FERTILITY AND RURAL LABOR MARKET INEFFICIENCIES: EVIDENCE FROM INDIA

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Abstract. Informational frictions are an important source of inefficiency in rural labor markets. I examine the role of family size in mitigating costs that arise due to these frictions. I show that an increase in family size decreases the demand for hired labor in tasks for which worker output and effort are difficult to observe (monitoring intensive tasks). In contrast, in tasks for which worker output is easily observed, I find no relationship between family size and hired labor use. I provide evidence that supervision costs drive the preference for family labor in monitoring intensive tasks. As a consequence, larger families spend less time in supervision. I develop a theoretical framework, that illustrates the empirical challenge of identifying the link between family size and labor demand: factors that determine labor demand on the farm also determine family size. To address this endogeneity problem, I use exogenous variation in fertility induced by both a family planning policy in India, which provides cash incentives for sterilization take up, and income shocks. I show that while neither incentive payments nor income shocks by themselves are valid instruments for completed fertility, their interaction is a valid instrument. I infer that population control policies must take into account market inefficiencies that make larger families profitable.

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1. Introduction

In developing countries and in India in particular, farms employ different types of labor. While hired labor represents an important source of farm labor (approximately 61% of farming households hire some labor), family members, including young and adult children, provide a large share of the labor employed on farms (family labor constitutes around 50% of total labor on the farm). The role of children and youth in agricultural production is likely to be very important for fertility decisions, since land area and farm size change very little over the years it takes children to mature from birth to become productive members on the household farm. Fertility decisions and the decision to employ labor are therefore closely linked. In this paper, I examine how family structure and labor demand in rural markets interact, with particular attention to the role of informational problems in rural labor markets.

A prominent feature of rural labor markets is the presence of informational problems (Foster and Rosenzweig 1996). Agricultural production often involves tasks where worker output and effort are difficult to observe. In such tasks, workers have incentives to shirk, and low effort in these such tasks can have large consequences for the overall harvest. For example, improper weeding can lead to almost 50% less harvest for some crops[1]. Such information problems result in high supervision costs - if labor is hired, almost 10% of total family time spent on the farm is spent on supervision activities (REDS 1999). The theoretical literature has focused on various forms of contracts like permanent labor contracts and sharecropping to deal with some of the issues of informational asymmetry (Eswaran and Kotwal 1985, Braverman and Stiglitz 1982). However, in rural India, only a small fraction of farming households seem to engage in these types of contracts. Almost 96% of farms in India are wholly owned and operated by families; sharecropping and other land lease contracts make up the remaining 4% (Ministry of Agriculture 1991). Moreover, permanent labor comprises a very small fraction of the total workforce: only 7% of the total agricultural workforce is employed under regular work contracts; while the remaining 93% is composed of casual hired labor and family labor (Ministry of Rural Development 2000). Hence, it appears that such contractual agreements do not fully resolve the information problems in the Indian rural labor market.

This paper examines the role played by family labor in alleviating informational problems. I test the hypothesis that informational inefficiencies are prevalent in rural labor markets in India and that family size helps mitigate the costs that arise due to informational inefficiencies. I start with the observation that family members are more likely to internalize the implications of their actions on output. Furthermore, family members repeatedly interact and therefore face worse consequences if caught shirking. In contrast, without supervision, hired labor has incentives to shirk. Hence, supervision costs in certain tasks drive the preference for family over hired labor. The theoretical framework of this paper provides simple testable predictions about the relationship between family size and hired labor use by task. In tasks where supervision must exist to extract optimal effort from a hired laborer, greater family size implies less hired labor and less family supervision used on that task. However, in tasks where worker output and effort are easily observed, we do not expect a systematic relationship between family size and hired labor use. I provide empirical evidence that

[1]Remesan, Roopesh, Remya, and Preman (2007) examine the effects of weeding on rice output in South India.

larger families employ less hired labor on tasks where output and effort are difficult to observe. However, this pattern of substitution between family and hired labor alone is not enough to make the case for asymmetric information. I show the existence of asymmetric information by directly examining the relationship between supervision and family size. I find that larger families spend less time in supervision, and this occurs only in tasks where hired laborers have incentives to shirk.

Empirically examining the relationship between family size and labor usage on the farm is challenging, as factors that determine labor use can also determine family size and vice-versa. The literature studying causal effects of family size has mainly used twins and/or family sex composition as strategies to create exogenous variation in family size (Rosenzweig and Wolpin 1980, Angrist and Evans 1998). For various reasons, using twins or sex composition as instruments for family size, particularly in a development setting is problematic (Rosenblum 2008, Schultz 2007).

My strategy for identifying the relationship between family size and labor use involves using incentive payments for male and female sterilization instituted by the Government of India and income shocks (which in rural India, I measure using rainfall shocks). I find that at a time of negative income shock, there is greater take up of sterilization. The instrument is best explained as a difference in difference estimator in the *first stage* of the IV.

$$
\begin{aligned}
\underbrace{\text{Rain Shock-No Rain Shock}}_{\textit{High Incentive}} &= \quad \text{Sterilization Take Up + Rain effect} \\
&\qquad\qquad\qquad\qquad - \\
\underbrace{\text{Rain Shock-No Rain Shock}}_{\textit{Low Incentive}} &= \qquad\qquad\quad \text{Rain effect} \\
&= \qquad\quad \text{Sterilization Take Up}
\end{aligned}
$$

For a given incentive amount, areas with and without rainfall shocks experience differential sterilization take up. This difference, however, also contains a direct rainfall shock effect. As noted in Rosenzweig and Wolpin (2000), rainfall shocks in rural areas affect a broad set of outcomes. To eliminate the rainfall main effect, I compare the effects of rainfall in high versus low incentive payments. As long as rainfall shocks affects $High$ and Low areas in the same way, the *difference in difference* nets out the rainfall main effect. Hence, the interaction of rainfall shocks and incentive payments predicts exogenous take up of sterilization. Since sterilization is an irreversible and permanent end to fertility, this results in exogenous changes in completed fertility. The essential aspect of the strategy lies in controlling for the main effects of rainfall shocks (income shocks) and incentive payments.

The idea that family labor and hired labor might be different is not new. Bardhan (1973), Desai and Mazumdar (1970), Deolalikar and Vijverberg (1983) and Benjamin (1992), among others, examine whether family and hired labor have different efficiencies in farm activities. These papers do not focus on fertility or family size and its interaction with labor demand, and they do not provide a mechanism through which family labor is more efficient. A contribution of this paper is that I can show informational frictions between hired and family labor as an important channel that drives the preference of family over hired labor. This paper also contributes to the literature that provides empirical evidence of information asymmetry in rural markets (Foster and Rosenzweig 1994, Jean-Jacques and Matoussi 1995).

The logical implication of this paper is that market structure and market inefficiencies can play an important role in fertility decisions. This paper highlights some of the larger consequences of a family planning policy that is based on incentives for sterilization take up. Sterilization is the most common form of contraception in India and many other developing countries. Nearly 70% of all female contraceptive users report sterilization as the method of contraception. Moreover, 65% of sterilized women report sterilization as the *first* form of contraception they used (REDS 1999). While incentives for sterilizations are often used to induce take up, it is unclear whether such policies have had any impact on overall fertility levels. As this paper shows, family labor mitigates costs associated with information asymmetry on the farm. Hence, population control policies must take into account market inefficiencies that make larger families more profitable. Indeed, higher-income families in rural areas of India have larger family size (NFHS 1998). If the planner's goal is to reduce family size, in light of the interaction between family size and labor demand on the farm, labor market interventions might play an important role in helping reduce family size.

The paper is organized as follows: Section 2 lays out the theoretical framework; Section 3 discusses in detail the identification strategy; Section 4 describes the data used to test the predictions; Section 5 describes the estimation strategy; Section 6 contains results; Section 7 concludes.

2. **Theoretical Framework**

This section derives testable predictions for detecting information asymmetry between family and hired labor. The model contains two essential parts - a production phase that involves family and hired labor in pre-harvest and harvest activities, and a pre-production phase when decisions about family size is made. I solve the model recursively, and so characterize the production phase first. I do this as profits from this stage will play a key role in the determination of family size. Hence, families are considered to be forward looking, taking into account production parameters while deciding how many children to have. This section concludes with a discussion of the assumptions made in this model.

2.1. **Agricultural Production Phase**

Following others in the literature, (Eswaran and Kotwal 1985, Frisvold 1994) I consider agricultural production to consist of 2 phases. Phase 1 involves pre-harvest tasks like weeding and fertilizer application, while Phase 2 consists of harvest tasks like harvesting and threshing. The timing is sequential in that Phase 1 decisions are made, and given a certain "unharvested" output, labor for harvesting and threshing is employed in Phase 2. Phase 1 tasks suffer from informational frictions as output and effort are not easily observable unless supervision is employed. An additional moral hazard problem is that hired labor using draught animals for tilling (a land preparation activity) tend to overwork these animals thereby leading to an inefficient use of inputs. Similarly, it is difficult to observe output in weeding, and hence it has to be supervised. Since output is difficult to observe, payment schemes for hired labor in Phase 1 involve a time rate wage and possibly include an efficiency wage markup (Walker and Ryan 1990, Roumasset and Uy 1980, Shapiro and Stiglitz 1984). Under such a payment scheme, if there is no supervision, it can be shown that hired labor on a short term contract will shirk (Eswaran and Kotwal 1985).

Phase 2 tasks on the other hand are easier to monitor, or a simple contract based on observable outcomes can deal with the informational problem. This is because the common method of payment in Phase 2 is a piece rate (Walker and Ryan 1990, Roumasset and Evenson 1986). Piece rates compensate workers according to the output they have gathered that day. By paying a piece rate, the burden of shirking is transferred to the agent - hence, the agent has no incentive to shirk. Output in Phase 2 is easily observed and hence it is possible to pay a piece rate.

The conclusions of the model hinge on the idea that family labor need not be supervised in Phase 1 tasks[2]. The could be due to many reasons. First, there is no incentive problem *within* families, or if there is, that family members are more easily and harshly punishable than hired labor. Second, family members are in essence residual claimants of the output - what is good for the farm in terms of output, is good for them. Thirdly, family members have repeated interactions with each other, hence shirking can have worse consequences as a result of a repeated game.

