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RAND Labor & Population

WR-1053

May 2014

This paper series made possible by the NIA funded RAND Center for the Study of Aging (P30AG012815) and the NICHD funded RAND Population Research Center (R24HD050906).

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Impact of Public Works on Household Occupational Choice: Evidence from NREGS in India

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July 2014

Abstract

I analyze the impact of India's public employment generation program (NREGS) on entrepreneurship. One of the main barriers to entrepreneurship in India is a lack of access to capital. My hypothesis is that NREGS allows liquidity constrained individuals to accumulate savings, enabling subsequent investment in a risky, but more profitable, venture, and ideally, permanent graduation from poverty. Taking advantage of the quasi-experimental nature of the program, I use a nationally representative data set to estimate the impact of NREGS on selection into entrepreneurship. I find that rates of non-agricultural entrepreneurship increase by 3 percentage points in NREGS districts (increasing rates from 15% to 18%), compared to areas that did not receive the program. This result is robust to various specifications, including two falsification tests. The results suggest that by acting as a source of credit, NREGS impacts household occupational choice, contributing to increased income, and ultimately promoting current and future family welfare.

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I thank Krishna Kumar, Peter Glick, Shanthi Nataraj, John Caloyeras, Christina Huang, Matthew Hoover, Sarah Kups, and participants in the Labor & Population Seminar at the RAND Corporation for valuable comments and suggestions. This work was supported by a Rosenfeld Dissertation Award. Any remaining errors are my own.

1 Introduction

Social safety net programs are viewed by governments as a powerful policy tool to support economically vulnerable groups. Increasingly, developing countries are relying on one type of social safety net, the employment generation program, as a way to enhance livelihood security for the poor, while also developing local infrastructure. Such programs have been successfully implemented in many middle and emerging economies, including Argentina, Ethiopia, India, and South Africa.¹ The goal is to provide households with increased income and food security in times when finding employment is difficult or to supplement insufficient incomes.

Whether employment generation programs can contribute to permanent reductions in poverty remains unclear. It is difficult to estimate the impact of these programs, as they are not randomized; without a comparable control group, a causal analysis cannot be conducted. In developed countries, there is evidence that public works programs are less successful in improving employment outcomes and fostering growth than other types of active labor market policies (Card et al., 2010; Kluge, 2010). The limited empirical evidence from programs in developing countries has focused on short-term outcomes, such as income, consumption, and participation in public employment (Ravi and Engler, 2012; Azam, 2012; Imbert and Papp, 2013), leaving unanswered the question of how employment generation programs affect growth in the long-run.

India's National Rural Employment Guarantee Scheme (NREGS) is the largest direct employment program in the world and has increased work opportunities for millions of the rural poor in India. Since its inception in 2006, the program has provided employment to nearly 50 million Indian households (about 36 percent of the rural labor force). The program continues to expand: NREGS participation rates more than doubled in the four years after implementation, and current program expenditures have risen to \$7.5 billion, approximately 1 percent of GDP. These costs cannot be maintained over an extended period. NREGS will be a sustainable and cost-effective policy only if it supports a graduation mechanism, enabling program participants to make a lasting change in their lives.

In this paper, I research how NREGS can result in sustained economic development and poverty alleviation, and in doing so, make three important contributions. First, I consider how NREGS

¹Devereux and Solomon (2006) provide a review of recent employment generation programs. See also Subbarao et al. (2013).

supports the transition to a new occupation by examining the impact of the program on a novel outcome: household entrepreneurship status. I propose that NREGS serves as a conduit for agents to overcome financial constraints and subsequently engage in entrepreneurial activities, fostering economic development. Banerjee and Newman (1993) show that with capital market imperfections, the initial wealth distribution and persistence of economic institutions determine whether an economy achieves prosperity (entrepreneurship) or stagnation (subsistence self-employment). The implication is that a one-time transfer, which changes the wealth distribution, can have permanent growth effects. NREGS may be viewed as such a finite transfer that can break the pattern of stagnation. Little is known about how active labor market policies can support entrepreneurship, apart from limited evidence on direct grants for self-employment in Europe (Carling and Gustafson, 1999).² Examining the impact on household occupation provides insight into one channel through which public works programs can provide long-term economic benefits to participants, thus informing policy in India and countries with similar programs.

Second, I adapt a model of household occupational choice (Evans and Jovanovic, 1989) to the context of a public works program, generating two testable predictions to support my empirical analysis. The first prediction is that a public works program will increase the extensive margin of entrepreneurship: the program allows households previously excluded from entrepreneurship to acquire the minimum level of capital necessary for a venture, thus increasing the share of households engaged in entrepreneurship. The second prediction is that a public works program will increase the intensive margin of entrepreneurship: after program implementation, households initially employing sub-optimal levels of capital are able to more intensively engage in the entrepreneurial venture, using income earned from the program to acquire the optimal level of capital. I then estimate the effect of NREGS on different measures of entrepreneurship, comparing Indian districts that received the program to those that did not. I exploit the timing of the program, implementing a differences-in-differences methodology, which allows me to control for time-invariant characteristics within districts that might also be correlated with entrepreneurship.

Third, I examine how NREGS differentially affects the rural non-farm and rural farm sectors. To date, there has been limited research on how government policies can promote rural non-farm

²Gilligan et al. (2008) find that participation in Ethiopia's social safety net programs is associated with increased household business activities. However, this study analyzes the impact of the employment generation program PSNP and other food security programs jointly. My research considers the effect of a public works program singularly.

entrepreneurship and current evaluations of NREGS do not analyze the rural non-farm sector separately. The Indian rural non-farm sector has grown steadily over the last thirty years (Coppard, 2001; Himanshu et al., 2011), while the agricultural sector has been shrinking.³ Within the rural non-farm sector, entrepreneurship has been the most dynamic source of income growth, driven by the expansion of productive household activities, rather than agrarian distress (Binswanger-Mikhize, 2012). However, there is evidence that a significant fraction of the rural population is precluded from engaging in such entrepreneurial opportunities. The main barrier to entry is a lack of access to credit (Coppard, 2001). Households are forced to use their own land as capital to finance entrepreneurial ventures (Lanjouw and Murgai, 2008), and households without sufficient assets are effectively barred from engaging in entrepreneurship altogether. Thus, for researchers and policymakers, understanding how government policy can support the expansion of the productive entrepreneurial rural non-farm sector is crucial, as it has played an important role in rural development and poverty reduction. My work analyzing the impact of NREGS on entrepreneurship attempts to shed light on this issue.

My results are consistent with the theoretical predictions. In my analysis of the impact of India's rural public works policy, NREGS, on entrepreneurship, I find that the program differentially affects rural non-farm and rural farm entrepreneurs. NREGS positively impacts rates of extensive rural non-farm entrepreneurship, resulting in a three percentage point increase (from 15 percent to 18 percent). Further, these results are robust to two separate falsification tests, indicating that the program does increase the extensive margin. The impact of NREGS on the intensive margin (measured by the share of household members engaged in entrepreneurship and the time spent on the main entrepreneurial activity) is less stark; I do not find a significant effect. The results provide some evidence that liquidity constrained households previously excluded from entering rural non-farm entrepreneurship are able to use NREGS as a source of credit to acquire the necessary capital for their entrepreneurial venture. In contrast, NREGS has little or no impact on rural farm entrepreneurship. Workers in this sector are often subsistence entrepreneurs who may be using NREGS to leave self-employment, substituting towards the better employment opportunities offered by the program.

³See Visaria and Basant (1994), Fisher et al. (1997), Dev and Ravi (2007), Himanshu (2007), Lanjouw and Murgai (2008), Abraham (2011), and Binswanger-Mikhize (2012) for detailed explanations on the historical and economic reasons behind this trend.

