ESSAYS ON DEVELOPMENT: EDUCATION AND INTRA-HOUSEHOLD DYNAMICS

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Declaration

I hereby declare that this thesis has not been and will not be submitted in whole or in part to another University for the award of any other degree.

This thesis consists of three papers. The first (*Do preschools add 'value'? Evidence on achievement gaps from rural India*) and third paper (*Female Adult Literacy Programme and Empowerment: Evidence from RCT in rural India*) are single-authored. I produced them under the guidance of my main supervisor Dr. Sonja Fagernas and my second supervisor Dr. Peter Dolton.

The second paper (*Intra-Household Efficiency in Extended Family Households: Evidence from rural India*) is co-authored with Dr. Annemie Maertens and Dr. Christopher Ksoll. The research was made possible through research grant from Social Sciences and Humanities Research Council, Canada on which Dr. Annemie Maertens and Dr. Christopher Ksoll were Principal Investigators. A concise and edited version of the thesis paper has been accepted for publication at the time of submitting this thesis.

I hereby declare that I have cleaned, managed and analysed the quantitative data under the guidance of Dr. Annemie Maertens. Dr. Christopher Ksoll provided input on further exploratory analyses. Dr. Annemie Maertens analysed the qualitative data and has co-written the paper with me.

Signature:

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DOCTOR OF PHILOSOPHY IN ECONOMICS

ESSAYS ON DEVELOPMENT: EDUCATION AND INTRA-HOUSEHOLD DYNAMICS

This thesis empirically investigates three questions of key importance to policy making in developing countries such as India.

The first chapter documents the effect of preschools on cognitive skills in rural India. Using a lagged score value added model, the analysis finds that children who attend preschool before starting primary school have a significant premium in cognitive test scores as compared to children who attend primary school without any preschool exposure. On further investigation into the management type of the preschool, I find that this result is driven by those who attended private preschool.

The second chapter studies the intra-household decision-making process in extended households in rural Uttar Pradesh, India. Using a series of public goods games played between pairs of adult household members, the study finds that spouses residing in extended households are less efficient than those in nuclear households. Moreover, the study finds that relationships within extended households are not equally efficient, with the relationship between daughter-in-law and mother-law particularly inefficient. Supplementary evidence suggests inefficiencies arise from fragmented decision-making power, and limited ability of young married women to assert their preferences in extended households.

The final chapter studies the impact of a two-month long female adult literacy programme on a range of female empowerment measures. Set up as a randomized control trial, the programme increases the treated woman's freedom of movement, such as, going to the shops or calling her natal family without requiring permission. Additionally, the treated woman is more likely to open a personal bank account. An investigation into spillovers reveals that the program has a positive externality on the control woman's freedom of movement as well. This suggests that while the freedom of movement effects might be a result of increased confidence and a 'role model' effect, opening a bank account might be linked directly to becoming literate.

To my parents, who have stood by me and my decisions against all odds

'Educate a girl and change the future.'

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At the very least, this PhD journey has made me realise the numerous blessings I have. In this realization, I am humbled and happy.

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1. Introduction to the thesis

This thesis aims to study three topics relevant for policy making and human development in low- and middle- income countries – early childhood education, decision-making in complex households, and adult literacy programmes as a means of female empowerment.

Educational participation is an important facilitator of economic and social development given its potential to correct for the 'accident of birth'. There is a strong case, in particular, for enrolment in the early years, considering the complementarity between inputs applied at various stages of growing up (Cunha et al., 2006). Participation in education in the early years can shield the child from the negative impact of being born amidst poverty or unstimulating home environment and bridge the skill development gaps, as well as improve access to future opportunities (Cunha & Heckman, 2008; Fryer & Levitt, 2006; Barnett, 2011). However, only about half the children across the world are enrolled at a preschool at the age of 5 (UNESCO, 2020).

While missed development opportunities during the critical years of life continue to drive national policy and international agenda, on the other end of the spectrum, the world has 773 million adults who have had no formal education (UNESCO, 2020). Of these adult illiterates, 63 percent are female (UNESCO, 2020). Hence, adult literacy programmes become crucial not just because functional literacy and numeracy skills have practical value, but also, they are of value to the society in increasing gender equality. The focus on female empowerment is justified because it is an inherent good and because it leads to other desirable development goals, such as investment in household public goods, children's education and nutrition, and a change in gender beliefs and aspirations (see, among others, Ashraf et al., 2010; Beamen et al., 2012; Beamen et al., 2009; Duflo, 2012; Duflo, 2003). Considering that illiteracy is a low- and middle-income country phenomenon, the puzzling question remains - whether adult literacy programmes can mimic some of the benefits of formal education. In particular, can adult literacy programmes targeted at women, be a cost-effective way of achieving empowerment?

One cannot study female empowerment without alluding to the gendered household dynamics that dictates the distribution of labour, income, investment and consumption.

While female empowerment is a wide-ranging concept investigating the relationship of the woman to her society – economic, social and political; the household remains a critical decision-making unit. Thus, understanding the intra-household decision-making process is key to uncovering how household- and gender-based development initiatives need to be designed. This is perhaps why the study of the household is not a new topic in economics and dates all the way back to Becker (1974). However, intra-household economics has continued to ignore the complex nature of household composition, one that may be more than just spouses and their children. Most complex households (co-residence of multiple generations, multiple adult married siblings, polygamous households) are situated in low- and middle-income countries. Add to this, the countries' struggle with human capital development, such as those related to illiteracy and gender inequality – these countries need effective policies to address the social ills. Without a better understanding of decision-making in complex households, we run the risk of poorly designed and targeted policies.

In the remainder of the introduction, I will summarise the methodology and main findings of the three empirical chapters and highlight their contribution to the political and scientific discourse.

In Chapter 2 (*Do preschools add 'value'? Evidence on achievement gaps from rural India*), I study the learning premium of attending a preschool before starting primary school. Moreover, I explore the heterogeneity in the value-added of preschools by their management type.

UNESCO (2020) estimates that almost 80 percent of the children who remain unenrolled in preschools at the age of five are situated in low- and middle-income countries. However, what sets India apart is that despite being a developing country it boasts a preschool enrolment rate equivalent to that of high-income developed nations. Among the sample of children in this study, the preschool enrolment rate is as high as 89 percent for rural households. This is attributable to India's preschool policy which first came into play in 1975. In recent years, India has re-affirmed the importance of preschools in child development in its new National Education Policy (Government of India, 2020) promising that 'provisioning of quality early childhood development, care and education must thus be achieved as soon as possible, and no later than 2030' (para 1.1).

Despite such a long-standing preschool policy and investment in preschool infrastructure in India, a rigorous evaluation of preschools remains virtually absent. Singh and Mukherjee (2017) using Young Lives data from Andhra Pradesh, find long-term effects of private preschool attendance on cognitive skills and subjective well-being at the age of 12. However, this study does not estimate the impact of having preschool exposure (public or private) versus none. Moreover, by looking at the impact of preschool exposure at age 12, it fails to consider the educational participation of the children between ages 6 and 12. A further limitation is the focus on data from Andhra Pradesh; thus the paper fails to address the question of regional heterogeneity in preschool quality in a country as geographically diverse as India. For instance, the preschool funding guideline in India is skewed to benefit economically underdeveloped regions[1]. While the Central government contributes 90 percent of the construction and operational costs in these states, in other states (such as Andhra Pradesh), the Central government contributes 75 percent of the construction cost and 60 percent of the operational cost.

In Chapter 2, I seek to improve on the limited evidence on Indian preschools. I use data from three geographically and economically distinct states in India to provide a more representative evaluation of preschools. I study the immediate (1 year) impact of preschool attendance to minimise the risk of other educational inputs confounding the results. Moreover, I estimate the effect of attending a preschool versus having no preschool exposure. I complement this analysis with a study of private-public gap in learning.

Employing a lagged-score Value Added Model (VAM), I find that there is a positive and significant premium of attending a preschool before starting primary school on the achievement test. However, the entire effect is driven by children who attend private preschools. I find that children who attend public preschools before starting primary school do not have a significant advantage over children who start primary school with no preschool experience. There is considerable regional heterogeneity in the private-public gap in learning levels with Andhra Pradesh exhibiting the highest private

[1] https://www.wcd.nic.in/sites/default/files/AR%202017-18%20Chapter%203.pdf

preschool premium. Conducting additional robustness checks on test score construction and investigating the bias due to child's and parent's motivation, I find that the VAM estimates are reliable.

This chapter's contribution to the literature is threefold. First, it is one of the few large-scale rigorous evaluation of the 40-year long preschool system in India. Second, it contributes to the current literature on the private-public learning gap in India, which has so far neglected the effect of preschools on primary school performance. Third, it contributes to the wider literature on evaluation of universal preschool provision. This literature is sparse, even in developed countries and the results continue to be mixed. While some studies find that universal preschool education is associated with improved literacy and numeracy skills at primary school entry age (for US, see Loeb et al., 2007; Fitzpatrick, 2008; for UK, see Melhuish et al., 2008; for Argentina, see Berlinski et al., 2009), others find that these positive effects dissipate as early as the end of first grade (for US, see Magnuson et al., 2007; for Quebec, see Baker et al., 2008).

The results of this chapter are particularly relevant in the backdrop of a rapidly changing education policy in India. The new National Education Policy (Government of India, 2020) stresses the need to improve foundational literacy and numeracy skills as early as in the preschool years. Given the findings of this chapter, public preschools would need considerable overhaul to be able to deliver on closing the learning gaps. Moreover, the varying levels at which children start primary school based on their preschool experience highlight the need for educators to develop innovative pedagogical tools to effectively address learning heterogeneity within the classroom. 'Teaching at the Right Level' is one such pedagogical innovation developed by Pratham NGO which has been shown promising results (Banerjee et al., 2017; Banerji & Chavan, 2020).

Chapter 3, co-authored with Dr. Annemie Maertens and Dr. Christopher Ksoll, (*Intra-household Efficiency in Extended Family Household: Evidence from rural India*) studies the intra-household decision-making process in complex households in rural Uttar Pradesh, India where multiple generations and/or married siblings co-reside. It documents that the traditional household model (nuclear households) studied in intra-household theoretical literature is qualitatively distinct from complex households. Additionally, even within complex households, the decision-making process is

governed by relationships between in-laws, particularly between a mother-in-law and a daughter-in-law.

The extended household is common in developing countries, especially in South Asia and Sub-Saharan Africa. The benefits of an extended family structure are, in general terms, akin to the benefits of marriage. Gains include cost-sharing of household public goods such as residence, meals, and children; economies of scale and specialization in the production process; and risk-sharing (Becker, 1974; Bergstorm, 1997; LaFave & Thomas, 2017; Rosenzweig & Wolpin, 1985). However, larger households might also suffer from significantly more free-riding, as more adults are in charge of production and public good provision (see Baland et al., 2016; Jakiela & Ozier, 2015, on the effect of a sharing tax and Cox & Fafchamps, 2007, for an overview on extended families and kinship networks more generally).

Despite its importance, there is relatively little literature in economics on the topic of extended families. Most existing literature focuses on the implications for agricultural productivity of African extended families (see Guirkinger et al., 2015; Kazianga & Wahhaj, 2013). However, decision-making in extended households can be key to understanding female empowerment. Dhanaraj and Mahambare (2019), show that norms around decision making of daughters-in-law in extended families prevent these women from taking up employment opportunities. Similarly, Saikia and Singh (2009) find that women in extended households are less likely to utilize maternal health services.

Using a lab-in-the-field public goods experiment in rural Uttar Pradesh, India, the chapter examines Pareto Efficiency of an allocation in which it is not possible to make one individual better off without making another individual worse off. The public goods experiment implemented in this study (and also extensively in other contexts, see Munro, 2015) is designed to uncover inefficiency which arises due to concealing of personal resources instead of contributing them to the household, with potentially larger shared benefits. We draw on the qualitative interviews to argue that household members in extended households do hide resources in processes that our experiment mimics.

Our study shows that spouses in extended households are less efficient in maximizing

surplus than spouses in nuclear households. Within extended households, not all relationships are equally inefficient. Household members related by blood are less prone to inefficient behavior than members related by in-law status, the relationship between mother-in-law and daughter-in-law displaying highest levels of inefficiency. We further supplement the experimental results with survey data on primary decision makers and qualitative interviews. We find that these inefficiencies within extended households exist due to multiple decision-makers and fragmented decision-making power, and limited ability of young married women to assert their preferences in extended households.

These findings fill an important gap in the literature, as the economics literature, has largely struggled to understand complex households, even though they are a central part of many non-Western societies. It is important to understand decision making in extended households in order to study allocation of resources within the household and better design policies that target households.

Expanding on the co-operative bargaining framework within a collective framework, Browning et al. (2014) note that the assumption of efficiency might be violated "when existing social norms impose patterns of behavior that may conflict with efficiency." We show how one such social norm, the norm of patrilocality, relevant in developing countries, undermines efficiency.

Cox and Fafchamps (2007), summarising the extensive literature on the role of kinship (friends and relatives) in risk sharing and inter-household transfers, highlight that 'Too often, economic models are gender blind, populated with generic parents and children and "spouses 1 and 2", rather than husbands, wives, fathers, mothers, sons and daughters.' In this chapter, we unpack these generic 'controls' into explicit relationships within the household that can potentially be used to enrich other economic models.

The results of this chapter have a direct implication for policies that target specific recipients within a household, such as cash-transfer programs in the context of societies with extended households. Duflo (2012) notes the importance of targeting transfers to the 'woman' in the household, with the aim of promoting gender equality as well as improving other desirable outcomes such as health and education. However, we show

that in the context of an extended household, the identity of this 'woman' is ambiguous, so that simply targeting transfers on the basis of gender might fail to achieve desired outcomes.

Chapter 4 (*Female Adult Literacy Programme and Empowerment: Evidence from RCT in rural India*) using a Randomised Control Trial (RCT), analyses the impact of an adult female literacy programme empowerment, measured as decision-making power within the household, freedom of movement, and control over assets.

India, the country in which this study is set, accounts for a third of the world's illiterate population at 252 million illiterate adults (UNESCO, 2020). The Indian Census (2011) puts the adult (18 years and above) female illiteracy rate at 43 percent versus 22 percent for men[2]. Given the size of the illiterate population in India and the associated gender bias, the provision of adult literacy programmes is common and desired. The Government of India launched the Sakshar Bharat adult literacy campaign in 2009 with an additional focus on closing the gender gap in literacy. By focusing on women, these campaigns also aim to promote female empowerment. However, there continues to be a paucity of evidence on adult literacy programmes beyond the intended effect of achieving functional literacy and numeracy.

In India, there are two main studies looking at the impact of literacy programme on female empowerment. While Banerji et al. (2017) find no significant impact of a maternal literacy programme on mothers' decision-making power, Kandpal et al. (2012) find a positive and significant impact of a female adult literacy programme on female empowerment measures. The insignificant intent-to-treat effects reported by Banerji et al. (2017) can be attributed to the low literacy programme take-up in their sample – self-reported records show that 40 percent of the mothers attended these classes. On the other hand, the positive impacts documented by Kandpal et al. (2012) could be due to selection on unobservables. The authors rely on propensity score matching between women from treated and un-treated districts based on observables combined with an IV strategy using the roll-out of the programme. The identification strategy raises the concern if the programme was purposely rolled out in some districts because of worse

[2] Calculated from Table DDW-0000C-O8, Census of India, 2011. Restricted to all persons above the age of 17 years. Literacy is defined as being able to read and write.

gender equality metrics in those areas, which can lead to biased estimates. Second, Kandpal et al. (2012) use a very narrow set of three measures for empowerment, namely having a National Rural Employment Guarantee card, the ability to leave the house without permission, and participation in village council meetings. It may be argued that these are not sufficient proxies for female empowerment and a more robust study is required that encompasses a wider range of measures.

I use an RCT to overcome the selection issue like Banerji et al. (2017), but with a greater treatment compliance of 79 percent of the treatment group women attending the literacy classes. To address limitations of female empowerment measurement, I use a wide range indicators and indices - decision-making on different subjects, the ability to leave the house freely for a range of tasks, and control of different financial assets.

I find that the literacy programme had a significant and substantial increase in woman's ability to leave the house without having to seek permission, and an increase in the likelihood of the woman having a personal bank account. I find no evidence of an increase in decision-making power over daily household decisions such as those involving household purchases and cooking. The chapter also studies a limited form of spillover where the control group woman lives at the same location as a treatment group woman. I find suggestive evidence of positive spillovers, but I have low power to detect significance. I hypothesise that these spillovers may be a 'role model' (Beamen et al., 2012) effect where the aspirations of the control group women shift due to observing the change in behaviour of the treatment group women. Investigating the correlates of the programme take-up, I find that women from backward and scheduled castes (the lower castes) were more likely to take up the literacy programme.

Since the literacy programme required women to leave the house daily to attend these classes, this might explain the impacts I find on mobility indicators. Alternatively, this change may be due to an increase in self-confidence and self-esteem as suggested by qualitative research (see Egbo, 2000 in Nigeria; Archer & Cottingham, 1997 in Bangladesh; Stromquist, 1997 in Brazil). The increased likelihood of owning a personal bank account, post the literacy programme, may be a more direct result of becoming literate if the women are now being able to read, fill out, and sign bank forms.

The null results for within household decision making may point to the strong

patriarchal family and societal structures that exist in India, particularly in rural Uttar Pradesh. Other studies in Uttar Pradesh looking at the impact of formal education on female empowerment found similar null results on decision-making power of the woman within the household, while finding positive impacts on women's mobility (Bloom et al., 2001; Jejeebhoy & Sathar, 2001).

This study contributes to the sparse literature on socio-economic impacts of adult literacy programmes. It is one of the few rigorous evaluations of adult literacy programmes. In finding positive impacts of the literacy programme on empowerment, this paper opens up exciting avenues for the evaluation of such programmes. There is a need for evaluation projects to include a wider range of measures to capture the change in self-esteem, beliefs about the self and gender roles, and aspirations to understand how literacy translates into female empowerment. Moreover, there is a need to push the frontier of the measurement of female empowerment to find more reliable measures that are appropriate for different contexts (such as, experimental measures explored in Almas et al., 2018).

In the final chapter, Chapter 5 of this thesis, I reflect on the limitations of each empirical chapter and discuss the potential for future research in the light of the findings of this thesis.

2. Do preschools add ‘value’? Evidence on achievement gaps from rural India.

2.1. **Introduction**

The preschool system in India, as anywhere else, is the first step towards education. India stands out from most developing countries, in its recognition of the importance of preschool education as early as 1975. In recent years, the Indian education policy has seen rapid and welcome changes. While the landmark Right of Children to Free and Compulsory Education (RTE) Act, 2009 guaranteed the access to free and quality education to all children aged 6-14 as a fundamental right, it excluded early childhood education from its legal ambit. The National Education Policy (Government of India, 2020) stands to correct its former mistake. The policy acknowledges that with lack of preschool exposure, a large proportion of children fall behind in learning levels, within a few weeks of starting Grade 1 (Government of India, 2020, para 2.5). Thus, preschools have been brought to centre stage by the new policy, promising that 'provisioning of quality early childhood development, care and education must thus be achieved as soon as possible, and no later than 2030' (Government of India, 2020, para 1.1).

The uniqueness of the Indian education system lies in the coexistence of two parallel sectors – a low cost fee-charging private and a free-of-cost public (government) sector. This introduces a degree of variability in the schooling trajectory followed by Indian children, and hence can potentially produce variability in learning levels.

Public preschools in India, commonly known as *anganwadis/balwadis* are part of the bigger umbrella program – Integrated Child Development Services (ICDS). The ICDS scheme has been in implementation since 1975 and performs six services – supplementary nutrition, preschool education, immunisation, health check-up, referral services, and nutrition and health education to mothers. There are 1.3 million ICDS centres across the country, with the policy stipulating that there be at least one centre in every village[3]. Public preschools are expected to cater to children in the age group 3 to 6 years of age, and contribute to the universalisation of primary education by providing

[3] https://www.wcd.nic.in/sites/default/files/AR%202017-18%20Chapter%203.pdf

necessary preparation for primary schooling.

Private preschools, on the other hand, are fee charging institutions, consisting of nursery and/or kindergarten classes. Their main draw is English language instruction. They are more formal in their structure and organisation with well-defined curricula and teaching hours.

The quality of public preschool education is often seen as poor, partly due to the Indian education policy failing to incorporate preschool education formally into its pedagogical framework. In reality, ICDS centres have come to be seen as health centres for children in early years, with the preschool function reduced to a free day care facility. A major shift in this realm comes with the new National Education Policy (2020), which will bring the preschool function of ICDS formally into what it terms 'school clusters'. This would imply that preschool education function of ICDS would shift from the Ministry of Women and Child Development to the Ministry of Human Resource Development[4]. This anticipated shift will integrate the preschool years with the rest of the education system in India, allowing the National Council of Educational Research and Training (NCERT)[5] to develop preschool curricula and pedagogy.

While variability in learning outcome due to the diverse private and public sector in education is well documented in India at the primary school level (Muralidharan & Sundararaman, 2015), very little is known at the school entry age or before that. Studies that document the learning gaps in the private and public sectors have overwhelmingly focussed on primary school without any knowledge of the early childhood years. One needs a careful assessment of the learning gap literature in India – whether these are gaps that arise due to primary school education or whether these are pre-existing gaps decreasing/increasing over time. From the policy perspective, it is vital to know when and where public spending should be focused to yield the highest return.

There is widespread recognition of the fact that early childhood factors and environment

[4] The Ministry of Human resource Development, India oversees all aspects of education – primary, secondary, higher education, technical and vocational training centres.
[5] NCERT currently oversees the development of curriculum, pedagogy, teacher training of all primary and secondary education in India.

have a significant impact on future outcomes, cognitive and non-cognitive. Quality early childhood education can improve children's learning skills and help with the transition to primary school (see Yoshikawa et al., 2013 for a review). Given such evidence, the less than satisfactory evaluation of preschool education in India is a major limitation. One of the main reasons for such an omission, is the lack of data in the education sector, and even more so in the preschool sector. The data set I use for this study is the only large-scale data set I know of which specifically aimed to collect information on preschools in India[6].

In this chapter, I attempt to address this gap in literature and study the differences in test scores, which exist even before starting primary school, due to preschool participation and the public-private preschool divide in India. Specifically, using the ASER data on Early Childhood Education collected in 2011-12, I present the estimates from Value Added Models (VAM) of the effect of participation in preschools on test scores. I explore the differential impact of attending a private and public preschool and discuss the quality differences in the two management types. Moreover, I study the regional heterogeneity and find that achievement gaps vary by Indian states, drawing caution on interpreting studies based on data from a single Indian state as a universal estimate for a country as diverse as India.

2.2. **Related Literature**

Early years of life are critical for the acquisition of skills and concepts. While positive experiences are thought to be crucial in determining the formation of cognitive and non-cognitive skills (Cunha & Heckman, 2008), negative experiences in the form of poverty, malnutrition, and unstimulating home environment can be detrimental to cognitive, motor, and socio-economic skill development (Grantham-McGregor et al., 2007). Since skill begets skill and there is complementarity between inputs applied at various stages of growing up (Cunha et al., 2006) there is a strong case for intervention in the preschool years. Although certain socio-emotional functions and health can be observed even before the age of three (preschool starting age), most successful early childhood

[6] ASER recently conducted a national survey in 2019 of children's enrolment status and skills with a focus on early years – ages 4 to 8. However, the survey does not have any information on the preschools themselves. It is also limited in its information on households.

interventions begin in preschool years. These can also be complemented with earlier 'antenatal investment' (Doyle et al., 2009).

There is now a large body of literature which documents the effectiveness of early childhood interventions, particularly in the US (Heckman & Mosso, 2014). In the context of the US, much of the literature to explain when and why gaps in cognitive (and non-cognitive) achievement surface has focused on the racial bias (see Fryer & Levitt, 2004, 2006). The second theme in early childhood intervention research in the US has been to document the persistent positive impacts of such interventions into adulthood – for example, Perry Preschool Project in the US (Schweinhart et al., 2005), and Head Start Preschool intervention (Garces et al., 2002). While the results from these studies are useful, the programmes evaluated involved disadvantaged children from select cities in the US. The evidence on the impact of universal preschool policy in developed countries remains scarce, and the evidence on short-run outcomes is mixed. While some studies find that universal preschool education is associated with improved literacy and numeracy skills at primary school entry age (for US, see Loeb et al., 2007; Fitzpatrick, 2008; for UK, see Melhuish et al., 2008), others find that these positive effects dissipate as early as the end of first grade (for US, see Magnuson et al., 2007; for Quebec, see Baker et al., 2008).

On the other hand, studies in developing countries overwhelmingly document positive effects of universal preschool on future educational outcomes. This is likely due to preschools providing an opportunity for positive developmental environment to children exposed to poverty, inadequate home stimulation and nutrition in developing countries (Grantham-McGregor et al., 2007). While an estimated 500 million children under the age of 5 live in developing countries with India accounting for 20 percent of the children, the evidence on the evaluation of preschools in such regions remain scarce.

In developing countries, the evidence on evaluation of universal preschool is even more limited. Berlinski, Galiani, and Manacorda (2008) study the effect of preschool education on years of education using a Uruguayan household survey. They use the within household estimator exploiting the variation in education trajectories between siblings. The authors report that by the age of 15, children who had attended preschools accumulate 0.8 years of extra education when compared to their untreated siblings.

In another study from Argentina, Berlinski, Galiani, and Gertler (2009) investigate the impact of large scale expansion of universal preschool education on subsequent primary school performance, and find that one year of preschool education increases the average third grade test scores by 23 percent of the standard deviation.

In Cambodia, Rao et al. (2012) evaluate the effectiveness of the different early childhood programmes and find that ,while some programmes are more effective than others, some preschool experience is better than none at all. However, a recent study evaluating the impact of preschool construction in Cambodia on children's short-term cognitive and socio-emotional development finds that there are no impacts of preschool attendance (Bouguen et al., 2018). Further, they find that there are significant negative impacts of preschool attendance on children with the longest exposure to preschools.

In urban Ethiopia, Woldehanna (2016) using Young Lives data find that preschool attendance is correlated with better cognitive performance at the primary school starting age of five.

Other than the above-mentioned studies, there have been smaller sample studies. Mwaura et al. (2008) study the impact of preschool experience on cognitive achievement in a sample of 423 children in East Africa under a quasi-experimental framework. They find that children who went to Madrasa type preschools (faith-based organization) performed better than those who attended non-Madrasa type preschools or none. Moore et al. (2008) design a pre-post intervention-control framework to evaluate the effect of revised preschool versus a regular preschool in rural Bangladesh. In their sample of 138 children, they find that after seven months, children in the revised program performed better than those in the regular program, although the quality of the regular program had also improved. Most of these studies suffer from the problem of small sample and focus on comparing different type of preschools rather than a universal preschool programme.

A related strand of literature from developing countries looks at the impact of quality of preschools on child outcomes – for instance, the effect of teacher quality (Araujo et al., 2016 in Ecuador; Wolf et al., 2019 in Ghana; Yoshikawa et al., 2015 in Chile) and the

effect of increasing preschool and parent communication (Ozler et al., 2016 in Malawi)

In the context of India, there are two papers that evaluate universal preschool provision. In Andhra Pradesh, India, Singh and Mukherjee (2017) employ propensity score matching and find long-term effects of private preschool attendance on cognitive skills and subjective well-being at the age of 12. However, this study does not estimate the impact of having preschool exposure (public or private) versus none. Moreover, by looking at the impact of preschool exposure at age 12, it fails to consider the educational participation of the children between ages 6 and 12. A further limitation is the focus on data from Andhra Pradesh; thereby, failing to address the question of regional heterogeneity in preschool quality in a country as diverse as India.

Another study using Young Lives data from Andhra Pradesh, India, demonstrates that test score gaps between children in schools exist even at the school-entry age, and this gap can in part be attributed to attending a preschool and type of preschool attended (Singh, 2014). However, the author mentions that drawing causality is beyond the scope of his paper and is at most able to establish correlations. This paper serves as a valid starting point for my exercise – once established, that test score gaps exist even before starting primary school, I attempt to explain such a gap through preschool attendance and the management type of the preschools.

In this study, I seek to improve on the limited evidence on Indian preschools. First, I use data from three geographically and economically distinct states in India to provide a more representative evaluation of preschools. Second, I study the immediate (1 year) impact of preschool attendance to minimise the risk of other educational inputs confounding the results. Third, I estimate the effect of attending a preschool versus having no preschool exposure. I complement this analysis with a study of private-public gap in learning.

The focus on management type of preschools is motivated by the existing literature on the private-public achievement gap divide in India. The private sector in Indian education has been growing rapidly in the last two decades (Kingdon, 2007), and it is now well-known that there are significant gaps in the average achievement scores between private and public schools in India. Muralidharan and Kremer (2008) find that

private unaided low fee-charging schools are widespread in rural India, particularly in areas where the public system is dysfunctional. This is a result of both demand-side variables (desire for English medium instruction, smaller classes, and more accountable teachers) and supply-side variables (availability of educated unemployed youth).

It has been found that private schools are associated with higher student achievement even after accounting for pre-existing differences in socio-economic background using a range of econometric methodologies. French and Kingdon (2010) use family fixed effects and within household variation to control for selection into private schools. Desai et al. (2009) use Heckman selection correction model using the existence of private school in the village as an exclusion restriction. Chudgar and Quin (2012) find positive effects of attending private primary schools while using regression analysis. However, when they conduct regressions on matched samples, the private school gain is less consistent across specifications. Muralidharan and Sundararaman (2015) do not find across the board gains of attending private schools in their experimental approach (school choice voucher scheme) and claim that private school children perform better in certain subjects (English and Hindi), but not in others (Telugu, Maths and Environmental Studies). Singh (2015) shows that private primary schools show significant positive gains in certain domains and age groups using Value Added Model, and that these results match up to the estimates of the experimental study of Muralidharan and Sundararaman (2015).

Most of these studies in India and beyond (with the exception of Singh & Mukherjee, 2017; Singh, 2014), have focused only on primary schools without any reference to prior preschool education. Given the widespread recognition of the importance of early childhood factors on future cognitive outcomes, this omission is a major limitation to the literature as it stands today.

I study the impact of preschool on cognitive achievement, and in particular, the differential impact of public versus private preschools. Since the question is similar to the literature which exists for primary schools in India, one could potentially use any one of the empirical strategies described earlier. However, family fixed effects are not satisfactory as parents can change their behavior based on preschool experience and it also requires assuming that there is perfect knowledge of intra-household allocation

between siblings. Coming across a valid instrument which only affects school choice and not educational outcome is also a tall order. The instrument used by Desai et al. (2009) being, whether the village has a private facility, cannot satisfy the exclusion restriction. As noted, the presence of private facilities can be driven by demand side variables like the aspirations of parents and community. This would also affect the educational outcome.