Production in Phase 1 and Phase 2 is:

$$Q_1 = f(L_1^f + L_1^h; A, Z) \tag{1}$$

$$Q_2 = min\{g(L_2^f + L_2^h; A, Z), Q_1\} \tag{2}$$

where Q_1 is production at the end of Phase 1 (unharvested crop), while Q_2 is the harvested amount. Q_2 is the result of activities in Phase 1 and subsequent harvesting. Hence, the most that can be achieved in Phase 2 is conditional on the amount Q_1 that was made in Phase 1. The functions $f(\cdot)$ and $g(\cdot)$ are standard concave production functions. A is the set of assets owned by the family at the beginning of Phase 1, and Z are fixed factors such as land size, land quality etc. L_t^f and L_t^h is the amount of family and hired labor used on the farm in Phase $t = [1, 2]$. In the course of this paper, I also refer to Phase 1 tasks as "pre-harvest tasks", and Phase 2 tasks as "harvest tasks". As mentioned before pre-harvest tasks require monitoring if hired labor is used, while harvest tasks do not require monitoring. It should be noted that since farmers are forward looking, using backward induction, it will always be optimal to harvest whatever is produced in Phase 1[3]. Hence, the Phase 2 profit maximization is solved first. The main assumption in the model to follow is the perfect substitutability between family and hired labor. I discuss relaxing this assumption towards the end of this section.

2.1.1. **Phase 2 - Harvest Tasks.** Consider the profit maximization problem faced by the household in Phase 2. Q_1 was produced in Phase 1, and this needs to be harvested in the least costly

[2]Even if family labor is supervised, they have to be supervised *less* than hired labor.

[3]In this simple model there is no role for uncertainty. If there are shocks to selling of output, or if shocks affect the opportunity cost of family labor then this might not hold.

manner possible:

$$\min_{L_2^f, L_2^h} w_2(L_2^h + L_2^f) \quad s.t. \quad g(L_2^f + L_2^h) = Q_1 \quad and \quad L_2^f \leq T(N) \tag{3}$$

where L_2^f, L_2^h is the amount of family and hired labor used on farm, N is an exogenously given family size, $T(\cdot)$ is an increasing function that maps family size to time endowments, and w_2 is the wage rate paid to family and hired labor in Phase 2.[4]

To make matters simple, consider the production function $g(\cdot)$ to be linear[5]:

$$\min_{L_2^f, L_2^h} w_2(L_2^h + L_2^f) \quad s.t. \quad k(L_2^f + L_2^h) = Q_1 \quad and \quad L_2^f \leq T(N) \tag{4}$$

As there is no uncertainty in the model, if it is at all profitable to harvest in Phase 2, all of Q_1 will be harvested. Hence the total amount of labor $(L_2^{*f} + L_2^{*h})$ used will be Q_1/k. Therefore, there is a continuum of solutions to the amount of total labor demanded (call this L_2^*), and hence any combination of family and hired labor can satisfy this optimum. In this simple setting, barring strong behavioral preferences, it is clear that the optimal amount of hired labor (L_2^{*h}) used on the farm is not systematically related to family size N while holding constant Q_1/k. Hence, in tasks like harvesting and threshing, we should not find a systematic relationship between family size and hired labor.

2.1.2. **Phase 1: Pre-harvest tasks.** As mentioned before, Phase 1 tasks are tasks where an agent's effort is not easily observed. For example, in a task like weeding, which is very important for the overall success of the harvest (Webster and Wilson 1966), it is difficult to observe output once the agent is done weeding. Hence, there are incentives to shirk in a task such as weeding. For every unit of hired labor, the family must spend a fraction θ in monitoring activities[6]. Moreover, the farmer knows the costs he will incur in Phase 2 and will incorporate these costs in his Phase 1

[4] While I impose perfect substitutability between family and hired workers, in Phase 2 tasks, since payment is by piece rate, this assumption can be relaxed. I only need that workers are paid their marginal product in Phase 2 tasks.
[5] Given that the solution is by backward induction, the linearity is purely for exposition.
[6] Another way to model this problem yielding similar qualitative result is to impose supervision as a direct addition θ to the wage paid for the hired laborer as in the Shapiro and Stiglitz (1984) solution to the moral hazard problem in labor markets. In this case the problem becomes:

$$\max_{L_1^f, L_h^1} (1 - \frac{w_2}{k}) f(L_1^f + L_h^1; A, Z) - (w_1 + \theta) L_h^1 - w_1 L_1^f \quad s.t. \quad L_1^f \leq K(N)$$

This problem yields similar predictions.

decisions. Hence, with monitoring, the maximization problem becomes:

$$\max_{L_1^f, L_h^1} f(L_1^f + L_h^1; A, Z) - w_1(L_h^1 + L_1^f) - \frac{w_2}{k} Q_1 \tag{5}$$

$$= \max_{L_1^f, L_h^1} (1 - \frac{w_2}{k}) f(L_1^f + L_h^1; A, Z) - w_1(L_h^1 + L_1^f)$$

$$s.t. \quad L_1^f + \theta L_h^1 \leq K(N)$$

At an interior[7], the amount of labor hired will be:

$$L_h^{1*} = \frac{1}{(1-\theta)}[(1 - \frac{w_2}{k}) f'^{-1}(w_1; A, Z) - K(N)] \tag{6}$$

Hence, in the presence of monitoring cost θ, the amount of hired labor used depends systematically on the endowment of family labor $T(N)$. If $K(\cdot)$ is an increasing function of N, then hired labor depends systematically on N. Moreover, since L_h^1 is a decreasing function of N, supervision (θL_h^1) is also decreasing in N. The model so far assumes perfect substitution between family and hired labor. I consider the case where family and hired labor are imperfect substitutes in Section 2.3.

This formulation also assumes that hiring any amount of casual labor requires supervision. This is true if without supervision, the productivity of the hired labor is so low that it would be profit maximizing to supervise.[8] In the data, households that do not supervise, hire very small amounts of labor on average as compared to households that do supervise (28 person days as opposed to 175 person days).

The following table summarizes the predictions from the model:

Consequences of increasing N

Task	Family Labor	Hired Labor	Supervision
Pre-Harvest	↑	↓	↓
Harvest	No change	No change	No change

Since family size matters for profits on the farm, the determination of family size in the period prior to production will take into account the role of family size in production.

2.2. **Determination of Family Size (N)**

Consider a Phase 0 where families decide how many children to have (N). Forward looking agents will consider the role of children or N on the farm, and these considerations will enter the demand for children and assets in Phase 0. The profit function from Phase 1 and 2 of farm

[7] ϵ amount of hired labor is used if $f(T(N) + (1-\theta)\epsilon) \geq f(T(N)) + w_1(1-\theta)\epsilon$. This is the condition that ensures that hiring some labor and monitoring them will lead to higher profits than not hiring any hired labor.

[8] If the productivity of the hired labor is λ when not supervised and $\lambda < 1$, supervision will always exist as long as $\lambda \leq 1 - \theta$.

production is given by:

$$\Pi = \pi(N, \underbrace{w_1, w_2, \theta, k, A, Z}_{\Phi}) \tag{7}$$

Parents in Phase 0 maximize:

$$\begin{aligned} &\max_{c_0, N} U(c_0) + \beta U(c_1) \\ &s.t. \quad c_0 + p_N N = I_0 \\ &and \quad c_1 = \pi(N; \Phi) \end{aligned} \tag{8}$$

Φ denotes production parameters, I_0 represents income in Phase 0, p_N is the price of children, and c_0 is the consumption in Phase 0 and c_1 represents consumption in the production period (inclusive of Phase 1 and 2). Hence, in Phase 0, parents will choose N, keeping in mind production parameters (Φ), as well as p_N and I_0. The demand for children in Phase 0 is given by:

$$N^* = d(I_0, p_N, \Phi) \tag{9}$$

Hence, any production parameter like land quality, wages, etc. will drive the demand for children via Φ. Equation 9 shows that monitoring plays a role in fertility decisions. Hence, information asymmetries that lead to differential monitoring between family and hired labor can drive the demand for family size. Certainly, this does not imply that families will increase family size until the point of not having to hire any labor - the price of children p_N mitigates the extent to which families can simply add more children to the existing stock. Hence, θ and p_N contribute opposite effects in determining the extent to which families increase their size to mitigate information costs.

Section 2.1.2 derives testable predictions using family size as an exogenous explanatory variable. However, family size is endogenously chosen keeping in mind production parameters. Hence, I need exogenous variation in family size to empirically test the predictions of the model.

2.3. **Discussion: relaxing assumptions of the framework**

In this section I discuss the implications for relaxing the central assumption of the framework, that hired and family labor are perfect substitutes. Under perfect substitutes, the wedge between family and hired labor is driven by monitoring costs due to asymmetric information. If family labor is preferred for some other reason, then the role of monitoring costs is not clear. Alternative theories, however, have to account for the fact that family labor is preferred *only in certain tasks* - hence a blanket imperfection like demand or supply rationing cannot explain the task wise pattern in hired labor demand.

A simple alternative theory could be comparative advantage of family labor over hired labor in certain tasks (i.e. differing efficiencies of family and hired labor). If family is better at a certain task due to better knowledge about the farm, or due to experience of the elderly living in the household (Rosenzweig and Wolpin 1984), then similar patterns of hired labor demand will be observed (as under the monitoring case). Hence, even without the need to monitor hired labor, family labor will be preferred to hired labor in certain tasks. In fact, since pre-harvest and harvest tasks occur at different times, family labor could be preferred to hired labor even in harvest tasks. It is entirely possible that a family member working on his own farm is more efficient than on an outside farm in both pre-harvest and harvest tasks. In this case we should observe that larger families use less hired labor in all production tasks. Let α_1 and α_2 be the efficiency parameters for family labor in Phase 1 and Phase 2 tasks respectively. The production functions can be written as:

$$\text{Phase 1} \quad f(\alpha_1 L_1^f + L_h^1; A, Z) \tag{10}$$

$$\text{Phase 2} \quad g(\alpha_2 L_2^f + L_2^h; A, Z) \tag{11}$$

If $\alpha_1 \geq \alpha_2$ then an additional family member will displace more hired labor in pre-harvest than in harvest activities. If $\alpha_1 < \alpha_2$, we get the same predictions as long as $\theta \geq 1 - \frac{\alpha_1}{\alpha_2}$. Due to the nature of the tasks involved, it is unlikely that family labor is more efficient at harvest than pre-harvest activities. The testable predictions of the model under perfect substitutability of hired and family labor assumes $\alpha_1 = \alpha_2 = 1$. However, as we just discussed, this assumption can be relaxed without changing the essence of the testable predictions. The key aspect of this discussion is that even under greater efficiency of family over hired labor, monitoring costs (θ) creates an *added* wedge between family and hired labor.