The paper proceeds as follows. Section 2 provides a description of the National Rural Employment Guarantee Scheme. In Section 3, I discuss the theoretical framework underlying the econometric approach of Section 4. Section 5 describes the data. In Section 6, I discuss the main findings, and extensions to and robustness of the baseline results. Section 7 concludes.

2 National Rural Employment Guarantee Scheme

The National Rural Employment Guarantee Scheme (NREGS) was implemented in 2006, after the passage of the National Rural Employment Guarantee Act (NREGA) in 2005.⁴ As the name suggests, NREGS is implemented only in rural areas. The program was rolled out in phases across rural India (see Figure 1). In 2006 (Phase 1), NREGS was implemented in the 200 least developed districts.⁵ In 2007 (Phase 2), the program was implemented in another 130 districts. The remaining 285 districts received the program in 2008-2009 (Phase 3).

Any rural household can opt in to the program. Each enrolled household is guaranteed a maximum of 100 days of labor, which may be allocated across the adult household members in any fashion, at any time of the year. Wages are paid according to Minimum Wage Act of 1948, and are not less than 60 rupees (\$1.12) per day, with equal wages for men and women.⁶ The program focuses on projects that improve agricultural infrastructure and productivity, such as water conservation and water harvesting, drought proofing, land development, flood control and protection, and rural connectivity (NREGA, 2005). As with other employment generation schemes, NREGS is a demand-driven program: work is provided to those who ask (enroll).

The high enrollment rates and vast geographic scale of NREGS make it the largest employment generation program in the world (Ravi and Engler, 2012). In the first year of NREGS (2006-2007), 21 million households (about 15 percent of the rural labor force) received employment, totaling 905 million person days of work, equivalent to 1.0% of government expenditure, about \$2.5 billion (NREGS website).⁷ In the most recent year of the program (2011-2012), nearly 50 million

⁴The Ministry of Rural Development (MRD) in India renamed NREGA/NREGS to the Mahatma Gandhi National Rural Employment Act/Scheme in 2009. I continue to refer to the act and scheme as NREGA and NREGS, respectively.

⁵A district is an administrative unit in India, similar to a U.S. county.

⁶Recently, NREGS increased the employment ceiling to 150 days per year and raised the daily minimum wage to 100 rupees (\$1.50). However, these changes took effect after the period I am studying, 2004-2007.

⁷Author's calculation based on current dollar-rupee exchange rates, using NREGS outlay data obtained from NREGS website (accessed August 27, 2013), 2001 Indian Census data, and Indian GDP data obtained from the World

households (about 36 percent of the rural labor force) received employment, totaling more than 2.1 billion person days of work, equivalent to 3.1% of government expenditure, about \$7.5 billion (NREGS website). This rapid growth rate raises questions about the sustainability and cost-effectiveness of NREGS, which I address in this paper by examining the impact on one possible path of program graduation: household entrepreneurship.

As NREGS was not randomized, but was implemented in high need areas first, this will impact my empirical approach. I exploit the quasi-experimental nature of the program: the timing of the NREGS rollout allows me to consider early (Phase 1/2) districts as the treated group, and late (Phase 3) districts as the untreated. To account for time invariant characteristics across early and late districts that may affect the outcome, I include district fixed effects in my econometric specifications. However, I cannot control for household level selection into the program. To address this issue, I use the survey household weights to generate aggregated district rates of entrepreneurship, and estimate an intent-to-treat effect. Table 1 shows there are pre-program differences across treatment and untreated districts (the groups are not balanced). This is to be expected given that NREGS districts were selected due to their poorer economic outcomes in 2004. I detail my econometric approach to address the differences in treatment and untreated groups in Section 4.

3 A Model of Household Occupational Choice

In this section, I discuss the analytical framework underlying my empirical approach. I adapt the Evans and Jovanovic (1989) model of household occupational choice under liquidity constraints to the context of a public works program. Consider a household prior to the implementation of a workfare scheme. The household has the option of engaging in a riskless employment option, with a known wage rate, resulting in a fixed income. Alternatively, the household could engage in a (risky) entrepreneurial activity, using an exogenous endowment to procure the required capital. There is no borrowing or lending in this economy, so a household's initial wealth is the only way to obtain capital for the entrepreneurial activity. This assumption reflects the fact that rural households in India, the population covered by NREGS, have very limited access to formal credit institutions. Informal institutions often charge exorbitant interest rates, which further

Bank website (accessed August 27, 2013).

excludes rural agents from the credit market (Arya, 2011; Bhattacharjee and Rajeev, 2010; Mahajan and Ramola, 1996; Hoff and Stiglitz, 1990). The profits from such an activity are directly related to the household's ability level (which is known and exogenous). Thus there will be households that become entrepreneurs and those that do not. Among those that do not become entrepreneurs, some will choose not to do so because of low ability; it is not profitable for these households to engage in the entrepreneurial activity. However, other households will be precluded from engaging in entrepreneurship though it is profitable, because they cannot afford the required capital; their initial wealth level is too low and so their liquidity constraint is binding.

With the onset of the workfare program, it is the households restricted from entrepreneurship that are of most interest. Anyone who wants a job in public works will obtain one (I assume there is no competition for work under the program), but there is a cap on the total income a household can earn through the workfare program each year. Thus previously constrained households now have the ability to earn additional income. They will use the program to earn enough to buy the capital required for the entrepreneurial activity, and overcome their financial constraint. Here the workfare program acts as a credit institution where instead of requiring interest for the loan, it requires work.

3.1 Household Decision

Households are endowed with wealth ω and ability level θ . There are two employment options. The first is to become a wage household, earning a fixed wage w . The second is to become an entrepreneurial household, with profit from the venture given by the (convex) function:

$$y = \theta k^\alpha, \alpha \in [0, 1) \tag{1}$$

Thus households with greater ability yield a higher total (and marginal) product, for all levels of capital. The minimum level of capital required to enter into entrepreneurship is \underline{k} . The household must decide whether to engage in the entrepreneurial activity or the wage earning option. All households are risk neutral.

In order to choose an occupation, the household must first determine its income under the entrepreneurial activity. It will solve for the profit maximizing level of capital, k^* . The price of capital

is normalized to one. Households purchase capital with their initial endowment. Households do not have access to credit markets, and there is no borrowing or lending between households. Thus a household can purchase a maximum level of capital equal to ω . The household solves:

$$\max_k \theta k^\alpha - k$$

$$FOC \Rightarrow k^* = (\theta\alpha)^{\frac{1}{1-\alpha}} \quad (2)$$

Where k^* is a function of household ability.

3.2 Implications of Liquidity Constraints

Case 0: Wage households

$$w > \theta k^\alpha - k, \forall k \quad (3)$$

Households facing Condition 3 will never choose entrepreneurship because the outside employment option is more attractive, regardless of the level of investment in entrepreneurship (these households have very low ability).

Case 1: Excluded households

$$w \leq \theta \underline{k}^\alpha - \underline{k}, \omega < \underline{k} \quad (4)$$

While these households would be better off by engaging in entrepreneurship, their wealth level is too low to procure even the minimum capital requirement, \underline{k} . As such, they are excluded from entrepreneurship and are forced to resort to wage employment.

Case 2: Constrained households

$$w \leq \theta \hat{k}^\alpha - \hat{k}, \underline{k} \leq \omega < k^* \quad (5)$$

These households have enough wealth to overcome the minimum capital requirement, but are constrained from acquiring the optimal level of capital k^* . In employing the sub-optimal level of capital \hat{k} , constrained households are unable to achieve the profit level possible if they were unconstrained.