An alternative identification strategy under-utilized in such research questions is one of lagged score Value Added Models (VAM). VAMs are used extensively in teacher and class effectiveness literature, particularly in the US. Overall evidence suggests that lagged score VAM estimates are valid and consistent, estimating average treatment effects with limited bias. Kane and Staiger (2008) while analyzing results from an experiment in Los Angeles that assigned children randomly across classrooms, report that teacher effects estimated from lagged score VAM yielded similar unbiased results. Andrabi et al. (2011) while documenting the evidence of public-private school test score gap in Pakistan, show that VAM estimates obtained from OLS provide similar results as the estimates from data extensive GMM estimation methods. Chetty et al. (2014) find no evidence of bias in VAM estimates when studying the long-term impact of teachers on adult outcomes.

2.3. **Data**

2.3.1. **Sampling**

The data for this paper has been provided by ASER, India which had been collected as part of their 5-year longitudinal study, Early Childhood Education Impact Study[7]. This chapter only covers two rounds of the data collection – the first round in September-December 2011 and the second round in October-December 2012.

The data covers three major states of India – Andhra Pradesh, Assam, and Rajasthan. States were purposively selected to maximize differences in geographical location as well as demographic, socio-economic, and educational characteristics. Within each state, two districts were selected at random for inclusion in the study - Medak and

[7] http://www.asercentre.org/Keywords/p/342.html#br03d

Warangal in Andhra Pradesh, Dibrugarh and Kamrup in Assam, and Ajmer and Alwar in Rajasthan. Within each district, a total of 50 villages were selected with a population of between 2000-4000. Given that the primary objective of this study was to examine the relationship between preschool and learning outcomes, sampling of villages was deliberately restricted to larger villages in order to maximize the likelihood of finding different types of preschool facilities (public and private) within a single village. Systematic random sampling was utilized in order to ensure that at least one village was included from each block in the district.

Within each village, the objective was to select 50 children in the age group 3.5-4.5 years at the time of the first round (September-December 2011). Integrated Child Development Services (ICDS) records were used to create a sample of all children in the above-mentioned age group. These records are maintained by government (Anganwadi) workers in each village. If the number of children in the required age group exceeded 50, then 50 children were randomly selected. If this number was less than 50, then all the children in the village were selected. In theory, at most 2500 children should have been selected for each district. However, in practice this was not achieved. Table 2.1 shows the distribution of the sampled children across the six districts and three states. While 42 percent of the children are in Rajasthan, 31 percent are in Assam and 27 percent are in Andhra Pradesh. Moving from Round 1 to Round 2 of the data collection, the study was able to track 89 percent of the children, with Rajasthan having the lowest attrition rate. This paper utilises the sample of 8124 children who are present in both Rounds 1 and 2.

Table 2.1. Distribution of sample by state and district

State	District	Sample Size at Round 1	Sample Size at Round 2	% of Round 1
Andhra Pradesh	Warrangal	1031	931	90.3
	Medak	1477	1265	85.6
Assam	Kamrup	1662	1450	87.2
	Dibrugarh	1163	998	85.8
Rajasthan	Alwar	1896	1762	92.9
	Ajmer	1892	1718	90.8
Total		**9121**	**8124**	**89.1**

This table presents the sample size in each district surveyed at Round 1 and at Round 2. Round 1 was conducted in Sept-Dec 2011 and Round 2 was conducted roughly a year apart in Oct-Dec 2012.

2.3.2. **Survey and Questionnaire**

During 2011-2012, sampled children were visited four times, approximately once every three months. The first round of data collection occurred in September-December 2011 and the second round in October-December 2012. Between Rounds 1 and 2, two tracking visits occurred. Table 2.2 shows the information collected in each round.

Table 2.2. Timeline of survey and information collected

Survey instrument	**Round 1** Sep - Dec 2011	**Tracking Visit 1** Feb – Mar 2012	**Tracking Visit 2** Jul – Aug 2012	**Round 2** Oct - Dec 2012
Household questionnaire	X			
Assessment	X			X
Child tracking	X	X	X	X
Preschool questionnaire	X			X

The household questionnaire includes detailed information on the level of education of the parents, employment status, religion, caste, consumer durables owned by the household, sampled child's learning environment, and questions on parent's aspirations and expectations from preschool. The questions on parent's aspirations and expectations from preschool were only administered to parents where the child was enrolled in a preschool.

The child tracking was used to only track the enrolment status of the child – whether the sampled child was going to a preschool, or a primary school.

The preschool questionnaire was conducted for all preschools in the village, irrespective of whether the sampled child was enrolled in them or not. Key aspects of infrastructure, classroom teaching observation, and availability of learning materials for children were observed in each preschool facility visited. However, the data provided for this paper did not link the preschool to the sampled child. Additionally, no unique preschool identifier was used between Rounds 1 and 2, which implies that I cannot link the preschools from Round 2 with Round 1.

The assessment tool used for this study is the School Readiness Inventory (SRI). It was administered one-on-one by a trained field investigator to the children at home. The test

was developed by the World Bank in conjunction with Centre for Early Childhood Education and Development, New Delhi. It is intended to test children's cognitive skill, and early language and numeracy skills[8]. Within these broad categories, the children were administered 24 items. Appendix A Table A.1 gives detail of the breakdown of the test.

I used a two-parameter logistic (2-PL) model of the Item Response Theory (IRT) to evaluate the performance of each item in uncovering the latent trait/skill parameter. Based on this model, I found Items 22 and 23 to perform poorly; and hence, excluded them for calculating the total score. Appendix A Section A.1 details the methodology used to construct the test score. While one can use test scores generated by IRT, for ease of interpretation, I do not do so in the main paper[9]. Instead, I assign a point for each of the 22 items administered and calculate the total test score. This is referred to as the raw score in the paper and ranges from 0 to 22. Second, I standardise this test score to have a mean of 0 and standard deviation of 1. This standardised score is used in all analyses. Children were assessed twice roughly a year apart. I shall refer to the test score from Round 1 as Lagged test score and the test score from Round 2 as Current test score.

2.3.3. **Participation status**

Table 2.3 summarises the participation trajectory of the 8124 sampled children from Round 1 to Round 2. Only 1.2 percent of the children (N=100) remain unenrolled by Round 2 and all these children were from Rajasthan. 89 percent of the children were already attending a preschool at the time of the first data collection (Round 1). This is unsurprising as enrolment rates have been consistently high for India in the recent years – for instance, ASER Early Years Report (ASER Centre, 2020) documents that 84 percent of their nationally representative rural sample of 4-year old children were enrolled in preschool. Of these children in my data, most continued to attend a preschool in Round 2. 1861 children started attending a primary school in Round 2 after preschool in Round 1 - 95 percent of these children were in Rajasthan or Andhra Pradesh. This is because the school starting age in Rajasthan and Andhra Pradesh is 5

[8]While most School Readiness instruments administered in early childhood studies also have dimensions on socio-emotional skills and motor skills (for e.g., see Yoshikawa et al., 2015; Wolf et al., 2019), the SRI tool administered in this study had a narrow focus on cognitive and language skills.

[9] In Appendix A Table A.3 and Table A.4, I report the main results using IRT constructed scores.

years, while it is 6 years in Assam. However, the slight anomaly, are the children (9 percent of the overall sample or 761 children) who were already attending a primary school at Round 1 and continue to do so in Round 2. While officially these children would be too young to be attending primary school, it is common for the enforcement of formal school entry regulations to be lax[10]. Given the difference in educational norms and trends by states, I control for village fixed effects in all my analysis. The choice to have village instead of state fixed effects is to capture the differences in facilities provision by village.

Table 2.3. Distribution of sample by educational participation

Participation	**Total**			**Andhra Pradesh**			**Assam**			**Rajasthan**		
Round 2 / Round 1	Not enrolled	Pre school	Primary school	Not enrolled	Pre school	Primary school	Not enrolled	Pre school	Primary school	Not enrolled	Pre school	Primary school
Not enrolled	100			0			0			100		
Preschool		5402	1861		1258	787		2350	98		1794	979
Primary school			761			151			0			610

This table presents the sample by each educational participation category, and by the states.

I categorise the children who were attending preschool in both Rounds 1 and 2, as children who have been to preschool (67 percent of the overall sample or 5402 children). The children who attended preschool in Round 1 and then enrolled in a primary school in Round 2 are categorised as children who have been to both preschool and school (23 percent of the overall sample or 1861 children). The children who were attending primary school in both Rounds 1 and 2 are treated as primary school goers (9 percent of the overall sample or 761 children) without having ever attended a preschool. The last category are the children who are never enrolled (1 percent of the overall sample or 100 children).

For additional analysis, I also categorise the children by the management type of preschools as shown in Table 2.4. 49 percent of those who were attending preschool, attended a private preschool – majority of these children are from Rajasthan. Children who attended a public preschool are mostly located in Assam. Most children, who start going to a primary school in Round 2 after preschool, come from a public preschool.

[10] For example, see Singh (2020) where he documents that there is no regression discontinuity involving official school age entry for the Indian sample. Also, see ASER Early Years (ASER Centre, 2020) which documents 8 percent of their nationally representative rural sample at age 4 were enrolled in primary school.

Table 2.4. Distribution of preschool goers by management type

	Overall	Andhra Pradesh	Assam	Rajasthan
Attending private preschool	2649	881	451	1317
Attending public preschool	2753	377	1899	477
Attending private preschool and school	623	117	5	501
Attending public preschool and school	1238	670	93	475

The table provides a further breakdown for children who either are or have attended a preschool by management type. The 5402 children who have been attending a preschool in both Rounds 1 and 2 are further distinguished into 'attending a private preschool' and 'attending a public preschool'. The 1861 children who were attending a preschool in Round 1 and started going to a primary school in Round 2 are further distinguished as attending a private preschool and school; and attending a public preschool and school.

I keep children who have attended both preschool and primary school as a separate category because the change in test scores from Round 1 to Round 2 is now a function of both preschool and primary school input. Using the data from tracking visits, I confirm that these 1861 children would still have had substantial exposure to preschool between Rounds 1 and 2. Table 2.5 shows that no child switched to a primary school in February. Most children switch in July – this is expected because the academic calendar runs from June/July in Rajasthan and Andhra Pradesh (and from January in Assam). Based on these tracking visits, I can confirm that these children would have had at least six months of preschool exposure after Round 1. Hence, it is reasonable to assume that the value added between Rounds 1 and 2 would be a function of both preschool and primary school.

Table 2.5. Switching from preschool to primary school

From preschool in Round 1 to primary school in Round 2	N
Switch occurs at tracking visit 1 (Feb-Mar 2012)	0
Switch occurs at tracking visit 2 (Jul-Aug 2012)	1215
Switch occurs at Round 2 (Sept-Dec 2012)	646
Sample size	1861

This table shows the approximate time when the 1861 children who were attending a preschool in Round 1 would have switched to a primary school.

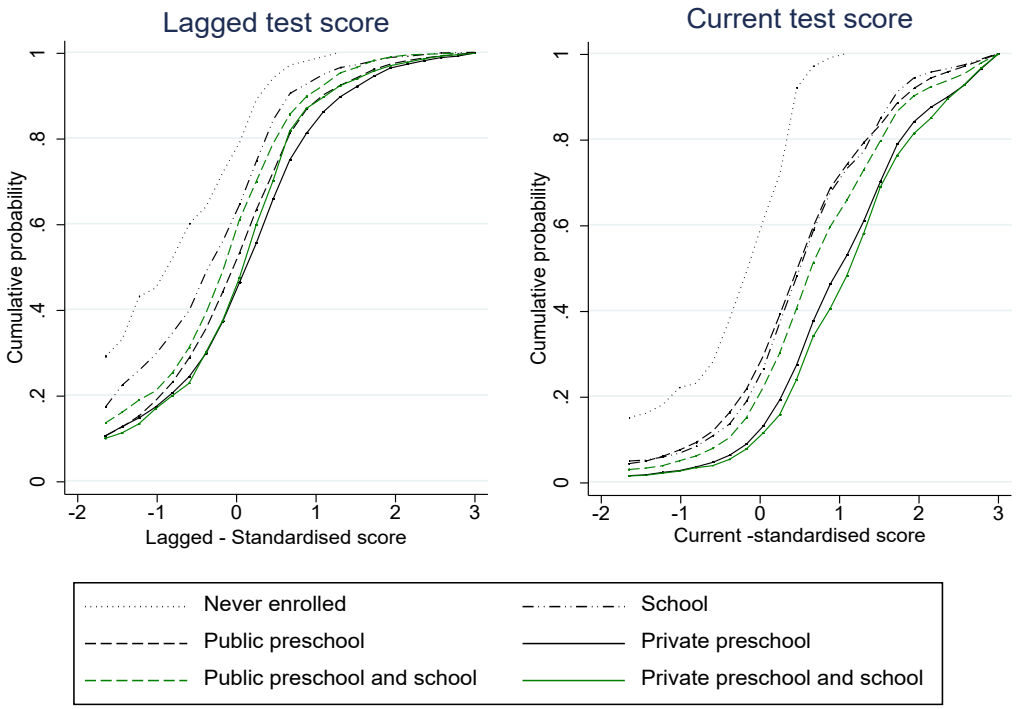
2.3.4. **Test scores**

Figure 2.1 presents the distribution of the standardised score at Round 1 and Round 2 by participation status of the children. I distinguish the preschool goers further by

management type. The test scores are presented by the following categories – never enrolled, going to a primary school (with no previous preschool exposure), going to a public preschool, going to a private preschool, going to a public preschool with primary school, and going to a private preschool with primary school.

Looking at the lagged test score, there emerges a clear hierarchy in selection – children who are not enrolled performing the worst, followed by children in primary school, then children in public preschool, and finally children in private preschool. When the children are tested again after one year in Round 2, all categories see a reduction in the proportion of children scoring very low. This could be a result of being tested on the same tool and the resulting familiarity or the effect of age. By Round 2, the primary school sample has caught up with the public preschool sample, with the two distributions almost overlapping. The public preschool goers who have started attending a primary school at Round 2 are slightly better off than those with only public preschool or with only primary school. The biggest gain in test scores come from the private preschool goers. The private preschool goers who may have attended at most six months of primary school before Round 2 testing are best performers in the sample.

Figure 2.1. Distribution of test scores by participation categories



I present further summary statistics on the test scores by particpation categories in Table 2.6. Looking at the raw test score, the average score for the overall sample is quite low at 7.8 (out of a total of 22) in Round 1 and just about half of the total at 11.8 in Round 2. There is a substantial proportion of children who score 0 on the test in Round 1 (12 percent of the overall sample). Most of these children are those who were not enrolled. This proportion drops across all particpation categories in Round 2. Additionally, while less than 1 percent of the overall sample score the full total of 22 in Round 1, in Round 2, 2.3 percent of the overall sample achieve a full score.

Table 2.6. Summary statistics on test score by participation categories

	Never enrolled	Primary School	Private preschool	Public preschool	Private preschool and school	Public preschool and school	Total
Round 1							
Raw score	4.46	6.36	8.55	7.89	8.26	7.19	7.84
Proportion scoring 0	0.29	0.17	0.11	0.11	0.10	0.14	0.12
Proportion scoring full	0	0.001	0.007	0.004	0.003	0	0.004
Standardised score	-0.714	-0.313	0.150	0.0101	0.0883	-0.138	0.0
Round 2							
Raw score	6.70	10.79	13.10	10.69	13.47	11.65	11.80
Proportion scoring 0	0.15	0.049	0.016	0.043	0.016	0.031	0.032
Proportion scoring full	0	0.013	0.034	0.015	0.032	0.020	0.023
Standardised score	-0.241	0.624	1.112	0.602	1.188	0.805	0.836
N	100	761	2649	2753	623	1238	8124

This table presents different statistics on the test score in Round 1 (lagged) and in Round 2 (current) by the educational participation categories. The categories are never enrolled, primary school with no preschool exposure, attending a private preschool in both Rounds 1 and 2, attending a public preschool in both Rounds 1 and 2, attending a private preschool before starting primary school, attending a public preschool before starting primary school, and the overall sample. The raw score is sum of correctly answered questions and ranges from 0 to 22. The standardised score is the raw score standardised using the Round 1 mean and standard deviation.

2.3.5. **Sample characteristics**

Table 2.7 reports the mean (and standard deviation) for the children by the participation categories. Column 1 reports the summary statistics for never enrolled; Column 2 for children in primary school (with no preschool exposure); Columns 3 for preschool (private and public) goers; Column 4 for private preschool goers; Column 5 for public preschool goers; Column 6 for children with both preschool (private and public) and primary school; Columns 7 and 8 differentiate the preschool participation among these children by private and public management types respectively; and Column 9 reports the summary statistics for the entire sample[11].

Older children are more likely to be in primary school or to have switched to primary school from preschool. Within preschools, private preschool goers tend to be marginally older than public preschool goers. Girls, muslims, scheduled caste, and scheduled tribe are less likely to have attended a private preschool. Children from scheduled caste and scheduled tribe are more likely to attend primary school, while muslims are more likely

[11] In Appendix A, Section A.6, I present the results of a multinomial logit on choice of educational participation for a more nuanced exercise of understanding how the observable characteristics affect participation.

to not be enrolled.

Parent's education, wealth index, and consumer durable index are associated with private preschool attendance. The poorest families are most likely to send their child to a public preschool. If both parents are employed outside the household, the child is more likely to have attended a preschool, in particular a public preschool. This might be because public preschool are more informal in set up and they tend to be used as free crèche facilities in villages. Households having children's reading material and play material are more likely to send the child to some educational institute as against not enrolling he child.

Table 2.7. Child and household characteristics by participation categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Preschool			**Mixed**		
	Never Enrolled	School	Total	Private preschool	Public preschool	Total	Private preschool and school	Public preschool and school	Full sample
Female	0.490	0.502	0.479	0.427	0.530	0.492	0.427	0.525	0.484
	[0.502]	[0.500]	[0.500]	[0.495]	[0.499]	[0.500]	[0.495]	[0.500]	[0.500]
Age in months	62.681	63.712	62.890	63.132	62.657	63.648	63.893	63.525	63.138
	[3.313]	[3.185]	[3.431]	[3.301]	[3.537]	[3.288]	[3.368]	[3.241]	[3.394]
Years of education - Father	4.960	5.506	6.279	7.747	4.866	5.500	7.398	4.545	6.012
	[4.452]	[4.546]	[4.699]	[4.653]	[4.293]	[4.898]	[4.956]	[4.582]	[4.743]
Years of education - Mother	0.630	1.970	4.180	5.144	3.254	2.794	3.909	2.233	3.612
	[1.905]	[3.282]	[4.412]	[4.701]	[3.896]	[3.792]	[4.195]	[3.439]	[4.244]
Both parents work outside of home	0.040	0.188	0.277	0.277	0.276	0.379	0.169	0.485	0.289
	[0.197]	[0.391]	[0.447]	[0.448]	[0.447]	[0.485]	[0.375]	[0.500]	[0.453]
Muslim (Base category: Hindu)	0.380	0.192	0.168	0.102	0.231	0.114	0.093	0.124	0.160
	[0.488]	[0.394]	[0.374]	[0.303]	[0.422]	[0.318]	[0.291]	[0.330]	[0.367]
Scheduled caste	0.160	0.233	0.136	0.127	0.145	0.228	0.141	0.272	0.167
	[0.368]	[0.423]	[0.343]	[0.333]	[0.352]	[0.420]	[0.349]	[0.445]	[0.373]
Scheduled tribe	0.100	0.129	0.091	0.067	0.114	0.114	0.108	0.118	0.100
	[0.302]	[0.335]	[0.287]	[0.250]	[0.318]	[0.318]	[0.310]	[0.323]	[0.300]
Backward castes	0.540	0.489	0.501	0.589	0.416	0.504	0.551	0.481	0.501
	[0.501]	[0.500]	[0.500]	[0.492]	[0.493]	[0.500]	[0.498]	[0.500]	[0.500]
Wealth index	-0.167	-0.167	0.026	0.458	-0.389	0.011	0.298	-0.133	0.002
	[0.872]	[0.862]	[1.031]	[0.967]	[0.913]	[0.936]	[0.980]	[0.879]	[0.995]
Ownership of durables index	-0.052	-0.068	-0.011	0.455	-0.459	0.082	0.271	-0.013	0.005
	[0.997]	[0.924]	[1.021]	[0.972]	[0.852]	[0.940]	[1.019]	[0.883]	[0.994]
HH has children's reading material	0.640	0.813	0.812	0.810	0.814	0.747	0.880	0.681	0.795
	[0.482]	[0.390]	[0.391]	[0.392]	[0.389]	[0.435]	[0.326]	[0.466]	[0.404]
HH has toys/games for child	0.140	0.229	0.372	0.440	0.305	0.325	0.379	0.298	0.345
	[0.349]	[0.420]	[0.483]	[0.497]	[0.461]	[0.469]	[0.485]	[0.458]	[0.475]
N	100	761	5402	2649	2753	1861	623	1238	8124

The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. Scheduled caste, scheduled tribe and backward castes are dummy variables with general caste as the base category. The wealth index comprises of household building material (bricks as against mud/straw), having a toilet, piped water, electricity and using higher grade fuel (LPG as against kerosene or wood or cow dung) for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.

Table 2.8 provides the summary statistics on additional variables capturing child and parent motivation. Enrolment in preschool is associated with higher likelihood of households to engage in home learning activities, such as reading a story to the child and helping the child with learning at home. However, if attending a preschool requires more home study, a household may be more likely to enage in such activities. While all information on these controls come from the Round 1 survey, as seen in Table 2.2, the children were already attending an institution in Round 1. Hence, it is likely that households have changed their input in response to the school/preschool input. Because of this concern, I do not use these variables in my main regressions, but only as robustness checks.

Additional questions relating to parent and child motivation were only adminitered to children in preschool. Parents who switch their child to a primary school by Round 2 report lower probability of engaging with preschool staff in Round 1. The proportion of parents wanting their child to learn to read and write is highest for children who go to a private preschool with primary school. Children who went to a private preschool and then a primary school are most likely to report liking going to a preschool. These additional questions are not used in the main regressions and only as robustness checks.

Table 2.8. Parent's and child's motivation by participation categories

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Preschool			**Mixed**		
	Never Enrolled	School	Total	Private preschool	Public preschool	Total	Private preschool and school	Public preschool and school	Full sample
Parent's motivation									
Reads story to child	0.040	0.089	0.393	0.308	0.475	0.204	0.199	0.206	0.317
	[0.197]	[0.285]	[0.489]	[0.462]	[0.499]	[0.403]	[0.400]	[0.405]	[0.465]
Helps with learning tasks	0.090	0.298	0.629	0.607	0.651	0.467	0.621	0.390	0.555
	[0.288]	[0.458]	[0.483]	[0.489]	[0.477]	[0.499]	[0.485]	[0.488]	[0.497]
Talk to staff about child's learning progress			0.329	0.318	0.339	0.275	0.345	0.239	
			[0.470]	[0.466]	[0.473]	[0.446]	[0.476]	[0.427]	
Wants child to read/write			0.775	0.760	0.789	0.760	0.827	0.726	
			[0.418]	[0.427]	[0.408]	[0.427]	[0.379]	[0.446]	
Child's motivation									
Child talks about preschool always			0.342	0.301	0.380	0.210	0.246	0.192	
			[0.474]	[0.459]	[0.486]	[0.407]	[0.431]	[0.394]	
Child talks about preschool sometimes			0.372	0.393	0.351	0.436	0.395	0.457	
			[0.483]	[0.488]	[0.477]	[0.496]	[0.489]	[0.498]	
Child likes going to preschool			0.610	0.598	0.622	0.538	0.748	0.433	
			[0.488]	[0.490]	[0.485]	[0.499]	[0.435]	[0.496]	
N	100	761	5402	2649	2753	1861	623	1238	8124

This table presents the mean and standard deviation in parenthesis on child and parent motivation variables by the educational participation categories. While all variables were administered to children attending preschool in Round 1, only a subset were administered to the full sample of children. All variables reported in this table come from Round 1 survey. Reads story to the child is a dummy variable which takes the value of 0 if no-one in the household reads story to the child at least once a week. Helps with learning tasks takes the value of 0 if no one in the household helps the child with homework at least once a week. Talks to staff about child's learning progress takes the value of 0 if the parent has not spoken to the staff in the past 3 months. The base category for the child talks about preschool always and sometimes is that the child never talks about the preschool.

2.4. **Method**

2.4.1. **Value Added Model – theoretical derivation**

The basis of the value-added model, used in recent literature, is a structural cumulative effects model developed by Boardman and Murnane (1979). Following Todd and Wolpin (2003) and Todd and Wolpin (2007), the general functional form is as follows,

$$T_{it} = T_t[F_i(t), S_i(t), X_i(t), \mu_{i0}, \varepsilon_{it}] \quad (1)$$

where T_{it} is a measure of achievement for child i at the end of the t-th year of life, F_i, S_i and X_i are the family, school and individual based input histories up to age t respectively, μ_{i0} is the time invariant individual endowment[12], and ε_{it} is a time varying error term.

Assuming the function in (1) is additively separable and non-age varying, we arrive at the cumulative effects model or the distributed lag model.

$$T_{it} = \alpha_1 F_{it} + \alpha_2 F_{i(t-1)} + \cdots + \alpha_t F_{i1} + \beta_1 S_{it} + \beta_2 S_{i(t-1)} + \cdots + \beta_t S_{i1} + \gamma_1 X_{it} + \gamma_2 X_{i(t-1)} + \cdots + \gamma_t X_{i1} + \phi_t \mu_{i0} + \varepsilon_{it} \quad (2)$$

It is important to note that linearity and additive separability are trivial assumptions to ease computability and interpretation. This is the most commonly used formulation of the cumulative effects model[13]. One can easily test if the functional form is mis-specified by introducing polynomials or using logarithmic transformation.

Second, non-age varying assumption implies that the impact of any input on achievement varies within the time period of application of the input and realization of achievement; however, it does not matter at which age or time period the input is applied. For example, it is assumed that the effect of a small class size at the age of 6 on achievement score at age 7 is the same as the effect of small class size at the age of 8 on the achievement score at age 9. This might seem like an unreasonable assumption, given the evidence for greater returns to investing in human capital in the early years (see

[12] This can be thought of as genetic endowment or ability which is fixed at conception and does not vary over time. This is not to say that the effect of the endowment is fixed with time. The functional form allows ability to have different effects over time, that is, it allows for the notion that higher ability children may learn faster.

[13] An exception is Harris (2007) who uses a trans-log functional form.

Cunha et al., 2006; Doyle et al., 2009). Although one can easily introduce extra interaction terms and allow for age varying intercepts, this is not ideal, due to loss of degrees of freedom and issues of multicollinearity.

It is important to note here that the μ_{i0} term in (2) remains untouched by the non-age varying assumption. The effect of ability can be interpreted in two ways in equation (2). First, ability can be thought of as fixed at conception, but having varying effects at different ages of the child, which is what ϕ_t would capture. Second, ability can be thought of as malleable and changing from the initial endowment. Given that ability cannot be observed, one cannot estimate the parameter on ability and observationally, both interpretations of the function of ability will give the same result.

Estimating equation (2) is difficult as data which tracks the child right from birth till current period and has information on inputs at every stage is impossible to come by. Also, one can easily see lag terms to be highly correlated with each other, giving little meaningful information to researchers and policy makers. If one is willing to assume geometric decay of prior inputs, and that this geometric decay parameter is the same for all prior inputs, we have $\alpha_t = \lambda\alpha_{t-1}$; $\beta_t = \lambda\beta_{t-1}$; $\gamma_t = \lambda\gamma_{t-1}$ where $0 \leq \lambda \leq 1$. The equation now becomes –

$$T_{it} = \alpha_1 F_{it} + \lambda\alpha_1 F_{it-1} + \cdots + \lambda^{t-1}\alpha_1 F_{i1} + \beta_1 S_{it} + \lambda\beta_1 S_{i(t-1)} + \cdots + \lambda^{t-1}\beta_1 S_{i1} + \gamma_1 X_{it} + \lambda\gamma_1 X_{i(t-1)} + \cdots + \lambda^{t-1}\gamma_1 X_{i1} + \phi_t\mu_{i0} + \varepsilon_{it} \quad (3)$$

Subtracting $\lambda T_{i(t-1)}$ from both sides of equation (3), we have,

$$T_{it} = \lambda T_{i(t-1)} + \alpha_1 F_{it} + \beta_1 S_{it} + \gamma_1 X_{it} + (\phi_t - \lambda\phi_{t-1})\mu_{ij0} + \varepsilon_{it} - \lambda\varepsilon_{i(t-1)} \quad (4)$$

The process described by geometric decay is well documented in literature – Banerjee et al. (2007) report that the 1-year treatment effect of educational intervention on test scores fade out by the 3rd year; Currie and Thomas (2000) and Lee et al. (1990) also show similar fading out of the Head Start preschool program, at least on achievement scores. If the effect of initial ability on achievement changes at a constant rate, then we finally have –

$$T_{it} = \lambda T_{i(t-1)} + \alpha_1 F_{it} + \beta_1 S_{it} + \gamma_1 X_{it} + v_i + \eta_i \quad (5)$$

where $v_i = \phi\mu_{i0}$ and $\eta_i = \varepsilon_{it} - \lambda\varepsilon_{i(t-1)}$

Equation (5), is commonly known as the lagged score value added model (VAM). This is not the only specification of VAM in common use. The other two versions are the highly restrictive contemporaneous VAM which assumes immediate decay of prior inputs or $\lambda = 0$, and the gain score specification, which assumes that there is perfect persistence or $\lambda = 1$. While the former assumes that inputs in previous years have no impact in current year, the latter assumes that inputs in previous years have full (the same effect as they would have had in *t*-1) effect in current year. Hence, lagged score VAM is the least restrictive. I use the lagged score VAM as my main specification throughout the paper, while also reporting the results from contemporaneous VAM and perfect persistence VAM.

2.4.2. **Value Added Model – estimated specification**

This paper uses the lagged VAM as the main specification in the analysis.

$$T_{it} = \lambda T_{i(t-1)} + \beta_{\text{pre}}\text{preschool}_{\text{it}} + \beta_{\text{pri}}\text{primary school}_{\text{it}} + \beta_{\text{mixed}}\text{preschool and school}_{it} + \delta_1\text{village}_{\text{i}} + \alpha_1 F_{it} + \gamma_1 X_{it} + \eta_i \quad (6)$$

where *preschool* is a dummy variable for having attended only preschool and not yet started primary school, *primary school* is a dummy variable for having attended only primary school with no preschool exposure, and *preschool and school* is a dummy variable for children who have started primary school after attending a preschool. The base category is for children who are not enrolled. The regression controls for village fixed effects ($village_i$) to ensure that differences in educational infrastructure provisioning at the village level is controlled for. The standard errors are clustered at the village level to account for the fact that sampling was not at random; deliberately choosing larger villages, and for spatial correlation within villages. I report equation (6) without household (F_{it}) and child (X_{it}) controls, and equation (6) with all controls. I also additionally report the results from contemporaneous VAM ($\lambda = 0$) and perfect persistence VAM ($\lambda = 1$).