Hence, empirically observing the pattern that family size matters for certain tasks on the farm is not conclusive proof of asymmetric information driving the wedge between family and hired labor. As will be made clear, having direct data on monitoring costs and time is critical to show the existence of asymmetric information and how this drives the preference of family over hired labor in certain tasks.

3. **Empirical Estimation**

The previous section outlined a testable prediction - larger families should decrease the amount of labor used in pre-harvest tasks, but not so systematically in harvest activities. The empirically estimable version of equation 6 is[9]:

$$L_{hi}^{1*} = \gamma N_i + \underbrace{(\frac{1}{1-\theta})(1 - \frac{w_2}{k})f'^{-1}(w_1; A_i, Z_i)}_{u_i} \tag{12}$$

Recall that L_{hi}^{1*} is the amount of hired labor used in Phase 1 in household i. The model predicts that $\gamma < 0$. As Section 2.2 showed however, u_i and N_i are correlated, and OLS will yield biased estimates of γ. Hence, we need instruments for N_i in order to obtain unbiased estimates of the relationship between family size and hired labor use. While the purpose of my test is not to give a structural interpretation to γ, if $K(N)$ is a linear function then $\gamma = -\frac{1}{1-\theta}K'(N)$.

In the Appendix I estimate equation 12 using a household fixed effects approach. While a household fixed effects approach can eliminate unobserved endogeneity at the household level, it cannot eliminate task specific aspects that are correlated with family size. Moreover, if returns

[9]Other estimating equations are simple variants of this equation. The important common element is that N is a dependent variable across all specifications.

to family size is heterogenous, then fixed effects is an incorrect approach. As will be seen the household fixed effects approach gives results largely consistent in direction with the IV approach.

3.1. Potential Instruments for Family Size (N)

This subsection considers 2 potential candidates as an instrument for family size - incentive payments for sterilization take up, and rainfall shocks. I argue that neither is a good instrument, but that the *interaction* of the two is a good instrument for predicting family size via exogenous take up of sterilization.

3.1.1. Incentive Payments for Sterilization.

India was the first country to introduce incentive payments for sterilization in 1952 (Cohen 1996)[10]. The amounts paid to acceptors were not trivial either - in 1959, acceptors of sterilization in Madras were paid around Rs.30 ($6.3 in 1959 dollars), a tremendous amount considering annual per capita income was around $70 (Connelly 2006). Starting in 1966, the Government started to officially provide funds to sterilization acceptors (Connelly 2006). While the Government set a rate for each IUD insertion (Rs.11) or sterilization (male and female sterilizations were paid differently, Rs.30 for a vasectomy and Rs.40 for a tubectomy), it was left up to the states to distribute that amount between the acceptor, staff and "motivators" (Connelly 2006). According to the current Government program, Rs.450 ($11.5) and Rs.500 ($12.5) are paid to each acceptor of tubectomy and vasectomy respectively (Govt of India documents). The fact that the Government took incentive payments seriously is highlighted in (Srivastava 1990) - "One third of the funds [of the Family Planning budget] are used to create awareness and motivate eligible couples to become acceptors, and two thirds to provide service facilities supplies equipment and services. The major consumer on the motivation side is the incentive payment" (pg 19).

A large number of studies show that these incentives actually worked in attracting people towards sterilization.[11] In a case study of sterilization acceptors in a small village in rural Andhra Pradesh, Reddy (1986) notes differences in whether people say incentives matter for take up of sterilization. While 45% of the Harijans (the lowest caste) responded in the affirmative, much fewer percentage of the upper caste (Kamma) said the incentives mattered. Sunil, Pillai, and Pandey (1999) and Khan and Prasad (1980) are other studies of whether financial incentives play a role in promoting the use of certain contraceptives. They too find increased take up under a financial incentive program.

[10]While India was the first country to establish incentives for family planning, it is certainly not the only one offering incentives. In a survey by Ross and Isaacs (1988) it is noted that almost all South Asian countries and Korea provide incentives to acceptors of family planning. Thapa, Abeywickrema, and Wilkens (1987) examine the impact of vasectomy payments in Sri Lanka, while Weeden, Bennett, Lauro, and Viravaidya (1986) examine a community based incentive program in Thailand. Each study finds a significant increase in family planning take up when incentives are introduced.

[11]The form of the incentive, i.e. an immediate payment is also important in understanding the type of monetary incentive that seemed to matter. It seems that immediate payments mattered more than a promise of payments later (Reddy 1986, Ridker 1980). As Satia and Maru (1986) suggest "It is clear that the respondents preferred incentives that would meet their immediate needs best...Old age pensions and other incentives that would benefit family planning acceptors at a future date were not very popular, particularly among young couples".

Since incentives matter for sterilization take up, variation in incentive payments is a potential instrument for fertility. Demand for N now takes the form:

$$N^* = d(I_s, I_0, p_N, \Phi) \tag{13}$$

Introducing incentive payments (I_s) as in equation 13 and using it as an instrument for N is not valid for two main reasons.

First, if people consider the incentive payments to last forever, then the variation in incentive payments need not affect *completed* fertility. It can be argued that a given level of incentive only induces families to reach their target size faster (presumably they get sterilized after reaching their target size). Hence, the only variation in family size that I obtain from using the incentive payments is by preventing unwanted births via sterilization. While this might be an important effect in terms of a population reducing policy, it cannot be used to test the predictions of the model as sterilized families under this scenario have already reached their optimal family size. I find that using the incentive payments as an instrument predicts a *positive* relationship between sterilization status and number of children. This is consistent with Rosenzweig and Schultz (1984), as more fecund women can have children faster and take up sterilization earlier. Hence, this strategy does not provide exogenous variation in completed (optimal) family size.

Second, according to Schultz (2007) simply using incentive payments is not enough to generate exogenous variation in N, as incentive payments in certain areas might themselves reflect preferences over N. Moreover, other direct income effects of the payment would make it a bad candidate instrument.

3.1.2. **Rainfall Shocks.** In rural areas, shocks to rainfall affect incomes. Hence a rainfall shock can be used as a shock to incomes. I can introduce rainfall (R_0) in equation 13 by making income a function of rainfall:

$$N^* = d(I_s, I_0(R_0), p_N, \Phi) \tag{14}$$

In this formulation, rainfall shocks can be candidate instruments for N. However, we know from Rosenzweig and Wolpin (2000) that rainfall shocks affect relative prices in rural areas of developing countries. Hence, rainfall shocks could directly affect w_1 or w_2 and have a direct impact on hired labor demand. However, if sterilization occurred a long time ago, it might be difficult to argue that rainfall shocks ten years ago (say) has an impact on relative prices today.

An additional issue with rainfall shocks is that they can also affect current farm asset holdings A, which in turn directly affect amount of labor used in the different phases of production. If farm assets that are used in Phase 1 and 2 are also determined in Phase 0, we can think of assets in Phase 0 as a function of income, and prices:

$$A^* = s(I_0(R_0), p_N, \Phi) \tag{15}$$

There is empirical evidence towards the idea that assets are affected by rainfall shocks. It has been shown (Rosenzweig and Wolpin 1993, Anagol 2008) that households in India smooth consumption at a time of negative income shock by selling farm animals. Thus, rainfall shocks do affect the path of asset accumulation and it would be tough to make the case that rainfall affects hired labor demand only through family size. The essential idea against using rainfall shocks as an instrument for fertility is that rainfall shocks affect a host of things in rural areas that might matter for both fertility and labor usage on farms.

3.2. **Interaction of Rainfall Shocks and Incentive Payments as an Instrument**

The identification strategy employed in this paper uses the interaction of incentive payments for sterilization and rainfall shocks to predict exogenous (conditional on the sequence of main effects of rainfall shocks and incentive payments) take up of sterilization and hence predict (exogenous) changes in family size. The advantage of using the interaction as the instrument is that it allows me to control for the direct effects of the incentive payment and the rainfall shock (since neither of these by themselves are good instruments). The interaction can be explained in terms of a difference in difference estimator, but for the first stage only.

Consider an area with a high incentive payment and an area with low incentive payment. These areas are either hit by low rainfall shocks, causing low incomes or not hit by rainfall shocks. Rainfall shocks induce people (say) to sterilize in high and low incentive areas - and they also affect the areas along various other dimensions. Hence, even though rainfall shocks are random, the *first difference* in sterilization take up due to rainfall shocks in the high incentive area includes all other changes that rainfall shocks might cause. To get around this difference, I use the first difference in sterilization take up in the low incentive area, which also includes differences in sterilization take up, and other changes due to rainfall shocks. Now, the strategy relies on the fact that the *difference in difference* between the two first differences across the high and low incentive areas nets out the "other changes" due to rainfall and only preserves the changes in the take up of sterilization.

$$
\begin{aligned}
\underbrace{\text{Rainfall Shock-No Rainfall Shock}}_{\textit{High Incentive}} &= \text{Sterilization Take Up} + \text{Rain effect} \\
& \qquad\qquad - \\
\underbrace{\text{Rainfall Shock-No Rainfall Shock}}_{\textit{Low Incentive}} &= \qquad\quad \text{Rain effect} \\
&= \quad \text{Sterilization Take Up}
\end{aligned}
$$

The assumption we need for the strategy to work is that rainfall shocks affect HI and LI areas in the same way along all other dimensions not related to sterilization take up. Anecdotal evidence supports the use of the interaction as an instrument for sterilization take up:

> Immediately after the incentive payments were announced there was a spike in the number of sterilizations and IUD insertions, particularly in the states that had started to go hungry. Bihar, for instance, had previously had the lowest rate of sterilization per capita of any state or union territory in India, performing just 2,355 such procedures in 1965. And, with 12,677 insertions, it had met only 12 percent of its IUD target. But in 1966, with some people eating leaves and bark, a total of 97,409 "acceptors" suddenly came forward. The next fiscal years performance was even better: 185,605, with 78 percent opting for sterilization (and the higher

> incentive payment). As a Ministry of Health and Family Planning analysis concluded, it was “the famine and drought conditions in various parts of the country like Bihar, Madhya Pradesh and Orissa, which attracted large numbers of persons towards sterilizations”. (Connelly 2006)

The maintained assumption is that the interaction does not affect asset accumulation, while the main effects of rainfall shocks and incentive payments are allowed to do so. This implies that conditional on the main effects of rainfall and the incentive payment, there is no differential (across RS and NRS areas) smoothing via asset accumulation or destruction. In Appendix Table 2, I show some evidence that farm investments are not differentially affected by the interaction. As an added check, I allow for differential smoothing via assets, and instrument for both, family size and asset holdings by using the interaction and direct rainfall shocks as instruments - I reserve discussion of these results until section 6. If there is heterogeneity in how sterilization affects family size, the interaction can still identify the sterilization effect, under some added assumptions. These are relegated to the Appendix. One advantage of using the interaction to predict sterilization take up is that sterilization is permanent as opposed to smoothing via assets. Even if at the outset (i.e. in Phase 0) there is differential smoothing that affects assets, families that lowered their assets (say) have a higher return to accumulating those assets. Given the time lag between when families get sterilized and labor decisions that I observe in the data, families with higher returns for accumulating assets might do so. However, the differential family size remains as sterilization is permanent. Hence, while differential asset movement might be a concern in the short run, given enough time between sterilization events and observation of labor hiring, this is not a first order concern.