Case 3: Unconstrained households

$$w \leq \theta k^* \alpha - k^*, k^* \geq \omega \quad (6)$$

Households facing Condition 6 are unconstrained: they are better off by engaging in entrepreneurship and they have enough wealth to acquire the optimal level of capital for their venture.

3.3 Implications of the Workfare Program

After implementation of a workfare program, households have an additional source of income. They can earn a maximum of $N > w$ through the program, and may invest λN in the entrepreneurial activity, where $\lambda \in [0, 1]$. This yields three predictions of the model.

Prediction 1: Wage work and unconstrained entrepreneurship unaffected

Wage households (Case 0) and unconstrained entrepreneurial households (Case 3) will not be affected by the implementation of a workfare program. For the former group, the program only serves to increase the value of the outside option compared to entrepreneurship, thus reaffirming the decision of a low ability household to choose wage employment. Households in the latter group were not bound by the liquidity constraint prior to program implementation, so the option to earn additional income does not affect their investment and occupational decisions.

Prediction 2: Extensive entrepreneurship increases

Households previously excluded from entrepreneurship (Case 1) now, under the workfare program, may face:

$$(1 - \lambda)N \leq \theta \underline{k}^\alpha - \underline{k}, \underline{k} \leq \omega + \lambda N \quad (7)$$

Excluded households are now able to purchase the minimum level of capital and engage in entrepreneurship. Thus the program will increase rates of entrepreneurship, or the extensive margin.

Prediction 3: Intensive entrepreneurship increases

Households constrained from purchasing k^* (Case 2) will now be able to invest at the optimal level, subject to total wealth (the initial endowment coupled with the workfare income) being

greater than the cost of k^* .

$$(1 - \lambda)N \leq \theta \hat{k}^\alpha - \hat{k}, \hat{k} \in [\underline{k}, \min \{k^*, \omega + \lambda N\}] \quad (8)$$

Thus entrepreneurial households previously employing sub-optimal levels of capital will now be able to more actively engage in their venture; the workfare program extends the intensive margin.

3.4 Transformative versus Subsistence Entrepreneurship

The discussion thus far rests on the assumption that entrepreneurship, for able households, is always a better option than wage work, implicitly focusing on the impact of a workfare program on transformational entrepreneurs, who drive economic growth (Schoar, 2009). It is possible that for households engaging in subsistence self-employment, the program might have a different impact on the supply of their entrepreneurial services. For example, individuals who engage in entrepreneurial activities because permanent employment opportunities are not available may substitute away from self-employment upon onset of the workfare program due to improved job prospects. This possibility does not negate the current hypothesis that workfare programs may act as a type of credit institution. Rather, the regression estimates will give the net change in entrepreneurship after onset of the program. The sign of the estimates will indicate whether the program promotes transformational entrepreneurship by acting as a source of credit over stable employment for subsistence entrepreneurs. I hypothesize that the workfare program will promote transformational entrepreneurship and will reduce subsistence entrepreneurship. This translates directly into my econometric specification, where I estimate the impact of NREGS on total rural entrepreneurship, and then separately on rural non-farm (transformational) and farm (subsistence) entrepreneurship.

4 Empirical Strategy

I take advantage of the timing of the NREGS phases and survey administration, employing differences-in-differences (DID) to estimate the impact of NREGS on rates of entrepreneurship. Treated districts include NREGS Phases 1 and 2, where NREGS was in operation by 2007. NREGS

Phase 3 districts comprise the untreated group, which received the program only after administration of the national survey was completed in 2008.

The baseline specification is given in Equation 9. To account for seasonality, I conduct the regressions at the quarter-district level. Households are randomly assigned to each quarter (by the survey). I account for the correlation of ϵ_{qdt} over time within each district by clustering the standard errors at the district level.

$$\ln(y_{qdt}) = \beta \text{treatment}_d * \text{post}_t + \delta \text{post}_t + \gamma X_{dt} + \theta Z_d * \text{post}_t + \eta_d + \tau_q + \epsilon_{qdt} \quad (9)$$

Where outcome y_{qdt} is a measure of entrepreneurship in quarter q , district d , period t . I first consider extensive entrepreneurship, which is defined as rates of total rural entrepreneurship, rural non-farm entrepreneurship, and rural farm entrepreneurship. The rates are calculated, using the survey weights, as the share of households engaged in (a particular category of) entrepreneurship, relative to the total labor force. I then consider two measures of intensive entrepreneurship. Measure 1 is the average share of working age (15-60) household members participating in the entrepreneurial activity, in each category (total, non-farm, and farm). Measure 2 is similarly defined, but considers the share of time working age household members spend in the main entrepreneurial activity. I analyze non-farm and farm entrepreneurs separately since, as noted above, they may engage in entrepreneurship for different reasons, and so are likely to differ in their response to the program.

The treatment_d indicator is equal to 1 if district d is in the treatment group (early districts, NREGS Phases 1 or 2); the post_t indicator is equal to 1 in the post-2007 period. The coefficient of interest is β , which measures the intent-to-treat effect of NREGS on entrepreneurship.

With the inclusion of district fixed effects η_d , Equation 9 compares rates of entrepreneurship within districts before and after program implementation to eliminate any time invariant characteristics that might also affect the outcome. This is crucial since NREGS was not randomly implemented; the district fixed effects address selection issues in treated versus untreated districts. The specification also controls for any national level trends in a particular year (with the post_t indicator) that could drive entrepreneurship by comparing rates of entrepreneurship across districts within each year. To account for seasonality, I include quarter fixed effects (τ_q).

One criticism of this approach is that differential pre-program trends across NREGS and non-NREGS districts might drive the results. Treated areas were less economically developed than untreated areas. In Table 2, I compare the pre-program trends across the treated and untreated groups for various demographic characteristics; there are significant differences in these trends. If these trends are also related to entrepreneurship, then my estimates of the treatment effect will be biased. I address this issue in multiple ways. First, I include time-varying district characteristics, given by X_{dt} in Equation 9. Second, I interact a vector of pre-program district level characteristics (Z_d in Equation 9) with the post period indicator. In addition, one might be also concerned with regression to the mean, implying that any specifications detecting a relationship between program status and entrepreneurship are actually spurious. I address this by including the pre-program trend in entrepreneurship in my regression specification as a robustness test. Finally, I run two falsification tests to ensure the results are not driven by factors unrelated to the implementation of NREGS.

5 Data

I incorporate data from a variety of sources. The outcome data, measures of entrepreneurship, come from the Employment and Unemployment Schedule of the National Sample Survey (NSS), which is administered by the National Sample Survey Office (NSSO) of India. The survey is administered throughout India and is nationally representative. It is conducted annually, with a different topic every year. The quinquennial surveys (conducted every five years) are the largest, collecting data on employment/unemployment, consumer expenditure, and demographics from approximately 125,000 households and 600,000 individuals (usually split 65% rural, 35% urban). However, in 2007-2008 the NSSO conducted a larger Employment and Migration survey (64th round), which was similar in size and scope to the quinquennial surveys.

I exploit the timing of the NSS survey rounds and the phased rollout of NREGS to analyze the impact of the program on entrepreneurship. Round 61 was administered in 2004-2005, prior to the start of NREGS, and so serves as the pre-program baseline period. Phases 1 and 2 of NREGS were rolled out in February 2006 and January 2007, respectively. NSS Round 64 was administered in 2007, prior to NREGS Phase 3, which began in April 2008. Thus Round 64 demarcates the post-

program period. Figure 2 depicts the timing of the NSS rounds, relative to the phases of NREGS.