The model in (6) is estimated using Dynamic OLS (DOLS). This estimation may be vulnerable to bias from two main sources. First, the identification of preschool and primary school effects relies on the assumption that the lagged test score is a sufficient

proxy for the unobserved ability (v_i in eq (5)). This assumption may be violated if parents use more information than those captured in equation (6) while making a decision to send their child to an educational institute. It may also be violated if the unobserved ability does not decay at the same rate as the lagged achievement or if it has time varying effects. Since one cannot observe inherent ability, this is akin to saying that talented children learn faster. Both these cases would lead to an upward bias in our β coefficients of interest. Following Singh (2015) who uses DOLS estimation of lagged score VAM to study the differential impact of private and public primary school, I employ a series of robustness checks to ascertain if indeed there is a potential bias from lagged score being a poor proxy of innate ability.

Second, conditioning on lagged test score may introduce a measurement bias, which would attenuate the persistence coefficient (λ), and consequently bias the β coefficients of interest in an unknown direction. The precise bias on β coefficients will depend on the degree of correlation with lagged inputs, which are all now a part of the error term. Ideally, one would want to control for IQ or mental ability along with test scores (as suggested in Todd & Wolpin, 2003), as this would circumvent the measurement error. However, I am unable to do so since there is no data on IQ for my sample. Andrabi et al. (2011) discuss this issue in depth and show how correcting only for measurement error in their sample results in worse estimates for the variable of interest.

There may be concern around using DOLS estimation with lagged score as the lagged test score will be correlated with $\eta_i = \varepsilon_{it} - \lambda\varepsilon_{i(t-1)}$. However, as long as $\lambda < 1$, the DOLS estimation is asymptotically consistent. Indeed, the literature on VAM has found the persistence parameter to be less than 0.5 in most cases (see Andrabi et al., 2011; Kane & Staiger, 2008; Rothstein, 2010).

VAMs have been used extensively in the education literature, mostly in the US teacher value added empirical work. However, a separate strand studying the effects of different management type of schools and its impact is closest in application to this paper here. The work on effects of charter schools (for instance, see Hanushek et al., 2007; Sass, 2006) and the effects of private school (for instance, see Andrabi et al., 2011; Singh, 2015) have shown that VAMs are indeed a reliable identification tool.

Additionally, work by Guarino et al. (2015) on comparing different estimators of VAMs have stressed the superiority of DOLS as an efficient and consistent estimator. They assess the reliability of different VAMs estimators for recovering teacher effects using simulated data with a variety of non-random teacher-student assignment structure. They find that DOLS estimator performed robustly across most scenarios; better than other estimators, namely, Arellano-Bond panel data estimators, pooled OLS on gain score VAM specification, random effects model on gain score VAM, fixed effects model on gain score VAM, and average residual approach. They report that 'the main strength of this (*referring to DOLS)* estimator lies in the fact that, by including prior achievement on the right hand side, it controls whether directly or indirectly for grouping and assignment mechanisms' (Guarino et al., 2015, p.30). Hence, by allowing the lagged test score and the variable of interest to be correlated, DOLS takes care of the selection issue.

Andrabi et al. (2011) while studying the impact of private schools on cognitive achievement for Pakistan report that 'despite ignoring measurement error and unobserved heterogeneity, the lagged value-added model estimated by DOLS gives similar results for the private school effects as our more data intensive dynamic panel methods, although persistence remains overstated. The relative success of the lagged VAM can be explained by the countervailing heterogeneity and measurement error biases on persistence parameter and because lagged achievement can also act as a partial proxy for omitted heterogeneity in learning' (Andrabi et al., 2011, p.31).

At this stage, I would like to draw a distinction between technology parameter (ceteris-paribus effect) and the policy effect (total effect) (Todd & Wolpin, 2003). Since VAMs are not the same as the cumulative effects structural model (equation (2)), one must remember that we are no longer estimating the technology parameter in the lagged score VAM. Thus, there is a need for caution as to which variables are included as controls – for example, one must not control for the channels through which private preschool choice would have an effect on learning because that would be part of the 'policy effect'. As soon as one controls for current family inputs or children's behavior, which might have changed due to the preschool choice, one is no longer calculating the average treatment effect, but the technology parameter. I will refrain from estimating the latter as there is not enough data or theory to guide the set of variables to be

included.

One of the implications of this distinction, is that much of the criticism around VAM applied to teacher performance literature, primarily in the US, is due to researchers trying to evaluate teacher value added without controlling for change in the family input, resulting from being assigned to a low quality (or high quality) teacher. Since most of the papers engaged in calculating teacher value added (technology parameter) use school administration data, they have little information on households. In such a scenario, estimation involves assuming that household effect is time-invariant. Such an assumption would lead to misclassification of teachers. As shown by Guarino et al. (2015) and Sass et al. (2014), varying VAM specifications and estimation methods typically misclassify teachers, even though they provide reliable estimates of the average effect. As such, the scope of this chapter is not to distill the individual preschool fixed effects, but to assess the average treatment effect of preschool. Thus, most of the criticism around VAM stemming from the application of this model to teacher value added is not valid for my exercise in this chapter.

2.5. **Results**

2.5.1. **Preschool value added**

In Table 2.9, I present the results of value added by preschools as compared to not enrolled, primary school (with no preschool exposure) and both preschool and primary school. Not enrolled serves as the base category in these estimations. However, as noted in Table 2.3, only 1 percent of the sample are not enrolled, and they are all located in Rajasthan. There might be concern over the reliability of the estimates using this category. In Appendix A Table A.5, I report the regressions on a sub-sample excluding the not enrolled category. The estimates remain significant and qualitatively similar to those reported here.

In Table 2.9, Columns 1 and 2 assume instant decay of input and are the results from contemporaneous VAM. Columns 3 and 4 assume perfect persistence of past inputs. Columns 5 and 6 are my preferred specification of the lagged score VAM. Straightaway, we find that our coefficients of interest are biased upwards in

contemporaneous VAM and biased downwards in a perfect persistence VAM. Columns 1, 3, and 5 have no controls. Columns 2, 4 and 6 have household and child level controls. The effects of controls are as expected and documented in the literature – girls and children belonging to socially disadvantaged groups perform worse on the test; older children, children from more educated parents and richer household perform better on the test.

Coming to the preferred specification (Column 6), there is a positive and significant effect of going to a preschool or a primary school or a preschool with primary school vis-à-vis children who are not enrolled anywhere. Going to a preschool increases the test score by 0.44 SD units, going to a primary school increases the test score by 0.53 SD units, and going to a preschool with primary school increases the test score by 0.67 SD units. These effects are large, but expected, as the base category are the children who have never been enrolled in any educational institute.

The more interesting comparison is children who attended primary school (with no exposure to preschool) and children who attended preschool. I find that there is no premium on test score of attending a preschool – in fact, these children perform worse than the children enrolled in primary school by 0.09 SD unit (significant at 5 percent). However, since teaching in primary school is more instructional and formal, and children are more familiar with test taking scenarios, it would be unfair to compare children who are yet to attend primary school with children who have been attending primary school for a while.

As discussed earlier in Section 2.3.3, some of the children who attended preschool also start going to primary school by Round 2. To truly gauge if attending a preschool before starting primary school has a premium, I compare the group of children with both preschool and primary school exposure to children with only primary school exposure. Children who attended preschool before starting primary school have a significant (at 1 percent) premium of 0.14 SD unit over children with only primary school experience. Hence, while it seems that preschool children lag behind in achievement tests at first glance, they seem to reap the benefits of their preschool experience when they enter

primary school[14].

Table 2.9. Preschool VAM estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
Lagged - Standardised score	0	0	1	1	0.276***	0.225***
					(0.019)	(0.017)
Preschool	0.657***	0.515***	0.196**	0.174*	0.530***	0.438***
	(0.081)	(0.085)	(0.093)	(0.095)	(0.079)	(0.083)
Primary school	0.625***	0.604***	0.245**	0.257**	0.520***	0.526***
	(0.085)	(0.088)	(0.100)	(0.101)	(0.083)	(0.086)
Preschool and school	0.837***	0.750***	0.378***	0.377***	0.711***	0.666***
	(0.084)	(0.087)	(0.097)	(0.098)	(0.082)	(0.086)
Female		-0.104***		0.004		-0.080***
		(0.020)		(0.024)		(0.019)
Age in months		0.026***		0.002		0.021***
		(0.004)		(0.004)		(0.003)
Years of education - Father		0.014***		0.007**		0.012***
		(0.003)		(0.003)		(0.003)
Years of education - Mother		0.024***		0.005		0.020***
		(0.003)		(0.003)		(0.003)
Both parents work outside		-0.101***		-0.008		-0.080**
		(0.037)		(0.044)		(0.036)
Muslim (Base category: Hindu)		-0.156***		-0.023		-0.126***
		(0.047)		(0.062)		(0.047)
Scheduled caste		-0.199***		-0.020		-0.159***
		(0.044)		(0.058)		(0.043)
Scheduled tribe		-0.184***		0.092		-0.122**
		(0.060)		(0.072)		(0.057)
Backward castes		-0.073**		0.074		-0.040
		(0.037)		(0.046)		(0.035)
Wealth index		0.039**		0.014		0.034**
		(0.016)		(0.021)		(0.016)
Ownership of durables index		0.082***		0.021		0.068***
		(0.015)		(0.019)		(0.015)
HH has children's reading material		0.042		-0.042		0.023
		(0.028)		(0.037)		(0.027)
HH has toys/games for child		0.042		-0.029		0.026
		(0.028)		(0.036)		(0.027)
Constant	-0.687***	-2.249***	-0.240***	-0.392	-0.564***	-1.832***
	(0.079)	(0.239)	(0.091)	(0.279)	(0.077)	(0.226)
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,124	8,124	8,124	8,124	8,124	8,124
R-squared	0.257	0.313	0.244	0.248	0.312	0.348
Preschool=School F-stat	0.411	3.987**	0.961	2.652	0.0413	4.215**
Preschool=Mixed F-stat	22.32***	44.16***	22.46***	26.88***	25.38***	44.21***

[14] There may be a concern that since the switch from preschool to primary school happens between Round 1 and Round 2, it could be due to an unobservable shock, which would no longer be captured by the controls and the lagged score. I re-run this analysis without the mixed (preschool and primary school) sample. The results are reported in Appendix A Table A.9. The results are qualitatively similar for the coefficients on preschool and primary school.

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
School=Mixed F-stat	18.38***	9.707***	5.311**	4.383**	15.80***	9.280***

All specifications control for village fixed effects. Standard errors are clustered at the village level. The table also reports the F-stat from testing equality of coefficient between preschool and primary school; between preschool and preschool with primary school (mixed); and between primary school and mixed. The variables of interest are preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), primary school (attending primary schools in Rounds 1 and 2 with no preschool exposure) and attending preschool before starting primary school. The base category is not enrolled. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

2.5.2. **Private preschool value added**

There is well-documented evidence of the public-private divide in the Indian context (see French & Kingdon, 2010; Desai et al., 2009; Chudgar & Quin, 2012). Given the rapidly growing private sector in the Indian education landscape and the significant gaps in learning due to management type heterogeneity, I delve deeper into the preschool effect. Instead of looking at just the preschool variable, I now differentiate the children as going to a public or private preschool.

Table 2.10 presents the results of value added by preschool type[15]. Columns 1 and 2 assume instant decay of input and are the results from contemporaneous VAM. Columns 3 and 4 assume perfect persistence of past inputs. Columns 5 and 6 are my preferred specification of the lagged score VAM. I find that the coefficients of interest are biased upwards in contemporaneous VAM and biased downwards in a perfect persistence VAM. Columns 1, 3, and 5 have no controls. Columns 2, 4 and 6 have household and child level controls.

Coming to the preferred specification (Column 6), there is a positive and significant effect of going to a private preschool. Children from private preschool have a value-added premium of 0.62 SD units (significant at 1 percent) when compared to children from public preschool. Additionally, they score 0.13 SD units higher (significant at 1 percent) on the test than children with only primary school exposure.

On the other hand, attending a public preschool barely has a premium on achievement even when compared to children who are not enrolled – a insignificant premium of 0.08

[15] In Appendix A Table A.6, I report the regressions on a sub-sample excluding the not enrolled category. The estimates remain significant and qualitatively similar to those reported here.

SD unit. These children from public preschool do significantly worse on test scores when compared to their private preschool counterpart as well as the primary school category.

When one looks at children with both public preschool and primary school exposure, the value-added coefficient is 0.59 SD unit. This is not significantly different from that of children with only primary school experience. Hence, the effects of preschool that we saw in Section 2.5.1, were entirely driven by children who attend private preschools[16].

[16] There may be a concern that since the switch from preschool to primary school happens between Round 1 and Round 2, it could be due to an unobservable shock, which would no longer be captured by the controls and the lagged score. I re-run this analysis without the mixed (preschool and primary school) sample. The results are reported in Appendix A Table A.9. The results are qualitatively similar for the coefficients on private preschool, public preschool and primary school.

Table 2.10. Private preschool VAM estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
Lagged - Standardised score	0	0	1	1	0.224***	0.198***
					(0.018)	(0.017)
Private preschool	0.942***	0.790***	0.339***	0.338***	0.806***	0.700***
	(0.079)	(0.083)	(0.097)	(0.098)	(0.079)	(0.083)
Public preschool	0.105	0.116	-0.058	-0.053	0.068	0.083
	(0.079)	(0.082)	(0.095)	(0.096)	(0.078)	(0.081)
Primary school	0.674***	0.645***	0.261***	0.272***	0.581***	0.571***
	(0.083)	(0.085)	(0.100)	(0.101)	(0.081)	(0.084)
Private preschool and school	1.157***	1.032***	0.373***	0.379***	0.981***	0.903***
	(0.090)	(0.094)	(0.105)	(0.106)	(0.087)	(0.091)
Public preschool and school	0.669***	0.638***	0.385***	0.399***	0.606***	0.591***
	(0.082)	(0.085)	(0.099)	(0.100)	(0.082)	(0.084)
Female		-0.066***		0.023		-0.048***
		(0.019)		(0.024)		(0.019)
Age in months		0.022***		-0.001		0.017***
		(0.003)		(0.004)		(0.003)
Years of education - Father		0.010***		0.005		0.009***
		(0.003)		(0.003)		(0.003)
Years of education - Mother		0.019***		0.002		0.015***
		(0.003)		(0.003)		(0.003)
Both parents work outside of home		-0.074**		0.007		-0.058*
		(0.036)		(0.044)		(0.035)
Muslim (Base category: Hindu)		-0.096**		0.008		-0.075
		(0.046)		(0.063)		(0.046)
Scheduled caste		-0.100**		0.022		-0.076*
		(0.043)		(0.058)		(0.042)
Scheduled tribe		-0.110*		0.133*		-0.062
		(0.057)		(0.072)		(0.055)
Backward castes		-0.043		0.089*		-0.017
		(0.035)		(0.046)		(0.034)
Wealth index		0.010		-0.001		0.008
		(0.015)		(0.020)		(0.015)
Ownership of durables index		0.060***		0.009		0.050***
		(0.015)		(0.019)		(0.014)
HH has children's reading material		0.027		-0.046		0.013
		(0.028)		(0.037)		(0.027)
HH has toys/games for child		0.017		-0.042		0.005
		(0.027)		(0.036)		(0.026)
Constant	-0.596***	-1.947***	-0.203**	-0.228	-0.508***	-1.606***
	(0.074)	(0.230)	(0.091)	(0.276)	(0.074)	(0.218)
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,124	8,124	8,124	8,124	8,124	8,124
R-squared	0.331	0.356	0.255	0.257	0.367	0.382
Private preschool=School F-stat	32.83***	10.54***	2.272	1.517	25.29***	8.930***
Public preschool=School F-stat	105.9***	97.13***	29.19***	30***	97.32***	90.52***
Private preschool=Public preschool F-stat	384.9***	239.1***	72.83***	67.93***	317.8***	210.8***

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
Private preschool and school=School F-stat	62.66***	40.27***	2.840*	2.508	47.10***	31.71***
Public preschool and school=School F-stat	0.00927	0.0211	4.019**	4.182**	0.250	0.169

All specifications control for village fixed effects. Standard errors are clustered at the village level. The table also reports the F-stat from testing equality of coefficient between private preschool and primary school; between public preschool and primary school; between private and public preschool; between private preschool with primary school and primary school only; and between private preschool with primary school and primary school only. The variables of interest are private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), primary school (attending primary schools in Rounds 1 and 2 with no preschool exposure), attending private preschool before starting primary school, and attending public preschool before starting primary school. The base category is not enrolled. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

2.5.3. **Preschool quality**

Given that I find such a remarkable difference in test score by the management type of preschool, the natural line of inquiry is to understand the nature of these preschools. To this end, I use the preschool survey conducted in Round 1. A total of 1159 preschools were surveyed across 300 villages in my sample, of which, 76 percent are public preschools[17]. Table 2.11 reports the mean and standard deviation on selected indicators by management type, as well as the t-test of difference between these public and private preschool characteristics[18].

Overall, I find that private preschools have better physical infrastructure. Public preschools are more likely to have a kitchen, and this is due to the government mandated meal scheme in India, which does not apply to the private education sector. Public preschools are also more likely to have a building made of bricks rather that mud. This may be because public preschools are seldom housed together with other arms of ICDS providing facilities such as child nutrition, child immunisation, child health check-up, and nutrition and health education for mothers.

[17] Here, I note that this data may be biased for several reasons. Not all preschools would have been surveyed, depending on whether these were open at the time of the visit and granted access to the investigators to conduct a survey. Private preschools may be more inclined to not grant such access; and the ones that did, could very well be 'better' quality. Indeed, substantially fewer private facilities were surveyed. See Appendix A, Section A.3 for details.

[18] Since the preschool data does not have unique identifier to link with the household survey, I am limited in my exercise and can only show the average characteristics by management type. I am unable to put these in a child level regression to study which aspect of preschool quality matters the most for the child's test score.

Private preschools have significantly lower student-teacher ratio than public preschools. The classrooms in public preschools have better display materials – artwork and alphabet/number charts. They also are more likely to be equipped with toys and games for children. However, a key difference is in the variable where teachers were observed to be teaching. It may be the case that private preschools have more formal instruction akin to primary schools, while public preschools are more focussed on developing a child's socio-emotional or motor skills through play-based activities. Indeed, the National Policy on Education (Government of India, 1986), and the National Early Childhood Care and Education Policy (Government of India, 2013) have discouraged any formal instruction of the 3R's and emphasised play-based learning. This could explain the difference in the test score between the two management types. It also suggests a need for a more complete evaluation exercise using data that captures socio-emotional skills in the early childhood phase.

This difference in learning styles across the two management types is confirmed when I use the household survey. The household survey asked parents a range of questions on the activities that happened at the preschools. Again, I find that children in private preschools are more likely to engage in formal study with reading and writing activities. Children in public preschools are more likely to engage in play-based activities – artwork, singing songs, playing with toys or puzzles, and listening to stories. However, this may be set to change with the new National Education Policy (Government of India, 2020). The policy, while emphasising the use of play-based learning, posits one of the aims of preschool education as developing early literacy and numeracy. Children in private preschool also report spending more hours at the facility, on average 4.4 hours as compared to 3.6 hours in public preschools. The lower number of hours in public preschools is in violation of the government mandate of 4 hours of educational instruction in public preschools[19].

[19] While not captured by the survey, it is crucial to mention here that the Government of India allows the hiring of staff with no experience and no high school diploma as a teacher at public preschool. See https://icds-wcd.nic.in/icds.aspx. There is a route for these staff to get appropriate training. However, even assuming that this would be done in a timely fashion, the training is for a mere 26-day period.

Table 2.11. Selected characteristics of preschools by management type

	(1) Public preschool	(2) Private Preschool	(3) Total	(4) t-test (1)-(2)
Preschool Survey				
Student teacher ratio	14.226	9.437	13.204	4.789***
	[8.115]	[7.648]	[8.251]	
Building made of bricks/mortar	0.930	0.864	0.915	0.066***
	[0.255]	[0.343]	[0.280]	
Has a toilet	0.447	0.733	0.514	-0.286***
	[0.497]	[0.443]	[0.500]	
Has water facility	0.550	0.813	0.612	-0.264***
	[0.498]	[0.390]	[0.488]	
Has boundary wall	0.360	0.718	0.444	-0.358***
	[0.480]	[0.451]	[0.497]	
Has a playground	0.749	0.740	0.747	0.010
	[0.434]	[0.439]	[0.435]	
Has a kitchen	0.283	0.059	0.230	0.225***
	[0.451]	[0.235]	[0.421]	
Classroom has children's art display	0.542	0.223	0.467	0.318***
	[0.499]	[0.417]	[0.499]	
Classroom has learning charts	0.888	0.740	0.853	0.148***
	[0.315]	[0.439]	[0.354]	
Classroom has toys/games/puzzles	0.691	0.542	0.656	0.149***
	[0.462]	[0.499]	[0.475]	
Classroom has books	0.868	0.810	0.854	0.058**
	[0.339]	[0.393]	[0.353]	
Teacher was seen teaching	0.690	0.777	0.710	-0.087***
	[0.463]	[0.417]	[0.454]	
Teacher was seen playing games	0.528	0.176	0.445	0.352***
	[0.499]	[0.381]	[0.497]	
Teacher was seen using books	0.650	0.667	0.654	-0.017
	[0.477]	[0.472]	[0.476]	
N(preschools)	886	273	1159	
Household Survey				
Hours spent at preschool	3.562	4.442	3.959	-0.881***
	[1.252]	[1.053]	[1.246]	
Child gets food	0.620	0.264	0.460	0.356***
	[0.485]	[0.441]	[0.498]	
Child learns to read and write	0.694	0.804	0.744	-0.110***
	[0.461]	[0.397]	[0.437]	
Child plays games	0.537	0.430	0.489	0.107***
	[0.499]	[0.495]	[0.500]	
Child draws and colours	0.176	0.105	0.144	0.072***
	[0.381]	[0.306]	[0.351]	
Child sings songs and poems	0.176	0.142	0.161	0.034***
	[0.381]	[0.350]	[0.368]	
Child plays with toys and puzzles	0.039	0.024	0.032	0.016***
	[0.194]	[0.152]	[0.176]	
Child listens to stories	0.229	0.191	0.212	0.038***
	[0.420]	[0.393]	[0.408]	
N(children)	3991	3272	7263	

This table presents some selected characteristics of public and private preschools. The last column is the difference between public and private preschools with t-test. The first set of characteristics comes from a preschool survey conducted in Round 1. See Section A.3 for details. The second set of characteristics comes from the household survey where parents would have answered these questions. The questions from household survey were only administered to sample of children attending preschool in Round 1. *** p<0.01, ** p<0.05, * p<0.1

2.5.4. State heterogeneity in value added

There can be considerable regional heterogeneity in preschool quality and hence, in the learning premium by the different states in India. This is driven by both, the variation in public preschool quality, and private preschool quality. Although the public preschools are governed by a central policy designed and implemented by the Ministry of Women and Child Development, the daily operation of these preschools is devolved at the state level. Most states are expected to raise at least 40 percent of the operational cost themselves. This can introduce a degree of variation in the quality of public preschools across the country.

While this heterogeneity exists even at the primary school level, I am restricted by the state-level distribution of the participation categories in my data set, and hence, can only explore the differences in preschools. As noted in Table 2.3, all children who are not enrolled come from Rajasthan; there are no children in Assam who attend primary school (without preschool exposure) and very few children in Assam who have switched from preschool to primary school. This is due to the primary school starting age being higher in Assam at six years as opposed to five years in the other states. Thus, in order to have adequate sample size in all three states, I restrict the analysis sample in this section to children who are attending preschool and have not yet started primary school. I distinguish these preschool goers by private-public management type, where going to a public preschool is the base category.

Table 2.12 presents the results of the lagged score VAM with full set of controls and village fixed effects for the sub-sample of children who are enrolled in preschool in Round 2 and have not yet started primary school. Column 1 estimates the value added of private preschool for the overall sample. Columns 2, 3 and 4 estimate the same specification for Rajasthan, Assam and Andhra Pradesh respectively. I find that the private preschool premium is highest in Andhra Pradesh, followed by Assam, and, lastly, Rajasthan. The findings here suggest that the limited empirical evidence on Indian preschools (Singh & Mukherjee, 2017; Singh, 2014) from Andhra Pradesh, need to be interpreted with caution as the results from these studies may not hold universally for a country as diverse as India.

Table 2.12. State level heterogeneity in value added for only preschool sample

	(1) Overall	(2) Rajasthan	(3) Assam	(4) Andhra Pradesh
Lagged - Standardised score	0.160***	0.352***	0.060***	0.082***
	(0.018)	(0.029)	(0.023)	(0.030)
Private preschool	0.628***	0.503***	0.633***	0.744***
(Base category: Public preschool)	(0.046)	(0.047)	(0.093)	(0.098)
Sample	Preschool (not started primary school)	Preschool (not started primary school)	Preschool (not started primary school)	Preschool (not started primary school)
Controls added	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Observations	5,402	1,794	2,350	1,258
R-squared	0.424	0.481	0.394	0.418

This table runs the lagged score VAM only on the subsample of children who attend preschool in both Rounds 1 and 2 and have not yet started primary school. All specifications control for village fixed effects and child and household level controls as in Table 2.9. Standard errors are clustered at the village level. The variables of interest are private preschool (attending private preschool in Rounds 1 and 2 and not yet started primary school). The base category is public preschool. *** p<0.01, ** p<0.05, * p<0.1

2.5.5. **Robustness check - Ability bias**

In this section, I revisit the problem of child heterogeneity. As discussed in Section 2.4.2, if child heterogeneity is left in the error term, it would cause the coefficient of interests as well the coefficient on lagged test score to be biased upwards. Child heterogeneity would be left in the error term if talented or motivated children learn faster, or if lagged test score is not a good proxy for ability. In either case, the lagged score VAM is no longer identified.

In the household questionnaire, the parents were asked "*Does the child speak about his day at the preschool?"* and "*If yes, how frequently?"*. I use the information from these two questions to construct dummy variables for whether the child speaks of preschool always, sometimes, and never (base category). Another question was asked to the child *"Do you like going to preschool?"*. I have also used this information as a dummy variable. Both these could serve as a proxy for a child's motivation and enthusiasm to learn. Since these questions were asked for the preschool sample, I can only conduct a check on the validity of my estimates for the subset of preschool goers (89 percent of the sample) comprising of those who were in preschool at Round 2, and those who had switched to a primary school after attending preschool.

Table 2.13 reports the results of the preferred lagged score VAM specification with full set of household and child controls and village fixed effects. Column 1 runs the preferred specification on the subsample of preschool goers where the base category is going to a public preschool. Column 2 reports the results of the same specification, but additionally controls for child motivation variables.

Table 2.13. VAM estimates robustness check with child motivation variables

	(1) Current score	(2) Current score
Lagged - Standardised score	0.183***	0.172***
	(0.017)	(0.016)
Private preschool	0.623***	0.619***
	(0.043)	(0.043)
Private preschool and school	0.831***	0.805***
	(0.060)	(0.060)
Public preschool and school	0.513***	0.517***
	(0.048)	(0.048)
Child talks about preschool always		0.110***
		(0.035)
Child talks about preschool sometimes		0.126***
		(0.029)
Child likes going to preschool		0.091***
		(0.029)
Sample	Preschool	Preschool
Controls	Yes	Yes
Village fixed effects	Yes	Yes
Observations	7,263	7,263
R-squared	0.383	0.387

This table runs the lagged score VAM only on the subsample of children who are either attending preschool in both Rounds 1 and 2 and have not yet started primary school or have attended preschool before starting primary school. All specifications control for village fixed effects and child and household level controls as in Table 2.9. Standard errors are clustered at the village level. The variables of interest are private preschool (attending private preschool in Rounds 1 and 2 and not yet started primary school); private preschool with primary school and public preschool with primary school. The base category is public preschool only with no primary school. The base category for child talks about preschool always/sometimes is child never talks about preschool. *** p<0.01, ** p<0.05, * p<0.1

I find that while talking about preschool activities and liking to go to preschool has a significant and positive effect on test score, the coefficient on variables of interest is revised downward only marginally[20]. However, note that the coefficient on lagged test score itself moves downwards on adding child motivation variables in Column 2. Further, in Appendix A Table A.8, I investigate this bias following Singh (2015). I look at the correlation between the lagged test score and the child motivation variables and find child motivation variables to be strongly correlated with lagged test score. This

[20] The null hypothesis of equality of the coefficient on private preschool and school from the two columns is rejected at 1 percent. However, there is no significant difference between the coefficients on private preschool and public preschool with school in Columns 1 and 2.

suggests that lagged test score does serve as a proxy for child motivation. Thus, despite the child motivation variables being significant, the coefficient on the variables of interest does not change greatly.

2.5.6. **Robustness check - Parent's Motivation**

Another source of bias with regards to VAM is when selection into type of educational institute is based on more information than those captured by the model. In particular, if the lagged test score is not a good proxy for this unobserved decision-making conducted in the past, the model would not be identified, and it would suffer from a positive selection bias.

I use information from the household questionnaire that could serve as indicators for parent's motivation and aspirations. I have made use of four variables to capture parental aspirations – whether parents read stories to the child at least once a week, whether they help him/her with learning at least once a week, whether they have spoken to a preschool staff about their child's learning progress at least once in the past three months, and whether they would like their child to learn to read and write. While the first two questions were administered to all households, the last two were only administered to the subset of parents whose children were in preschool in Round 1.

Table 2.14 reports the results of the preferred lagged score VAM specification with full set of household and child controls and village fixed effects. Column 1 reports the results of the preferred specification, which we have seen previously in Table 2.10. Column 2 reports the results of the same specification, but additionally controls for two variables capturing parent's motivation. Column 3 runs the preferred specification on the sub-sample of preschool goers where the base category is going to a public preschool. Column 4 reports the results of the same specification as in Column 3, but additionally controls for all four indicators of parent's motivations. In Column 5, I run the same specification as in Column 3 by only adding indicators on talking to preschool staff and parents wanting their child to read and write. I do this because the variables 'reads story to the child' and 'helps with learning' could be an adjustment in parental inputs in response to the educational institute being attended. For example, if private preschools assign homework to children and in response to this, parents have to help the child with learning, then this variable is part of the private preschool effect. It becomes

a mechanism through which private preschools have a positive impact. Hence, one would expect the coefficient on private preschool to adjust downwards, even if there was no selection bias.