4. **Data**

The household data used in this paper comes from the Rural Economic and Demographic Survey (REDS) conducted by the National Council of Applied Economic Research (NCAER) in India. They survey round I use is from 1999.

The demographic schedule of the survey contains extensive questions about contraceptive behavior, as well as asking women when they got sterilized. This information is key in constructing the panel for sterilization take up. The population of women in the demographic survey are ever married women between the ages of 14-49. Labor use data is obtained from detailed questionnaires regarding labor use by type (hired, permanent etc.) and task (weeding, harvesting etc.). Labor use is collected in “person-days” measure.

For robustness checks of the IV methodology, two other Indian data sets are used - International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) data, and the National Family Health Survey 1998 (NFHS 1998). The ICRISAT data is a panel dataset of sample households from 6 villages over a 10 year period from 1975-1985. I use yearly data on farm asset sales and purchases from this data set. The NFHS is a nationally representative sample of Indian women between the ages of 14-49 who are married at the time of the survey. The NFHS's demographic schedule is similar to the REDS, and is used to verify that the interaction predicts sterilization take up. However, the NFHS contains no information on household production, and hence cannot be used to test the labor usage-family size relationship.

The rainfall data used in this paper is from the Center for Climactic Research at the University of Delaware, specially from their Global Precipitation Monthly and Annual Data Series for 1950-99. Rainfall is measured at a 0.5 degree by 0.5 degree longitude-latitude grid. To compile this data series, researchers combined data from 20 nearby weather stations, using an interpolation algorithm based on the spherical version of Shepard's distance-weighting method. In order to match this rainfall data to villages in REDS, I calculated the distance between the center of each village and the Delaware grid using the Haversine formula for measuring distance between two longitude-latitude points, and matched each villages to the closest point on the grid. Rainfall shock is measured as deviation of annual rain from historical mean of annual rain. The definition of a shock as used in this paper is a deviation of more than 30% from historical annual mean.[12]

Data on incentive payments were collected from archives in the Family Planning Commission in Delhi, India. The data is at the annual, national level starting in 1969 (state specific data is available for some states, but not for the entire time period). I exploit variation in prices of staple grains across districts in India to compute an index for the incentive payment in terms of the quantities of staple grains people could potentially buy. This creates cross sectional variation in "perceived" incentive payment. Staple grain prices are from the World Bank Climate and Agriculture Dataset for India. A different way to do this is to use the CPI at the district or state level to create the value of the "perceived" incentive payment. Both methods yield similar overall results.[13]

5. **Implementing the IV Strategy**

5.1. **Overall Estimation Method**

The first step in the estimation strategy is to obtain for each woman in the REDS sample, the probability that she is sterilized by the time she is surveyed in 1999. I subsequently use this estimated probability of sterilization (call it $\widehat{p_i}$) as a predictor of actual sterilization status in 1999 (call it S_i). By instrumenting for S_i using $\widehat{p_i}$, I can estimate the (causal) effect of sterilization on the number of children in the family. In subsequent outcome equations I directly use $\widehat{p_i}$ as an instrument for the number of children.

The overall empirical strategy consists of 3 steps:

(1) Estimate from a panel (how I "construct" a panel will be discussed in the following section) the probability a woman is sterilized in each time period - $\widetilde{p_{it}}$. From $\widetilde{p_{it}}$'s compute the probability that the woman is sterilized in 1999 - $\widehat{p_i}$. This is similar to estimating a survival function for each woman.

[12]The Indian Meteorological Department classifies a "moderate drought" as between 26-50% deficit from the historical average (Infochange 2008). Results are robust to other measures as well. Table 8 in the Appendix shows whether other shock/drought measures interacted with sterilization payment predict take up. Moreover, this shock measure and Rosenzweig and Wolpin's (1993) shock measure yield similar effects on crop income/profits in the ICRISAT data.

[13]One might potentially be worried that the rainfall shocks directly affect grain prices or the CPI. This is unlikely as the price information/CPI is at the district level, while the rainfall is at a finer level. Moreover, three year averages for the price data are taken to mitigate any direct effect of rainfall on prices.

(2) In the cross section, use $\widehat{p_i}$ as an instrument for actual sterilization status S_i and estimate the effect of sterilization on number of children:

$$\text{First Stage} \quad S_i = \theta_1 \widehat{p_i} + \theta_2 X_i + \zeta_i \tag{16}$$

$$\text{Second Stage} \quad N_i = \rho_1 \widehat{S_i} + \rho_2 X_i + \upsilon_i \tag{17}$$

Where N_i is the number of children born to woman i and X_i's are control variables such as state fixed effects, education of the woman, inherited land holding, and a polynomial in the woman's age. X_i's also include controls for the mean and standard deviation of the rainfall shocks and the incentive payments.

(3) Once established that the instrument $\widehat{p_i}$ impacts number of children through sterilization status in 1999, subsequent equations that examine the effect of family size on labor hiring decisions (Y_i), directly use $\widehat{p_i}$ as an instrument for number of children

$$\text{First Stage} \quad N_i = \omega_1 \widehat{p_i} + \omega_2 X_i + \chi_i \tag{18}$$

$$\text{Second Stage} \quad Y_i = \beta_1 \widehat{N_i} + \beta_2 X_i + \epsilon_i \tag{19}$$

5.2. **Estimating $\widehat{p_i}$**

Since the data contains information on when women were sterilized, I can construct for each woman in the sample, a "panel" where in each year, I can observe the rainfall shock, the incentive payment as well as her sterilization status. I use the interaction in this "constructed" panel to obtain the probability that she is sterilized in each period.

With the panel, I can use the entire history of incentive payments and rainfall shocks the woman has experienced, as opposed to just her last period incentive payment and rainfall shock. In fact, utilizing only the last period information would lead to inconsistent estimates (Shumway 2001). The woman starts being "at risk" (i.e. enters the panel) upon marriage. For each woman-year observation I know the incentive payment that existed and the rainfall shock that period. The goal in this first step is to estimate the probability of sterilization in each time period. The regression is simply:

$$S_{it} = \beta_1 I_{it} + \beta_2 R_{it} + \beta_3 I_{it} * R_{it} + \epsilon_{it} \tag{20}$$

Here S_{it} is a binary variable, 0 if not sterilized, 1 if sterilized. I_{it} and R_{it} is the incentive payment and rainfall shock variable, while $I_{it} * R_{it}$ is the interaction. The implication of the anecdotes is that β_3 is positive and predictive. Although the model is agnostic about this, one would a-priori think that β_1 be positive as well. The goal is to obtain predicted survivor probabilities in each time period for each woman (i.e. obtaining $\widetilde{p_{it}}$).

I can estimate equation 20 using various hazard estimation methods, or by using a panel logit estimation method. Shumway (2001) shows in a simple formulation the equivalence between discrete time hazard models and panel logit estimation. In the empirical results I show equation 20 estimated using a Cox proportional hazard method as well as panel logit method. Using the estimated β's I can estimate $\widetilde{p_{it}}$.

Once I estimate $\widetilde{p_{it}}$ for every woman, I can calculate the probability that the woman is sterilized by 1999. A woman who is sterilized sometime before 1999, (call this time T) exits the data subsequently, so for some women, the probability of being sterilized is estimated as of time T, since years between T and 1999 do not alter this probability (sterilization is presumably irreversible in this context). $\widehat{p_i}$ is given by:

$$\widehat{p_i} = \widetilde{p_{iT}} * \prod_{t=1}^{T-1} [1 - \widetilde{p_{it}}] \tag{21}$$

Here T refers to the last time period we observe the woman in the data. As mentioned earlier, this could be 1999, or earlier depending on when the woman was sterilized. Given the IV strategy outlined in 3.2, I only need to use variation in $\widetilde{p_{it}}$ that comes from the interaction term (RI). The main advantage of using the interaction is that the main effects of rainfall and the incentive payment that are not excludable are accounted for. However, when I use $\widehat{p_i}$ in the cross sectional analysis, I do need to account for an "aggregated" version of the rainfall shocks (R_{it}) and the incentive payment (I_{it}). Hence, I include the mean and standard deviation of the incentive payments and the rainfall shocks to account for these main effects. I discuss and perform robustness checks by including other variants of the main effects in the Appendix.

5.3. **Outcome Equations**

The final regression specifications involve three outcome variables - hired labor (L_{ih}), supervision (L_{is}) and family labor (L_{if}) as the dependent variables. In each case, the main independent variable is family size N_i, which is instrumented by $\widehat{p_i}$. Moreover, each regression is done by task (k). The final regression specifications are:

$$\text{First stage: } N_i = \phi_1 \widehat{p_i} + \phi_2 X_i + \mu_i \tag{22}$$

$$\text{Second stage: } L_{ij}^k = \psi_1^k \widehat{N_i} + \psi_2^k X_i + \upsilon_i^k \tag{23}$$

Where $j = [h, f, s]$ denotes the type of labor used (hired, family or supervision). X_i is a list of controls as mentioned before. It is important to note that the mean and standard deviation of incentive payments and rainfall shocks experienced by the woman during her time in the hazard is included in all specifications. Doing so utilizes variation in sterilization take up coming from the interaction between rainfall shocks and incentive payments.

6. **Results**

6.1. **Does the interaction predict take up?**

The key to the identification strategy is that the interaction of rainfall shock and incentive payment should predict sterilization take up. As mentioned in an earlier section, I estimate this interaction coefficient using a constructed panel with sterilization status on the left hand side. As a first check, we should only see the interaction predicting take up in rural areas. Urban areas are areas where rainfall has little or no effect on incomes. Table 2 uses data from the NFHS and splits the analysis by rural and urban. It is apparent that while the incentive has a large positive main effect in both areas, rainfall only plays a role in the rural areas (since rainfall determines incomes more so in rural areas), and moreover the interaction has predictive power only in the rural areas. Table 3 shows various specifications where the coefficient of interest is the interaction. The anecdotal evidence suggests that this interaction is positive. Evidence from Connelly (2006) says that precisely at a time of income shock, or in his example in case of a drought, sterilization take up was higher. The coefficients across all specifications in Table 3 confirm such a story where during times of rainfall shock and the presence of incentive payment, take up is higher. To ensure that this is not dependent on functional form, specification 1 is a Cox proportional hazard model. The fact that the interaction coefficient is greater than 1 is indicative of positive take up.