I combine NSS Rounds 61 and 64, aggregating household data to create a district-level panel. I matched districts using borders from the 2001 Indian Census borders, to track consistent geographic units over time. The panel was then matched with the NREGS rollout data, to identify districts that received the program by 2007, and those that did not. Phase 1 and 2 (early) districts combined represent the treated group; Phase 3 (late) districts comprise the untreated group. Details of the data management and variable construction are provided in the Appendix.

District and state covariates come from the 2001 Census, India's statistical agency (Ministry of Statistics and Programme Implementation, MOSPI), and the Indian Planning Commission. The Census provides demographic data, poverty data, labor force characteristics, and rural development measures. From MOSPI, I gather state-level GDP data. In 2003, the Planning Commission developed a Development Index, identifying the 200 least developed districts in India according to various criteria. The Ministry of Rural Development incorporated this ranking when deciding which districts would receive NREGS first. To control for the non-random implementation of NREGS, I include the components of the Development Index in my regressions.

6 Results & Discussion

6.1 Impact of NREGS on Extensive Entrepreneurship

The results of the baseline regression (Equation 9) estimating the impact of NREGS on extensive entrepreneurship are provided in Table 3, for rates of total rural (column 1), rural non-farm (column 2), and rural farm (column 3) entrepreneurship. This specification includes only time and district fixed effects. Tables 4, 5, and 6 extend the baseline regressions, presenting results for different specifications, in which groups of controls are additively included. In Tables 3-6, the outcome is defined as the share of households engaged in entrepreneurship relative to the total labor force, in a given quarter, district, and period. In all specifications, the standard errors are clustered at the district level.

The baseline results in Table 3 provide evidence consistent with the hypotheses presented in Section 3: NREGS increases rates of extensive entrepreneurship (prediction 2), but in a specific sector (likely promoting transformative ventures over subsistence activities). To see this, first con-

sider column 1, which provides results for the impact of NREGS on rates of total entrepreneurship. The difference-in-difference estimate (-0.02) is small in magnitude and statistically insignificant. However, columns 2 and 3 show the importance of disaggregating into the non-farm and farm sectors. Farm entrepreneurs outnumber non-farm entrepreneurs nearly 3 to 1, and the results in column 1 (total entrepreneurship) reflect this composition. However, it is clear that the impact of the program on non-farm entrepreneurship differs greatly from the impact on farm entrepreneurship. The coefficient for the impact on non-farm entrepreneurs (column 2) is 0.13; NREGS is promoting ventures (likely transformative) in this sector. Further the results indicate that NREGS may pull farm entrepreneurs away from their subsistence activities into the workfare program, as the coefficient in column 3 is negative (-0.02), though it is not statistically significant.

The results in Tables 4 (total entrepreneurship), 5 (non-farm entrepreneurship), and 6 (farm entrepreneurship) are consistent with the baseline results of Table 3. There is no impact of NREGS on total entrepreneurship, consistent with the results in Table 3. Column 1 of Table 5 shows that for the rural non-farm entrepreneurial population, the baseline treatment effect (0.13) is likely an underestimate, when compared to the results in columns 2-5 of Table 5, which control for a range of time-varying and time-invariant district characteristics. The DID estimates increase to 16-18% in columns 2-5, consistent across these specifications. Prior to the program implementation (in 2004), baseline levels of non-farm entrepreneurship were 15%; the program has a net effect of 3 percentage points, increasing the rate to 18%. For farm entrepreneurs (Table 6), the estimated effect is also consistent across the various specifications, at around -3 to -3.5%, but is not significant.

Thus there is clear evidence that NREGS increases rates of rural non-farm entrepreneurship by approximately 3 percentage points. The estimated increase in extensive rural non-farm entrepreneurship is consistent with the prediction that NREGS may relax credit constraints for excluded households, allowing them to invest income earned from the program into their venture. The results follow recent empirical trends as well. The rural non-farm sector in India has been expanding steadily over the last 5-10 years, with a range of high-paying jobs, while proper, salaried employment in agriculture has been shrinking. Further, casual and self-employment opportunities in agriculture are not well-paying nor are they consistently available (Lanjouw and Murgai, 2008). In this context, we would expect to see a negative impact of NREGS on rural farm self-employment, as the rural farm self-employed substitute away from subsistence work in favor of

higher paying employment opportunities offered by the program. While the estimated impact of NREGS on farm entrepreneurship is negative, it is not statistically significant.

6.2 Impact of NREGS on Intensive Entrepreneurship

While there is evidence that NREGS increases the extensive margin of entrepreneurship, the results in Tables 7 and 8 indicate that there is little impact on the intensive margin of entrepreneurship. I first consider if the program affects the share of working age household members engaged in entrepreneurship (Table 7). I also estimate the impact on the share of time working age household members spend on the main entrepreneurial activity (Table 8). In considering measures of intensive entrepreneurship, I analyze whether NREGS can also promote entrepreneurship for households engaged in a venture, but not at an optimal level, prior to program implementation (prediction 3).

From the results in Tables 7 and 8, we see that NREGS does not increase the intensity of the entrepreneurial activity within the household when considering the total entrepreneurial population (Panel A). The coefficient is small and not statistically significant. This result holds under disaggregation into the non-farm and farm sectors (Panels B and C). Although these results differ from the model prediction, they do provide useful insight into the behavior of entrepreneurial households and their response to a workfare program. In particular, the empirical evidence points to the possibility that households involved in transformative entrepreneurship in the non-farm sector prior to program implementation may already be operating at or near optimal levels of capital. As a result, these households do not increase the intensity of entrepreneurship post-NREGS.

It is somewhat surprising that farm entrepreneurs do not exhibit a decrease in the intensity of the entrepreneurial activity, as evidenced by Panel C of Tables 7 and 8. The results from Table 6 indicate that households in farm entrepreneurship do not use NREGS as a complete substitute for their subsistence activities. However, it seems likely that these households could use NREGS as a partial substitute, which would affect the intra-household allocation towards entrepreneurship. This lack of evidence on partial substitution may indicate that even with access to NREGS, households in this sector must still engage in a subsistence activity to achieve a minimum level of income.

Combined, the results in Tables 4-8 provide evidence that NREGS may relax credit constraints, allowing previously excluded households to now afford the minimum capital requirement, thus facilitating a discrete jump into entrepreneurship in the non-farm sector. There is little evidence that the program supports existing entrepreneurial households in acquiring more capital, as there is no increase in the intensity with which households engage in entrepreneurship. This may reflect the fact that the minimum level of capital is relatively high compared to average wealth in this sector and that, in many ventures, this minimum level is very close to the optimal level. Thus we see increases along the extensive margin and not the intensive margin.

6.3 Robustness Tests

Given that the evidence is strongest in Tables 4-6, which reflect the impact of NREGS on extensive entrepreneurship, I subject these results to a variety of robustness tests. To determine if the effect estimated in Equation 9 is truly due to NREGS and does not result from differing trends in entrepreneurship across treatment and control groups prior to the program, I conduct a falsification test. The main assumption of the differences-in-differences approach is that, in the absence of treatment, rates of entrepreneurship in the treatment and control groups would have continued along their same relative trajectories. While I cannot observe this counterfactual, I can test for a program effect during years in which NREGS was not implemented. Thus I estimate Equation 9 using data from the two NSS rounds prior to NREGS rollout: Round 55 in 1999-2000 and Round 61 in 2004-2005. I compare the same treatment and control groups as in Equation 9, but there is no program in effect. If the false treatment has no effect on the outcomes of interest, this provides additional evidence that the original estimates are indeed causal effects of NREGS.