Table 2.14. VAM estimates robustness check with parent's motivation variables

	(1) Current score	(2) Current score	(3) Current score	(4) Current score	(5) Current score
Lagged - Standardised score	0.198***	0.195***	0.183***	0.177***	0.178***
	(0.017)	(0.017)	(0.017)	(0.016)	(0.016)
Private preschool	0.700***	0.690***	0.623***	0.617***	0.620***
	(0.083)	(0.083)	(0.043)	(0.043)	(0.043)
Public preschool	0.083	0.077			
	(0.081)	(0.081)			
Primary school	0.571***	0.569***			
	(0.084)	(0.084)			
Private preschool and school	0.903***	0.889***	0.831***	0.813***	0.817***
	(0.091)	(0.091)	(0.060)	(0.061)	(0.060)
Public preschool and school	0.591***	0.586***	0.513***	0.515***	0.515***
	(0.084)	(0.085)	(0.048)	(0.049)	(0.049)
Reads story to child		0.049		0.048	
		(0.031)		(0.032)	
Helps with learning tasks		0.059**		0.043	
		(0.027)		(0.029)	
Talk to staff about child's learning progress				0.035	0.044
				(0.029)	(0.029)
Wants child to read/write				0.075***	0.083***
				(0.029)	(0.028)
Sample	Full	Full	Preschool	Preschool	Preschool
Controls	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	8,124	8,124	7,263	7,263	7,263
R-squared	0.382	0.383	0.383	0.385	0.384

This table runs the lagged score VAM only on the full sample (Columns 1 and 2) and on the subsample of children who are either attending preschool in both Rounds 1 and 2 and have not yet started primary school or have attended preschool before starting primary school (Columns 3, 4 and 5). All specifications control for village fixed effects and child and household level controls as in Table 2.9. Standard errors are clustered at the village level. The variables of interest are private preschool (attending private preschool in Rounds 1 and 2 and not yet started primary school); public preschool only with no primary school, private preschool with primary school, public preschool with primary school, and primary school with no preschool exposure. The base category for Columns 1 and 2 is not enrolled. The base category for Columns 3, 4 and 5 is public preschool only with no primary school. Reads story to the child is a dummy variable which takes the value of 0 if no-one in the household reads story to the child at least once a week. Helps with learning tasks takes the value of 0 if no one in the household helps the child with homework at least once a week. Talks to staff about child's learning progress takes the value of 0 if the parent has not spoken to the staff in the past 3 months.*** p<0.01, ** p<0.05, * p<0.1

For the full sample (in Column 2), while 'helps with learning tasks' has a positive and significant impact, the change in the coefficients of interest is marginal[21]. Next when I look at the subsample of preschool goers only (in Column 4), parents wanting their child to read/write is positive and significant. However, the coefficient on variables of

[21] The null hypothesis of equality of the coefficient on private preschool and private preschool with school in Columns 1 and 2 is rejected at 5 percent. However, there is no significant difference between the coefficients on public preschool, primary school, and public preschool with school in Columns 1 and 2.

interest, once again, shows only a marginal change[22]. Moving to Column 5, where I do not control for variables that could be assumed to be parental inputs in response to attending a type of educational institute, I find that parents wanting their child to read/write to be positive and significant. The coefficients on variable of interest are not significantly different from those in Column 3. This indicates that the lagged test score is a sufficient proxy for past inputs including the parent's decision-making process regarding their child's enrolment (also see Appendix A Table A.8, for the significant correlation between the parent's motivation and lagged test score).

2.5.7. **Robustness check – Excluding zeroes on test score**

As seen in Table 2.6, 12 percent of the sample scored zero on the test in Round 1. This proportion reduces to 3 percent in Round 2. A concern arising from this change in the distribution at the lower end, is that I may be overestimating the value added of preschools. The change could have been because the children were older and more familiar with the test or less nervous at Round 2. In this section, I re-run the preferred lagged score VAM with controls and village fixed effects on a sub-sample of children who did not score zero in Round 1. Table 2.15 reports the results of this exercise[23].

Column 1 reports the results as seen in Column 6 of Table 2.9 for the full sample. Column 2 reports the results of the same specification but excludes children who scored zero on the test in Round 1. Column 3 reports the results as seen in Column 6 of Table 2.10 for the full sample differentiating preschools by management type. Column 4 reports the results of the same specification but excludes the children who scored zero on the test in Round 1.

While the coefficient on variables of interest moves downwards (except that on *preschool),* the results remain significant and qualitatively similar. Thus, the main results are not an artefact of the test or testing environment but driven by the

[22] The null hypothesis of equality of the coefficient on private preschool in Columns 3 and 4 is rejected at 5 percent. The null hypothesis of equality of the coefficient on private preschool with school in Columns 3 and 4 is rejected at 1 percent. There is no significant difference between the coefficients on public preschool with school in Columns 3 and 4.

[23] Of the children scoring zero, the majority were not enrolled. Excluding the children who score zero in Round 1, also implies excluding 29 children from the base category of not enrolled. This means that the estimates are now based on 71 children in the not enrolled base category. Given this very small sample size, I re-run Table 2.15 excluding children who are not enrolled and using primary school (with no preschool exposure) as the base category in Appendix A Table A.11. The results are similar to those discussed here.

participation in preschool or primary school.

Table 2.15. VAM estimates excluding children scoring zero in Round 1

	(1) Current Score	(2) Current Score	(3) Current Score	(4) Current Score
Lagged - Standardised score	0.225***	0.240***	0.198***	0.209***
	(0.017)	(0.019)	(0.017)	(0.019)
Preschool	0.438***	0.441***		
	(0.083)	(0.094)		
Primary school	0.526***	0.507***	0.571***	0.553***
	(0.086)	(0.100)	(0.084)	(0.097)
Preschool and school	0.666***	0.656***		
	(0.086)	(0.096)		
Private preschool			0.700***	0.694***
			(0.083)	(0.094)
Public preschool			0.083	0.076
			(0.081)	(0.095)
Private preschool and school			0.903***	0.883***
			(0.091)	(0.102)
Public preschool and school			0.591***	0.575***
			(0.084)	(0.095)
Sample	Full	Excluding zeroes on lagged score	Full	Excluding zeroes on lagged score
Controls Added	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Observations	8,124	7,162	8,124	7,162
R-squared	0.348	0.348	0.382	0.383

This table presents the results of Table 2.9 (Column 1) and Table 2.10 (Column 3) for the full sample of children. In Columns 2 and 4, it re-runs the same specifications for the sub-sample of children excluding children who scored 0 on the tests in Round 1. All specifications control for village fixed effects and child and household level controls as in Table 2. 9. Standard errors are clustered at the village level. The variables of interest are private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), primary school (attending primary school in Rounds 1 and 2 with no preschool exposure), attending private preschool before starting primary school, and attending public preschool before starting primary school. The base category is not enrolled. .*** p<0.01, ** p<0.05, * p<0.1

2.6. **Conclusion**

In this chapter, I investigated the extent of value added of preschool attendance using data from three geographically and culturally distinct states in India. I find that children who attend preschool before starting primary school have a significant premium of 0.14 SD units in cognitive test scores as compared to children who attend primary school without any preschool exposure. On further investigation into the management type of the preschool, I find that this result is driven by those who attended private preschool. Children who attend public preschool before starting primary school are no better off than those who start primary school directly. I conduct a series of robustness checks to

asses if lagged score VAM are sufficient proxies for child's and parent's motivation; if the results are an artefact of the test itself or testing environment, and find the results remain qualitatively similar in magnitude and significant.

I provide a descriptive study of the preschool quality by management type to understand the private preschool premium. Private preschools have lower student-teacher ratio, longer hours of operation and a focus on formal instructional style of teaching. On the other hand, public preschools conduct more play-based activities. While this may explain the difference in test scores, it stresses the importance of undertaking a more comprehensive evaluation of preschools in India.

The test used in this paper has a narrow focus on cognitive skills, early literacy and numeracy. However, empirical evidence shows that one of the main benefits of early childhood education lies in nurturing of a child's non-cognitive or socio-emotional skills (see Barnett, 1995, 2011, for a review). In this light, there is need to supplement the findings of this paper with outcome measures on non-cognitive skills. The play-based activities used in public preschools may nurture soft-skills, and it would be incorrect to conclude that they have no effect on child development based only on the results of this chapter.

However, this chapter contributes to the current literature on the private-public learning gap in India, which has so far neglected the effect of preschools on primary school performance. Additionally, the limited empirical evidence which exists on preschools in India is based on data from Andhra Pradesh. I find that the private preschool premium displays considerable state level heterogeneity with Andhra Pradesh adding the highest private preschool premium on test score and Rajasthan adding the least. Not only is the preschool funding guideline in India skewed to benefit economically underdeveloped regions, most states are expected to raise at least 40 percent of the operational costs themselves. This would imply a variation in public preschool quality depending the state's revenue generating capacity. States may also exhibit a variation in attitudes and norms around educational attainment which would in turn be another source of variation in the quality of educational institutions. As such, one needs to adopt caution to not interpret results from a single region in India to hold true for the entire country. More research is required using nationally representative data on preschools.

This study also contributes to the literature on evaluation of universal preschool provision. This literature is sparse, even in developed countries and the results continue to be mixed. While some studies find that universal preschool education is associated with improved literacy and numeracy skills at primary school entry age (for US, see Loeb et al., 2007; Fitzpatrick, 2008; for UK, see Melhuish et al., 2008; for Argentina, see Berlinski et al., 2009), others find that these positive effects dissipate as early as the end of first grade (for US, see Magnuson et al., 2007; for Quebec, see Baker et al.,2008).

The results of this paper are particularly relevant in the backdrop of a rapidly changing education policy in India. The new National Education Policy (Government of India, 2020) sees an important shift towards early years and stresses the need to improve foundational literacy and numeracy skills as early as in the preschool years. Given the findings of this paper, public preschools would need considerable overhaul to be able to deliver on closing the learning gaps.

The policy acknowledges that with lack of preschool exposure, a large proportion of children fall behind in learning levels, within a few weeks of starting Grade 1 (National Education Policy, Government of India, 2020, para 2.5), a concern that is reiterated in the findings of this chapter. However, the policy fails to recognise that this gap in learning at school starting age is not as much due to lack of preschool exposure as it is due to lack of 'quality' preschool exposure. 89 percent of the sample in this chapter attend some form of preschool. Hence, the bigger focus for policy is to improve the quality of public preschools in India. Further, the varying levels at which children start primary school based on their preschool experience, highlights the need for educators to develop innovative pedagogical tools that target children with lower levels of learning. 'Teaching at the Right Level' is one such pedagogical innovation developed by Pratham NGO which has been shown promising results (Banerjee et al., 2017; Banerji & Chavan, 2020).

3. Intra-household Efficiency in Extended Family Households: Evidence from rural India

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3.1. **Introduction**

Family is the primary institution in society within which social and economic activities are carried out. As Mani (2011) puts it, family is a universal and enduring institution that forms the basis of economic interactions – from allocating time to work, human capital investment to issues of marriage, consumption and child rearing. The extended family household[27] structure, is where multiple adults live together, other than spouses and their unmarried children. An easier way to define an extended household is to say any structure other than a nuclear household. The nuclear household comprising of the spouse and the unmarried children, has been the basis of all economic intra-household models. Elsewhere in sociology and anthropology, researchers have further categorized family structures (see Khatri, 1975; D'Cruz & Bharat, 2001; Niranjan et al., 2005), but as a first attempt to bring non-nuclear household structure into the purview of economics, we implement a simplistic distinction between nuclear and extended (non-nuclear) households. The institution of the extended households in this context is closely connected to the social norm of patrilocality, which prescribes cohabitation of young married couples with the husband's parents.

The extended household is common in developing countries, especially in South Asia and Sub-Saharan Africa. An estimated 50 percent of children in India live in families which include adults other than their parents (calculated using data from the 2011 Indian Human Development Study). The benefits of an extended household structure are, in general terms, akin to the benefits of marriage. Gains include cost-sharing of household public goods such as residence, meals, and children, economies of scale and specialization in the production process, and risk-sharing (Becker, 1974; Bergstorm, 1997; LaFave & Thomas, 2017; Rosenzweig & Wolpin, 1985). However, larger households might also suffer from significantly more free-riding, as more adults are in charge of production and public goods provision (see Baland et al., 2016; Jakiela & Ozier, 2015, on the effect of a sharing tax and Cox & Fafchamps, 2007, for an overview on extended families and kinship networks more generally). In addition, the presence of many adults might

[27] We define a household as members who eat together on a daily basis from the same kitchen. This is the most commonly used definition of a household, also referred to as the pot in developing countries' research.

introduce additional conflicting preferences, which might further encourage cheating and free riding tendencies.[28]

The fact that most individuals voluntarily enter into a marital contract, or join another household, does not imply that the relationship between spouses is co-operative and efficient. However, economic theoretical intra-household models either imply efficiency (the unitary approach) or assume efficiency in the decision-making process (the collective approach), that is, married partners will always take advantage of opportunities that make both better off. The empirical literature on intra-household has found mixed results for this efficiency assumption.

However, the purpose of this chapter is not to settle the debate of (in)efficiency in intra-household decision making, but to bring to the forefront a discussion on what constitutes a household and how that might affect the dynamics within the household. We use a novel dataset collected in 2014 in rural Uttar Pradesh, India that combines survey data with lab-in-the-field intra-household experiments. The experiment was set up to measure individual household members' willingness to forego personal monetary rewards for larger, collective monetary rewards. We find significant differences in contribution rates depending on the relationship of the game participants. A key finding is that participants linked through in-law relationships contribute less to the common resource pool (akin to a household public good) than members related by blood. We also find a significant difference in the contribution made to the common account by couples residing in nuclear versus extended families. We complement these findings with insights from survey and qualitative data.

Our motivation for undertaking this descriptive study can be nicely summarized in the words of Cox and Fafchamps (2007) – 'Too often, economic models are gender blind, populated with generic parents and children and "spouses 1 and 2", rather than husbands, wives, fathers, mothers, sons and daughters. This modelling choice is in part a legacy of the nature of economics, which has little to say about gender in and of itself

[28] It should be noted that the long-term nature of the extended family household, as well as altruism between family members, might counter some of these pressures. In addition, enforcement through violence and lack of privacy might turn any household into a "unitary" household, that is, a family where all production and consumption decisions are made according to the preferences of the head or "dictator" in the family, de facto achieving efficiency. For a recent nuanced account on the use of violence see Lentz (2018).

– such as the nature of motherhood versus fatherhood'. Cox and Fafchamps (2007) summarising the extensive literature on the role of kinship (friends and relatives) in risk sharing and inter-household transfers, highlight that a rich analysis of 'controls' is indispensable to understanding intra-household decision making process. We hope to be able to shed light on the power balance in the decision-making process and the efficiency implications by unpacking the generic 'controls' in economic models.

Second, Browning et al. (2014) while expanding on the co-operative bargaining framework note that the assumption of efficiency might be violated 'when existing social norms impose patterns of behavior that may conflict with efficiency' (p. 122). We show how one such social norm of patrilocality, relevant in developing countries, undermines efficiency. In developing countries, such as India, it is a common practice for the son of the household to continue to live with his parents even after marriage. This often creates tension between the daughter-in-law (wife) and the mother-in-law (mother) over the control of power not just over the son (and husband) but also power over decision making with the household.

Third, Porter and Adams (2016) argued that there is a need to study sharing rules within the family as it has consequences for the design of intra- and inter-generational redistributive programs. This is particularly true for developing countries, where time and again empirical economic literature has supported targeted transfers to the 'woman' in the household with the aim of promoting gender equality (see Duflo, 2012). However, the ambiguous identity of this 'woman' in relationship to the other household members, implies that gender targeted transfers might fail to achieve their desired outcome. The generic 'woman' selected for the transfer may not play a primary role in the allocation of the resources within the household. Through this study, we hope to shed light on which 'woman' in the household is primary.

3.2. **Related Literature**

Unitary models predict efficiency as a result of common set of preferences or the existence of an altruistic head (Samuelson, 1956; Becker, 1991). In the collective model, the household maximizes a weighted average of individual utilities, the weights

capturing the balance of power within the household. Both these models are based on co-operation and assume efficiency. Prima facie, this efficiency assumption may seem natural as married partners who are aware of each other's preferences can act co-operatively, resulting in decisions that make both better off. Alternatively, due to co-residence and repeated interactions, these married partners would achieve efficiency (similar to 'folk theorem' from game theory).

However, empirical evidence on intra-household efficiency has been mixed. Udry (1996) studies agricultural yields on the plots of men and women within the same household in Burkina Faso and finds that the inputs are not allocated efficiently within the household, resulting in production losses (see also Duflo & Udry, 2004). Dercon and Krishnan (2000) reject risk-sharing within the households in rural Ethiopia.

On the other hand, Bobonis (2009) finds evidence in favour of Pareto Optimality using data from Opportunidades program in Mexico. Browning and Chiappori (1998) are unable to rule out efficient households using Canadian household data.

In contrast, the case seems to be settled in a growing experimental literature which studies intra-household decision making between spouses. They consistently report failure to maximize surplus for the household, and hence inefficiency (Ashraf, 2009; Iversen et al., 2011; Mani, 2011; Munro et al., 2014; Cochard et al., 2016; Kebede et al., 2013).

Sizeable empirical and experimental literature has focused on spouses. Despite its importance, there is relatively little literature in economics on the topic of extended households. Most existing literature focuses on the implications for agricultural productivity of African extended households. Guirkinger et al. (2015)—building on Udry (1996)—document that land yields are larger on plots where an individual has control over inputs and the use of resources, compared to extended household plots in Mali. But Kazianga and Wahhaj (2013) find the opposite results in Burkina Faso, a difference that Guirkinger et al. (2015) attribute to the relatively large and complex households in their sample.

Experimental evidence which studies non-spousal relationships is also rare and limited.

Peters et al. (2004) and Porter and Adams (2016) focus on inter-generational cooperation between parents and children in Ithaca city and Oxford respectively. The former study finds that parents are less likely to free ride than children, and that there is more altruism between parents and children than between parents and strangers. However, the surplus is never maximized and hence, inefficiency exists. The latter study also found that children are more likely to give when paired with parents rather than strangers. However, there is no experimental literature that focusses on extended households[29].

There is another strand of literature related to our research, which focusses on woman's 'empowerment' and its erosion due to the social norm of patrilocality in South Asia. Using the large sample of the India Human Development Survey, Dhanaraj and Mahambare (2019), show that norms around decision making of daughters-in-law in extended households prevent these women from taking up employment opportunities. They suggest that limited autonomy of young married women within extended households, characterized by, among other things, the practice of purdah, or generally low mobility, to be the primary explanation. Similarly, Saikia and Singh (2009) find that women in extended households are less likely to utilize maternal health services. Harris-Fry et al. (2017) systematically review the literature on food allocation in South Asian families and link social hierarchies and patrilocality with unequal status and access to food. Thus, there is evidence that belonging to an extended household is correlated with inefficient outcomes, particularly for the younger married woman.

3.3. **Data**

The study is based in the state of Uttar Pradesh, one the biggest Indian states in terms of land area and population, and makes use of the baseline data of the TARA Akshar Evaluation Project (Wang, Maertens, Ksoll, & Deshpande, 2018). TARA Akshar is a computer-based female adult literacy programme implemented by a Delhi based NGO, Development Alternatives (DA). The Evaluation Project (Wang et al., 2018) was

[29] There is also a growing, literature on polygynous households in Western Africa. See Akresh et al. (2011), Rossi (2019), Munro et al. (2019), and Barr et al. (2018). Dynamics in polygynous households are necessarily quite different from the type of multi-adult households in which we are interested, so we do not discuss their findings in more detail.

designed as a Randomised Control Trial (RCT) to study the impact of a female adult literacy program (Tara Akshar) in the state of Uttar Pradesh, India. Note that as this chapter uses the baseline collected for the evaluation of the adult female literacy program, this naturally implies a degree of selectivity for the sample. However, the results remain relevant for substantial population as female illiteracy is common in the area. Using data from the 2011 Census of India, we compute illiteracy rate of 55 percent for women in Uttar Pradesh and 47 percent for women in India, overall[30].

This chapter focusses on the public goods experiment implemented in five villages in April-May 2014[31], before the implementation of the adult female literacy programme. In the nuclear households, only one game was played, that is, between the selected illiterate woman and her husband (spousal game). In the extended households, we selected one additional married adult male and one additional married adult female at random from among the present members. Thus, in extended households up to six games could be played: spousal, woman and other male, woman and other female, husband (of the 'woman') and other male, husband and other female, and the other male and female.

Our analysis centers on efficiency. We define efficiency in the sense of Pareto Efficiency: a resource allocation in which it is not possible to make one individual better off without making another one worse off. In the public goods experiment, we measure the individual household members' willingness to forego personal monetary rewards for larger, collective monetary rewards. This is a test of a key implication of Pareto Efficiency, because an efficient household will co-ordinate to use the greater collective reward to compensate the individual for forgoing personal monetary rewards. While other measures of efficiency are possible, this experiment is a common tool among economists. A more elaborate discussion of the measurement of efficiency has been dealt with in Section 3.3.1.

The experimental data is supplemented with survey data (also collected as part of the

[30] Calculated from Table DDW-0000C-O8, and SC-09-00-008-2011-DDW.XLS, Census of India, 2011. Restricted to all persons above the age of 18 years. Literacy is defined as being able to read and write.
[31] Since the experiment involved monetary payouts, it was not possible to implement the experiment in the other seven of the 12 villages due to ongoing elections.

baseline survey). In particular, we will make use of the decision-making process within the household as captured by the woman's questionnaire. The woman was asked 'who in the household has the most say?' and the domains covered were - what to cook on a daily basis, what and how much to purchase at the local shop, what and how much to purchase at the market outside the village, what to do when your child falls ill, your child's enrolment in school, your child's attendance at school. The woman could name up to three household members, including herself, who have decision making power over these domains. The order in which the household members were named was inconsequential.

Additionally, we complement our quantitative analysis with insight from a short qualitative study conducted in December 2016. We revisited two villages to conduct a series of qualitative semi-structured interviews. The goal of the interviews was to gain a better understanding of the dynamics of the relations and decisions within the household. We interviewed three nuclear households and five extended households. Among the extended households, respondents included both daughters-in-law and mothers-in-law. The interviews were semi-structured, following a set of open questions guiding the interview, but allowing the respondent to talk freely and at length about each topic. We covered perceptions, advantages, and disadvantages of extended versus nuclear households, division of labor and output, relationships, and decision making within the household.

3.3.1. **Public Goods Experiment**

As part of the Evaluation Project (Wang et al., 2018), a public goods experiment was employed to measure intra-household efficiency. Many variations of the public goods experiment has been used in experimental economics as a tool to measure co-operation between spouses and intra-household efficiency as well as to test different theories of intra-household efficiency (Ashraf, 2009; Castilla & Walker, 2013; Mani, 2011; Munro et al., 2014; Cochard et al., 2016; Iversen et al., 2011; Kebede et al., 2013). As Munro (2015) points out that monetary incentivized intra-household experiments have taken place in over 20 different countries including Bangladesh (1), Benin (1), Brazil (2), P.R. China (3), Colombia (1), Ethiopia (2), France (5), Germany (2), Ghana (1), India (5), Japan (1), Kenya (3), Nigeria (1), Malawi (1), Mongolia (1), Peru (2), Turkey (1), Uganda (2), UK (4), USA (3) and Zambia (1).

In the baseline, up to six experiments per household could be played - a spousal (selected illiterate woman-her husband) experiment, the woman and another male member, the woman and another female member, the husband (from the spousal experiment) and another female member, the husband and another male member, the other male and other female. The male and female member (other than the selected illiterate woman and her husband) were selected randomly from the household roster's list of adult married members.

In the basic version of the experiment with spouses, the woman and her husband were invited to a central location in the village (where their privacy could be ensured while playing the experiment). If the husband was not available, the enumerators returned the following day and re-issued the invitation. If the husband was still not available on the next day, the enumerators did not proceed with the experiment. Once the spouses were present, they were split into two different rooms where the experiment was explained simultaneously. They were first showed ten tokens and two boxes. One box was coloured blue and the other was coloured yellow, the colours chosen as they lack any religious or other meaning. Each one of them would receive ten tokens and would be asked to divide the ten tokens over the two boxes. The tokens in the blue box were worth more than the tokens in the yellow box: The tokens in the blue box are converted at a rate of four Rupees (10 US cents), while the tokens in the yellow box are converted at a rate of three Rupees (7.5 US cents). In addition, the use of these funds differ. The participant her(him)self could decide on the use of the funds from the tokens in the yellow box. The enumerators gave a few examples of such use: clothing, food, savings and emphasized that it was the participant 'you' who could decide on the use of the funds. The funds from the tokens in the blue box, on the other hand, would be decided upon by both experiment participants, in this case, the spouses.

The participants were then handed the ten tokens and invited to make the decision as to how many tokens should be placed into each box. It was made clear that the decision the participant made would not be observed or shared with the other participant. A random amount of 42 Rupees (68 US cents) was contributed by the enumerators to the (common) blue box, so that the participant could not figure out how much the other had contributed to the blue box. Once the decisions were made, one of the enumerators left

the location with the four boxes and counted the total amount of tokens. The enumerator returned to the location, paid out the participants for the funds from the (individual) yellow boxes in private, brought the two participants together and then paid out the funds from the (common) blue box, plus the random amount of 42 Rupees[32].

In the larger households (consisting of adult members other than the spouses and their child), this experiment was repeated with a randomly selected adult male and a randomly selected adult female. The same protocols were followed as described above.

In this experiment set up, there are higher returns from contribution to the common account (four times the amount as opposed to three times for contribution to the private account). This ensures that full contribution to the common account by all participants is the socially optimal or Pareto Efficient solution. However, not contributing to the common account when others do, benefits the free rider. Thus, contribution to the common account becomes a measure of the extent of co-operation or intra-household efficiency.

Theoretically, we can use a linear voluntary contribution model used in public goods literature to represent the experiment played between *N* players.

$$\pi_i = 1 - q_i + \lambda \sum_{i=1}^{N} q_i \; where \; q_i \in [0,1]$$

Where π_i denotes the payoff to player *i,* q_i denotes the contribution of player *i* to the common account, and λ is the private payoff of public good also known as Marginal Per Capita Return (MCPR) in the public goods literature.

The dominant strategy here for each player acting independently is to contribute nothing to the common account -

$$q_i = 0 \; if \; \lambda < 1 \qquad (1)$$

[32] The decision to keep contributions private is common in experimental literature. It is motivated for ethical reasons where studies such as Schuler et al. (1998) and Angelucci (2008) have found an increase in domestic violence related to cash flow to the woman in the household.

If each player would contribute everything to the common account, the payoff would be –

$$\pi_i^C = \lambda Q_i \ \ where\ Q_i = \sum_{i=1}^{N} q_i$$

$$or\ \pi_i^C = \lambda N$$

In order for this to be a Pareto Efficient (socially optimum) solution, we require that the payoff from contribution to the common account be greater than the payoff from non-contribution.

$$\pi_i^C > \pi_i^{NC}$$

$$\lambda N > 1$$

$$\lambda > \frac{1}{N}$$

Thus,

$$q_i = 1\ if\ \lambda > \frac{1}{N} \qquad (2)$$

Combining equations (1) and (2), we have a prisoner's dilemma problem where $q_i \in [0,1]\ if\ \frac{1}{N} \leq \lambda \leq 1$.

Specific to our experiment, q_i is the proportion of the 10 tokens that the player contributes to the common account, so $q_i \in [\frac{1}{10}, 1]$. Since each three Rupees token taken out of the private account and placed in the common account is paid off at four Rupees and then divided equally among the two players (N=2), the private payoff of contribution to the common account is $\lambda = \frac{4}{3} * \frac{1}{2} = \frac{2}{3}$. For the experiment in this data set, we have a prisoner's dilemma problem as $\lambda = 2/3$ lies between 1/2 and 1.

Note that the two individuals could together earn up to 80 Rupees, equivalent to two USD or twice the daily wage at the time, if they contributed everything to the common account, whereas they would only receive 30 Rupees each or 60 Rupees in total if they contributed all to the private account. Hence, contributing all tokens to the common account maximizes joint surplus and is the Pareto Efficient outcome.

While free riding would be a dominant strategy for each individual, contribution to the common account is driven by altruism, trust, social norms, alignment of preferences, reciprocity[33], and social relations. This chapter explores some of these factors – the social norm of patrilocality and efficiency between spouses; how an individual changes behaviour as a function of the relationship (within the family) with the other individual in the experiment; the influence of the distribution of decision-making power on efficiency.

A common criticism levelled against lab-in-the-field experiments is whether they mimic real life decisions sufficiently. The experiment implemented in this study is designed to uncover a particular dimension of inefficiency within households: concealing personal resources instead of contributing them to the household as a whole, with potentially larger shared benefits. There are other forms of efficiency that this study cannot comment on, for instance, production efficiency (see Guirkinger et al., 2015; Kazianga & Wahhaj, 2013).

Another possible concern for this measurement of efficiency is that the experiment was a one-off play. The participant's best response in this experiment will be based on the knowledge about the behaviour of others which a participant learns through repeated play of the experiment (Arifovic & Ledyard, 2012). Since the experiment is played with individuals who have a base of common knowledge and experience through living together; and as far as the decision-making in this experiment mimics real life intra-household decision making (evidenced through the widespread use in literature stated earlier) this is not a problem.

3.3.2. **Sample**

There were 393 households across the five villages of the baseline sample with at least one adult illiterate female. However, we were able to play the public goods experiment with 266 women and their households (68% of the baseline sample). Table 3.1 shows

[33] The idea of reciprocity is one where participants are willing to contribute when others contribute. In Appendix B Table B.6, we look at the correlation between the contribution to the common account by participant 1 and participant 2. Overall, we find these contributions to be positively correlated suggesting reciprocity. However, these correlations are stronger and significant in experiments excluding the spousal experiment.

the characteristics of the overall sample (Column 1), the sample omitted from the experiment (Column 2) and the experiment sample used in this chapter (Column 3). The last column reports the t-test of difference in the characteristics between the omitted and experiment sample.

While 61 percent of the households in the baseline sample are extended, the sample subset used for the analyses in the chapter has a higher proportion of extended families at 77 percent. This is because in nuclear households, if the selected illiterate woman was widowed or her husband is a migrant labour, the spousal experiment (the only possible experiment in nuclear households) could not be played. However, in extended households, other experiments, such as those with a randomly selected adult married male or female were still possible.

The only other significant difference between the experiment sample and the omitted sample is in the household size. Given that 63 percent of the omitted sample belong to nuclear households, it is expected that this sample would have a smaller household size, on average.