The main effect of the shock variable in Table 3 is negative; however, we do not have a strong a priori reasoning for its sign. However, we might expect that the incentive payment itself is positively correlated with take up, and we find this to be the case in Table 3. As long as the interaction is positive, it suggests that during times of rainfall shock, as the incentive payment increases, take up also increases. Similarly, for a situation when incentive payment is high, the interaction suggests that during times of rainfall shock, take up is even higher. Recall from the previous section that the panel hazard essentially acts as my "first stage" in my estimation strategy.

I can verify the fact that take up is higher by using a completely different data set. The National Family Health Survey from 1998 is a nationally representative survey covering around 80,000 women. Like the REDS, this survey also asks women when they got sterilized so I can create the same type of "panel" with sterilization status on the left hand side. The results from estimating the take up for this data is presented in the Appendix (Table 1). The coefficients again suggest that the take up is positive via the interaction.

One of the underlying assumptions is that the interaction does not predict other things that might affect labor usage on the farm. For example, if upon sterilization at a time of rainfall shock, the incentive payment is used towards farm investments, then the strategy will not be valid. To test this assumption, I need a history of farm investments or asset investments to create a similar panel as in Table 3. While the REDS data does have detailed information on farm and other assets, the only asset where I know the date of investment is tubewell construction. I construct a similar panel as used in Table 3, to examine whether the interaction predicts tubewell take up. The results are in Appendix Table 2. Notice that while the interaction does not predict take up, the main effect of the incentive payment does predict tubewell take up. This reaffirms the idea that using the main effects is not a robust strategy.

I further test the validity of using the interaction as my instrument by using a 10 year panel of households with detailed asset information from the ICRISAT Village Level studies (Columns 2-4 in Table 2 in the Appendix). In the 10-year panel I find that while asset sales and purchases are affected by rainfall shocks, there is no *differential* asset purchase or sale. That is to say, the interaction is not predictive of asset sale or purchase.

As mentioned earlier in the text, another way of dealing with differential asset accumulation is to instrument for assets along with family size. In this case, I need an added instrument since I have to instrument for 2 endogenous variables - family size and assets. Under the assumption that rainfall shocks in the past *only affect current labor hiring decisions via asset accumulation and family size*, I can use rainfall shocks and the interaction of rainfall shocks and the incentive payment to instrument for the two endogenous variables. I find that while the interaction and the main effect of the rainfall shocks affect family size, only the rainfall shocks affect asset accumulation. Moreover, instrumenting for both does not alter the coefficient on family size in any of the labor-use regressions (Appendix Table 7). Hence, it appears that the interaction does not predict differential asset accumulation.

6.2. **Impact on fertility**

Does the predicted sterilization probabilities from this section predict number of children in the cross section from 1999? As mentioned in the empirical strategy section, I use the predicted survival probabilities to predict sterilization take up in the cross section from 1999, and estimate the impact of sterilization on total number of children. Table 4 estimates the causal impact of sterilization on number of children. The estimates suggest that sterilized women have between 0.6 to 0.7 fewer children. Presumably women who are above the age of 35 have completed their fertility and hence comparing the number of kids born to sterilized and unsterilized women above the age of 35 gives us the impact on completed fertility. However, the estimates between the full sample and the above age 35 sample is not that different. For ease of presentation, I use the entire sample for all future results. However, the results are qualitatively similar across outcomes for women above the age of 35.

The OLS coefficient (columns 1 and 4) of the impact of sterilization shows a positive and significant relationship between sterilization take up and number of children. The difference between the OLS and IV estimates suggests that there is significant selection into who takes up sterilization (Rosenzweig and Schultz 1984).

The empirical strategy hinges on the fact that I can control for the main effects of rainfall shocks and value of the incentive payment - I only want to use variation in sterilization take up as predicted by the interaction of these main effects. Hence to control for the main effect in the 1999 cross section, I use the distribution of the main effects as experienced by the mother during her fertile lifetime. Hence, I control for the mean and standard deviation of the incentive payment and the shocks she experienced through her life up to 1999. The table reports the coefficients on the averages (columns 2 and 5). I conduct further specification checks to ensure that my results are not sensitive to additional main effects being included. These are shown in the Appendix (Table 4).

To further investigate whether using the distribution of main effects is an effective control, I construct the interaction variable using the distribution. Hence I create a version of an “average” instrument by multiplying the means of the incentive and shock variables. The estimates on sterilization take up and on fertility as implied by this “average” instrument is shown in the Appendix (Table 3). Table 3 in the Appendix confirms that the interaction predicts higher take up, and that the impact of sterilization on total number of births is negative and significant.

6.3. **Casual Labor on Farm**

The main analysis examines the relationship between family size (as measured by number of children) and average (per woman) casual labor use by task. The REDS data has extensive information on labor use by task. For ease of presentation, I aggregate agricultural tasks into pre-harvest tasks and harvest tasks.[14] The labor usage measure is in total number of person days employed - for example if a household employs 2 workers for 2 days, the total days worked is taken to be 4. The explanatory variable of interest, family size, is measured as the number of living children born to women in the household for whom information is available in the demographic component of the survey.

Table 5 estimates the relationship between number of children and hired labor use. In pre-harvest tasks, we see a strong negative relationship with family size. Particularly in weeding, higher family size reduces hired labor used on the farm. In tasks like harvesting, family size does not appear to play a significant role. Moreover, since I have data on expenditures on hired labor, we see that expenditure on hired labor in weeding reduces significantly as family size increases. This is not the case in harvesting, where expenses do not vary systematically with family size. A concern might be that the specifications include children who are too young to work, and this somehow creates a spurious correlation. The presence of young children might indicate a *lack* of a result rather than a strong negative result. In the extreme consider if all children are below 1 year in age - in this case we should clearly not find children participating in the labor force and reducing hired labor use in any task. I cannot instrument for the age composition of the children. However, the instrument should be orthogonal to the age or even the sex composition (see Table 9 in the Appendix). Since the timing of when women get sterilized is exogenous, and if the time of survey is orthogonal to a woman's fertility, then the age or sex composition observed in the household where a woman was exogenously sterilized should not be predicted by the instrument. In tables available upon request, I show that altering the right hand side variable to reflect only working age children (age 7 and up), or restricting the sample to women whose children are at least 10 years of age does not alter the basic result of this table.

Table 7 in the Appendix examines how the coefficient on family size changes if I includes farm assets as an endogenous variable. Although I have provided evidence that the interaction does not predict differential farm asset accumulation, a slightly different approach is to treat the assets on the farm as endogenous, and instrument using rainfall shocks. Including assets as an endogenous variable does not alter the coefficient on family size. In fact, precision of the estimates increases as a result of controlling for farm assets.

6.4. **Supervision on Farm**

The relationships between hired labor and family size is consistent with a story of asymmetric information between family and hired labor. To demonstrate a potential channel that drives

[14]This classification is similar to Maluccio (1997) where he labels pre-harvest tasks as tasks that require "care", like land preparation, fertilizer application, weeding, transplanting and sowing, and irrigation management. Tasks not require "care" include the tasks of harvesting and threshing.

this relationship, however, I need data on monitoring. This is because a story of greater family efficiency in pre-harvest tasks can explain the relationship described above: family members are more efficient at weeding, and hence are preferred to hired laborers. I can address this (and other) competing explanations by directly estimating the effects of family size on monitoring activities of the family.

The REDS contains data on time spent monitoring and the cost of monitoring, and I can estimate the relationship between family size and monitoring effort. Since monitoring is a share of hired labor activity, as hired labor decreases, supervision should also decrease. Intuitively, if informational problems exist and is one of the underlying reasons for the relationship between family size and hired labor use, we should observe monitoring in tasks pre-harvest tasks to decrease with family size.

Table 6 provides evidence that this is indeed the case. Tasks like weeding see a drop in the amount of supervision (as well as costs of supervision) done as family size increases. For every added family member, supervision in weeding decreases by 4 person days. This amounts to almost a 50% decrease in average supervision for the family. However, for tasks like harvesting and threshing we see no significant relationship. Table 7 in the Appendix also shows the same relationship while accounting for endogenous asset formation. By controlling for assets, the results for supervision are more precisely estimated, while the coefficient size remains about the same.

6.5. **Own Farm Family Labor**

Table 7 is similar in spirit to Table 5, except that the dependent variable is days worked by the entire family in the household. As predicted in the theoretical framework, increasing family size causes hired labor usage to decrease in pre-harvest tasks, and family labor should increase in these tasks. In all pre-harvest tasks like weeding and fertilizer application we see an increase in the family labor usage as family size increases, though this is significant only for weeding. In tasks like harvesting and threshing however, while there is no significant effect individually, the combined effect is an increase in family labor. Hence, while the theoretical prediction about casual labor seemed to bear out quite accurately in the data, it appears that family labor increases across the board when family size increases.

6.6. **Alternative ways of avoiding information frictions**

Although the focus of this paper is on labor hired in the spot market, a potential way to overcome informational frictions is to engage in land transactions (either renting of the land or sharecropping) to hire "permanent" labor, or by perhaps even altering crop choice to produce a crop that requires low levels of pre-harvest activity. I examine whether family size systematically relates to one of these ways of avoiding informational frictions.

Land transactions in the data is extremely low (less than 5% of households engage in land transactions over a 20 year period). I examine whether family size plays a role in land contracts in the Appendix (Table 5). It is apparent that land transactions or sharecropping type contracts are not systematically related to family size.

Appendix Table 6 examines the relationship between family size and permanent labor across various tasks. If permanent labor does not suffer from informational problems, possibly through long term relationships with the landlord (Eswaran and Kotwal, 1985), we should perhaps not see a systematic relationship between permanent labor and family size in *any* task. Appendix Table 6 shows that this is indeed the case. However, the incidence of permanent labor on farms is so low that these results should be treated with some caution.

Crop choice is examined in Table 10 in the Appendix. Crops differ in the amount of pre-harvest labor that they require. For example, rice requires plenty of pre-harvest labor in the form of weeding, where as wheat requires much less weeding (Ranjit 1998). It is possible that in order to spend less time in supervision, families switch to growing wheat (say). Hence, we should find that larger families engage in more pre-harvest labor intensive crops than smaller families. However, as Table 10 in the Appendix shows, family size does not play a role in determining crop choice. This is not alltogether surprising as crop choice primarily depends on soil and climactic conditions.