The nature of NREGS itself permits another falsification test. Since the program was offered only in rural areas, there should not be any treatment effect in urban parts of the country unless a national secular trend was driving rates of entrepreneurship at the same time NREGS was implemented. I re-estimate Equation 9, comparing the original treated and untreated districts, but restricting the sample to urban areas. Once again, if the false treatment has no effect, this is further proof that NREGS is responsible for the increased rates of entrepreneurship.⁸

⁸In this falsification test, I consider only the non-farm entrepreneurial sector, as there is no farm sector in urban areas.

The results from the falsification tests (Tables 9 and 10) indicate that the effect of NREGS on rural non-farm rates of entrepreneurship can be attributed to the program and not to other trends. In Table 9, the difference-in-difference coefficient (β) is not significant, indicating that the results in Table 5 are due to the implementation of NREGS, and not differential trends in the treated and untreated districts. The urban falsification test in Table 10 corroborates this result: there is no evidence of an impact of NREGS in urban areas, where the program was not implemented.

In the pre-program falsification test (Table 9) I exclude some controls from the regression. These controls are from the 2001 Indian Census and were interacted with a post-NREGS indicator to capture district trends that might also be correlated with rates of entrepreneurship during the program period, 2004-2007. In Table 9, I consider the period 1999-2004, and so excluded controls from 2001. However, I conduct the falsification test for all entrepreneurial, non-farm entrepreneurial, and farm entrepreneurial households with the entire set of controls (from 2001) and find that the results are the same.⁹ There is no effect of NREGS on entrepreneurship outside of the program era.

The magnitude of the DID estimates (5-8%) in Table 9 indicates there may be upward bias in my estimate of the effect of NREGS on rural non-farm entrepreneurship. I include the pre-program trend in entrepreneurship (interacted with the post-period indicator) in my specification as a robustness check; the results are presented in Table 11. Qualitatively, the results remain the same as in Tables 4, 5, and 6; the effect of NREGS on rural non-farm entrepreneurship now increases to more than 18%.

It is worth noting that the coefficient on the entrepreneurship trend in Table 11 is negative and significant. This indicates possible reversion to the mean: districts that had particularly high positive trends in entrepreneurship are now falling back to the national average. Historically, however, rates of rural self-employment in India have been increasing, especially since the mid-1990s (Lanjouw and Murgai, 2008; NSSO, 2010). In 2004, the rate was quite high, above the value the trend from earlier years would have predicted. It is likely that the abnormally high rate of entrepreneurship in the pre-period (2004) may be manifesting as mean reversion within the current specification. To investigate this possibility, I estimate Equation 9, on extensive entrepreneurship, but now using 1999 as the pre-program period (instead of 2004). The results in Table 12 are consistent with

⁹These results are available upon request.

the results in Tables 4-6. In fact, the estimated impact on rates of rural non-farm entrepreneurship (column 2) more than doubles (to 0.36), although the coefficient on the post-period indicator is negative and significant.

6.4 Extension: Dosage Response

The original specification given in Equation 9 groups NREGS Phase 1 and 2 districts together, comparing them to Phase 3 districts, which had not received the program in 2007, when NSS Round 64 was administered. However, by 2007, Phase 1 districts had been exposed to NREGS for more than a year longer than Phase 2 districts. To allow for a dosage response, that the impact of NREGS on entrepreneurship may be related to the length of time the program had been in effect, I modify Equation 9 as follows:

$$\ln(y_{qdt}) = \beta_1 \text{Phase}_1 * \text{post}_t + \beta_2 \text{Phase}_2 * \text{post}_t + \delta \text{post}_t + \gamma X_{dt} + \theta Z_d * \text{post}_t + \eta_d + \tau_q + \epsilon_{qdt} \quad (10)$$

Differences in β_1 and β_2 will indicate the extent to which the impact of NREGS on rates of entrepreneurship is related to the length of exposure to the program.

Table 13 presents the results for the differential effect, or dosage response, of NREGS on Phase 1 and Phase 2 districts. The results are similar to the cumulative effect of NREGS presented in Tables 4, 5, and 6, but now the contribution of each phase to increases in entrepreneurship is apparent. For the rural non-farm population (column 2), Phase 1 increases the rate of extensive entrepreneurship by 10% (1.5 percentage points) and Phase 2 results in about a 20% increase in the rate of entrepreneurship, although insufficient power of the regression prevents detecting statistical significance for the first phase. The lack of power is evident in the F-test, which indicates that the Phase 1 and Phase 2 coefficients are not statistically different. Similar to the baseline results, there is no effect of the program on the total rate of entrepreneurship or rates of rural farm entrepreneurship (columns 1 and 3).

7 Conclusion

I utilize a nationally representative dataset to analyze the impact of India's rural public works policy, NREGS, on entrepreneurship. I find that the program differentially affects rural non-farm and rural farm entrepreneurs. NREGS positively impacts rates of extensive rural non-farm entrepreneurship, resulting in a three percentage point increase. The rural non-farm sector, which includes transformational entrepreneurial activities, has been expanding rapidly in India. The results presented provide some evidence that liquidity constrained households that had been excluded from entering rural non-farm entrepreneurship are able to use NREGS as a source of credit to acquire the necessary capital for their entrepreneurial venture. In contrast, NREGS has little or no impact on rural farm entrepreneurship. Workers in this sector are often subsistence entrepreneurs who may be using NREGS to leave self-employment, substituting towards the better employment opportunities offered by the program.

Over the period 1983-2004, prior to NREGS, the annualized growth rate of employment in the rural non-farm sector was 4.0% each year (Himanshu et al., 2011). From my results, I estimate that in NREGS districts, the size of the rural non-farm self-employed workforce grew by 6.2% every year, more than a fifty percent increase over the previous two decades. Effects of this magnitude could translate into considerable wealth gains, pulling individuals out of poverty, even if the baseline share of rural non-farm self-employment in total GDP is low. Further, there may be intergenerational effects: children in households that have graduated from poverty are less likely to be poor themselves. In addition, the implications for the sustainability of NREGS are significant. The estimates indicate that the net number of rural non-farm entrepreneurial households has increased. To the extent that this population reduces its dependency on NREGS and contributes to the Indian economy, the per capita cost of NREGS will decrease, making the program more economically viable. Thus analyzing this issue has important consequences for the program itself and economic development in India.

In ongoing work, I am working to identify sectors that have experienced significant expansion or contraction after the implementation of NREGS. Doing so will clarify that NREGS is promoting transformational entrepreneurship and reducing subsistence activities. I am also utilizing data sources that provide information on the incomes of the self-employed (NSS does not have

this information) to estimate the welfare increase for rural non-farm entrepreneurs post-NREGS. Combining income data with pre-program asset information would allow me to verify that the capital constrained are indeed benefiting most from NREGS. I can further corroborate my results with additional robustness tests, such as controlling for states in which NREGS was particularly well implemented (i.e. “star” states identified by Dreze and Khera, 2009). Finally, I am working to estimate the effect of NREGS nonparametrically, similar to the method proposed by Athey and Imbens (2006).

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Data Appendix

To create a district-level panel using NSS Rounds 61 and 64 that tracked geographically consistent units over time and also contained NREGS phase data, I converted all district borders to match that of the 2001 Census. In 2001, India had 593 districts. NSS Rounds 61 and 64 used 2001 Census district borders, sampling from 585 and 588 districts, respectively (inaccessible areas in Jammu and Kashmir, Nagaland, and Andaman and Nicobar Islands were not surveyed). I started with the 585 districts common to NSS Rounds 61 and 64. When rolling out the program, NREGS used current district borders, which differed from that of the 2001 Census because of newly created districts. Between the 2001 and 2011 Censuses, 47 districts were created, by splitting existing districts or combining areas across districts. I converted the NREGS districts to 2001 Census borders to match the NSS districts. I dropped 31 districts where the new district and the original district received NREGS in different phases. A further 11 districts where NREGS was not administered were dropped from the sample, leaving a core sample of 543 districts with consistent borders from 2004 to 2007.