Table 3.1. Characteristics of omitted and experiment sample

	(1) Baseline sample	(2) Omitted Sample	(3) Experiment Sample	(4) t-test
Nuclear household	0.387	0.634	0.233	0.401***
	[0.488]	[0.483]	[0.439]	
Selected woman's current age	39.237	38.684	38.72	-0.036
	[9.918]	[9.397]	[10.175]	
Selected woman's husband's age	42.003	41.522	41.75	-0.228
	[9.970]	[9.715]	[9.982]	
Number of years married	24.232	23.907	23.44	-0.467
	[10.913]	[10.450]	[11.24]	
Husband's education in years	6.209	6.237	6.254	-0.017
	[4.723]	[4.717]	[4.732]	
Backward caste	0.363	0.336	0.365	-0.029
	[0.481]	[0.474]	[0.482]	
Scheduled Caste	0.490	0.500	0.491	0.009
	[0.501]	[0.502]	[0.501]	
PPIscore	25.368	25.455	25.12	0.335
	[10.581]	[10.369]	[10.811]	
Number of household members	7.656	6.410	8.649	-2.239***
	[3.836]	[3.103]	[4.391]	
Number of adult male household members	2.224	1.746	2.471	-0.725***
	[1.380]	[1.168]	[1.418]	
Number of adult female household members	2.099	1.657	2.328	-0.671***
	[1.135]	[0.935]	[1.163]	
Number of migrant members	1.285	1.201	1.328	-0.127
	[1.771]	[1.481]	[1.906]	
Number of adult male migrant members	0.957	0.881	0.996	-0.116
	[1.033]	[0.823]	[1.126]	
Number of adult female migrant members	0.109	0.082	0.124	-0.041
	[0.372]	[0.302]	[0.404]	
Sample size	393	127	266	

This table presents the mean and standard deviation (in parenthesis) of the full baseline sample, the omitted sample for households where no public goods experiment could be conducted, and the experiment sample for households where at least one experiment was conducted. Column 4 presents the difference between omitted and experiment sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.1 and Table B.2 in Appendix B provide the characteristics of the experiment and omitted sample by nuclear and extended structure. We find that our experiment sample is similar to the omitted sample of extended households. However, in nuclear households, the omitted sample has a higher level of husband's education than those

present in our experiment sample. This would explain the husband's migration, and hence, our inability to play the spousal experiment in nuclear households.

Table 3.2 lists all the experiments that were played in the extended and nuclear households. We note here that despite having 204 extended households who played at least one public goods experiment, the number of experiments played for each type varies.

Table 3.2. Number of experiments implemented by household structure

	Extended	Nuclear
Spousal (selected illiterate woman and her husband)	111	62
Woman and another male	86	
Woman and another female	124	
Husband and another male	38	
Husband and another female	52	
The other male and female	63	
Total number of games played	474	62

This table presents the number of experiments played by experiment type (row variable) and household structure (column variable). A total of 536 experiments were conducted.

The most common reason for not having played the spousal public goods experiment was due to the husband being a migrant labour[34]. We discuss the bias due to migration in the results related to spousal experiment in Section 3.4.2.

For the non-spousal experiments, there could be two reasons for not having played them. First, the extended household may not have any other eligible male or female member to select from. For instance, an extended household consisting of the selected woman, her husband and mother-in-law, would not play the experiments – 'Woman and another male', 'Husband and another male', and 'The other male and female'. We control for the bias stemming from household composition by including household fixed effects in our analysis of the experiments played within the extended households.

Second, the non-spousal experiments may not have been played for reasons of migration.

[34] We infer that 57% of the spousal experiments that were not implemented was due to the husband being a migrant labour (not residing in the household for at least six months but intending to return). See Appendix B Table B.3 for details

Relative proximity of the study area to major cities such as Varanasi, Allahabad, and Delhi, combined with low living standards resulted in a high migration rate, especially among the men. In so far as the household member had been away for at least six months prior to the survey but intended to return, the household member would be classified as migrant labour. However, this limits our ability to infer other patterns of migration, such as seasonal migration. Based on this limited measurement of migration, on average, we infer that 30% of the non-spousal experiments that should have been played within extended households could not be implemented due to long-term (more than six months) migrant labour.[35] We will return to the implication of temporal migration for our results within extended households in Section 3.4.3.

3.4. **Results**

3.4.1. **Descriptive statistics**

Table 3.3 presents the mean and standard deviation (in parenthesis) of selected variables by household structure. The final column also reports the p-value of the test of difference in the means between nuclear and extended households[36].

[35] See Appendix B Table B.4 for details.
[36] Appendix B Table B.5 reports the descriptive statistics for the full sample (experiment and omitted) by nuclear and extended household structure.

Table 3.3. Descriptive statistics of experiment sample by household structure

	Total	Extended	Nuclear	*p*-value
Wife's current age	38.72	38.71	38.76	0.969
	(10.175)	(10.616)	(8.768)	
Husband's current age	41.75	41.68	41.95	0.850
	(9.982)	(10.360)	(8.874)	
Number of years married	23.44	23.47	23.33	0.920
	(11.24)	(11.94)	(8.902)	
Husband's education in years	6.254	6.83	4.60	0.001
	(4.732)	(4.689)	(4.492)	
Backward Caste	0.365	0.38	0.31	0.302
	(0.482)	(0.487)	(0.467)	
Scheduled Caste	0.491	0.44	0.64	0.004
	(0.501)	(0.498)	(0.483)	
PPI score	25.12	24.98	25.55	0.699
	(10.81)	(10.95)	(10.43)	
Number of household members	8.649	9.838	5.030	0.000
	(4.391)	(4.352)	(1.714)	
Observations	266	204	62	

This table presents the mean and standard deviation (in parenthesis) of the full experiment sample where at least one experiment was conducted, the extended households in the experiment sample and the nuclear households in the experiment sample. The last column reports the p-value of the t-test of difference in means between extended and nuclear households. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

The average age of the woman is 39 years, whereas that of the husband is 42 years. The average length of marriage is 23 years. There is no significant difference in the length of marriage by household structure. Although all women in our sample were uneducated (as we selected only illiterate women who were eligible for an adult literacy program), the average level of education attained by husbands is six years. Husbands in an extended household have two extra years of education, on average, compared with those in a nuclear household[37]. Caste classification shows that almost all households belong to the lower castes (rather than the General category), although nuclear households are more likely to belong to Scheduled Caste. In order to establish a comparable metric for the living standard of each household, we computed a Progress out of Poverty Index (PPI) ranging from 1 to 100. In 2009, a PPI score of 20-24 corresponded to a 75 percent chance of being under the poverty line[38] (Schreiner, 2008). The average PPI score is 25 for our sample. The extended household, on average, has 10 members, whereas the nuclear household has, on average, has five members.

[37] As noted earlier in Section 3.3.2, husbands with higher education level in nuclear households are more likely to migrate and hence the average education level of husbands in our experiment nuclear household sample is low.
[38] Reserve Bank of India given poverty line for rural India

Table 3.4 presents the average contribution to the common account by household structure, and relationship[39]. Panel A presents information on contributions of wives and husbands in the spousal experiment, by nuclear and extended households. Panel B presents average contributions across all non-spousal experiments (recall, there were up to five other experiments played in an extended household with a randomly selected male and female) in the extended household. The contribution is presented in percentage terms where 100 percent would denote that all 10 tokens were contributed to the common account. Contributions were private knowledge and not shared with the other experiment participants. A random amount of 42 Rupees (68 US cents) was added to the common account before payout, to ensure unobservability (See Section 3.3.1 for details). All games point to Pareto inefficiency since members do not maximise the surplus.

The spousal experiment acts as a reference point, as it is the one that has been played in many different contexts. Consistent with past studies using a public goods experiment in rural India (see, for instance, Castilla, 2015; Mani, 2011; and Munro et al., 2014), we find that both participants contribute their full endowment to the common account in only 2 to 3 percent of the experiments. However, consider the percentage of individuals contributing everything to the common account – for instance, in a nuclear household 11 percent of the wives and 19 percent of the husbands contribute everything. This discrepancy with the experiment-level and individual-level contributions suggests low degree of correlation between players' contributions[40].

In very few experiments (1 to 3 percent) both spouses contributing nothing at all to the common account. These statistics are similar when looking at the non-spousal experiments in Panel B.

[39] See Appendix B Figure B.1and 0 for the histogram of contribution to the common account by each experiment type.
[40] See Appendix B Table B.6 for the correlation between the two players' contributions to the experiment

Table 3.4. Average contribution by experiment type and household structure

	Extended	Nuclear	t-test
Panel A: Spousal experiment			
Wife's contribution	47%	53%	-6.6*
Husband's contribution	56%	64%	-8.4**
Total contribution	51%	59%	-7.5***
% of experiments wife contributes nothing	10%	10%	
% of experiments husband contributes nothing	4.5%	8%	
% of experiments both players contribute nothing	1%	3%	
% of experiments wife contributes everything	4.5%	11%	
% of experiments husband contributes everything	7%	19%	
% of experiments both players contribute everything	2%	3%	
N	111	62	
Panel B: Other experiments			
Total contribution	51%		
% of experiments both players contribute nothing	2%		
% of experiments both players contribute everything	2%		
N	363		

This table presents the average contribution by participants to the common account. It also reports the total contribution to the common account from both participants in an experiment, in the row labelled "Total." The contribution is shown in percentage terms, with 100 percent denoting that all 10 tokens were contributed to the common account. The averages are shown by experiment type (row) and household structure (column). The last column reports the difference in average contribution by household structure and the significance level associated with the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1.

The average contribution to the common account in the spousal experiment is 8 percentage points lower in the extended household compared to the nuclear household (this difference is statistically significant at the 1 percent level). In monetary terms, this translates into an efficiency loss of 1.60 Rupees on average for spouses in extended households. This is driven by lower contributions by both wife (6 percentage points lower; statistically significant at the 10 percent level) and husband (8 percentage points lower; statistically significant at the 5 percent level) in the extended household. Wives in both, nuclear and extended households, contribute less than the husbands. This is a common finding in experimental literature based in developing countries (see, for instance, Inversen et al., 2011; Kebede et al., 2013). Specifically, in the context of our study, this may be a result of limited autonomy enjoyed by women in rural Uttar Pradesh (Dyson & Moore, 1983; Bloom et al., 2001; Jejeebhoy & Sathar, 2001). Chapter 4 studies women's empowerment in rural Uttar Pradesh, and indeed, finds women to have low decision-making power within the household, lower mobility and exposure to the outside world, and low levels of financial independence (see Table 4.2)

3.4.2. **Household structure and Spousal Experiment**

We first look at the correlations between the type of household structure and the level of contributions to the spousal experiment[41]. The exact regression specification is given by –

$$C_{s,j} = a_0 + a_1 NUCLEAR_{s,j} + a_2 X_{s,j} + a_3 HH_j + \epsilon_{s,j} \quad (3)$$

where $C_{s,j}$ is the contribution to the common account by the spouse s - wife or husband or both in household j; *NUCLEAR* is the dummy variable that takes the value of 1 if the spouses belong to a nuclear household and 0 if the spouses belong to an extended household; $X_{s,j}$ is a set of individual characteristics such as age and education; HH_j is a set of household characteristics such as caste, economic status and number of household members.

In Table 3.5, we present the results from OLS regression in equation (3), where the dependent variables are the total contribution of both spouses to the common account (Column 1), the contribution of the wife to the common account (Column 2), and the contribution of the husband to the common account (Column 3). Contributions are measured in proportion of total feasible contribution, that is, a value of 1 corresponds to 100 percent and a value 0.1 corresponds to 10 percent. The main independent variable of interest is 'nuclear household'.

[41] Appendix B Table B.7 presents the descriptive statistics for only the spousal experiment sample – 173 households out of 266.

Table 3.5. Regression results of spousal experiment on household structure

	Total contribution (1)	Wife's contribution (2)	Husband's contribution (3)
Nuclear household	0.073**	0.063	0.083*
0 = extended household; 1 = nuclear household	(0.036)	(0.046)	(0.049)
Wife's age	-0.061	-0.097*	-0.025
	(0.045)	(0.054)	(0.070)
Husband's age	0.065	0.107*	0.024
	(0.045)	(0.056)	(0.067)
Wife's age squared	0.001	0.001*	0.000
	(0.000)	(0.001)	(0.001)
Husband's age squared	-0.001	-0.001**	-0.000
	(0.000)	(0.001)	(0.001)
Number of years married	-0.002	-0.001	-0.002
	(0.005)	(0.007)	(0.006)
Husband's education	0.001	-0.004	0.006
	(0.003)	(0.005)	(0.005)
Backward Caste	0.044	0.040	0.049
	(0.049)	(0.071)	(0.072)
Scheduled Caste	0.045	0.007	0.084
	(0.051)	(0.069)	(0.075)
PPI score	0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)
Number of household members	0.000	0.002	-0.001
	(0.004)	(0.004)	(0.006)
Constant	0.229	0.062	0.395
	(0.261)	(0.426)	(0.367)
Observations	159	159	159
R-squared	0.063	0.048	0.057

This table reports the results of the regression mapping the contribution to the common account in spousal experiment. Contributions are measured in proportions. Total contribution is the contribution by both players to the common account. Standard errors are clustered at the household level. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration. Note that the sample is less than the expected 173 due to missing co-variate variables. Appendix B Table B.8 presents the descriptive statistics for only the spousal experiment regression sample. *** p<0.01, ** p<0.05, * p<0.1.

We find that being in a nuclear household is positively and significantly correlated with the husband's contribution as well as with the total contribution in the spousal experiment. The total contribution to the common account is approximately 7 percentage points larger when the spousal experiment is played in a nuclear household versus extended households (an effect size of 14 percent), whereas the husband's contribution is 8 percentage points larger (an effect size of 15 percent). The difference in the wife's contribution in extended versus nuclear households is not significantly different. This may be due to the strong patriarchal structure and social norms that is seldom associated with adverse gender outcomes in Uttar Pradesh – lower female labour

force participation, higher fertility, less female say in household decisions and lower entitlement to household resources (Dyson & Moore, 1983; Foster & Rosenzweig, 1996Bloom et al., 2001; Jejeebhoy & Sathar, 2001; Mani, 2011; Munro et al., 2014).

Among the covariates, only age and age squared terms are significantly correlated with wife's contribution. An increase in the wife's age is correlated with a decrease in the wife's contribution to the common account (at an increasing rate). On the contrary, an increase in the husband's age is correlated with an increase in the wife's contribution to the common account (at an increasing rate).

Since the spousal experiment could only be conducted in households where the husband was present (alive and not a migrant labour), these results might be biased. Joseph et al. (2018)[42] in studying remittances to India from migrant labour in UAE, finds that international migrants whose salaries increase over time, remit a constant amount, a behaviour that is consistent with hiding of additional resources from the households. This finding is closest to the type of (in)efficiency that our field experiment uncovers. Combining this with the finding from Morten (2019) using ICRISAT data, that Indian households with more than one adult male (more likely to be an extended household) are more likely to have migrant labour[43], our estimate on nuclear households may be biased downwards[44].

We note here that these results can be at most interpreted as correlations. In order to fully study the causal implication of household structure for household consumption, production, and investment decisions, one would require access to long-term panel data and a strategy to deal with the endogeneity of choice of household structure. Foster and Rosenzweig (2002), using a national level data set from India (ARIS/REDS), note that one-third of the households documented in the early 1970s with more than one male heir had split during re-interview in the early 1980s; in many cases this split was linked

[42] Joseph et al (2018) studies long-term or permanent migration, while in India short-term migration of up to six months is more common (Munshi & Rosenzweig, 2016; Morten, 2019).
[43] In our full baseline sample of 393 households, we indeed find that 43% of the extended and 40% of the nuclear households have a migrant husband (not residing in the household for at least six months but intending to return).
[44] We are assuming here that the degree of inefficiency associated with migrant labour is the same (or less inefficient) in nuclear vis-à-vis extended households. If migrant labour in nuclear households display more inefficient behaviour as compared to their counterparts in extended household, then one cannot predict the direction of the bias in Table 3.5.

to the death of a parent.

Although our analysis across household structures is exploratory and cannot be used to predict what would happen if the households separated, we believe, however, that the estimates we present here are useful and the appropriate estimates to take into account when discussing policy implications.

3.4.3. **Experiments within Extended Households**

We now turn our attention to the inner dynamics of the extended household. In Table 3.6, we analyse how patterns of contributions in the extended household experiments change with the relationship between participants. We present the results using outcomes of all experiments (recall there were up to six, including the spousal experiment) played within the extended household[45]. The dependent variable is the total contribution of both players to the common account (again in proportion). Due to the complexity and variety of household structures and potential players, and the limits in terms of sample size, we estimate determinants of the contribution to the experiment as a function of set of dummy variables - blood relation, gender, generation, and marital links. Hence the estimating equation is given by –

$$C_{i,j} = b_0 + b_1 MALE_{i,j} + b_2 DIFFGEND_{i,j} + b_3 BLOOD_{i,j} + b_4 GENERATION_{i,j} + b_5 SPOUSE_{i,j} + \theta j + \epsilon_{i,j} \quad (4)$$

where $C_{i,j}$ is the contribution to the common account by pair i in household j and θ is the household fixed effects. *MALE* and *DIFFGEND* take the value of 1 if both players are male and both players are of different gender respectively, with the base category as both players being female. *BLOOD* takes the value of 1 if both players are related by blood and 0 if not, that is, for players related by in-law relationship. *GENERATION* takes the value of 1 if both players are of the same generation, such as, siblings or spouses, and 0 if not, such as, parents. *SPOUSE* takes the value of one if both players are married to each other and 0 otherwise.

Note that the mother-in-law/daughter-law relationship is captured by the constant—both

[45] The experiment literature has found that the participant's contribution to the common account would decline in repeated games (see Ledyard, 1994; Chaudhuri, 2011; for an overview). However, we think this observation might not be relevant in our setting because the participants were known to each other and would have interacted in a similar decision-making setting that the public goods experiment mimics. As such, we don't hypothesise that learning happens during the playing of this experiment. Indeed, when we control for the order of the experiment, the controls for order (or repetition) remain insignificant. See Appendix XX Table XX

players female, not related by blood, of different generations and not spouses. Column 1in Table 3.6 does not control for household fixed effects; Column 2 includes household fixed effects. Since each household could have played up to six public goods experiments, the fixed effects control for observable and unobservable household characteristics that are fixed across experiments played within the household (such as size of household, income, caste, religion). Additionally, household fixed effects help us control for the various kinds of structures that may exist with-in the extended households, such as, extended households with no father-in-law. It also partly controls for migration patterns in so far as migration patterns are determined by household composition and characteristics.

The results in Table 3.6 indicate that when players are of different gender, the contribution to the common account increases by possibly 3 to 5 percentage points as compared to an experiment where both players are female (the omitted dummy variable category). Blood relatives contribute significantly more to the common account (9 to 10 percentage points) as compared to in-laws.

Table 3.6. Regression results of experiments within extended households

	Total contribution (1)	Total contribution (2)
Both players male	-0.010	-0.048
(0 = players are not both male; 1 = both players are male)	(0.046)	(0.041)
Both players different gender	0.049**	0.034
(0 = both players not different gender; 1 = both players are different gender)	(0.020)	(0.022)
Blood relatives	0.089***	0.098***
(0 = players are not related by blood; 1 = players are related by blood)	(0.025)	(0.029)
Same generation	-0.021	-0.030
(0 = players belong to different generations; 1 = players are from same generation)	(0.025)	(0.033)
Spouses	0.031	0.055
(0 = players are not married to each other; 1 = players are married to each other)	(0.027)	(0.034)
Constant	0.462***	0.469***
	(0.017)	(0.017)
HH fixed effects	No	Yes
Number of experiments	474	474
R-squared	0.053	0.626

This table reports the results of the regression mapping the contribution to the common account in all experiments (including the spousal experiment) played in the extended households. Contributions are measured in proportions. Total contribution is the contribution by both players to the common account. In Column (1), standard errors are clustered at the household level. Column (2) employs household fixed effects. *** p<0.01, ** p<0.05, * p<0.1

To gain a better understanding of the relative efficiency of each relationship within the extended household, we report the results of a series of joint hypothesis tests in Table 3.7. The tests are performed using the OLS results (with household fixed effects) from Column 2, Table 3.6. As a reference point, on average, mother-in-law/daughter-in-law pairs contribute 47 percent of the total endowment to the common account. Similarly, different combinations of these dummy variables capture the other relationships within a household – see Column 4.

Table 3.7. Hypothesis testing of experiments by relationship within extended households

Relationship			Coefficient			
Player 1	Player 2	Number of experiments		Direction	Percentage point difference	p-value
(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-law relations						
Mother-in-law	Daughter-in-law	96	b0	base category		
Father-in-law	Daughter-in-law	85	b0+b2	More	+3	0.115
Brother-in-law	Sister-in-law	32	b0+b2+b4	More	+0.5	0.905
Sister-in-law	Sister-in-law	23	b0+b4	Less	-3	0.366
Blood relations						
Mother	Daughter	5	b0+b3	More	+10	0.001
Father	Son	35	b0+b1+b3	More	+5	0.108
Father Mother	Daughter Son	47	b0+b2+b3	More	+13	0.000
Sister	Sister	0	b0+b3+b4	More	+7	0.149
Brother	Brother	3	b0+b1+b3+ b4	More	+2	0.669
Brother	Sister	1	b0+b2+b3+ b4	More	+10	0.033
Spousal relationship						
Husband	Wife	147	b0+b2+b4+ b5	More	+6	0.010

This table reports the hypothesis tests of coefficients from the household fixed effects regression in Column (2) of Table 3.6. Note that as the father-daughter pair and the mother-son pair share the same specification, no separate hypotheses testing can be conducted. There were 5 father-daughter experiments and 42 mother-son experiments. A t-test comparing the contributions across these two pairs however reveals no statistically significant difference. The number of experiments for spouses is higher than 111 reported and used in the spousal experiment section. This is because 'the other male' and 'the other female' could also be related by marriage.

We conduct tests to assess whether contributions in these relationships differ from those in the mother-in-law/daughter-in-law relationship. Column 6 reports the mean difference in contribution to the common account between the base category and each of the other relationships in terms of percentage point difference. Column 7 reports the *p*-value of the hypothesis test comparing each relationship to the base category, that is, the mother-in-law/daughter-in-law pair. Column 3 notes the number of experiments within each category. We note that some categories have a very small sample size,

especially among the blood relations[46], and results need to be interpreted with this caveat in mind.

Compared to the mother-in-law/daughter-in-law pair, parents and children contribute significantly more. Mothers and daughters contribute 10 percentage points more to the common account, on average, whereas fathers and sons contribute about 5 percentage points more, on average. Fathers paired with daughters and mothers paired with sons contribute the highest on average to the common account (60 percent on average, which is about 13 percentage points more than the mother-in-law/daughter-in-law pair). Spouses also contribute significantly more, by about 6 percentage points, on average. The only relationship that contributes less than the mother-in-law/daughter-in-law pairing is two sisters-in-law. However, this result is not statistically significant.

3.4.4. **Decision-making within Extended Households**

We further explore the (in)efficiency within extended households by linking our public goods experiment with survey data. The (in)efficiency within the extended household is not only determined by the relationship to the household member, but also by the division of decision-making power within the household.

In the household survey, we asked the respondent to identify up to three key decision makers on six different household tasks – what to cook on a daily basis, household daily purchases at the local shop, household purchases at shops outside the village, what to do when a child is sick, decision related to if a child should be enrolled in school, and decisions related to daily attendance at school. We asked for three decision makers to be able to identify the household members other than the married couple who play an important role.

Using this decision-making data, we distinguish between three cases: (1) both individuals (or participants in the experiment) have a say, (2) only one individual has a say, and (3) neither of the two individuals has a say. The regression sample for Table

[46] The experiment protocol was to select at random an adult married male/female. Given partilocality, it is obvious that we have very few games with the daughter of the household as a married daughter would no longer reside at her natal household. Second, as Morten (2019) finds that households with more than one adult male are likely to have more migrant labour, this may be the reason why we find low number of experiments (only 3) between two brothers.

3.8 is all experiments excluding the spousal experiment and hence, by definition, only includes extended households. The base category for each decision-making realm is neither of the two participants has a say in decision making.

First, across all decision-making domains, one or both participants having a say in decision making is negatively correlated with contributions to the common account relative to neither player having a say. Second, across all domains of decision making except those related to the child's education, when both participants are decision-makers, the contribution to the common account falls to a larger extent than when only one of the two participants is a decision-maker. This fragmented decision-making power and consequent power struggle may be one of the reasons for driving the inefficient intra-household behaviour we observe in extended households.

Table 3.8. Regression results of experiments within extended households on decision-making power

Distribution of decision making power	Total contribution (1)	Total contribution (2)	Total contribution (3)	Total contribution (4)	Total contribution (5)	Total contribution (6)
Cooking - one participant	-0.036					
	(0.024)					
Cooking - both participants	-0.065*					
	(0.036)					
Purchase at the local shop - one participant		-0.032				
		(0.024)				
Purchase at the local shop - both participants		-0.068**				
		(0.032)				
Purchase outside the village - one participant			-0.038			
			(0.024)			
Purchase outside the village - both participants			-0.052			
			(0.039)			
Child is sick - one participant				-0.033		
				(0.022)		
Child is sick - both participants				-0.061*		
				(0.034)		
Child is enrolled in school - one participant					-0.034*	
					(0.021)	
Child is enrolled in school - both participants					-0.010	
					(0.029)	
Child attends school - one participant						-0.036*
						(0.020)
Child attends school - both participants						-0.026
						(0.028)
Number of experiments	363	363	363	363	363	363
R-squared	0.075	0.074	0.073	0.074	0.071	0.072

This table reports the results of the effect of decision-making power distribution on the total contribution by both players to the common account in all experiments (excluding the spousal experiment). Contributions are measured in proportion. Controls added are both players are male, both players are female (base category), both players are different gender, both players are related by blood, both players are from the same generation (such as two brothers). Standard errors are clustered at the household level. *** p<0.01, ** p<0.05, * p<0.1

3.4.5. **Results from Qualitative Study**

We interviewed three nuclear households and five extended households using a semi-structured qualitative interview in December 2016. Appendix B Table B.11 and Table B.12 summarise the qualitative sample. Among the extended households, respondents included both daughters-in-law and mothers-in-law. All households were involved in a diverse set of economic activities. Half of the households interviewed have migrant members, and remittances are an important source of income[47].

We draw upon these interviews to provide a better understanding of the in-law relationships within the extended household. As we interviewed women, the natural focus was on the mother-in-law/daughter-in-law relationship. We start with their general perceptions on the costs and benefits of extended households. We then proceed with a description of the inefficiencies mentioned by the respondents (note that respondents did not use the term 'inefficiency', rather, we flagged something to be inefficient when the respondent described cases of free-riding, moral hazard, cheating, and so on). We conclude by linking up these inefficiencies with elements of the household decision making process.

Household structure appears to be both transient and complex in our sample as all respondents in nuclear households reported having lived in an extended household setting in the past[48]. All except one respondent (interview 2) perceived extended households to be overall superior to nuclear households. The perceived benefits of extended households include risk sharing, emotional support, specialization (division of labour), and household public goods and joint assets. Some examples:

- Respondent 7 noted that if her husband, a migrant, did not send money one month, it would not matter, as her mother-in-law ensures that she is taken care of.

[47] Our list for the qualitative interviews distinguished between migrant and non-migrant households. We randomly selected three households from the nuclear family list and five households from the extended family list (but ensuring we had at least one migrant household from each type).

[48] In three cases, the productive unit surpasses the consumption unit (interviews 4, 7, and 8), meaning that although we define the household in the traditional manner as a group of people who eat at least one meal together each day, in three cases the group of people who worked together on the land owned or co-owned is larger than this consumption unit. This is not an uncommon situation (see, for instance, Udry 1996, and Beaman & Dillon 2012) and affects the functioning of the household, as we discuss below.

- Respondent 1, who owns a small store, regretted the loss of gas for cooking when shifting to her current nuclear household.
- Respondent 4 mentioned that she had less work when the extended household was even larger and she did not have to cook every day, as this task was done by the other women in the household.

However, four out of five respondents in extended households noted inefficiencies, whereas none of the nuclear household respondents mentioned any event, activity, or behaviour that could be construed as inefficient. Most of these inefficiencies relate to labour and effort. All respondents noted a fixed set of daily duties that can include cooking, fetching water, gathering firewood, making dung cakes (used as fuel), feeding cattle, and taking care of young children and the elderly. Few women noted agricultural duties (interviews 4 and 5), including collecting fodder and threshing.

In extended households, all women reported being assigned to a subset of these tasks. For example, respondent 7 is in charge of cooking and looking after her own son. Her elder sister-in-law visits the field, collects fodder for the animals, and feeds them. Her mother-in-law, according to her account, does little and mainly takes care of her father-in-law, who had been unwell recently. Consistent with the literature (Jeffery & Jeffery, 1996), the set of tasks an individual is engaged is governed by social norms. None of the mothers-in-law interviewed were involved in cooking, for instance, something which was left to the youngest daughter-in-law.

Although these social norms may protect the household from excessive free riding, meaning the norm ensures that meal preparation—a public good within the household—will get done, the lack of observability of effort might introduce inefficiencies. For example, respondent 7 admitted (to us) to cooking the food slowly so as to avoid receiving other tasks. In addition, some household members might (be perceived to?) contribute little due to the hierarchical assignment of tasks: All daughters-in-law we spoke to referred to their mothers-in-law as being ‘idle’ or ‘somewhat useless’[49].

[49] The men in the extended households are engaged in agriculture, casual labor, sales jobs, or migration. Here too, social norms appear to be at play; in all farming households (with the exception of interview 5, where only one male was involved in farming), decisions regarding the household land (that is, which crops to cultivate and other input decisions) are usually made by the eldest able man, sometimes in discussion with the other adult men. All available

Following the decision-making process for particular events can shed light on household relations. Agricultural decisions were made by the eldest capable male member. When a decision had to be made which concerns another realm, such as clothing, education and health, there would usually be no obvious decision maker. Instead, we noted alliances with negotiation at the centre. For example, respondent 8, when she wishes to purchase something personal, will approach her husband who is usually amenable to the request and will either purchase the item himself or approach his elder brother for funding. When respondent 7 wanted to attend the literacy program she approached her sister-in-law, who in turn approached her mother-in-law, who then together with the sister-in-law, made the decision and informed the father-in-law.

This last example illustrates the complex position of junior women in extended households. In effect, all junior women interviewed noted having 'little' to 'no' decision making power, but when pressed for examples, except for the daughter-in-law of respondent 6, all described forging alliances when need be, and perhaps they are not as powerless as they claim themselves to be[50]. In contrast, women in nuclear households note having considerable say. This is especially the case when the husband is a migrant worker and the day-to-day decisions are left to his spouse. In this case, the respondents all noted making the decisions themselves as to what to cook, how much to cook, and what to spend on clothing, medicine, and small educational items. The migrant husband would be asked to give his approval, though, when it comes to less frequent decisions, such as visits to the natal family, attending adult educational classes, and school enrolment. Even though such permission would be necessary, all women in the nuclear households that we interviewed noted that they usually come to a consensus with their husband through discussion, and the initial request would be approved in most cases.

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men work on the land to a certain extent. In all families, the harvest is shared equally between the households who work on the land. In the cases where the production unit exceeded the consumption unit, this gave rise to free-riding. For instance, respondent 8 noted that although everyone receives an equal share from the harvest, her youngest brother-in-law contributes little to the activities and is mostly preoccupied with his carpet weaving activity, the returns of which are only used by himself and his nuclear unit.