7. **Conclusion**

This paper examines the link between fertility and labor market inefficiencies in rural India. In particular, I examine the relationship between family size, hired labor demand and supervision costs across various agricultural tasks. The theoretical framework suggests that due to the moral hazard induced by informational asymmetry, family labor is preferred to hired labor in some tasks like weeding and fertilizer application, where monitoring costs are high. The framework suggests a simple test: in tasks such as weeding, larger family size should imply less hired labor use, while in tasks such as harvesting, there should be no systematic relationship between family size and hired labor use.

To test these predictions, I use exogenous variation in family size induced by both, cash incentives for sterilization take up and income shocks. I find that during times of low income due to exogenous rainfall shocks, sterilization take up is greater. Using a difference in difference estimator in the first stage of an IV regression, I examine the impact of family size on labor allocations. The empirical results support the theoretical predictions - an increase in family size decreases hired labor use in tasks where worker effort and output is difficult to observe. Moreover, family supervision in these tasks also decreases. By contrast, in tasks where worker effort and output is easily observed, I do not find a relationship between family size and hired labor use.

Hence, population control policies must consider the impact of market inefficiencies on family size in rural areas. For example, I estimate that every added child decreases *total* family labor costs by 6% just through reduced supervision. Hence policies that promote lower family size may be less effective if market inefficiencies exist that make larger families profitable. As this paper shows, labor demand on the farm and fertility decisions are linked. Labor market interventions could thus have implications for family size.

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8. Appendix

8.1. **Modeling the endogeneity problem**

Another way to think about the endogeneity problem is to have families choose N in Phase 1 along with production decisions in that period. With foresight, Phase 2 parameters are already embedded in Phase 1 decisions. Hence the problem can be modeled as:

$$\max_{c,N,L_1^h,L_1^f} U(c,N) \tag{24}$$

$$s.t. \quad c + p_N N = (1 - \frac{w_2}{k}) f(L_1^f + L_1^h; A) - w_1 L_1^h + w_1 L_{fo}^1$$

$$and \quad L_{fo}^1 + L_1^f + \theta L_1^h \leq T(N) \tag{25}$$

L_{fo}^1 is the amount of labor that is supplied outside the household. Given the setup, the optimal amounts of hired and family labor will depend on the parameters of the problem. In particular due to supervision costs, the price of children p_N is now a determinant of the demand for hired labor in Phase 1 tasks:

$$L_h^{*1} = d(p_N, \theta, k, w_1, w2, A) \tag{26}$$

If supervision were not necessary, under the perfect substitution set up above, p_N will not be part of the function determining the demand for hired labor. This is the classic result from agricultural household models regarding the separability of household and production decisions. In Phase 2, where supervision is not required, conditional on obtaining Q_1 from Phase 1, the amounts of hired and family labor used are not a function of p_N. The test for market inefficiency now simply is a test of whether p_N enters the demand for any type of labor in Phase 1 and Phase 2. We expect that Phase 1 labor demand (hired and family) depends on p_N, but that Phase 2 demand, conditional on Q_1 does not depend on p_N. Since observing p_N in the data is almost impossible (Rosenzweig and Wolpin 1980), we can get around having to know prices if we were to treat N as a parameter $\overline{N}$ and vary it exogenously around the planned or optimal N (Tobin and Houthakker 1950), we would get:

$$\frac{dL_1^h}{d\overline{N}} = \frac{\partial L_1^h / \partial p_n}{\partial N / \partial p_n}$$

Hence, exogenously varying N around its optimal amount approximates a price change. An IV strategy hence, approximates a price change in this case. If there were no wedge due to monitoring in Phase 1, $\partial L_1^h / \partial p_n = 0$ and we should find that the $\frac{dL_1^h}{d\overline{N}} = 0$ (this is because $\partial N / \partial p_n \neq 0$). In Phase 2 tasks, conditional on Q_1, we do expect that $\frac{dL_h^2}{d\overline{N}} = 0$. Hence, while the qualitative implications of the tests are the same in this model, and the model presented in the paper, the interpretation of what the instrumental variables regression is doing is slightly different.

8.2. **Assumptions under heterogeneity**

If there is heterogeneity in how sterilization affects family size, further assumptions are needed to identify the coefficient of interest. Under heterogeneity, we can rewrite the equation of interest as:

$$N_i = \beta_i S_i + u_i \tag{27}$$

In the presence of heterogeneity (β_i), we can rewrite the estimating equation in terms of a constant coefficients model:

$$N_i = \overline{\beta} S_i + \underbrace{[u_i + (\beta_i - \overline{\beta}_i) N_i]}_{\epsilon_i} \tag{28}$$

Given that S_i is in the new error term u_i, it is not possible to recover $\overline{\beta}$, even with an IV strategy, without more assumptions. For identification under heterogeneity, we rely on the fact that rainfall is random, that is:

$$E[\beta_i R_i] = E[\epsilon_i R_i] = 0 \tag{29}$$

Since rainfall itself is not an excludable instrument (as argued above), I exploit the fact that the value of the incentive payments (I_i) vary across space. As mentioned before, I cannot use just the incentive payment as an instrument either. However, the interaction is an excludable instrument.

For expositional purposes, consider 2 types of incentive payment areas - high incentive and low incentive areas. Moreover, consider there are 2 types of rainfall - high and low rain.

$$R_i = \begin{cases} 1 & \text{if High Rainfall} \\ 0 & \text{if Low Rainfall} \end{cases}$$

$$I_i = \begin{cases} 1 & \text{if High value of incentive} \\ 0 & \text{if Low value of incentive} \end{cases}$$

Since rainfall is random, *within* the set of high and low incentive payment areas, high and low rainfall should balance the mean return to sterilization:

$$E[\beta_i | I_i = 1, R_i = 1] = E[\beta_i | I_i = 1, R_i = 0] \tag{30}$$

$$E[\beta_i | I_i = 0, R_i = 1] = E[\beta_i | I_i = 0, R_i = 0] \tag{31}$$

The above equations are the core of the identifying strategy under heterogeneity. The interaction of rainfall and the incentive payment as an instrument serves as a difference in difference estimator in this simple framework. While assignment to high and low incentive areas might themselves not be random, the distribution of unobservables that induce people to get sterilized under a random rainfall shock is the same across high and low incentive areas.

$$E[\beta_i | R_i = 1, I_i = 1, N_i = 1] - E[\beta_i | R_i = 0, I_i = 1, N_i = 1] \tag{32}$$

$$= E[\beta_i | R_i = 1, I_i = 0, N_i = 1] - E[\beta_i | R_i = 0, I_i = 0, N_i = 1] \tag{33}$$

Hence, the IV strategy works to identify the coefficient of interest even under heterogeneous returns to sterilization.

8.3. **Controlling for main effects in the cross section**

As mentioned before, the first stage of the hazard framework involves a regression of the following type:

$$S_{it} = \beta_1 I_{it} + \beta_2 R_{it} + \beta_3 I_{it} * R_{it} + \epsilon_{it} \tag{34}$$

Using the hazard framework as explained in section 5.2 I can estimate $\widehat{\beta_1}, \widehat{\beta_2}$ and $\widehat{\beta_3}$. These parameters are key to estimate $\widetilde{p_{it}}$.

Accounting for the history of main effects in the cross sectional second stage is an essential part of the strategy. At a minimum, at least the history of shocks and incentive payments might linearly affect the outcome variable of interest. What if the history of shocks or incentive payments affected the outcome in a non linear fashion?

I present a simple 2 period example that will help clarify the implicit form of the data generating process in the second stage, and how I can perform robustness checks to mitigate any misspecification. In the 2 period case, $\widehat{p_i}$ is calculated as:

$$\widehat{p_i} = \widetilde{p_{iT}} * [1 - \widetilde{p_{iT-1}}] \tag{35}$$

Now consider the *reduced form* second stage, where I have outcomes in 1999 on the left hand side and $\widehat{p_i}$ on the right hand side, while controlling for the mean of rainfall and incentive payment (as mentioned before, at a minimum the histories might affect the outcome linearly).

$$Y_i = \phi_1 \widehat{p_i} + \phi_2 \overline{R_i} + \phi_3 \overline{I_i} + u_i \tag{36}$$

$$\begin{aligned} Y_i = \; & \phi_1[(\widehat{\beta_1} I_{iT} + \widehat{\beta_2} R_{iT} + \widehat{\beta_3} I_{iT} * R_{iT}) \\ & *(1 - (\widehat{\beta_1} I_{iT-1} + \widehat{\beta_2} R_{iT-1} + \widehat{\beta_3} I_{iT-1} * R_{iT-1}))] \\ & + \phi_2 \frac{(R_{iT-1} + R_{iT})}{2} + \phi_3 \frac{(I_{iT-1} + I_{iT})}{2} + u_i \end{aligned} \tag{37}$$

On expansion of equation 37, terms like $R_{iT} R_{iT-1} I_{iT}$ start showing up. These multiplicative terms are a concern depending on what we consider the true data generating process in the cross section to be. If Y_i in the cross section depends linearly on the history of rainfall shocks and incentive payments, then simply putting in the mean of these rainfall shocks and incentive payments is sufficient to rid ϕ_1 of bias[15]. However, if Y_i depends on rainfall shocks and incentive payments in a multiplicative manner, i.e. if terms like $R_{iT} R_{iT-1}$ are in u_i, then just controlling for the mean of R_i and I_i is not enough. For this reason empirical tests are performed using products of the rainfall and the incentive payments as control variables, while controlling for the mean. If rainfall and the incentive payment affect the outcome in non linear ways, then terms other than the mean should play a significant role in determining Y_i. These robustness checks are detailed in Appendix Table 4.

[15]This is true only if we assume that each shock or incentive payment has the *same* effect on the outcome

Another potential way to get around making assumptions about how the way in which main effects affect the outcome variable would be to simply control for the entire history of interactions and main effects for each woman. The problem with such an approach is that women exist in the panel for varying lengths of time. Hence, for some women we only need say five data points, while for others we might need fifteen.