I employed the same approach when preparing the data for the first falsification test, comparing NREGS areas and non-NREGS areas from 1999 (NSS Round 55) to 2004 (NSS Round 61), when the program was not implemented. In order to track consistent units, I converted Rounds 55 and 61 to 1991 Census borders. In 1991, India had 466 districts. Again dropping cases where a parent and child district received NREGS in different phases and dropping urban districts, I was left with a sample of 395 districts from 1999 to 2004.

The controls used in the regressions came from three sources: the 2001 Census, India's statistical agency (Ministry of Statistics and Programme Implementation, MOSPI), and the Indian Planning Commission. The District Profiles of the Census provided demographic data (average household size, proportion Scheduled Caste and Scheduled Tribe, literacy rate), poverty data (average household monthly consumption and expenditure), labor force characteristics (share in main employment, share in marginal employment, workforce participation rate), and rural development measures (share of households occupying permanent structures, share of villages with safe drinking water, share of villages with a primary school, share of villages with a medical facility) at the district level. I obtain state GDP and NDP data for 2000 from MOSPI. I also control for

the development ranking used by the Ministry of Development to decide which districts would receive NREGS first. This ranking came from an Indian Planning Commission study conducted in 2003, to identify the 200 least developed districts in India. The commission used three criteria in its ranking: share of the population that was Scheduled Caste/Tribe (from the 1991 Census), daily agricultural wages (from 1996-1997), and average agricultural productivity per worker (over 1990-1993).

Figure 1: Map of NREGS Phased rollout

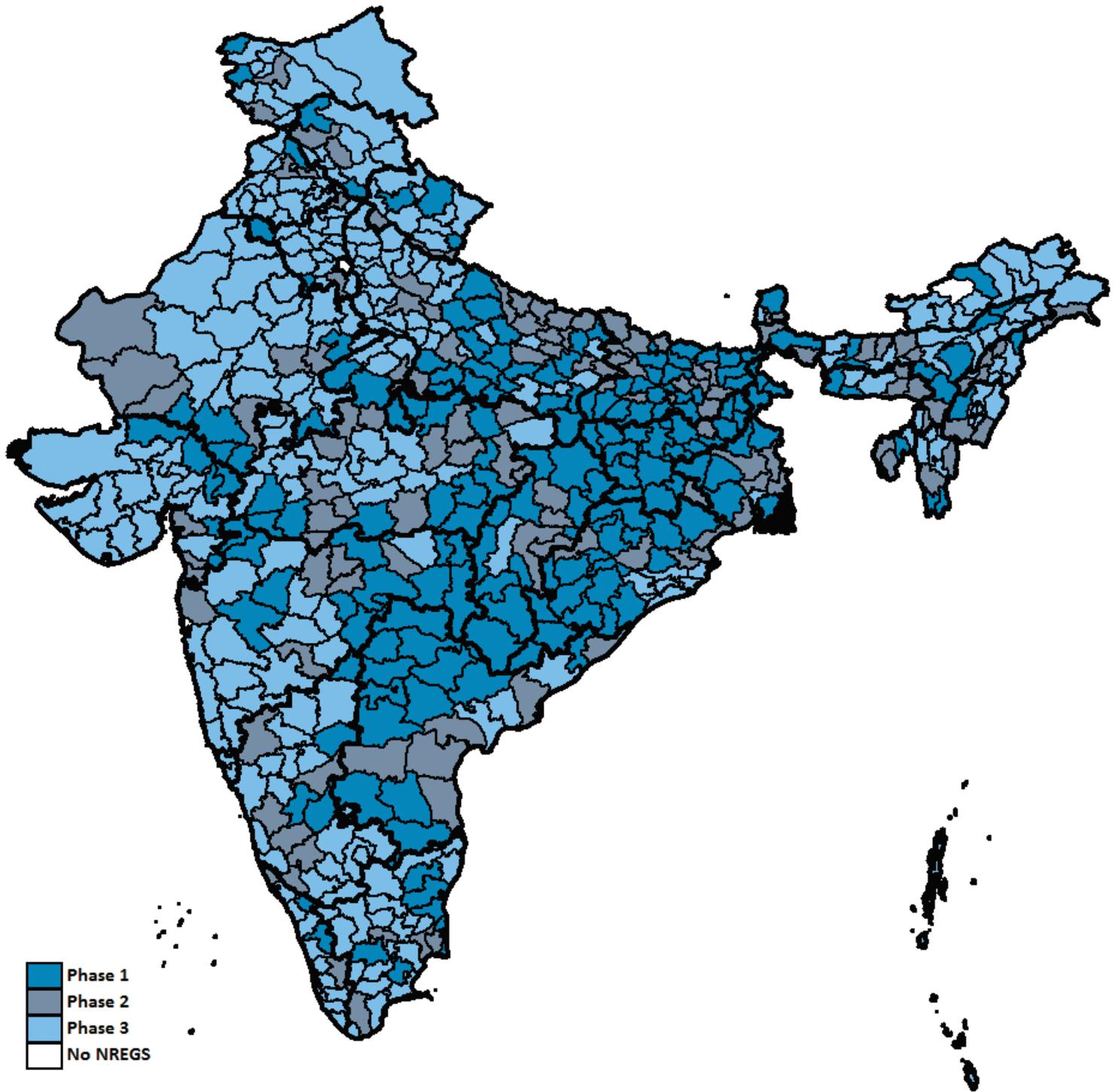


Figure 1 presents a map of the phased implementation of the National Rural Employment Guarantee Scheme (NREGS) in India. In 2006 (Phase 1, dark blue), NREGS was implemented in the 200 least developed districts. In 2007 (Phase 2, gray), the program was implemented in another 130 districts. The remaining 285 districts received the program in 2008 (Phase 3, light blue). Urban areas (white) did not receive the program.

Figure 2: Timeline of NSS Rounds and NREGS Rollout

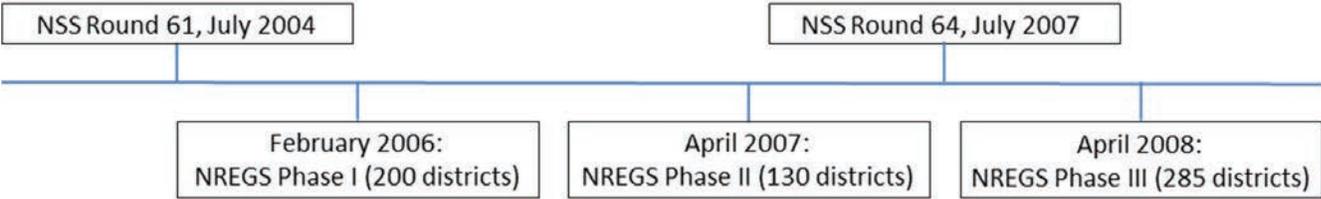


Figure 2 depicts the timing of the National Sample Survey (NSS) rounds, relative to the phases of NREGS. I exploit the timing of the NSS survey rounds and the phased rollout of NREGS to analyze the impact of the program on entrepreneurship.