[50] These inconsistencies in an individual's narrative were not uncommon, and we attribute them to the complexity of the family relations where an individual tries to reconcile many contradictory aspects of her reality, but also to the limitations of our method. A carefully executed ethnographic approach, as in Caldwell et al. (1984), Jeffery and Jeffery (1996) and Lentz (2018), would have likely yielded many additional insights.

It is this lack of decision-making power of junior women, together with discrepancies in preferences and goals and the opportunity to hide one's efforts, which can lead to the inefficiencies the women mentioned. Unable to assert one's preferences or achieve one's goals, junior women in extended households resort to behaviours that improve their personal well-being but might reduce the household's collective welfare. These behaviours can include slacking off on assigned tasks, avoiding tasks altogether, or hiding income or resources. For instance, respondent 7, who has a migrant husband, noted that she hides around 20 percent of the remittance her husband sends her from her mother-in-law. Our public goods experiment effectively mimics this situation of funds that arrive in the household, and asks the players to make a decision as to what to do with these 'remittances'.

To conclude this section, we note that all women recognized that decision making power was subject to change. This is consistent with the literature (Jeffery and Jeffery, 1996; Uberoi, 1994). The relationship between the various extended family household members is complex and changes over time when life events take place. For instance, a younger daughter-in-law might have very little say when she joins the household, relative to the unmarried daughters living in the household; however, her position might change after the birth of a son or daughter. The mother-in-law's power can change after her husband dies. It is in these periods of change that households see new bargaining patterns around household chores, expenses, and the allocation of resources (Sharma, 1980).

3.5. **Conclusion**

Using a series of public goods experiments conducted with adults in extended and nuclear households in India, we investigate efficiency in within household decision making. We focus on Pareto Efficiency of an allocation in which it is not possible to make one individual better off without making another individual worse off. The experiment implemented in this study is designed to uncover inefficiency which arises due to concealing personal resources instead of contributing them to the household as a whole, with potentially larger shared benefits. We find three interrelated sets of results. First, we find that households are inefficient across household structures, whether it be a

nuclear or an extended household. Indeed, all but 2 percent of pairs failed to maximize the surplus in the public goods experiment. In terms of magnitude of inefficiency, on average, the money earned is about 10 Rupees less than what could have been earned has household pairs exhibited efficient behaviour. Second, we provide descriptive evidence that spouses are less efficient in extended households than in nuclear households. Comparing extended households with nuclear households, it should be noted that the difference in magnitude between the two types of structures, while statistically significant and substantial in relative terms, is not that large in absolute terms. In effect, the difference of 7 percentage points between nuclear and extended spousal pairs (after controlling for observable characteristics) is equivalent to an efficiency loss of only 1.40 Rupees. Third, using household fixed effects for experiments conducted in extended households, we find that relationships within the extended households are not all equally inefficient, with the mother-in-law and daughter-in-law relationship being particularly inefficient. Survey and qualitative evidence further point at an unequal distribution of power between generations and genders as an underlying factor in these results.

Despite our sample being restricted to illiterate women, our findings from the spousal experiment are consistent with those from experimental literature in rural India (see, for instance, Castilla, 2015; Mani, 2011; and Munro et al., 2014). Additionally, our findings are consistent with the survey literature on limited autonomy of married women in rural Uttar Pradesh (Dyson & Moore, 1983; Bloom et al., 2001; Jejeebhoy & Sathar, 2001)

These findings fill important gaps in the literature, as the economics literature, albeit with some notable exceptions (see, Section 3.2), has largely struggled to understand complex households, even though they are a central part of many non-Western societies. Just as the study of within-household decision making in nuclear households has led to a better understanding of the allocation of resources within households and more appropriately designed policies, it is equally important to understand decision making in extended households.

The most direct policy implication of our findings on patterns of inefficiency within extended households is for policies that target specific recipients within a household, such as cash-transfer programmes in the context of societies with extended households.

Duflo (2012) notes the importance of targeting transfers to the 'woman' in the household, with the aim of promoting gender equality as well as improving other desirable outcomes such as health and education. However, in the context of an extended family household, the identity of this 'woman' is ambiguous, so that simply targeting transfers on the basis of gender might fail to achieve desired outcomes. In general, our combined survey, qualitative and experimental results point at the overall lack of decision making power among certain junior women (daughter-in-law) of the household, implying that, if government programmes maintain the household head as the beneficiary, these members are unlikely to have much say in whether and how the benefits of these programmes may be used. This point is also argued by Porter and Adams (2016), who note the need to study sharing rules within households, as this has consequences for the design of intra- and inter-generational redistributive programs. Thus, what might work in nuclear households might not be straightforward in extended households with fragmented and inequitable distribution of decision-making power. Bertrand et al. (2003) find that a South African pension program reduced the labour supply of prime age individuals in extended households, especially when the pensioner was a woman, suggesting that resources are pooled to some extent in this context. In the Indian context, given the conflictual nature of the mother-in-law/daughter-in-law relationship, as recognised in this study, it is not clear to what extent transfers targeted to the female head of household would lead to improvements in the situation of the daughter(s)-in-law in that extended household. Thus, particular attention needs to be paid to designing cash transfer programs in extended households.

A common criticism levelled against lab-in-the-field experiments is questioning whether they mimic real life decisions. The public goods experiment we implement is designed to uncover a particular dimension of inefficiency within households: concealing personal resources instead of contributing them to the household as a whole, with potentially larger shared benefits. We draw on the qualitative interviews to argue that household members in extended households do hide resources in processes that our experiment mimics. For example, as in the experiment, the wife of a migrant husband decides to hide a share of the remittances from her family-in-law. However, the qualitative work also uncovered additional patterns and dimensions of inefficient behaviour that other experiments could better mimic, such as production inefficiencies (slacking off and other forms of free riding). Developing experiments or other empirical

methods, perhaps building on Udry (1996), to unpack these other dimensions of inefficiency within the extended households and to relate them to observed behaviour, is one fruitful avenue for future research.

We conclude with a note on household formation and household structure. Although we find both higher inefficiency the in-laws within extended households, and higher inefficiency between spouses in extended households relative to nuclear households, this does not mean that household members would be better off if they split into nuclear households. This would be true even if the correlations we present were causal relationships, given that there are economies of scale and specialization gains in production that favour larger households. However, in order to fully study the causal implication of household structure for household consumption, production, and investment decisions, one would require access to long-term panel data and a strategy to deal with the endogeneity of choice of household structure. As Jeffery and Jeffery (1996) note, and as we confirmed in our qualitative interviews, households change. A young couple may start off their married life in an extended household but split off later and form a nuclear household as their family continues to grow. Further inspiration for plausible identification methods can be drawn from the numerous studies in the other social sciences, both demographic accounts as well as detailed ethnographic studies (Caldwell et al., 1984; Jeffery & Jeffery, 1996; Lentz, 2018; Ram & Wong, 1994).

4. **Female Adult Literacy Programme and Empowerment: Evidence from RCT in rural India**

4.1. **Introduction**

According to UNESCO (2020), there are 773 million illiterate adults (over 15 years of age) in the world, of which 63 percent were women. Nearly half of these live in South Asia, where illiteracy is still largely a female phenomenon. India, the country in which this study is set, accounts for a third of the world's illiterate population at 252 million illiterate adults (UNESCO, 2020). The Indian Census (2011) puts the adult (18 years and above) female illiteracy rate at 43 percent versus 22 percent for men. It is worse for rural areas where over half the adult female population remain illiterate.

Given the size of the illiterate population, achieving literacy remains central to international community efforts. The Education for All goals included 'achieving a 50 percent improvement in levels of adult literacy by 2015, especially for women, and equitable access to basic and continuing education for all adults'. This global goal received a refreshed impetus in the Sustainable Development Goals which pledges to eliminate adult illiteracy by 2030, while ensuring the elimination of gender disparity in education.

In the light of this call to eliminate illiteracy levels internationally, the provision of adult literacy programmes is common. However, most large-scale adult literacy programmes rarely met their learning targets (for a full review see Abadzi, 2003) mainly due to low enrolment rates, and a loss of acquired literacy skills in the long run. Abadzi (2003) notes that the World Bank financed almost no adult literacy in the 1980s and continued a cautious approach in the 1990s. This begs the question whether adult literacy programmes are the right tool to achieve 'education for all'. Notwithstanding the literacy goal, adult literacy programmes may also be tools for achieving other socio-economic outcomes. This is particularly true for developing countries where there is some empirical evidence of impacts of adult literacy programmes on other welfare measures (for instance, see Blunch, 2013; Blunch & Portner, 2011; Banerjee et al., 2017). Hence, countries continue to invest in universal adult literacy campaigns. The Government of India launched the Sakshar Bharat adult literacy campaign in 2009 with an additional focus on closing the gender gap in literacy. By focusing on women, these campaigns aim to promote female empowerment.

However, there is a paucity of evidence on adult literacy programmes beyond the intended effect of achieving functional literacy and numeracy. Even the evidence that does exist on the impact of adult literacy programmes on literacy itself, is fraught with selection bias. This study is one of the few adult literacy programme evaluations that employs a Randomised Control Trial (RCT) to rigorously estimate the impact of the programme on female empowerment. The chapter studies the 8-month long TARA Akshar Plus (TA+) programme delivered by Development Alternatives in rural Uttar Pradesh. Uttar Pradesh not only presents the worst female literacy rate in India (Indian Census, 2011), but is also noted for low levels of female empowerment (Dyson & Moore, 1983; Bloom et al., 2001; Jejeebhoy & Sathar, 2001; Jayachandran, 2015). Deshpande et al. (2017) documents the positive effects of the TA+ programme in Uttar Pradesh on women's literacy and numeracy. But the question remains if this improved literacy can translate into greater female autonomy over decision making, greater freedom of movement and access to financial resources.

It has been assumed by policy makers that adult literacy programmes, especially those for women, may be a cost-effective way to encourage female empowerment. However, little is known if this is true and relatedly the mechanisms through which empowerment is brought about. Drawing on the literature on formal education, better educated women have increased labour market participation and consequent increase in women's contribution to the household income (Rahman & Rao, 2005; MacPhail & Dong, 2007; Hashemi et al., 1996). This is likely to increase the woman's bargaining power (Almas et al., 2018) and position as a decision maker within the household. However, other studies have found maternal education to be a significant predictor of decisions regarding children's education and health investment through the channel of increased knowledge and empowerment (Thomas et al., 1991; Glewwe, 1999; Andrabi et al., 2012; Aslam & Kingdon, 2012). In fact, in their Pakistan study, Andrabi et al. (2012) found that mother's education did not increase bargaining power within the household, but still increased her investment in children's education. This is particularly relevant for this chapter which is based in rural Uttar Pradesh with similar patriarchal structure and adverse gender norms. Given the gender-related cultural norms resulting in the lower female labour force participation in this region (Dyson & Moore, 1983; Foster & Rosenzweig, 1996), I hypothesise that in so far as literacy skills enable women to acquire knowledge on household decisions, it would shift the woman's role in intra-

household decision making, without necessarily changing her bargaining power.

Most literacy programmes targeted at women already encourage attaining such knowledge by including discussion on a wide variety of welfare topics related to children's education and health, maternal health, hygiene practices (for example, TA+ Programme discussed in Deshpande et al., 2017 and in this Chapter, the Ghanaian National Functional Literacy Programme discussed in Blunch, 2013; Blunch & Portner, 2011). This increased knowledge together with the newly acquired literacy and numeracy skills may boost the woman's confidence (as documented in qualitative studies of literacy programmes, for instance, Egbo, 2000; Archer & Cottingham, 1997; Stromquist, 1997). Additionally, going to literacy classes regularly unaccompanied by household members would improve self-efficacy among these women, making it more likely for these women to be able to leave the house for other tasks (such as, visiting the market, natal family, local fair, cinema).

The literacy classes in themselves present an opportunity for women to talk to other women and create peer networks that may not have existed. An increase in the woman's peer networks is shown to enable women overcome mobility constraints in developing countries (Kandpal & Baylis, 2019; Anukriti et al., 2020). Relatedly, social networks have also been key to other aspects of women's empowerment, such as adoption of reproductive health technology (Kohler & Buhler, 2001; Kohler et al., 2000) and increase in financial independence (Field et al., 2016; Banerjee et al., 2013). Increase in confidence, self-efficacy, and peer networks are all possible mechanisms through which literacy programmes may also drive a shift towards greater control of financial resources among women, such as opening a bank account. Additionally, opening a personal bank account is a direct function of literacy and numeracy skills, enabling women to read, complete and sign bank forms.

In this chapter, I find that the TA+ programme had significant positive impacts on aspects of female empowerment, despite the deeply rooted gender-based cultural norms of rural Uttar Pradesh. The literacy programme increased the overall freedom of movement index by 13 percent, by substantially increasing the likelihood of women going to local shops and going to call their natal family without requiring permission. Moreover, the programme increased the probability of the women opening a bank account by 13 percent. I do not find any impacts of the programme on the women's

decision making power within the household. While it is unlikely that an 8-month long literacy programme would increase the woman's bargaining decision making power through employment and increased income, literate women are known to be efficient consumers of knowledge (Thomas et al., 1991; Andrabi et al., 2012). This increase in knowledge has been documented to improve the woman's role in household decision making. However, the null effects I find in this study, may be due to lack of exposure to such sources of information (such as, newspapers and television) in rural Uttar Pradesh (Thomas et al., 1991).

Female empowerment has increasingly become a policy goal as reflected in United Nations' Sustainable Development Goal No. 5, which stipulates the importance of achieving gender equality. This concern was reiterated by the World Bank (2012) pushing for gender equality in employment, income, assets and agency. The importance of achieving female empowerment is magnified by the evidence that this leads to other development goals, such as higher investment in household public goods, children's education and nutrition, and changing gender beliefs and aspirations (see, among others, Ashraf et al., 2010; Beamen et al., 2012; Beamen et al., 2009; Duflo, 2012; Duflo, 2003).

If these effects could be studied and established, adult literacy programmes could receive new impetus as an important cost-effective tool contributing to the social and economic development of countries with low human capital. Overall, the body of rigorous evidence on adult literacy programmes remains small and needs to be strengthened.

4.2. **Related Literature**

There is a vast literature in developing countries which reports the positive effects of formal education on female empowerment such as women's decision-making ability within the household, women's freedom of movement and control of resources (for a full review, see Pande et al., 2005). Closely related to formal education is access to employment opportunities. For instance, better educated woman earning higher wages than their husbands have greater freedom of movement and a say in household decision making (Rahman & Rao, 2004). Another study in China finds that irrespective of the wage gap between spouses, an employed woman enjoys a higher household status than

unemployed woman (MacPhail & Dong, 2007). Similarly, Boateng et al. (2014) finds that educated and employed women in Ghana are more likely to have an opinion on all aspects of household decision-making. This higher status of an employed woman within the household may come by way of the greater economic value that an employed woman represents. Hashemi et al (1996) show that participation in a microcredit programme in Bangladesh increases the women's economic contribution to the household, thereby leading to female empowerment within the household and community.

These studies suggest that formal education is associated with female empowerment by way of the woman having access to employment opportunities and economic resources. Drawing on intra-household model bargaining model, access to income would influence the 'sharing rule' thereby influencing the woman's role in intra-household decision making. However, it would be erroneous to assume that adult literacy programmes would translate into female empowerment through a similar mechanism. Literacy programmes narrowly focus on developing functional reading and writing skills and are of too short a duration to allow women to enter the workforce. Add to this the context of this study – deeply rooted patriarchal structure with patrilocality (Dyson & Moore, 1983; Anukriti et al., 2020) and gender related social norms on labour force participation (Foster & Rosenzweig, 1996). It is unsurprising that even formal education has found dismal effects on woman's bargaining and decision making power in the context of Uttar Pradesh (Bloom et al., 2001; Jejeebhoy & Sathar, 2001; Jayachandran, 2015).

However, another strand of formal education literature studying the effects on children's health and educational investments, has proposed an increase in access to information to be key to woman's role as a decision maker within the household. Glewwe (1999) find mother's knowledge of health acquired using the literacy and numeracy skills taught at school to be an important predictor of investment in child health. Relatedly, Thomas et al. (1991) finds that almost all the impact of maternal schooling on child height can be explained through mother's access to information through newspaper, radio, and television in urban Brazil. Thus, in so far as the adult literacy programmes increase women's knowledge and access to information, this can increase the woman's role in intra-household decision making without necessarily shifting the bargaining power (as

seen in Andrabi et al., 2012 study in Pakistan).

While increase in knowledge may be one of the channels through which female empowerment is realised over decision making, it is less clear that this could have effects on other aspects of empowerment, such as mobility and control of financial resources. An indirect effect of literacy on female empowerment may be through the channel of peers or social networks. Women, particularly in rural Uttar Pradesh as in other developing countries, seldom have access to social networks of friends, peers, or relatives (Kandpal & Baylis, 2019; Anukriti et al., 2020). In attending literacy classes, the women can create a network of peers outside of their household which may reduce mobility constraints. Anukriti et al. (2020) in the Uttar Pradesh sample finds that having an additional outside peer almost doubles the likelihood of a woman able to freely visit a family planning clinic. They discuss the role of peer support through companionship in helping the women to overcome mobility constraints imposed by the family. Kandpal and Baylis (2019) study the impact of peer effects on social norm driven behaviour and find positive impacts on the likelihood of the married women's ability to leave the house without permission.

Yet another channel of female empowerment is the increase in woman's own self-efficacy, confidence, and aspiration as result of becoming literate. A small body of qualitative research documents evidence that illiteracy has a negative impact on women's self-esteem, while literate women report being more confident (see Egbo, 2000 in Nigeria; Archer & Cottingham, 1997 in Bangladesh; Stromquist, 1997 in Brazil).

Such evidence assessing the impact of adult literacy programmes on socio-economic outcomes, and the associated pathways, is hard to come by in quantitative research. There are a few exceptions. Blunch and Pörtner (2011) report substantial, significant, and positive effects of an adult literacy programme in Ghana on the standard of living, as measured by household consumption expenditure. Blunch (2013) studying the same adult literacy programme in Ghana concludes that mother's participation in the programme had a substantial impact on reducing child mortality. However, the Ghanaian literacy programme is one of the few long-term programmes running for 21 months and covering 28 different themes such as nutrition, family planning and farming practices.

These few studies still have nothing to say about the agency of the literate woman herself. They additionally suffer from selection bias as they rely on ex-post comparisons with non-participants. Even studies of the effect of adult literacy programmes on the literacy outcome itself (Carron, 1990; Ortega & Rodríguez, 2008) suffer from similar problems. This chapter is one of the few evaluations of adult literacy programmes to provide causal estimates. A few exceptions are the literacy programme evaluations using an RCT by Aker et al. (2012) in Niger, and Deshpande et al. (2017) in India.

Only two studies of literacy programmes report on the impact on female empowerment. First, Banerji et al. (2017) employ a randomised controlled trial in Bihar and Rajasthan, assigning households to either adult literacy classes for mothers, training for mothers to enhance their children's learning, or a combination of both treatments. They find that the maternal literacy classes had no significant impact on children's learning levels, but a combination of both treatments did. All three treatment arms have a significant impact on mothers' test scores, mothers' participation in their children's education, and availability of educational assets in the household. Moreover, measuring empowerment as a decision index comprised of mothers' involvement in decisions regarding household purchases and child's schooling decisions, they find no significant impact of the literacy programme by itself. However, their insignificant intent-to-treat effects can be attributed to the low literacy programme take-up in their sample – self-reported records show that only 40 percent of the mothers attended these classes. Second, the purpose of this study is to add to the limited evidence on the impacts of adult literacy programme on female empowerment as an end in itself; while that of Banerji et al. (2017) is 'searching for (cost-effective) methods to improve levels of learning (children's education) in developing countries' (p. 303).

A second closely related paper, Kandpal et al. (2012), evaluates the effect of a different female literacy programme in Uttarakhand, India and finds a positive, significant effect on women's empowerment outcomes. Participants are more likely to have access to outside employment and are able to leave the house without permission across all regression specifications. However, the study is restricted in two respects. First, its identification strategy relies on an IV strategy using roll-out of the programme. This raises concerns regarding selection on unobservables and if the programme was purposely rolled out in some districts because of worse gender equality metrics in those

areas. Second, it uses a very narrow set of three measures for women empowerment, namely access to employment is measured as having a National Rural Employment Guarantee card; mobility is measured as the ability to leave the house without permission; and political participation is measured as participation in village council meetings.

This chapter improves on the above-mentioned studies by using an RCT to overcome the selection issue like Banerji et al. (2017), but with a greater treatment compliance of 79 percent of the treatment group women attending the literacy classes. Moreover, the study makes use of a wide range female empowerment indicators and indices - decision-making on different household domains, the ability to leave the house freely for a range of tasks, and control of different financial assets.These studies suggest that formal education is associated with female empowerment by way of the woman having access to employment opportunities and economic resources. However, it would be erroneous to assume that adult literacy programmes would translate into female empowerment through a similar mechanism. Literacy programmes narrowly focus on developing functional reading and writing skills and are of too short a duration to allow women to enter the workforce. However, adult literacy programmes might work through the channel of bolstering confidence and aspirations which would translate into improving a woman's position in her household and her community. It is difficult to predict the impacts of adult literacy programmes due to the limited evidence on the same. A small body of qualitative research documents evidence that illiteracy has a negative impact on women's self-esteem, while literate women report being more confident (see Egbo, 2000 in Nigeria; Archer & Cottingham, 1997 in Bangladesh; Stromquist, 1997 in Brazil).

Such evidence assessing the impact of adult literacy programmes on socio-economic outcomes is hard to come by in quantitative research. There are a few exceptions. Blunch and Pörtner (2011) report substantial, significant, and positive effects of an adult literacy programme in Ghana on the standard of living, as measured by household consumption expenditure. Blunch (2013) studying the same adult literacy programme in Ghana concludes that mother's participation in the programme had a substantial impact on reducing child mortality. However, the Ghanian literacy programme is one of the few long-term programmes running for 21 months and covering 28 different themes

such as nutrition, family planning and farming practices.

However, these studies rely on ex-post comparisons with non-participants. Even studies of the effect of adult literacy programmes on the literacy outcome itself (Carron, 1990; Ortega & Rodríguez, 2008)[51] suffer from similar problems. This chapter is one of the few evaluations of adult literacy programmes to provide causal estimates.

In a randomised control trial in Niger, Aker et al. (2012) find a significant effect of the adult literacy programme on math and reading scores, with an even larger effect for individuals assigned to a treatment arm of the literacy programme with a monitoring system in place. Second, Deshpande et al. (2017) find a statistically significant impact of the adult literacy programme (Tara Akshar+)[52] on literacy outcomes in rural Uttar Pradesh in India, with the study set up as a randomised control trial.

Exploring effects on a wider range of socio-economic outcomes other than literacy outcomes; and arguably closer to the purpose of this study, there are two papers of interest. Banerji et al. (2017) employ a randomised controlled trial in Bihar and Rajasthan, assigning households to either adult literacy classes for mothers, training for mothers to enhance their children's learning, or a combination of both treatments. They find that the maternal literacy classes had no significant impact on children's learning levels, but a combination of both treatments did. All three treatment arms have a significant impact on mothers' test scores, mothers' participation in their children's education, and availability of educational assets in the household. Moreover, measuring empowerment as a decision index comprised of mothers' involvement in decisions regarding household purchases and child's schooling decisions, they find no significant impact of the literacy programme by itself. However, their insignificant intent-to-treat effects can be attributed to the low literacy programme take-up in their sample – self-reported records show that only 40 percent of the mothers attended these classes. Second, the purpose of this study is to add to the limited evidence on the impacts of adult literacy programme on female empowerment as an end in itself; while that of Banerji et al. (2017) is 'searching for (cost-effective) methods to improve levels of

[51] Carron(1990) finds significant effects of the Kenyan large-scale literacy programme, but report low levels of take up and poor coverage. Ortega and Rodriguez (2008) finds small insignificant positive effects of the Venzuelan large scale literacy programme.

[52] The effect of the same programme is studied in this paper in the same geographical location. However, the sampled villages and sample size are different as this chapter makes use of data collected in Phase 2 of the programme.

learning (children's education) in developing countries' (p. 303).

A second closely related paper, Kandpal et al. (2012), evaluates the effect of a different female literacy programme in Uttarakhand, India and finds a positive, significant effect on women's empowerment outcomes. Participants are more likely to have access to outside employment and are able to leave the house without permission across all regression specifications. However, the study is restricted in two respects. First, its identification strategy relies on an IV strategy using roll-out of the programme. This raises concerns regarding selection on unobservables and if the programme was purposely rolled out in some districts because of worse gender equality metrics in those areas. Second, it uses a very narrow set of three measures for women empowerment, namely access to employment is measured as having a National Rural Employment Guarantee card; mobility is measured as the ability to leave the house without permission; and political participation is measured as participation in village council meetings.

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4.3. **The Female Adult Literacy Programme**

TARA Akshar Plus (TA+) is a female adult literacy programme implemented by a Delhi based NGO, Development Alternatives (DA). This is an 8-month long computer-based programme that enables illiterate women to read and write in Hindi and to achieve basic numeracy skills. The programme includes 56 days of daily instructional classes, followed by 6-month long reading club meetings. The programme was created by ReadingWise[53] and adapted by the TARAhaat team in 2004-2006 for India.

The programme spans 37 days of reading (Tara Akshar) and 19 days of numeracy (Tara Ganit). The 37 days of reading aspect is further divided into 26 days of computer based

[53] Details of the ReadingWise initiative can be accessed on their website http://readingwise.com/story/

instruction lasting 100 minutes daily with 20 minutes of revision and doubts clarification, 10 days of practice sessions, and finally one exam day where the learners are assessed on their ability to recognise Hindi letters, write words, phrases and sentences, and apply reading skills. The instructional session themselves are structured to incorporate the following activities – 4 minutes of video, 12 minutes of big flash cards, 20 minutes of writing, 20 minutes of small flash cards for revision, 10 minutes of exercise on computer software, another 20 minutes of writing practice, 10 minutes of practice with peers, and finally 4 minutes of a follow up video.

The numeracy programme lasts 105 minutes for 19 days including a last day reserved for assessment. Each session is structured as – 13 minutes of word problems, 10 minutes of big flash cards, 12 minutes of writing practice, 10 minutes of small flash cards for revision, 10 minutes of exercise on computer software, 10 minutes of writing numbers, 10 minutes of repetition in group, and 30 minutes of writing practice. The 7th, 9th, 11th, 13th, 16th, and 18th days of the programme are reserved for tests on counting, addition, subtraction, multiplication, division, and multiplication tables respectively.

After the programme, a reading club called ‘gyan choupali’ is formed which lasts for 6 months with meetings held two to three times a week. It is led by a paid ‘TA Saheli’ who is given a two-day training prior to the beginning of reading clubs. These sessions are based on reading and writing books, interactive games, and watching movies.

The programme was delivered at the hamlet level (subdivision of a village). It was delivered at the same time across hamlets and followed the same structure. Each session has approximately a class size of 10 women.

Since its inception, TA+ has been made available to over 200,000 women in the Indian states of Bihar, Madhya Pradesh, Uttar Pradesh, Haryana, Jharkhand, Rajasthan, Uttarakhand, and Delhi. The program boasts a ‘success rate’ of over 90% in terms of achieving functional literacy and numeracy goals[54]. Deshpande et al. (2017) evaluate the TA+ programme using an RCT and find statistically significant impact of the adult literacy programme. While this chapter studies the same geographical area as Deshpande et al. (2017), the data is not identical.

[54] https://taraakshar.org/Result.aspx

4.4. **Female Empowerment Outcome Measures**

The definition of female empowerment is broad and involves options, choices, control, and power (Malhotra & Schuler, 2005). It remains a debated term and has been assigned different definitions, often in the backdrop of varying socio-economic contexts (see, for an overview of contesting conceptual framework of empowerment, Malhotra & Schuler, 2005; and Ibrahim & Alkire, 2007). However, most definitions either directly or indirectly allude to the notion of 'agency' – women consider themselves as not only able, but also entitled to make choices (Kabeer, 1999).

I apply this consensus in conceptualisation of empowerment in this study. I define female empowerment as the expansion in women's ability to make choices (Alsop et al., 2005; Kabeer, 1999; Narayan-Parker, 2005) through a process where women either gain the ability to control resources (Uphoff, 2005) or gain power (Kabeer, 1999) or are able to affect decisions regarding important life outcomes (Malhotra & Schuler, 2005).

The data used in this study has a rich set of variables that help capture empowerment. These include a range of 'most frequently used indicators' in empirical studies (Malhotra & Schuler, 2005).

First, I use within household decision-making on domestic and child related issues. The survey included a range of questions which asked the woman "who in the household has the most say?", and the matters covered were what to cook on a daily basis, what and how much to purchase at the local shop, what and how much to purchase at the market outside the village, what to do when your child falls ill, your child's enrolment in school, your child's attendance at school. For each of these decisions, the woman was asked to identify up to three key decision makers, including herself. I use this measure to construct a binary variable which takes the value of 1 if the woman identified herself as a key decision maker, and 0 if other household members excluding herself were involved in the decision making.

Second, I capture a measure of mobility through a range of questions, which asked the woman if she would need permission to conduct an activity. The activities covered were leaving the house, going to the local shop, going to a market outside the village, visiting a primary health centre, visiting natal family, making calls to the natal family, going out

for entertainment (cinema, fair). These questions only capture if there was a constraint on woman's movement in the form of requirement of permission, and do not capture if the woman conducted these activities.

Finally, I capture control over resources through a range of questions which asked if the woman has personal ownership of a mobile phone, if the woman has a personal bank account, and if the woman keeps her jewellery with herself (rather than with the husband or a parent-in-law).

Each of the three domains have also been aggregated into indices using the method specified in Kling et al. (2007)[55]. Since questions on decision making include decisions involving a school-aged child, and 15 percent of the sampled women have no child (see Table 4.2), I also construct an index excluding these decisions. Thus, I have four indices that I refer to – 'Decision', 'Decision excluding the child decisions', 'Mobility', and 'Control of assets'.

4.5. **Data**

The study is based in the state of Uttar Pradesh, one the biggest Indian states in terms of land area and population. However, it has consistently been associated with low levels of economic and social development. In particular, the illiteracy rate in the state is the worst in India with 55 percent of the adult (18 years and above) women, and 27 percent of adult men classified as illiterate[56].

The data used in this chapter was collected as part of a larger Tara Akshar Evaluation Project and is available on FigShare (Wang et al., 2018). The Evaluation Project (Wang et al., 2018) was set up in two phases – Phase 1 and Phase 2. The main difference between the two Phases was that Phase 1 villages already had TA+ presence and Phase 2 villages were entirely new to the programme. This chapter only utilises the Phase 2 data. It comes from 12 villages located in two separate blocks in the district of Sant Ravidas Nagar – 7 are located in Gyanpur block and 5 in Abholi block, making the setting exclusively rural. Overall, the villages are well connected by bus service and

[55] The normalized value of each variable based on control group mean and standard deviation is computed. Then after taking an average of the normalized variables, the resulting index is renormalized using the control group mean and standard deviation. The final index would have a mean of 0 and standard deviation of 1 for the control group.
[56] Calculated from Table DDW-0000C-O8, Census of India, 2011. Restricted to all persons above the age of 18 years.