8.4. **Household fixed effects specification**

A different approach is to eliminate the source of endogeneity by using a household fixed effect. The basic idea is to convert the cross sectional data on households, into a panel where each cell is household i in task k on the farm. Suppose task c is a pre-harvest task (like in weeding and fertilizer application), and task d is a harvest task. To examine whether higher family size implies less hired labor L_{ih} in task c, I simply define a dummy D^c, which is 1 if the task is c and 0 otherwise and interact this with family size and include a household fixed effect. While the fixed effect eliminates the main effect of number of children, the differential effect by task on labor hiring is still identified. The specification is:

$$L_{ih}^k = \alpha D_i^c + \rho D_i^c * N_i + \eta_i + u_i^k \tag{38}$$

In such a regression, it is clear that η_i controls for unobserved elements that affect L_{ih} as well as N_i. However, this specification does not allow for correlations between u_i^k and N_i. Hence, while household fixed effects controls for unobservables *across* tasks that might bias ρ, it is not effective against task specific errors that might bias ρ. The theoretical framework specified earlier would in fact suggest that it is precisely the correlations between u_i^k and N_i that we should be worried about. However, the results from such a regression should yield qualitatively similar results. We should expect $\alpha < 0$ and $\rho < 0$ as well. If there is no bias from u_i^k, then the $\rho < 0$ should be similar in magnitude to the IV estimates.

8.4.1. **Results from Household fixed effects model.** The results from Appendix Table 12 are broadly consistent with what I find using the IV strategy. The dummy for pre-harvest tasks is negative and significant for casual labor, indicating that in general less casual labor is hired for these tasks. Moreover, the interaction with number of children is also negative and significant, indicating that even less hired labor is used as family size increases. The results for supervision are also interpreted the same way - while pre-harvest activities in general involve more supervision, as family size increases, less supervision is done in these tasks. The only result that deviates from what I discussed earlier is the result on family labor. While the dummy is positive, the interaction is negative suggesting that less family labor is employed on pre-harvest tasks as family size increases.

TABLE 1: Summary Statistics

	REDS 1999	Mean	Std Deviation
Children	Number of living children	2.83	1.53
	Number of sons	1.65	1.14
	Average age of children	10.32	6.58
Sterilization	Percentage of women sterilized	30.54	
	Average value of sterilziation amount (in 1999 rupees)	499	46.56
	Average value of sterilization in terms of consumption index (kilos of staple grain)	87.11	15.89
	Average number of shocks experienced until sterilization	2.45	2.52
Women	Years of education of women	3.25	3.94
	Average age of women	32.52	7.64
Labor	% of households employing permanent labor for any activity	2.68%	
	% of households employing casual labor for any activity	61.95%	
	Average person days of casual labor employed	122.08	263.53
	Average person days of casual labor employed in pre-harvest tasks	49.36	120.6
	Average person days of family labor employed	124.46	161.4
	Average person days of family labor employed in pre-harvest activities	56.42	76.47
	Average person days spent in supervision	9.78	29.09
	Average person days spent in supervision in pre-harvest tasks	6.57	25.51
Land	Median inherited land holding (acres)	4.24	2.51
	% of households involved in sharecropping	4%	
	% of households involved in renting/leasing of land	7.50%	

TABLE 2: Shocks, Incentives and Sterilization Take Up

Data from NFHS 1998		
	Urban	Rural
	1	2
Shock X Value of Incentive	0.175	0.271
	[0.148]	[0.097]***
Shock	-0.114	-0.268
	[0.133]	[0.091]***
Value of incentive	1.48	1.57
	[0.096]***	[0.062]***
Constant	-4.332	-4.627
	[0.111]***	[0.070]***
Observations	17298	39349

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: Panel logit used for estimation. Years spanned in the data - 1969-1995. State fixed effects and education of the woman included. Shock is defined as 1 if annual rainfall is less than or more than 30% of historical rainfall in that state. Women enter the panel upon marriage and exit in 1995 or upon sterilization, whichever comes first.

TABLE 3: Probability of Sterilization, Shocks and Incentive Payments

Data from REDS 1999				
Specification	Cox Model	Panel Logit		
	1	2	3	4
Shock X Value of incentive	1.906	0.748	0.741	0.785
	[0.661]*	[0.314]**	[0.315]**	[0.374]**
Shock	0.435	-0.938	-0.934	-0.979
	[0.141]***	[0.294]***	[0.294]***	[0.348]***
Value of incentive	1.33	1.047	1.05	0.26
	[0.203]*	[0.135]***	[0.135]***	[0.258]
Observations	63691	63691	63691	63691
Additional controls		State FE	+ Land, Education	3 + Dummies for Calendar Year, Polynomial in Age

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: For each woman, the dependent variable is 0 as long as un-sterilized, 1 at the time of sterilization, and missing thereafter. Data is from REDS 1999. Women are exposed to hazard when they get married, and exit due sterilization, 1999 or age 49, whichever happens first. Sample involves 4,385 women. Predictions from Column 3 used to construct estimated survival function. Shock is defined as 1 if annual rainfall is less than or more than 30% of historical rainfall in that village.

TABLE 4: Number of Living Children and Sterilization Status

Data from REDS 1999						
	All women			Women >=35 yrs		
	OLS	IV Estimates		OLS	IV Estimates	
		First Stage	Second Stage		First Stage	Second Stage
	1	2	3	4	5	6
Sterilization status	0.155		-0.7	0.341		-0.624
	[0.053]***		[0.234]***	[0.086]***		[0.298]**
Probability of Sterilization		17.341			22.084	
		[1.010]***			[1.375]***	
Average Incentive		0.889	3.64		0.781	3.725
		[0.104]***	[0.481]***		[0.142]***	[0.774]***
Average number of shocks		0.888	-0.868		1.936	-0.445
		[0.084]***	[0.422]**		[0.177]***	[1.177]
Observations	4385	4385	4385	1965	1965	1965

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: All IV specifications include standard deviations of the shock and the incentive payment - total number of rainfall shocks experienced over the lifetime (in the hazard) is also included. State fixed effects and polynomial in mother's age also included. Other controls used are education of the woman and amount of inherited land. First stage uses sterilization status in 1999 as the dependent variable. Probability of survival is obtained from the panel hazard model from Table 3.

TABLE 5: Number of Living Children and Hired Labor on Farm

Person days per reference period/Season	Days Spent by Task in Pre-Harvest Activities			
	OLS	IV Estimates		
	Total	Total	Weeding	Fertilizer
Instrumented number of living children[a]	-2.648	-49.064	-32.359	-6.945
	[1.669]	[25.590]*	[13.010]**	[4.094]*

Person days per reference period/Season	Days Spent by Task in Harvest Activities			
	OLS	IV Estimates		
	Total	Total	Harvesting	Threshing
Instrumented number of living children[a]	-1.812	7.457	11.238	-3.781
	[1.294]	[12.630]	[11.657]	[4.124]

Value of Labor in Rs	Imputed Value of Hired Labor			
	OLS	IV Estimates		
	Total	Total	Weeding	Fertilizer
Instrumented number of living children[a]	-181.367	-2628.22	-1081.35	834.297
	[108.936]*	[1634.056]	[433.217]**	[545.690]
Observations	4385	4385	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. Total number of shocks over the lifetime also included. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman, the amount of inherited land and farm assets. The dependent variable in each case is aggregated across all crops and plots belonging to the household. Regressions for harvest activities include labor used in pre-harvest activities as control.

[a] Instrument used is the predicted probability of sterilization as estimated from the hazard. First stage F-stat is around 14. Coefficient on instrument is -14.51 and t-statistic is 3.74.

TABLE 6: Number of Living Children and Supervision on Farm

Person days per reference period/Season	Days Spent by Task in Pre-Harvest Activities			
	OLS	IV Estimates		
	Total	Total	Weeding	Fertilizer
Instrumented number of living children[a]	-0.124	-4.355	-4.441	-0.612
	[0.310]	[4.511]	[1.862]**	[0.448]
Person days per reference period/Season	**Days Spent by Task in Harvest Activities**			
	OLS	IV Estimates		
	Total	Total	Harvesting	Threshing
Instrumented number of living children[a]	-0.055	-1.324	-0.803	-0.647
	[0.107]	[1.195]	[0.913]	[0.563]
Value of Labor in Rs	**Imputed Value of Supervising Labor**			
	OLS	IV Estimates		
	Total	Total	Weeding	Harvesting
Instrumented number of living children[a]	-1.71	-208.549	-38.21	-29.156
	[8.674]	[115.278]*	[24.893]	[20.698]
Observations	4385	4385	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. Total number of shocks over the lifetime also included. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman, the amount of inherited land and farm assets. The dependent variable in each case is aggregated across all crops and plots belonging to the household. Regressions for harvest activities include labor used in pre-harvest activities as control.

[a] Instrument used is the predicted probability of sterilization as estimated from the hazard. First stage F-stat is around 14. Coefficient on instrument is -14.51 and t-statistic is 3.74.

TABLE 7: Number of Living Children and Family Labor on Farm Labor

Person days per reference period/Season	Days Spent by Task in Pre-Harvest Activities			
	OLS	IV Estimates		
	Total	Total	Weeding	Fertilizer
Instrumented number of living children[a]	1.954	9.487	11.356	4.092
	[0.786]**	[27.942]	[6.308]*	[2.180]*
Person days per reference period/Season	**Days Spent by Task in Harvest Activities**			
	OLS	IV Estimates		
	Total	Total	Harvesting	Threshing
Instrumented number of living children[a]	1.149	18.35	13.292	5.058
	[0.428]***	[11.610]	[8.733]	[3.505]
Value of Labor in Rs	**Imputed Value of Child/Youth Labor**			
	OLS	IV Estimates		
	Total	Total	Weeding	Harvesting
Instrumented number of living children[a]	98.143	702.653	562.511	341.649
	[37.888]***	[958.908]	[322.868]*	[213.947]
Observations	4385	4385	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. Total number of shocks over the lifetime also included. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman, the amount of inherited land and farm assets. The dependent variable in each case is aggregated across all crops and plots belonging to the household. Regressions for harvest activities include labor used in pre-harvest activities as control.

[a] Instrument used is the predicted probability of sterilization as estimated from the hazard. First stage F-stat is around 14. Coefficient on instrument is -14.51 and t-statistic is 3.74.

APPENDIX TABLE 1: Robustness Check: Probability of Sterilization, Shocks and Incentive Payments

Data from NFHS 1998 - Rural Sample				
Specification	Cox Model	Panel Logit		
	1	2	3	4
Shock X Value of incentive	2.82	0.271	0.221	0.165
	[0.647]***	[0.097]***	[0.101]**	[0.102]
Shock	0.278	-0.268	-0.229	-0.172
	[0.059]***	[0.091]***	[0.095]**	[0.096]*
Value of incentive	2.171	1.57	1.383	1.194
	[0.258]***	[0.062]***	[0.065]***	[0.065]***
Constant		-4.627	-5.115	-8.167
		[0.070]***	[0.076]***	[0.206]***
Observations	415477	415477	415477	415477
Additional controls		State FE, Education	+ Dummies for time since marriage	+ Polynomial in Age

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: For each woman, the dependent variable is 0 as long as un-sterilized, 1 at the time of sterilization, and missing thereafter. Shock is defined as 1 if annual rainfall is less than or more than 30% of historical rainfall in that village. Data is from NFHS 1998. Rainfall is at the state level, hence coeffcients are different from similar table using REDS 1999 data. Only rural sample is used.