Table 1: Treated vs. Untreated Districts in 2004, Pre-NREGS

	Treated	Untreated	p-value
Poverty and Demographics			
Avg HH size	5.43	5.42	0.937
Scheduled caste share	15.92	14.77	0.119
Scheduled tribe share	19.72	12.77	0.003
Literacy rate	59.43	69.10	0.000
Avg monthly household consumer exp	2,884	3,600	0.000
Land owned (hectares)	1.08	1.1	0.831
Labor Force Characteristics			
Main worker share	30.48	31.84	0.014
Marginal worker share	10.97	8.37	0.000
Workforce participation rate	41.39	40.22	0.053
Rural Development Measures			
Share of HH in permanent housing	38.79	57.55	0.000
Share of villages with safe water	96.57	96.33	0.794
Share of villages with primary school	80.97	85.42	0.001
Share of villages with medical facility	33.94	46.51	0.000
Share of villages with communication facility	41.29	60.71	0.000
Share of villages with bus services	35.70	57.24	0.000
State Expenditures			
Real GDP trend, 2000-2005	4.23	4.76	0.014
Real per capita NDP trend, 2000-2005	3.14	3.03	0.567
Development Index			
Agricultural wages, 1996-1997	33.89	47.72	0.000
Agricultural output per worker, 1990-1993	5,962	12,126	0.000
N (districts)	248	294	

Table 1 compares the treated and untreated districts in 2004, prior to NREGS implementation, for a variety of characteristics. There are pre-program differences, as Phase 1 and 2 districts were selected due to their poorer economic outcomes in 2004, e.g. treated districts were less developed and faced lower rates of workforce participation in 2004 (compared to untreated districts).

Notes: 1 hectare = 10,000 square meters.

Source: Author calculations using data from the National Sample Survey, 2001 Indian Census, Ministry of Statistics and Programme Implementation, and the Indian Planning Commission.

Table 2: Pre-NREGS Trends in Treated and Untreated Districts, 1999-2004

1999-2004 Change	Treatment	Untreated	p-value
Average HH size	-0.21	-0.22	0.799
Scheduled caste share	-.01	0.02	0.024
Scheduled tribe share	-0.01	0.01	0.062
Literacy rate	0.07	0.04	0.000
Average monthly HH expenditure	-73.84	-146.76	0.295
Average land holdings (hectares)	0.06	-0.08	0.182
N (districts)	205	184	

Table 2 compares the trends in various demographic characteristics across the treated and untreated districts over the 1999 to 2004 period, prior to NREGS implementation. There are significant differences in these trends; to the extent they are related to entrepreneurship, my estimates of the program effect will be biased. I address this issue by including time-varying district characteristics and interacting a vector of pre-program district characteristics with a post period indicator, in addition to implementing robustness tests (see Section 6.3).

Notes: 1 hectare = 10,000 square meters. Household expenditure adjusted to 2004 rupees.

Source: Author calculations using data from the National Sample Survey, 2001 Indian Census, Ministry of Statistics and Programme Implementation, and the Indian Planning Commission.

Table 3: Baseline: Effect of NREGS on Rates of Entrepreneurship

	Total	Non-farm	Farm
	(1)	(2)	(3)
NREGS (β)	-0.020 (0.031)	0.126* (0.066)	-0.024 (0.046)
Post indicator (δ)	-0.050** (0.022)	-0.240*** (0.048)	-0.051 (0.032)
Quarter fixed effects (τ_q)	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes
N	4,332	4,010	4,301

Table 3 provides baseline estimates of the impact of NREGS on rates of total (column 1), non-farm (column 2), and farm entrepreneurship (column 3), using a differences-in-differences model, which controls for time and district fixed effects. There is no impact of NREGS on total or farm entrepreneurship, but the program results in a 0.13% increase in rural non-farm entrepreneurship. This is consistent with the idea that the program increases entrepreneurship on the extensive margin in the transformative non-farm sector over the subsistence farm sector (Prediction 2 of Section 3).

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Effect of NREGS on Total Entrepreneurship

	Outcome: Total entrepreneurship				
	(1)	(2)	(3)	(4)	(5)
NREGS (β)	-0.020 (0.031)	0.007 (0.030)	-0.005 (0.033)	0.003 (0.033)	-0.004 (0.040)
Post indicator (δ)	-0.050** (0.022)	-0.142 (0.088)	-0.275 (0.171)	-0.222 (0.179)	-0.579 (0.677)
Poverty and demographics		yes	yes	yes	yes
Labor force characteristics		yes	yes	yes	yes
Rural development measures			yes	yes	yes
Real per capita NDP				yes	yes
Development index components					yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes
N	4,332	4,332	4,284	4,263	3,381

Table 4 estimates the impact of NREGS on rates of total (rural) entrepreneurship, using a differences-in-differences model. Column 1 presents the baseline results; columns 2-5 extend the baseline regressions, controlling for a range of district characteristics. There is no impact of NREGS on total entrepreneurship, consistent with the results in Table 3. Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effect of NREGS on Non-farm Entrepreneurship

	Outcome: Non-farm entrepreneurship				
	(1)	(2)	(3)	(4)	(5)
NREGS (β)	0.126*	0.176**	0.164**	0.161**	0.170*
	(0.066)	(0.067)	(0.072)	(0.073)	(0.089)
Post indicator (δ)	-0.240***	-0.324*	-0.216	-0.216	-2.467
	(0.048)	(0.196)	(0.543)	(0.545)	(1.971)
Poverty and demographics		yes	yes	yes	yes
Labor force characteristics		yes	yes	yes	yes
Rural development measures			yes	yes	yes
Real per capita NDP				yes	yes
Development index components					yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes
N	4,010	4,010	3,970	3,949	3,155

Table 5 estimates the impact of NREGS on rates of (rural) non-farm entrepreneurship, using a differences-in-differences model. Column 1 presents the baseline results; columns 2-5 extend the baseline regressions, controlling for a range of district characteristics. In line with Prediction 2 (of Section 3), NREGS increases extensive non-farm entrepreneurship by 18%, equivalent to about 3 percentage points, consistent with the results in Table 3.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 6: Effect of NREGS on Farm Entrepreneurship

	Outcome: Farm entrepreneurship				
	(1)	(2)	(3)	(4)	(5)
NREGS (β)	-0.024 (0.046)	-0.010 (0.045)	-0.036 (0.049)	-0.028 (0.049)	-0.036 (0.060)
Post indicator (δ)	-0.051 (0.032)	-0.138 (0.127)	-0.427* (0.250)	-0.348 (0.252)	-0.565 (1.000)
Poverty and demographics		yes	yes	yes	yes
Labor force characteristics		yes	yes	yes	yes
Rural development measures			yes	yes	yes
Real per capita NDP				yes	yes
Development index components					yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes
N	4,301	4,301	4,253	4,237	3,364

Table 6 estimates the impact of NREGS on rates of (rural) farm entrepreneurship, using a differences-in-differences model. Column 1 presents the baseline results; columns 2-5 extend the baseline regressions, controlling for a range of district characteristics. In all specifications, the impact of NREGS is negative, consistent with the hypothesis that farm entrepreneurs would substitute away from subsistence self-employment towards stable employment in the program, but these estimates are not significant.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7: Effect of NREGS on Share of Household Members in Entrepreneurship

	Panel A: Total		Panel B: Non-farm		Panel C: Farm	
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS (β)	-0.028 (0.024)	0.005 (0.029)	-0.035 (0.034)	-0.013 (0.043)	-0.024 (0.025)	0.007 (0.032)
Post indicator (δ)	-0.079*** (0.017)	-0.439 (0.448)	-0.091*** (0.026)	-0.837 (0.560)	-0.084*** (0.018)	-0.195 (0.455)
Poverty and demographics		yes		yes		yes
Labor force characteristics		yes		yes		yes
Rural development measures		yes		yes		yes
Real per capita NDP		yes		yes		yes
Development index components		yes		yes		yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes	yes
N	4,330	3,380	3,988	3,145	4,295	3,362