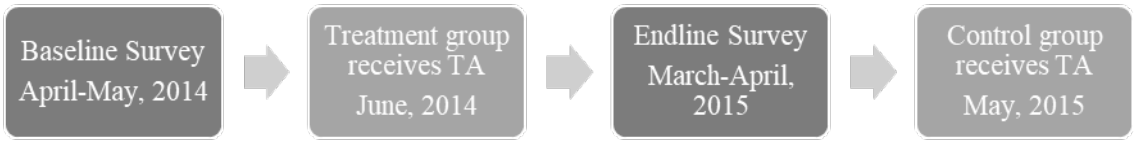
tarred roads to the nearest town and railway station (within 5 km and 12 km, respectively). All villages have access to electricity, cellular phone coverage, and water (though not all of these services might be available throughout the day and to everyone).

In April-May 2014, DA drew up a list of all adult illiterate women in the 12 villages – a total of 1061 women. These women were part of the baseline survey, following which 173 (16 percent) women were declared ineligible by DA because of their education/literacy status. Before the randomisation, the remaining 888 women were approached about their willingness to participate in the TA+ programme, of which 725 expressed interest. Next, within each hamlet[57] (village subdivisions), the women were randomly assigned to treatment and control groups through a public lottery. Since the TA+ programme was delivered at the hamlet level where both treatment and control group women co-reside, there was a possibility that a woman from the control group could attend the literacy classes at the discretion of the TA+ instructor. However, from the design perspective this was sought to be minimised in three ways. First, class size was restricted to 10 due to limited learning materials and infrastructure. So, a control group woman could only end up attending a class if a treatment group woman from the hamlet dropped out of the programme. Second, crossovers were discouraged by providing control group women with clear assurance that they would be able to participate a year later. Third, literacy classes across all hamlets happened simultaneously to discourage control group women from traveling from one class to the next in search of a vacant seat in the TA+ programme

There are 376 women in treatment and 349 women in control groups. The treatment group was invited to participate in the TA+ programme in June 2014, while the control group received the TA+ programme at a later date, May 2015. A follow up endline survey was conducted in March-April 2015. At the endline survey, 672 women of the 725 could be surveyed (an attrition rate of 7.3%). The timeline of the study has been summarised in Figure 4.1 below.

[57] There are 28 hamlets over the 12 villages selected for this study.

Figure 4.1. Timeline of treatment and data collection



At the baseline survey in April-May 2014, information was collected through a village questionnaire, household questionnaire, woman's questionnaire, cognitive test for women, literacy and numeracy tests for the woman and a randomly selected child of the woman, rapid literacy testing of other adult household members of the household[58], and a lab-in-the-field public goods experiment to capture intra-household efficiency. Forward Digit Span (FDS) was used as the cognitive test for the woman. FDS is a subtest of both of Wechsler Adult Intelligence Scale (WAIS) and the Wechsler Memory Scale (WMS) with a maximum score of 16.

The household questionnaire consisted of information on household assets, household composition, religion, caste, and educational level of household members. The household assets section of the questionnaire was constructed in line with Progress out of Poverty Index (renamed as Poverty Probability Index in 2016)[59] . The Progress out of Poverty (PPI) score ranges from 0 to 100. In 2009, a PPI score of 20-24 corresponded to a 75 percent chance of being under the poverty line in rural India (Schreiner, 2008).

The woman's questionnaire captured information on self-reported participation in the literacy programme, woman's school-aged children, and aspects of empowerment as described in Section 4.4.

At the endline survey conducted in March-April 2015, the survey administered the same woman's questionnaire as in baseline, literacy and numeracy test for the woman and her child, and a shorter public goods experiment. The household questionnaire was not administered at the endline, except to update the household members' information.

[58] All tests were administered in Hindi
[59] http://www.progressoutofpoverty.org/india

4.5.1. **Selection**

As mentioned in the previous Section 4.5, 163 women of 888 women eligible for the TA+ programme refused to participate in the programme. This raises concern over selection of the sample that was eventually randomised. Since the baseline survey was administered to all the women, Table 4.1 presents the baseline characteristics of the 163 women as compared to the 725 women who were selected for randomisation. Column 2 presents the mean (and standard deviation) for sample selected for randomisation; Column 3 presents the mean (and standard deviation) for the sample of women who refused to participate; Column 4 presents the results of the t-test of difference in the mean of the two groups; and Column 1 presents the overall mean (and standard deviation).

Table 4.1. Baseline characteristics by randomisation 'selection'

	(1)	(2)	(3)	(4)
	Total	Randomised	Refused	t-test (2)-(3)
Nuclear household	0.320	0.330	0.276	0.054
	[0.467]	[0.470]	[0.448]	
Backward caste	0.490	0.472	0.571	-0.099**
	[0.500]	[0.500]	[0.497]	
Scheduled caste	0.439	0.488	0.221	0.267***
	[0.497]	[0.500]	[0.416]	
Progress out of Poverty Index	23.191	22.690	25.462	-2.773***
	[9.963]	[9.475]	[11.696]	
Number of adult HH members	5.792	5.639	6.472	-0.834**
	[4.328]	[4.220]	[4.734]	
Woman's age in years	35.144	35.068	35.485	-0.417
	[8.516]	[8.501]	[8.597]	
Woman in paid labour	0.065	0.063	0.074	-0.010
	[0.247]	[0.244]	[0.262]	
Forward Digit Span score	6.387	5.603	9.870	-4.268**
	[22.290]	[1.504]	[51.901]	
Number of children 4-18	2.449	2.461	2.399	0.062
	[1.739]	[1.706]	[1.887]	
Woman has no child 4-18	0.155	0.153	0.166	-0.013
	[0.362]	[0.360]	[0.373]	
Baseline value of outcomes				
Decision Index	0.050	0.029	0.144	-0.114
	[0.997]	[0.998]	[0.993]	
Decision Index excluding child decisions	0.084	0.065	0.169	-0.103
	[1.014]	[1.010]	[1.031]	
Cooking	0.639	0.632	0.669	-0.037
	[0.481]	[0.483]	[0.472]	
Purchases at local shop	0.523	0.512	0.571	-0.059
	[0.500]	[0.500]	[0.497]	
Purchases outside village	0.476	0.469	0.509	-0.040
	[0.500]	[0.499]	[0.501]	
Child's Illness	0.557	0.550	0.589	-0.039
	[0.497]	[0.498]	[0.494]	
Child's enrolment	0.528	0.521	0.558	-0.037
	[0.499]	[0.500]	[0.498]	
Child's attendance	0.534	0.521	0.589	-0.068
	[0.499]	[0.500]	[0.494]	
Mobility Index	-0.032	-0.009	-0.137	0.128
	[0.967]	[0.985]	[0.879]	
Leave house without permission	0.161	0.166	0.141	0.024
	[0.368]	[0.372]	[0.349]	
Go to local shop without permission	0.407	0.421	0.344	0.077*
	[0.491]	[0.494]	[0.476]	

	(1)	(2)	(3)	(4)
	Total	Randomised	Refused	t-test (2)-(3)
Go to shop outside the village without permission	0.205	0.218	0.147	0.071**
	[0.404]	[0.413]	[0.355]	
Visit health clinic without permission	0.191	0.193	0.184	0.009
	[0.394]	[0.395]	[0.389]	
Visit natal family without permission	0.135	0.141	0.110	0.030
	[0.342]	[0.348]	[0.314]	
Call natal family without permission	0.725	0.716	0.767	-0.051
	[0.447]	[0.451]	[0.424]	
Go out for entertainment without permission	0.180	0.193	0.123	0.070**
	[0.385]	[0.395]	[0.329]	
Control of assets Index	0.001	-0.006	0.031	-0.037
	[1.018]	[1.025]	[0.990]	
Owns mobile phone	0.409	0.407	0.417	-0.010
	[0.492]	[0.492]	[0.495]	
Own back account	0.400	0.407	0.368	0.039
	[0.490]	[0.492]	[0.484]	
Keeps her own jewellery	0.725	0.714	0.773	-0.059
	[0.447]	[0.452]	[0.420]	
N	888	725	163	

This table presents the mean and standard deviation (in parenthesis) of the overall sample approached for the TA+ programme, the sample of women who were randomised and the sample who refused to participate. Column 4 presents the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. Forward Digit Span Test is a subtest of both of Wechsler Adult Intelligence Scale (WAIS) and the Wechsler Memory Scale (WMS) with a maximum score of 16. There are 6 missing values on the FDS score. Progress out of Poverty Score ranges from 1 to 100 and is a composite index of education level of household head, household's main occupation, cooking fuel, ownership of durables such as cupboard, vehicle, TV, VCR/DVD/VCD player, sewing machine, thermoware.

Women who refused participation in the TA+ programme are more likely to belong to backward caste, are richer and belong to slightly bigger households. They also scored 77 percent higher on the cognitive test, Forward Digit Span. Of concern is the significant difference in the mobility measure of female empowerment. Women who refused to participate are less likely (approximately 7 percentage point difference) to the leave the house for shopping or entertainment without permission. This could be the reason for the unwillingness to participate in the TA+ programme – the inability to secure permission to attend such classes, as it would have required the woman to leave the house on a regular basis. For the results presented in this chapter, I control for baseline observable characteristics as well the baseline levels of all female empowerment measures to account for any selection bias. However, from the point of view of large-scale policy or programme implementation, it is worth noting that such programmes may miss the least empowered women.

4.5.2. **Balance**

For the 672 women selected for randomisation and present at the endline survey, I present the descriptive statistics by treatment and control group in Table 4.2. Column 1 presents the mean (and standard deviation) for the total sample. Column 2 presents the mean (and standard deviation) for the sub-sample of control women and Column 3 presents the mean (and standard deviation) for the sub-sample of treatment women. In Column 4, I report the t-test of difference between control and treatment groups. Appendix B Table C.1 presents the characteristics by control and treatment for the full sample of 725 women randomised at baseline – it follows the same trends as those reported here.

Table 4.2. Baseline characteristics by treatment assignment

	(1) Total	(2) Control	(3) Treatment	(4) t-test (1)-(2)
Nuclear Household	0.344	0.352	0.336	0.015
	[0.475]	[0.478]	[0.473]	
Backward caste	0.469	0.462	0.475	-0.014
	[0.499]	[0.499]	[0.500]	
Scheduled caste	0.491	0.495	0.487	0.008
	[0.500]	[0.501]	[0.501]	
Progress out of Poverty Index	22.525	22.572	22.481	0.091
	[9.409]	[9.653]	[9.185]	
Number of adult HH members	5.588	5.697	5.484	0.213
	[4.264]	[4.465]	[4.068]	
Woman's age in years	35.247	35.125	35.362	-0.237
	[8.317]	[8.022]	[8.597]	
Woman in paid labour	0.064	0.061	0.067	-0.006
	[0.245]	[0.240]	[0.250]	
Forward Digit Span score	5.615	5.574	5.654	-0.080
	[1.503]	[1.517]	[1.490]	
Number of children 4-18	2.510	2.471	2.548	-0.077
	[1.708]	[1.714]	[1.705]	
Woman has no child 4-18	0.147	0.159	0.136	0.023
	[0.355]	[0.366]	[0.344]	
Baseline value of outcomes				
Decision Index	0.048	0.000	0.094	-0.094
	[1.000]	[1.000]	[1.000]	
Decision Index excluding child decisions	0.079	0.000	0.153	-0.153**
	[1.007]	[1.000]	[1.010]	
Cooking	0.641	0.612	0.670	-0.058
	[0.480]	[0.488]	[0.471]	
Purchases at local shop	0.513	0.477	0.548	-0.071*
	[0.500]	[0.500]	[0.498]	
Purchases outside village	0.475	0.437	0.510	-0.073*
	[0.500]	[0.497]	[0.501]	
Child's Illness	0.557	0.547	0.565	-0.018
	[0.497]	[0.499]	[0.496]	
Child's enrolment	0.530	0.538	0.522	0.016
	[0.499]	[0.499]	[0.500]	

	(1) Total	(2) Control	(3) Treatment	(4) t-test (1)-(2)
Child's attendance	0.536	0.523	0.548	-0.025
	[0.499]	[0.500]	[0.498]	
Mobility Index	0.000	-0.000	0.001	-0.001
	[0.993]	[1.000]	[0.987]	
Leave house without permission	0.167	0.177	0.157	0.021
	[0.373]	[0.383]	[0.364]	
Go to local shop without permission	0.427	0.416	0.438	-0.022
	[0.495]	[0.494]	[0.497]	
Go to shop outside the village without permission	0.216	0.220	0.212	0.009
	[0.412]	[0.415]	[0.409]	
Visit health clinic without permission	0.202	0.214	0.191	0.023
	[0.402]	[0.411]	[0.394]	
Visit natal family without permission	0.143	0.135	0.151	-0.016
	[0.350]	[0.342]	[0.358]	
Call natal family without permission	0.714	0.703	0.725	-0.021
	[0.452]	[0.457]	[0.447]	
Go out for entertainment without permission	0.195	0.196	0.194	0.002
	[0.396]	[0.397]	[0.396]	
Control of assets Index	0.014	0.000	0.026	-0.026
	[1.016]	[1.000]	[1.033]	
Owns mobile phone	0.411	0.410	0.412	-0.002
	[0.492]	[0.493]	[0.493]	
Own back account	0.414	0.391	0.435	-0.043
	[0.493]	[0.489]	[0.496]	
Keeps her own jewellery	0.722	0.731	0.713	0.018
	[0.448]	[0.444]	[0.453]	
N	672	327	345	

This table presents the mean and standard deviation (in parenthesis) of the overall sample randomised and surveyed at both baseline and endline, the sample of women who were assigned to control and the sample of women who were assigned to treatment. Column 4 presents the difference between control and treatment groups and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. Forward Digit Span Test is a subtest of both of Wechsler Adult Intelligence Scale (WAIS) and the Wechsler Memory Scale (WMS) with a maximum score of 16. There are 4 missing values on the FDS score. Progress out of Poverty Score ranges from 1 to 100 and is a composite index of education level of household head, household's main occupation, cooking fuel, ownership of durables such as cupboard, vehicle, TV, VCR/DVD/VCD player, sewing machine, thermoware.

While there is no significant difference in most of the observed characteristics between the control and treatment group, the women in the treatment group are more likely to be a primary decision maker on activities involving purchases at the local shop or at a shop outside the village. 55 percent of the treatment group women report making the decision on purchases made at a local shop as compared to 48 percent of the control group women. There is a similar 7 percentage point difference in favour of treatment group women with regards to decision on purchases made at shops outside the village. Hence, in all my main analyses, I control for the baseline value of the female empowerment measures.

4.6. **Results**

4.6.1. **Intent-to-treat Effects**

I begin by exploring the average differences in the outcome variables at the endline by control and treatment group in Table 4.3[60]. Column 1 presents the mean (and standard deviation) for the total sample. Column 2 presents the mean (and standard deviation) for the sub-sample of control women and Column 3 presents the mean (and standard deviation) for the sub-sample of treatment women. In Column 4, I report the t-test of difference between control and treatment groups.

There is a significant difference in the means of control and treatment group on mobility measures and control of assets. In particular, women in the treatment group are more likely to go to local shops and call their natal family without requiring permission at the endline as compared to the control group women. They are also more likely to have a personal bank account.

[60] The endline means of all the within-household decision-making indicators are lower (although not significantly) than the baseline means for both the treatment and control group. There might be a concern that these survey measures of decision-making are noisy and not very effective, as highlighted by Almas et al. (2018). They find no effects on within household decision-making of a conditional cash transfer programme when they use survey measures, but find significant effects when using experimental measures. They additionally find their survey measures to be poorly correlated with the experimental measures.

Table 4.3. Endline outcome variables by treatment assignment

	(1) Total	(2) Control	(3) Treatment	(4) t-test (1)-(2)
Endline value of outcomes				
Decision Index	-0.006	0.000	-0.011	0.011
	[1.019]	[1.000]	[1.039]	
Decision Index excluding child decisions	-0.010	0.000	-0.019	0.019
	[1.002]	[1.000]	[1.005]	
Cooking	0.557	0.566	0.548	0.018
	[0.497]	[0.496]	[0.498]	
Purchases at local shop	0.506	0.508	0.504	0.003
	[0.500]	[0.501]	[0.501]	
Purchases outside village	0.429	0.431	0.426	0.005
	[0.495]	[0.496]	[0.495]	
Child's Illness	0.496	0.498	0.493	0.006
	[0.500]	[0.501]	[0.501]	
Child's enrolment	0.385	0.391	0.380	0.012
	[0.487]	[0.489]	[0.486]	
Child's attendance	0.458	0.450	0.467	-0.017
	[0.499]	[0.498]	[0.500]	
Mobility Index	0.069	0.000	0.134	-0.134*
	[1.026]	[1.000]	[1.048]	
Leave house without permission	0.129	0.128	0.130	-0.002
	[0.336]	[0.335]	[0.337]	
Go to local shop without permission	0.379	0.333	0.423	-0.090**
	[0.486]	[0.472]	[0.495]	
Go to shop outside the village without permission	0.155	0.141	0.168	-0.027
	[0.362]	[0.348]	[0.375]	
Visit health clinic without permission	0.177	0.159	0.194	-0.035
	[0.382]	[0.366]	[0.396]	
Visit natal family without permission	0.098	0.092	0.104	-0.013
	[0.298]	[0.289]	[0.306]	
Call natal family without permission	0.628	0.572	0.681	-0.109***
	[0.484]	[0.496]	[0.467]	
Go out for entertainment without permission	0.104	0.101	0.107	-0.006
	[0.306]	[0.302]	[0.310]	
Control of assets Index	0.061	-0.000	0.119	-0.119
	[0.974]	[1.000]	[0.947]	
Owns mobile phone	0.432	0.422	0.441	-0.019
	[0.496]	[0.495]	[0.497]	
Own back account	0.622	0.575	0.667	-0.092**
	[0.485]	[0.495]	[0.472]	
Keeps her own jewellery	0.868	0.865	0.870	-0.004
	[0.339]	[0.342]	[0.337]	
N	672	327	345	

This table presents the mean and standard deviation (in parenthesis) of the overall sample randomised and surveyed at both baseline and endline, the sample of women who were assigned to control and the sample of women who were assigned to treatment. Column 4 presents the difference between control and treatment groups and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1.

I further investigate the significant differences in the mean outcomes by treatment status, using the following regression -

$$y_{1i} = \alpha + \beta_{ITT} * ITT_i + \theta X_i + \pi y_{0i} + \epsilon_i \quad (1)$$

Where y_i denotes the different empowerment measures for woman *i* at the endline (period=1) and at the baseline (period=0), ITT_i denotes the randomised treatment allocation (=1 for woman assigned to treatment, and =0 for woman assigned to control), β_{ITT} denotes the intent-to-treat effect. The vector of control variables X_i includes the characteristics of the woman (age, occupation, FDS cognitive test, number of school-aged children), and household characteristics (nuclear or extended household structure, caste, PPI asset score, number of adult household members). The standard errors are clustered at the hamlet level. In Appendix B, I report the intent-to-treat results for regression specification without any controls (see Table C.2, Table C.5 and Table C.8), only with baseline value of outcome variables as controls (see Table C.3, Table C.6, and Table C.9), only with woman and household characteristics as controls (see 0, Table C.7, and Table C.10).

Table 4.4 presents the results of intent-to-treat effects of the TA+ programme on various aspects of decision-making. The outcome variables are decision index, decision index excluding the decision involving the child, binary variable on if the woman is a primary decision maker on matters related to cooking, purchases at the local shop, purchases at shops outside the village, child's illness, child's enrolment at school, child attending the school on a given day (See Section 3.3.1 for more details on the dependent variables).

Table 4.4. Intent-to-treat effects on decision-making power

	(1) Decision Index	(2) Decision Index excl child	(3) Cooking	(4) Purchases at local shop	(5) Purchases outside village	(6) Child's Illness	(7) Child's enrolment	(8) Child's attendance
Treatment	-0.047	-0.061	-0.032	-0.018	-0.024	-0.011	-0.013	0.003
	(0.072)	(0.078)	(0.035)	(0.038)	(0.039)	(0.039)	(0.037)	(0.024)
Nuclear household	0.321**	0.324**	0.144**	0.161**	0.169**	0.198***	0.073	0.115***
	(0.122)	(0.137)	(0.064)	(0.070)	(0.068)	(0.055)	(0.047)	(0.041)
Backward caste	-0.088	-0.069	-0.022	-0.031	-0.050	0.015	-0.061	-0.102*
	(0.137)	(0.132)	(0.063)	(0.076)	(0.071)	(0.085)	(0.066)	(0.060)
Scheduled caste	-0.099	-0.008	0.013	0.023	-0.041	-0.011	-0.069	-0.145**
	(0.137)	(0.137)	(0.066)	(0.080)	(0.075)	(0.086)	(0.068)	(0.062)
Progress out of Poverty Index	-0.005	-0.002	0.000	-0.001	-0.001	-0.000	-0.003	-0.003
	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.002)
Number of adult HH members	-0.002	-0.007	-0.006	-0.005	-0.001	0.004	-0.001	-0.005
	(0.018)	(0.019)	(0.009)	(0.010)	(0.009)	(0.008)	(0.007)	(0.006)
Woman's age in years	0.020***	0.027***	0.015***	0.013***	0.012***	0.005*	0.005**	0.006**
	(0.005)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
Woman in paid labour	0.250	0.243*	0.088	0.139*	0.124	0.157**	0.048	0.110
	(0.160)	(0.140)	(0.071)	(0.070)	(0.073)	(0.071)	(0.081)	(0.089)
Forward Digit Span score	0.029	0.024	0.010	0.012	0.011	0.011	0.013	0.014
	(0.023)	(0.023)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)
Number of children 4-18	0.030	-0.010	0.002	-0.004	-0.009	0.038**	0.028**	0.042***
	(0.028)	(0.027)	(0.014)	(0.013)	(0.013)	(0.016)	(0.011)	(0.014)
Outcome at baseline	0.298***	0.237***	0.148***	0.198***	0.233***	0.223***	0.216***	0.174***
	(0.060)	(0.053)	(0.052)	(0.051)	(0.046)	(0.042)	(0.041)	(0.049)
Constant	-0.828***	-1.051***	-0.122	-0.108	-0.124	-0.038	0.066	0.138
	(0.294)	(0.313)	(0.178)	(0.150)	(0.147)	(0.169)	(0.107)	(0.115)
Control group mean at endline	0.002	0.001	0.568	0.506	0.432	0.497	0.392	0.454
Observations	668	668	668	668	668	668	668	668
R-squared	0.204	0.199	0.143	0.173	0.176	0.141	0.106	0.129

This table presents the results of regression of decision-making power on treatment assignment with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score.

I find no significant effect of the TA+ programme on any of the decision-making indicators. This is unsurprising as previous studies on formal education of women have documented null effects on women's decision making power within the household in Uttar Pradesh (Dyson & Moore, 1983; Bloom et al., 2001; Jejeebhoy & Sathar, 2001). In the sample studied in this chapter, about only half the women indicated having some say in decision making (see Table 4.2 and Table 4.3), implying low levels of decision making power both before and after the programme. Given the low female labour force participation in rural Uttar Pradesh, attributed in part to the less labour-intensive cultivation of wheat (Foster & Rosenzweig, 1996) and in part to gender related social norms (Dyson & Moore, 1983; Bloom et al., 2001; Jejeebhoy & Sathar, 2001), it is unlikely that the literacy programme created employment and income opportunities for these women. Thus, it follows from the prediction of intra-household bargaining models that women would not see a shift in their bargaining power and consequent decision making roles (Rahman & Rao, 2005; MacPhail & Dong, 2007; Hashemi et al., 1996; Almas et al., 2018). However, other studies on investment in children's health and education document a greater role of women in decision making due to an increase in women's access to knowledge (Thomas et al., 1991; Glewwe, 1999; Aslam & Kingdon, 2012). The access to such sources of knowledge, for instance, newspapers and television, may be limited in the context of rural Uttar Pradesh (Thomas et al., 1991). Additionally, Dyson and Moore (1983) in explaining the north-south India divergence in gender equity, emphasised that the unfavourable position of women in North India stemmed from the cultural norm of patrilocality. Married women live with their husband's family in an inferior position to the household power hierarchy (Anukriti et al., 2020). Chapter 3 of this thesis finds support for this theory in finding the inefficient sharing of household resources between daughters-in-law and mothers-in-law.

Among the covariates, I find being part of a nuclear household and being an older woman to be strongly positively correlated with the decision-making power within the household. These correlations may be related to the findings in Chapter 2 – extended households exhibit fragmented decision-making power with indication of higher levels of inefficiency between a mother-in-law (older woman in the household) and a daughter-in-law.

For certain outcomes, a woman in paid employment is positively correlated with the

probability of being a primary decision maker. This is similar to the findings in the literature on economic empowerment and gender empowerment – an employed woman is more likely to have a greater say in decision making within the household. However, only 6 percent of the women in our sample are employed in income generating activities (see Table 4.1).

Table 4.5 presents the results of intent-to-treat effects of the TA+ programme on various aspects of freedom of movement. The outcome variables are mobility index, binary variable on if the woman can leave the house, shop at the local market, shop at the market outside the village, visit a health clinic, visit her natal family, call her natal family, and go out for entertainment without requiring permission (See Section 3.3.1 for more details on the dependent variables).

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Table 4.5. Intent-to-treat effects on mobility

	(1) Mobility Index	(2) Leave house	(3) Go to local shop	(4) Go to shop outside the village	(5) Visit health clinic	(6) Visit natal family	(7) Call natal family	(8) Go out for entertainment
Treatment	0.126*	0.005	0.075***	0.029	0.037	0.010	0.098***	0.005
	(0.062)	(0.022)	(0.027)	(0.025)	(0.026)	(0.020)	(0.035)	(0.022)
Nuclear household	0.146	0.102***	0.058	0.082**	0.119***	0.028	-0.118***	0.005
	(0.116)	(0.033)	(0.052)	(0.036)	(0.034)	(0.032)	(0.042)	(0.039)
Backward caste	-0.049	0.027	0.063	-0.048	-0.012	-0.018	0.037	0.003
	(0.107)	(0.044)	(0.083)	(0.055)	(0.054)	(0.044)	(0.112)	(0.068)
Scheduled caste	-0.042	0.029	0.068	-0.046	-0.031	-0.006	-0.015	0.014
	(0.136)	(0.053)	(0.084)	(0.059)	(0.056)	(0.040)	(0.118)	(0.079)
Progress out of Poverty Index	-0.014***	-0.005***	-0.004*	-0.004**	-0.005**	-0.004***	-0.001	-0.001
	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Number of adult HH members	-0.001	0.003	-0.015***	0.001	0.002	0.006	-0.012***	-0.002
	(0.009)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Woman's age in years	0.019***	0.007***	0.015***	0.008***	0.008***	0.001	-0.003	0.006***
	(0.005)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Woman in paid labour	0.689***	0.134*	0.128**	0.227***	0.245***	0.187***	0.189***	0.178***
	(0.142)	(0.065)	(0.051)	(0.067)	(0.044)	(0.060)	(0.064)	(0.047)
Forward Digit Span score	0.007	-0.002	0.007	0.001	0.008	-0.005	0.018	-0.009
	(0.024)	(0.006)	(0.014)	(0.009)	(0.009)	(0.006)	(0.011)	(0.008)
Number of children 4-18	-0.058**	-0.019**	-0.013	-0.014	-0.019**	-0.024**	0.025*	-0.016**
	(0.027)	(0.009)	(0.011)	(0.009)	(0.008)	(0.010)	(0.014)	(0.008)
Outcome at baseline	0.367***	0.149**	0.158***	0.260***	0.233***	0.315***	0.080*	0.052
	(0.051)	(0.058)	(0.048)	(0.049)	(0.030)	(0.055)	(0.041)	(0.031)
Constant	-0.289	-0.086	-0.180	-0.070	-0.116	0.144*	0.570***	-0.002
	(0.235)	(0.089)	(0.127)	(0.104)	(0.105)	(0.084)	(0.149)	(0.111)
Control group mean at endline	0.006	0.13	0.336	0.142	0.16	0.093	0.574	0.102
Observations	668	668	668	668	668	668	668	668
R-squared	0.239	0.108	0.157	0.198	0.172	0.201	0.056	0.065

This table presents the results of regression of freedom of movement on treatment assignment with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score.

I find a significant effect of the TA+ programme on the mobility index. Being part of treatment group increases the mobility index by 0.126 standard deviations. Investigating the individual components of the mobility index, I find that being part of the treatment increases the probability of going to the local shops and calling the natal family without requiring permission. Women in the treatment group are 7.5 percentage points (an effect size of 22 percent) more likely than the control group women to go to the local shops without permission. They are also 9.8 (an effect size of 17 percent) percentage point more likely than the control group women to be able to call their natal family without requiring permission.

A direct mechanism driving the positive impacts on freedom of movement is the increase in the woman's confidence and self-esteem due to newly acquired literacy skills (Archer & Cottingham, 1997; Stromquist, 1997; Egbo, 2000). Alternatively, women overcoming mobility constraints may have less to do with literacy itself, and more to do with the act of attending the literacy class. Firstly, literacy classes presented these women an opportunity to move freely and unsupervised out of their homes on a regular basis, thereby, shifting the woman's belief of own self-efficacy as well as her household members' belief of the woman's self-efficacy. Secondly, attending literacy classes with other women would have expanded the network of peers which has shown to have a positive effect on freedom of movement (Kandpal & Baylis, 2019; Anukriti et al., 2020).

Among the covariates, I continue to find that being part of a nuclear household, an older woman, and a woman in paid employment to be positively correlated with the mobility indicators. The estimates on a woman involved in income generating activity are higher and stronger in the regressions on mobility than the regressions on decision-making. This might be because an employed woman has to leave the house on a regular basis and is hence less prone to be restricted in her movements. Additionally, I find the number of children to be negatively correlated with the mobility indicators. This might be due to childcare duties, which may limit the woman's ability to leave the house.

Finally, I look at the intent-to-treat effects of the TA+ programme on various aspects of control of assets. The outcome variables in Table 4.6 are control of assets index, binary variable on if the woman owns a personal mobile phone, has her own bank account, and keeps her own jewelry with herself. I find a significant effect of the TA+ programme

only on the probability of having a personal bank account. Women in the treatment group are 7.3 percentage points (an effect size of 13 percent) more likely than the control group women to have a personal bank account. This may be a direct result of becoming literate with the newly acquired ability to read, fill out, and sign bank forms (see Deshpande et al., 2017 on significant impacts of the TA+ programme on women's reading and numeracy skills). Also related to the previous finding on increased freedom of movement, the woman would feel more confident to go to a bank herself unaccompanied and without requiring permission of other household members. Another hypothesis is found in social networks literature, where an increase in the interaction with peers results in an increase in financial independence (Field et al., 2016; Banerjee et al., 2013).