APPENDIX TABLE 2: Does interaction predict farm investment?

Dataset used	REDS 1999	ICRISAT 1975-1985		
	Tubewell installation	Sale of farm assets (value)	Buying livestock asset	Selling livestock asset
	(1)	(2)	(3)	(4)
Shock X Value of incentive	0.136	-731.997	1.836	-1.968
	[0.313]	[486.320]	[1.905]	[1.955]
Shock	-0.09	714.062	-0.576	2.859
	[0.273]	[392.270]*	[1.330]	[1.510]*
Value of incentive	-0.366	343.762	1.767	1.387
	[0.196]*	[686.235]	[2.485]	[3.020]
Constant	-3.676	142.001	-1.317	0.41
	[0.751]***	[388.171]	[2.423]	[2.662]
Observations	86593	1104	1104	1104

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Col 1: Dependent variable is 1 when tubewell was installed, 0 before that and missing after installation. State and year fixed effects included. Estimated using panel logit. Col 2: Dependent variable is the value of farm assets sold in that year. Col 3 & Col 4: Dependent variable is number of livestock animals bought or sold. Also in Col 2-4, year and household fixed effects are included. Cols 2-4 estimated as a linear dependent model. Shock is defined as 1 if annual rainfall is less than or more than 30% of historical rainfall in that state.

APPENDIX TABLE 3: Predicting Sterilization Take Up and Fertility

Data from REDS				
Number of Living Children	All women		Women above 35 years	
	First Stage (Sterilization)	Second Stage (Children)	First Stage (Sterilization)	Second Stage (Children)
Sterilization status		-1.837		-1.043
		[0.787]**		[0.899]
Average of Shock X Incentive	0.657		0.058	
	[0.331]**		[0.467]	
Average Shock	-0.501	-0.094	0.184	0.527
	[0.279]*	[0.226]	[0.397]	[0.489]
Average value of incentive	-0.264	-0.383	-0.734	-1.038
	[0.083]***	[0.408]	[0.091]***	[0.866]
Constant	0.659	1.452	0.98	3.52
	[0.125]***	[0.660]**	[0.129]***	[1.113]***
Observations	4385	4385	1965	1965
F-Statistic		9.1		5.9
Partial R-Squared		0.0042		0.0052

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include standard deviations of the interaction, the shock and the incentive payment. State fixed effects and polynomial in mother's age. First stage uses sterilization status in 1999 as the dependent variable.

APPENDIX TABLE 4: History of Main Effects and their Functional Form

PANEL A	Casual Labor Hired in Pre-Harvest Tasks (days)			
	1	2	3	4
Number of living children	-43.066	-43.15	-42.149	-44.079
	[13.472]***	[18.859]**	[18.587]**	[19.607]**
Observations	4385	4001	4001	4001
Controls	A	B	C	D

PANEL B	Number of living children			
	1	2	3	4
Steriization status	-0.705	-1.086	-1.092	-1.116
	[0.152]***	[0.270]***	[0.272]***	[0.291]***
Observations	4385	4001	4001	4001
Controls	A	B	C	D

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

A : Mean and Standard Deviation of rainfall shocks and incentive payments

B : A+ 3 period lags of rainfall shocks and incentive payments from last period of observation in data

C : B + products of 3 period lags

D : C + squares of 3 period lags

Note: all specifications include mean and standard deviations of the shock and the incentive payment. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman and the amount of inherited land. First stage uses number of living children in 1999 as dependent variable in Panel A and sterilization status in 1999 is the dependent variable in Panel B. Instrument used is the predicted probability of sterilization computed from the hazard model. The dependent variable in each case is aggregated across all crops and plots belonging to the household.

APPENDIX TABLE 5: Land Sales, Sharecropping and Number of Living Children

	Land in Acres			
	Land Rented	Land Shared	Net Sales	Net Purchase
Instrumented number of living children[a]	-0.415	-0.031	-0.628	0.551
	[0.357]	[0.158]	[0.569]	[0.473]
Constant	-8.535	-3.054	-4.982	3.567
	[3.790]**	[1.684]*	[2.889]*	[5.081]
Observations	4179	4179	4120	4120

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman and the amount of inherited land. First stage uses number of children in 1999 as the dependent variable. Instrument used is the predicted probability of sterilization computed from the hazard model.

APPENDIX TABLE 6: Number of Living Children and Permanent Labor on Farm

Days per reference period/Season	Days Spent by Task in Pre-Harvest Activities		
	Total	Weeding	Fertilizer
Instrumented number of living children[a]	8.297	0.727	0.326
	[5.334]	[0.711]	[0.753]
Constant	26.253	-0.21	-3.532
	[52.197]	[6.959]	[7.371]
Observations	4385	4385	4385
Days per reference period/Season	Days Spent by Task in Harvest Activities		
	Total	Harvesting	Threshing
Instrumented number of living children[a]	-0.689	-0.048	-0.642
	[1.777]	[1.215]	[0.713]
Constant	-21.901	-10.343	-11.558
	[17.384]	[11.890]	[6.978]*
Observations	4385	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. Total number of shocks over the lifetime also included. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman and the amount of inherited land. The dependent variable in each case is aggregated across all crops and plots belonging to the household. Regressions for harvest activities include labor used in pre-harvest activities as control.

[a] Instrument used is the predicted probability of sterilization as estimated from the hazard. First stage F-stat is around 14. Coefficient on instrument is -14.51 and t-statistic is 3.74.

APPENDIX TABLE 7: Number of Living Children and Labor Used on the Farm (Assets instrumented by rainfall shocks)

Person days per reference period/Season	Hired Labor IV Estimates for tasks	
	Pre-Harvest	Harvest
Instrumented number of living children	-41.039	4.208
	[11.564]***	[5.018]
Instrumented Value of Assets ('000)	0.52	0.31
	[0.655]	[0.330]
Constant	-212.529	-82.829
	[240.288]	[129.271]
Observations	4385	4385
Person days per reference period/Season	**Supervision IV Estimates for tasks**	
	Pre-Harvest	Harvest
Instrumented number of living children	-4.82	-0.63
	[2.292]**	[0.599]
Instrumented Value of Assets ('000)	-0.057	-0.008
	[0.130]	[0.039]
Constant	-32.547	-9.184
	[47.629]	[15.429]
Observations	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: all specifications include mean and standard deviations of the shock and the incentive payment. State fixed effects and a polynomial in mother's age also included. Other controls used are education of the woman and the amount of inherited land. First stage uses number of children in 1999, and total value of farm assets as the dependent variable. Instrument used is the predicted probability of sterilization computed from the hazard model, as well as history of rainfall shocks captured by the mean, standard deviation and the total number of rainfall shocks. Regressions for harvest activities include labor used in pre-harvest activities as control. First stage F-stats are 4.80 and 27.3 for assets and number of children, respectively.

APPENDIX TABLE 8: Shock Measure Sensitivity

REDS Data			
Logit Model, Dependent variable is sterilization status			
	1	2	3
Shock X Value of Incentive	0.908	1.474	2.162
	[0.331]***	[0.855]*	[1.232]*
Shock	-0.715	-1.785	-2.436
	[0.310]**	[0.777]**	[1.135]**
Value of incentive	0.501	1.13	0.066
	[0.175]***	[0.129]***	[0.309]
Constant	-28.305	-4.538	-27.91
	[2.751]***	[0.325]***	[2.896]***
Measure used	Dummy for 1 std deviation from historical mean	Dummy for less than 50% of historical rain	Percent deivation from historical mean (continuous, 0)
Observations	63691	63691	63691

Standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Note: For each woman, the dependent variable is 0 as long as un-sterilized, 1 at the time of sterilization, and missing thereafter. Data is from REDS 1999. Women are exposed to hazard when they get married, and exit till sterilization, 1999 or age 49, whichever happens first. Sample involves 4,385 women. All regressions use state fixed effects, and controlf for the education and land holding of the woman.

APPENDIX TABLE 9: Household Composition and Instrumented Sterilization

Data from REDS	Sex Composition	Average Age of Children	Brothers and Sisters of HH Head
Instrumented sterilization status	0.126	1.006	0.086
	[0.081]	[0.863]	[0.128]
Constant	0.001	16.742	0.119
	[0.542]	[4.197]***	[0.754]
Observations	4137	4137	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: All regressions control for the mean and standard deviation of the rainshocks as well as the incentive payment. Total number of shocks, education, inherited land holding, and a polynomial in the age of the woman are other included controls. F-stat on first stage is 270.

Appendix Table 10: Number of living children and crop choice

Data from REDS	Crops by weeding intensity	Weed intensive crops
Instrumented number of living children[a]	-0.044	-0.029
	[0.032]	[0.104]
Constant	-0.121	-0.368
	[0.295]	[0.959]
Observations	4385	4385

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Weeding intensity is the fraction of total time spent farming that is spent on weeding. This is calculated at the crop level using national level data. Households growing a certain crop are assigned the particular weeding intensity level. Weed intensive crops in the second column are crops that require more than 40% of total time spent on weeding. Rice, for example is a weed intensive crop, while wheat is not.

[a] Instrument used is the predicted probability of sterilization as estimated from the hazard. First stage F-stat is around 14. Coefficient on instrument is -14.51 and t-statistic is 3.74.

APPENDIX TABLE 11: Household Fixed Effects Estimates

Person days per reference period/Season	Labor Usage		
	Hired	Supervision	Family
Dummy for Pre-Harvest Activities X Number of children	-0.503	-0.102	-0.124
	[0.202]**	[0.018]***	[0.015]***
Dummy for Pre-Harvest Activities	-7.177	0.12	0.167
	[0.811]***	[0.070]*	[0.060]***
Households	3489	3489	3489

Robust standard errors in brackets

* significant at 10%; ** significant at 5%; *** significant at 1%

Notes: Level of observation is household-task. Tasks included are weeding, fertilizer application, sowing, tilling, irrigation management, harvesting and threshing. Only harvesting and threshing are designated as "harvest" tasks. Everything else is a "pre-harvest" task.