Table 7 estimates the impact of NREGS on the intensive margin of entrepreneurship, measured by the share of working age household members engaged in the entrepreneurial activity, using a differences-in-differences model. The results are not statistically significant for the combined sectors (Panel A), or when the sectors are disaggregated into non-farm (Panel B) and farm (Panel C) entrepreneurship.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Effect of NREGS on Time Spent in Main Entrepreneurial Activity

	Panel A: Total		Panel B: Non-farm		Panel C: Farm	
	(1)	(2)	(3)	(4)	(5)	(6)
NREGS (β)	-0.017 (0.020)	0.000 (0.023)	-0.034 0.029	-0.030 (0.041)	0.001 (0.025)	0.008 (0.029)
Post indicator (δ)	-0.041*** (0.016)	0.381* (0.206)	-0.039* (0.023)	0.241 (0.429)	-0.057*** (0.020)	0.509** (0.226)
Poverty and demographics		yes		yes		yes
Labor force characteristics		yes		yes		yes
Rural development measures		yes		yes		yes
Real per capita NDP		yes		yes		yes
Development index components		yes		yes		yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes	yes
N	4,327	3,380	3,967	3,129	4,276	3,352

Table 8 estimates the impact of NREGS on another measure of intensive entrepreneurship, the share of time working age household members spend engaged in the main entrepreneurial activity, using a differences-in-differences model. The results are not statistically significant for the combined sectors (Panel A), or when the sectors are disaggregated into non-farm (Panel B) and farm (Panel C) entrepreneurship. Combined with Table 7 the results indicate that NREGS has little impact on intensive entrepreneurship, differing from Prediction 3 of Section 3.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Pre-NREGS Falsification Test

	Total	Non-farm	Farm
	(1)	(2)	(3)
NREGS (β)	0.052 (0.040)	0.070 (0.080)	0.048 (0.054)
Post indicator (δ)	0.102 (0.064)	0.046 (0.128)	0.049 (0.091)
Poverty and demographics	yes	yes	yes
Labor force characteristics	no	no	no
Rural development measures	no	no	no
Real per capita NDP	no	no	no
Development index components	yes	yes	yes
Quarter fixed effects (τ_q)	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes
N	2,372	2,291	2,367

Table 9 presents the results from a falsification test: I estimate the impact of NREGS on rates of total, non-farm, and farm entrepreneurship in years when the program was not implemented, 1999-2004. There is no impact of NREGS on entrepreneurship outside of the program era. Note that some controls are excluded from the regression. These controls are from the 2001 Indian Census and were interacted with a post-NREGS indicator to capture district trends that might also be correlated with rates of entrepreneurship during the program period, 2004-2007. In Table 9, I consider the period 1999-2004, and so excluded controls from 2001. However, I have conducted the falsification test for all entrepreneurial, non-farm entrepreneurial, and farm entrepreneurial households with the entire set of controls (from 2001) and find that the results are the same.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 10: Urban Falsification Test

	Outcome: Non-farm entrepreneurship				
	(1)	(2)	(3)	(4)	(5)
NREGS (β)	0.019 (0.050)	0.013 (0.051)	-0.011 (0.059)	0.010 (0.060)	-0.010 (0.072)
Post indicator (δ)	-0.073** (0.035)	-0.134 (0.149)	-0.441 (0.362)	-0.309 (0.378)	0.765 (0.815)
Poverty and demographics		yes	yes	yes	yes
Labor force characteristics		yes	yes	yes	yes
Rural development measures			yes	yes	yes
Real per capita NDP				yes	yes
Development index components					yes
Quarter fixed effects (τ_q)	yes	yes	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes	yes	yes
N	4,046	4,046	4,008	3,894	3,235

Table 10 presents the results from a falsification test: I estimate the impact of NREGS on rates of non-farm entrepreneurship in urban areas, where the program was not implemented. I consider only the non-farm sector (as there is no farm sector in urban areas). The program has no effect in urban districts. Combined with the results from Table 9, it is clear NREGS has no effect on entrepreneurship in years or areas in which it was not implemented.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Controlling for Pre-program Trend in Entrepreneurship

	Total	Non-farm	Farm
	(1)	(2)	(3)
NREGS (β)	0.016 (0.037)	0.182* (0.097)	-0.021 (0.056)
Post indicator (δ)	0.372 (0.555)	-2.226 (1.856)	1.033 (1.000)
Entrepreneurship trend, 1999-2004	-0.293*** (0.107)	-0.340*** (0.098)	-0.283* (0.157)
Poverty and demographics	yes	yes	yes
Labor force characteristics	yes	yes	yes
Rural development measures	yes	yes	yes
Real per capita NDP	yes	yes	yes
Development index components	yes	yes	yes
Quarter fixed effects (τ_q)	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes
N	2,381	2,278	2,376

Table 11 presents the results from a robustness check: I estimate the impact of NREGS on rates of total, non-farm, and farm entrepreneurship while controlling for pre-program (1999-2004) trends in entrepreneurship. The results are consistent with Table 3-6; NREGS increases non-farm entrepreneurship along the extensive margin, but has no impact on farm entrepreneurship.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 12: Impact on Extensive Entrepreneurship, Base Period = 1999

	Total	Non-farm	Farm
	(1)	(2)	(3)
NREGS (β)	0.057 (0.046)	0.358*** (0.097)	0.007 (0.069)
Post indicator (δ)	0.061 (0.070)	-0.494*** (0.166)	0.100 (0.105)
Poverty and demographics	yes	yes	yes
Labor force characteristics	no	no	no
Rural development measures	no	no	no
Real per capita NDP	no	no	no
Development index components	yes	yes	yes
Quarter fixed effects (τ_q)	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes
N	2,393	2,298	2,388

Table 12 presents the results from a robustness check against possible mean reversion: I estimate the impact of NREGS on rates of total, non-farm, and farm entrepreneurship using 1999 as the base period instead of 2004. The results are consistent with Tables 3-6.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13: Dosage Response

	Total	Non-farm	Farm
	(1)	(2)	(3)
Phase 1 * Post indicator (β_1)	-0.018 (0.042)	0.098 (0.104)	-0.041 (0.065)
Phase 2 * Post indicator (β_2)	0.013 (0.046)	0.192** (0.092)	-0.019 (0.071)
Post indicator (δ)	-0.539 (0.673)	-2.355 (1.907)	-0.517 (0.993)
Poverty and demographics	yes	yes	yes
Labor force characteristics	yes	yes	yes
Rural development measures	yes	yes	yes
Real per capita NDP	yes	yes	yes
Development index components	yes	yes	yes
Quarter fixed effects (τ_q)	yes	yes	yes
District fixed effects (η_d)	yes	yes	yes
N	3,541	3,302	3,524
Dosage response, F-test	$F_{1,44} = 0.46$	$F_{1,442} = 1.05$	$F_{1,442} = 0.09$

Table 13 presents the results from estimating the differential effect, or dosage response, of NREGS Phases 1 and 2 on rates of total, non-farm, and farm entrepreneurship. The results of the F-test in each column indicate there is no statistical difference across Phases 1 or 2.

Notes: Each observation corresponds to a quarter-district. The entrepreneurship outcome is defined at the household level. Household responses are aggregated to the quarter-district level in a given year, using the survey weights. Standard errors (in parentheses) are clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.