Table 4.6. Intent-to-treat effects on control of assets

	(1) Control of assets Index	(2) Owns mobile phone	(3) Own back account	(4) Keep own jewellery
Treatment	0.097	0.014	0.073*	0.002
	(0.076)	(0.034)	(0.039)	(0.022)
Nuclear household	0.134	0.039	0.014	0.057*
	(0.087)	(0.042)	(0.039)	(0.032)
Backward caste	0.012	0.012	-0.052	0.044
	(0.146)	(0.092)	(0.087)	(0.083)
Scheduled caste	-0.161	-0.088	-0.099	0.037
	(0.168)	(0.092)	(0.094)	(0.086)
Progress out of Poverty Index	0.005	0.002	-0.000	0.002
	(0.004)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.011	-0.007*	0.000	-0.001
	(0.009)	(0.004)	(0.006)	(0.003)
Woman's age in years	0.018***	-0.003	0.008***	0.012***
	(0.006)	(0.002)	(0.002)	(0.003)
Woman in paid labour	-0.102	0.039	-0.156**	-0.004
	(0.086)	(0.046)	(0.057)	(0.065)
Forward Digit Span score	0.033	0.024**	0.022	-0.007
	(0.026)	(0.010)	(0.014)	(0.009)
Number of children 4-18	0.072**	0.019*	0.022*	0.020*
	(0.027)	(0.010)	(0.013)	(0.011)
Outcome at baseline	0.296***	0.397***	0.268***	0.050
	(0.037)	(0.040)	(0.032)	(0.033)
Constant	-1.010***	0.188	0.087	0.327**
	(0.272)	(0.150)	(0.156)	(0.128)
Control group mean at endline	0.001	0.423	0.577	0.864
Observations	668	668	668	668
R-squared	0.191	0.201	0.149	0.118

This table presents the results of regression of control of assets on treatment assignment with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score.

4.6.2. **Programme take-up**

The intent-to-treat effects assume that there was perfect compliance to the randomisation process. In practice, this is seldom the case. Both the randomisation and the delivery of literacy classes were conducted at the hamlet level within the village. The proximity of the classes to control group women implied that there were possibilities of cross-over from control to treatment. However, class size was restricted to 10 due to limited learning materials, implying that usually a control group woman could attend a class unnoticed if the assigned treatment group woman dropped out. At the endline, all women were asked if they participated in the TA+ programme. Based on this self-reported measure in Table 4.7, I find that 20.6 percent of the treatment group women reported not having attended the TA+ programme and 18.7 percent of the control group women reported having attended the TA+ programme. Since the TA+ instructors were not familiar with the names and faces of the treatment group women, and given these classes generated curiosity, it is likely for control group women to have attended the literacy classes in the initial days unnoticed. This may explain why the non-compliers among control group women attended only 47 percent of the classes. The 56-day instructional component was followed by a six month long reading club which was attended by 41 percent and 18 percent of the treatment and control group women, respectively.

Table 4.7. Programme take-up by treatment assignment status

	(1) Control		(2) Treatment	
	N	Mean	N	Mean
Woman attended TA+	327	0.187	345	0.794
		[0.390]		[0.405]
Days attended (out of 56)	61	0.473	274	0.773
		[0.357]		[0.277]
Woman attended reading club	61	0.177	274	0.409
		[0.385]		[0.493]

This table reports the average attendance in TA+ programme by the treatment assignment status. TA+ has a 56 day instructional component followed with a reading group that meets biweekly for six months. The days attended and whether a woman attended the reading club is calculated only for women who participated in the programme. All measure are self-reported by the woman at endline. Standard deviations are in parenthesis.

While later in Section 4.6.3, I estimate the treatment effects of the TA+ programme on the compliers, it is important to note that the intent-to-treat effects may be more policy

relevant. In case an adult literacy programme is rolled out for all the illiterate women in the country, policy makers might be interested in knowing the impact of offering the programme on the entire population, even if some women might not take it up.

Relatedly, one would like to know the determinants of program take up. Table 4.8 reports the regression results of the correlates of women's participation in the TA+ programme[61]. The following specification is used to estimate Table 4.8 –

$$Participation_i = \alpha + \theta X_i + \pi y_{0i} + \epsilon_i \quad (2)$$

Where X_i is the vector of household and individual characteristics of woman i; y_{0i} is the vector of baseline value of empowerment variables. The regression is run separately by treatment and control group assignment status. Column 1 reports the determinants of program take up among the treatment group women and Column 2 reports the determinants of program take up among the control group women.

[61] Appendix B, Table C.12 reports the same regressions but with hamlet fixed effects. The results remain the same with minor changes in the magnitude of coefficients.

Table 4.8. Correlates of programme take-up

	(1) Participation within treatment	(2) Participation within control
Nuclear household	-0.055	0.081
	(0.046)	(0.056)
Backward caste	0.398*	0.110*
	(0.211)	(0.061)
Scheduled caste	0.452**	0.175**
	(0.195)	(0.080)
Progress out of Poverty Index	0.003	0.002
	(0.003)	(0.003)
Number of adult HH members	-0.010	0.001
	(0.007)	(0.006)
Woman's age in years	0.005	0.001
	(0.003)	(0.003)
Woman in paid labour	-0.193**	-0.042
	(0.091)	(0.095)
Forward Digit Span score	0.004	-0.009
	(0.017)	(0.015)
Number of children 4-18	0.031**	0.017
	(0.014)	(0.015)
Decision Index at baseline	0.038	0.008
	(0.028)	(0.022)
Mobility Index at baseline	0.000	-0.028
	(0.020)	(0.025)
Control of assets index at baseline	-0.003	-0.000
	(0.027)	(0.020)
Constant	0.122	-0.040
	(0.288)	(0.219)
Observations	344	324
R-squared	0.093	0.029

This table presents the results of regression of program take up within treatment and control groups on observables. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score.

Among the treatment group women, I find that belonging to lower castes (backward and scheduled castes as against general upper caste) and number of children is positively associated with program participation. A woman in paid employment is negatively correlated with program participation. This could be due to the employed woman being unavailable on a regular basis or that an employed woman might consider the benefits from a literacy programme to be low. Among the control group women, belonging to lower castes is significantly and positively associated with program participation. These results suggest that the women from lower castes are more likely to take up the female adult literacy programme, making such programmes inclusive of the socially

marginalised groups.

While I don't find aspects of empowerment – decision-making power, mobility and control of assets to be correlated with programme take up in Table 4.8, it may be because the sample finally randomized excluded the 163 women who refused to participate in the study. As seen in Table 4.1, these women were less likely to leave the house without permission. Hence, although a large-scale female literacy policy may lead to the desirable selection into the programme from the socially marginalised caste groups in India, it may still miss the least empowered women limited in their ability to leave the house freely.

4.6.3. Local Average Treatment Effects

Given that there wasn't perfect compliance to the programme, I use an instrumental variable approach to estimate the treatment effect of the TA+ programme on the compliers. The first stage regression is given by instrumenting self-reported participation on random treatment assignment status -

$$Participation_i = \alpha + \beta_{stage1} ITT_i + \theta_{stage1} X_i + \pi_{stage1} y_{0i} + e_i \quad (3)$$

And final estimated instrumental variable (IV) regression is -

$$y_{1i} = \alpha_{IV} + \beta_{IV} * Participation_i + \theta_{IV} X_i + \pi_{IV} y_{0i} + \epsilon_i \quad (4)$$

The coefficient β_{IV} captures the local average treatment effect (LATE) of TA+ programme among the women who complied with the treatment assignment. The local average treatment effect is expected to be higher than the intent-to-treat effects as it is the effect of treatment among those women who took it up. The higher the compliance, the closer the LATE will be to ITT estimates. Appendix B Table C.13 reports the results of IV regressions without any controls, with only baseline value of outcome variable as control, and with only woman and household characteristics as controls.

Table 4.9 presents the effects of the TA+ programme on various aspects of decision making among the compliers. The outcome variables are decision index, decision index excluding the decision involving the child, binary variable on if the woman is a primary decision maker on matters related to cooking, purchases at the local shop, purchases at

shops outside the village, child's illness, child's enrolment at school, child attending the school on a given day. I find no significant effect of the TA+ programme on any of the decision-making indicators among the compliers.

Table 4.9. Treatment effect on decision-making power for compliers

	(1) Decision Index	(2) Decision Index excl child	(3) Cooking	(4) Purchases at local shop	(5) Purchases outside village	(6) Child's Illness	(7) Child's enrolment	(8) Child's attendance
Participation	-0.078	-0.101	-0.053	-0.030	-0.039	-0.018	-0.021	0.005
	(0.115)	(0.125)	(0.056)	(0.061)	(0.063)	(0.064)	(0.060)	(0.040)
Nuclear Household	0.322***	0.324**	0.145**	0.161**	0.170**	0.198***	0.074	0.115***
	(0.118)	(0.132)	(0.062)	(0.068)	(0.066)	(0.054)	(0.045)	(0.040)
Backward caste	-0.069	-0.045	-0.009	-0.024	-0.041	0.019	-0.056	-0.103*
	(0.127)	(0.122)	(0.061)	(0.069)	(0.067)	(0.084)	(0.060)	(0.058)
Scheduled caste	-0.075	0.023	0.029	0.032	-0.029	-0.005	-0.062	-0.147**
	(0.130)	(0.134)	(0.068)	(0.075)	(0.071)	(0.085)	(0.063)	(0.061)
Progress out of Poverty Index	-0.005	-0.002	0.000	-0.001	-0.001	-0.000	-0.003	-0.003
	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Number of adult HH members	-0.003	-0.007	-0.006	-0.005	-0.001	0.003	-0.001	-0.005
	(0.017)	(0.019)	(0.009)	(0.010)	(0.009)	(0.008)	(0.006)	(0.006)
Woman's age in years	0.020***	0.027***	0.015***	0.013***	0.012***	0.005**	0.005**	0.006**
	(0.005)	(0.005)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Woman in paid labour	0.239	0.230*	0.081	0.135**	0.118	0.154**	0.046	0.111
	(0.154)	(0.136)	(0.068)	(0.068)	(0.072)	(0.069)	(0.079)	(0.087)
Forward Digit Span score	0.028	0.024	0.009	0.012	0.011	0.011	0.013	0.014
	(0.022)	(0.022)	(0.010)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)
Number of children 4-18	0.032	-0.008	0.003	-0.004	-0.008	0.039**	0.028***	0.042***
	(0.027)	(0.027)	(0.014)	(0.013)	(0.013)	(0.016)	(0.011)	(0.014)
Outcome at baseline	0.299***	0.240***	0.151***	0.199***	0.234***	0.224***	0.216***	0.174***
	(0.059)	(0.052)	(0.051)	(0.050)	(0.045)	(0.041)	(0.041)	(0.048)
Constant	-0.846***	-1.074***	-0.136	-0.116	-0.134	-0.042	0.060	0.140
	(0.292)	(0.320)	(0.182)	(0.151)	(0.149)	(0.165)	(0.099)	(0.113)
Observations	668	668	668	668	668	668	668	668
First stage F-stat	58.56	59.45	59.02	55.39	60.96	55.96	59.72	55.88

This table presents the results of IV regression of decision-making power on programme participation with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at the hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score. The first stage regression is reported in Appendix B Table C.14.

Table 4.10 presents the results of the effects of the TA+ programme on various aspects of freedom of movement among the compliers. The outcome variables are mobility index, binary variable on if the woman can leave the house, shop at the local market, shop at the market outside the village, visit a health clinic, visit her natal family, call her natal family, and go out for entertainment without requiring permission. I find a significant effect on the mobility index, going to the local shops and calling the natal family without requiring permission, similar to the intent-to-treat effects. The magnitude of the estimates is larger than ITT estimates. Participation in the TA+ programme increases the mobility index by 0.21 standard deviations among the compliers. Treatment group women participating in the programme are 12.4 percentage points more likely than the non-participant control group women to go to the local shops without permission. They are also 16.3 percentage points more likely to be able to call their natal family without requiring permission.

Table 4.10. Treatment effect on mobility for compliers

	(1) Mobility Index	(2) Leave house	(3) Go to local shop	(4) Go to shop outside the village	(5) Visit health clinic	(6) Visit natal family	(7) Call natal family	(8) Go out for entertainment
Participation	0.210**	0.008	0.124***	0.048	0.062	0.017	0.163***	0.009
	(0.100)	(0.036)	(0.043)	(0.041)	(0.041)	(0.032)	(0.055)	(0.036)
Nuclear Household	0.141	0.101***	0.056	0.081**	0.118***	0.027	-0.121***	0.005
	(0.114)	(0.032)	(0.051)	(0.034)	(0.033)	(0.031)	(0.041)	(0.038)
Backward caste	-0.100	0.025	0.033	-0.060	-0.027	-0.022	-0.003	0.001
	(0.098)	(0.047)	(0.071)	(0.057)	(0.057)	(0.044)	(0.104)	(0.064)
Scheduled caste	-0.107	0.027	0.030	-0.061	-0.050	-0.011	-0.065	0.011
	(0.125)	(0.053)	(0.077)	(0.061)	(0.060)	(0.039)	(0.110)	(0.075)
Progress out of Poverty Index	-0.015***	-0.005***	-0.004*	-0.004**	-0.005**	-0.004***	-0.002	-0.001
	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Number of adult HH members	0.000	0.003	-0.014***	0.001	0.002	0.006	-0.011**	-0.002
	(0.009)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)
Woman's age in years	0.018***	0.007***	0.014***	0.007***	0.008***	0.001	-0.003	0.006***
	(0.005)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)
Woman in paid labour	0.715***	0.135**	0.144***	0.233***	0.253***	0.190***	0.209***	0.179***
	(0.142)	(0.062)	(0.054)	(0.065)	(0.044)	(0.060)	(0.066)	(0.045)
Forward Digit Span score	0.008	-0.002	0.008	0.001	0.009	-0.005	0.018	-0.009
	(0.024)	(0.006)	(0.015)	(0.008)	(0.009)	(0.006)	(0.011)	(0.008)
Number of children 4-18	-0.063**	-0.019**	-0.017	-0.015*	-0.021**	-0.024**	0.021	-0.017**
	(0.026)	(0.009)	(0.011)	(0.009)	(0.008)	(0.009)	(0.014)	(0.008)
Outcome at baseline	0.369***	0.149***	0.157***	0.261***	0.234***	0.315***	0.091**	0.052*
	(0.050)	(0.056)	(0.046)	(0.048)	(0.029)	(0.053)	(0.040)	(0.030)
Constant	-0.231	-0.084	-0.147	-0.057	-0.099	0.149*	0.608***	-0.000
	(0.245)	(0.089)	(0.127)	(0.106)	(0.110)	(0.081)	(0.144)	(0.109)
Observations	668	668	668	668	668	668	668	668
First stage F-stat	60.62	62.43	53.54	55.89	53.64	53.99	64.68	56.65

This table presents the results of IV regression of freedom of movement on programme participation with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score. The first stage regression is reported in Appendix B Table C.15.

Finally, I look at the effects of the TA+ programme on various aspects of control of assets among the compliers. The outcome variables in Table 4.11 are control of assets index, binary variable on if the woman owns a personal mobile phone, has her own bank account, and keeps her own jewellery with herself. I find a significant effect of participation only on the probability of having a personal bank account. Treatment group women participating in the programme are 12.1 percentage points more likely than the control group non-participants to have a personal bank account.

Table 4.11. Treatment effect on control of assets for compliers

	(1) Control of assets Index	(2) Owns mobile phone	(3) Own back account	(4) Keeps own jewellery
Participation	0.161	0.024	0.121*	0.004
	(0.123)	(0.055)	(0.064)	(0.036)
Nuclear Household	0.132	0.038	0.012	0.057*
	(0.085)	(0.042)	(0.038)	(0.031)
Backward caste	-0.026	0.007	-0.080	0.043
	(0.145)	(0.093)	(0.083)	(0.082)
Scheduled caste	-0.210	-0.095	-0.135	0.036
	(0.170)	(0.095)	(0.086)	(0.086)
Progress out of Poverty Index	0.004	0.002	-0.001	0.002
	(0.004)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.010	-0.007*	0.001	-0.001
	(0.008)	(0.004)	(0.005)	(0.003)
Woman's age in years	0.018***	-0.003	0.008***	0.011***
	(0.006)	(0.002)	(0.002)	(0.002)
Woman in paid labour	-0.081	0.042	-0.141***	-0.004
	(0.081)	(0.044)	(0.053)	(0.064)
Forward Digit Span score	0.033	0.024**	0.022*	-0.007
	(0.024)	(0.010)	(0.013)	(0.009)
Number of children 4-18	0.068**	0.018*	0.019	0.020*
	(0.027)	(0.010)	(0.012)	(0.011)
Outcome at baseline	0.296***	0.398***	0.265***	0.050
	(0.037)	(0.039)	(0.031)	(0.032)
Constant	-0.966***	0.194	0.118	0.328***
	(0.256)	(0.147)	(0.146)	(0.123)
Observations	668	668	668	668
First stage F-stat	54.48	55.20	54.66	53.34

This table presents the results of IV regression of control of assets on programme participation with individual level, household level and value of outcome variable at baseline controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. The regression sample is slightly smaller than 672 due to missing values on Forward Digit Span score. The first stage regression is reported in Appendix B Table C.1.

4.6.4. **Attrition**

At the baseline, 725 women were randomized into treatment and control groups. However, only 672 women completed the endline survey. Attrition can be of concern if it is correlated with the treatment assignment. I have a low overall attrition rate of 7.3 percent with the corresponding rates among control and treatment group at 6.3 percent and 8.2 percent respectively. This difference in attrition rate by treatment status is not significant in itself[62]. However, attrition might remain correlated with observable characteristics or the outcomes.

In Table 4.12, I present the characteristics and the baseline value of outcome variables of the attrition sample. Column 1 reports the mean (and standard deviation) of the overall attrition sample, Column 2 reports the mean (standard deviation) of the control group attrition sample, and Column 3 reports the mean (and standard deviation) of the treatment group attrition sample.

In the treatment group, the attrition sample is from relatively richer households, households with larger number of adult members, and higher test score on the Forward Digit Span test. I also find the attrition sample in the treatment group to have higher decision-making power on child's enrolment in school and child's daily attendance at school. They are less likely to be able to go to local shop without permission. Since the women who attrit in the treatment group have higher decision-making power than the control group women who attrit, I may be underestimating the intent-to-treat effects on decision making indicators. Similarly, I may be overestimating the intent-to-treat effects on mobility indicators.

[62] The p-value for a t-test of difference in attrition rates by treatment and control group assignment is 0.3165

Table 4.12. Characteristics of attrited women by treatment assignment

	(1) Total	(2) Control	(3) Treatment	(4) t-test (1)-(2)
Nuclear Household	0.151	0.227	0.097	0.130
	[0.361]	[0.429]	[0.301]	
Backward caste	0.509	0.500	0.516	-0.016
	[0.505]	[0.512]	[0.508]	
Scheduled caste	0.453	0.500	0.419	0.081
	[0.503]	[0.512]	[0.502]	
Progress out of Poverty Index	24.774	21.636	27.000	-5.364*
	[10.139]	[10.247]	[9.609]	
Number of adult HH members	6.283	5.273	7.000	-1.727*
	[3.586]	[2.815]	[3.933]	
Woman's age in years	32.792	32.091	33.290	-1.199
	[10.391]	[9.631]	[11.028]	
Woman in paid labour	0.057	0.045	0.065	-0.019
	[0.233]	[0.213]	[0.250]	
Forward Digit Span score	5.442	4.818	5.900	-1.082**
	[1.526]	[1.435]	[1.447]	
Number of children 4-18	1.830	1.864	1.806	0.057
	[1.553]	[1.612]	[1.537]	
Woman has no child 4-18	0.226	0.273	0.194	0.079
	[0.423]	[0.456]	[0.402]	
Baseline value of outcomes				
Decision Index	-0.210	-0.409	-0.069	-0.340
	[0.940]	[0.951]	[0.921]	
Decision Index excluding child decisions	-0.100	-0.159	-0.058	-0.101
	[1.046]	[1.059]	[1.053]	
Cooking	0.509	0.500	0.516	-0.016
	[0.505]	[0.512]	[0.508]	
Purchases at local shop	0.491	0.455	0.516	-0.062
	[0.505]	[0.510]	[0.508]	
Purchases outside village	0.396	0.364	0.419	-0.056
	[0.494]	[0.492]	[0.502]	
Child's Illness	0.472	0.364	0.548	-0.185
	[0.504]	[0.492]	[0.506]	
Child's enrolment	0.415	0.273	0.516	-0.243*
	[0.497]	[0.456]	[0.508]	
Child's attendance	0.340	0.182	0.452	-0.270**
	[0.478]	[0.395]	[0.506]	
Mobility Index	-0.126	0.019	-0.229	0.248
	[0.882]	[1.063]	[0.729]	
Leave house without permission	0.151	0.227	0.097	0.130
	[0.361]	[0.429]	[0.301]	
Go to local shop without permission	0.340	0.500	0.226	0.274**
	[0.478]	[0.512]	[0.425]	
Go to shop outside the village without permission	0.245	0.273	0.226	0.047
	[0.434]	[0.456]	[0.425]	
Visit health clinic without permission	0.075	0.091	0.065	0.026
	[0.267]	[0.294]	[0.250]	
Visit natal family without permission	0.113	0.182	0.065	0.117
	[0.320]	[0.395]	[0.250]	
Call natal family without permission	0.736	0.636	0.806	-0.170
	[0.445]	[0.492]	[0.402]	

	(1) Total	(2) Control	(3) Treatment	(4) t-test (2)-(3)
Go out for entertainment without permission	0.170	0.182	0.161	0.021
	[0.379]	[0.395]	[0.374]	
Control of assets Index	-0.249	-0.288	-0.222	-0.066
	[1.106]	[1.158]	[1.086]	
Owns mobile phone	0.358	0.409	0.323	0.087
	[0.484]	[0.503]	[0.475]	
Own back account	0.321	0.318	0.323	-0.004
	[0.471]	[0.477]	[0.475]	
Keeps her own jewellery	0.623	0.545	0.677	-0.132
	[0.489]	[0.510]	[0.475]	
N	53	22	31	

This table presents the mean and standard deviation (in parenthesis) of the overall attrited sample, the sample of women who attrited from control group and the sample of women who were attrited from treatment group. Column 4 presents the difference between control and treatment groups and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

In order to deal with the differential attrition rates and characteristics between the treatment and control groups, I construct bounds on the intent-to-treat estimates using the trimming method suggested by Lee (2009)[63]. I trim the control group sample as this was the group that experienced lower attrition, using 2.1 percent (or 7 women) as the trimming fraction. The trimming fraction is determined by the difference in attrition rates between treatment and control groups. By dropping the lowest 2.1 percent of the outcomes of the control group, I estimate the lower bound; and by dropping the highest 2.1 percent of the outcomes of the control group, I estimate the upper bound.

Table 4.13 reports the bounds on intent-to-treat effects on all outcome variables[64]. Column 1 reports the intent-to-treat effects from Section 4.6.1 (Table 4.4, Table 4.5 and Table 4.6). Column 2 and Column 3 report the lower and upper bounds on the intent-to-treat effects respectively. As expected, the main intent-to-treat effects in Column 1 lie between the estimated lower and upper bounds. The lower and upper bounds are significant only for the outcome variables where I report a significant impact of the TA+ programme. Given the low levels of attrition, the estimated bounds are tight. For instance, the estimated effect size of the TA+ programme on mobility index ranges from 0.115 to 0.182 standard deviations. From Table 4.12, we noted that the women who attrited from the treatment group had relatively lower levels of mobility as compared to the women who attrited from the control group. In this case, we would be worried that

[63] I also estimate the bounds using Horowitz and Manski (2000), which is not reported here. However, as has been noted in the empirical literature, this method gives very wide bounds with little inference ability.
[64] Appendix B Table C.17 reports the bounds on IV estimates for all outcome variables.

our intent-to-treat effect of the TA+ programme is overestimated. However, looking at our lower bound estimates in Column 2, these remain significant and close to our intent-to-treat effects on mobility indicators. Thus, there is little evidence that differential attrition influenced our intent-to-treat effects on female empowerment outcomes.

Table 4.13. Intent-to-treat effects correcting for attrition using Lee bounds

	(1)	(2)	(3)
Endline outcomes	Treatment	Lower Bound	Upper Bound
Decision Index	-0.047	-0.065	-0.023
	(0.072)	(0.077)	(0.077)
Decision Index excluding child decisions	-0.061	-0.080	-0.046
	(0.078)	(0.076)	(0.072)
Cooking	-0.032	-0.043	-0.021
	(0.035)	(0.040)	(0.034)
Purchases at local shop	-0.018	-0.028	-0.009
	(0.038)	(0.039)	(0.038)
Purchases outside village	-0.024	-0.031	-0.013
	(0.039)	(0.039)	(0.036)
Child's Illness	-0.011	-0.020	-0.001
	(0.039)	(0.039)	(0.038)
Child's enrolment	-0.013	-0.022	-0.001
	(0.037)	(0.038)	(0.037)
Child's attendance	0.003	-0.006	0.014
	(0.024)	(0.026)	(0.026)
Mobility Index	0.126*	0.115*	0.182***
	(0.062)	(0.060)	(0.059)
Leave house without permission	0.005	0.003	0.023
	(0.022)	(0.021)	(0.023)
Go to local shop without permission	0.075***	0.070***	0.089***
	(0.027)	(0.025)	(0.028)
Go to shop outside the village without permission	0.029	0.028	0.046*
	(0.025)	(0.027)	(0.025)
Visit health clinic without permission	0.037	0.036	0.051*
	(0.026)	(0.025)	(0.027)
Visit natal family without permission	0.010	0.009	0.024
	(0.020)	(0.019)	(0.019)
Call natal family without permission	0.098***	0.086**	0.107***
	(0.035)	(0.036)	(0.034)
Go out for entertainment without permission	0.005	0.004	0.024
	(0.022)	(0.023)	(0.021)
Control of assets Index	0.097	0.054	0.119
	(0.076)	(0.070)	(0.074)
Owns mobile phone	0.014	0.010	0.024
	(0.034)	(0.035)	(0.034)
Own back account	0.073*	0.062*	0.081**
	(0.039)	(0.037)	(0.039)
Keeps her own jewellery	0.002	-0.016	0.004
	(0.022)	(0.023)	(0.022)

This table presents the results of regression of endline empowerment outcomes on treatment assignment after trimming the sample using the method described in Lee(2009). Column (1) reports the intent-to-treat effects from Section 4.6.1..All regressions control for individual and household level controls, as well as the value of the outcome variables at baseline. Standard errors for Columns (2) and (3) were bootstrapped with 250 repetitions and clustered at thehamlet level. The number of observations for bounds regressions is 661, after trimming 7 observations. ***p<0.01, ** p<0.05, * p<0.1.

4.6.5. **Spillover**

Spillover could occur when those in the control group who did not participate in the TA+ programme, could still be affected by it. Since the randomisation design for this study was at the household level, control group women were present within the hamlet along with the treatment group women. One cannot rule out the possibility of treatment group women discussing the lessons from the TA+ programme with other control group women present in their social network. For instance, Basu et al. (2001) find in their rural Bangladesh sample that an illiterate adult earns significantly more if living with at least one literate household member. These results are strongest for women in their sample. Alternatively, spillovers can also be driven by 'role-model' effect (see Beamen et al., 2012 for the effect of female leadership on women's aspirations in rural India). Exposure to treatment group women who exhibited a change in behaviour as a result of TA+ programme, such as free and frequent movement, could have shifted the beliefs and aspirations of the control group women.

Given these channels of possible positive spillovers, the intent-to-treat effects reported in Section 4.6.1 are underreporting the effects of the programme. The data did not incorporate any measure of social networks or exact residence location of each woman to conduct a thorough estimation of the extent of spillovers. However, I attempt to provide a crude estimate of the spillover, which in all likelihood, is an underestimation of the true spillover effect.

It is relatively common in India to find that multiple households, although distinct consumption units, would reside within the same building or compound. These are still different households, only sharing a residence. The data identifies such households and I find that 49 of the 327 control group women live in the same physical residence as a treatment group woman. Using this indicator variable as an additional control, I re-run the estimations of the intent-to-treat effects reported in Table 4.4, Table 4.5 and Table 4.6. The exact specification used is –

$$y_{1i} = \alpha + \beta_1 * Treat_i + \beta_2 * Control\ woman\ lives\ close\ to\ treatment\ woman + \theta X_i + \pi y_{0i} + \epsilon_i \quad (5)$$

Where the base category is now all control group women who do not live at the same

residence location as a treatment group woman. Table 4.14 reports the intent-to-treat effects of the TA+ programme on all empowerment outcomes after controlling for spillover. I first report the intent-to-treat effects which do not control for spillovers (as reported in Section 4.6.1 – β_{ITT} from equation 1) and then report the intent-to-treat effects adjusted for spillovers (β_1 from equation 5). I additionally also report the coefficient on the binary variable capturing spillover where the control group woman lives in the same compound as a treatment group woman (β_2 from equation 5). This coefficient is never significant and perhaps, it is because of the low power (only 49 such women).

However, it is interesting to note that the intent-to-treat estimates after controlling for potential spillover (Column 2), are now higher for the outcomes that were significantly impacted by the TA+ programme[65]. Curiously, I also find a marginal significant (at 10 percent) effect of the TA+ programme on the likelihood of going to a health clinic without requiring permission.

[65] There is no significant difference in the coefficients on intent-to-treat between Columns (1) and (2).

Table 4.14. Intent-to-treat effects controlling for spillover

	(1)	Regression controlling for spillover (2)	(3)
Endline Outcomes	Original ITT estimates	New ITT estimates	Control woman lives close to treatment woman
Decision Index	-0.047	-0.059	-0.079
	(0.072)	(0.075)	(0.148)
Decision Index excluding child decisions	-0.061	-0.064	-0.019
	(0.078)	(0.079)	(0.148)
Cooking	-0.032	-0.028	0.026
	(0.035)	(0.036)	(0.074)
Purchases at local shop	-0.018	-0.022	-0.027
	(0.038)	(0.039)	(0.075)
Purchases outside village	-0.024	-0.027	-0.022
	(0.039)	(0.040)	(0.071)
Child's Illness	-0.011	-0.024	-0.087
	(0.039)	(0.039)	(0.093)
Child's enrolment	-0.013	-0.022	-0.063
	(0.037)	(0.042)	(0.068)
Child's attendance	0.003	-0.000	-0.022
	(0.024)	(0.028)	(0.083)
Mobility Index	0.126*	0.150**	0.153
	(0.062)	(0.066)	(0.134)
Leave house without permission	0.005	0.011	0.040
	(0.022)	(0.023)	(0.046)
Go to local shop without permission	0.075***	0.085***	0.068
	(0.027)	(0.029)	(0.084)
Go to shop outside the village without permission	0.029	0.030	0.005
	(0.025)	(0.027)	(0.039)
Visit health clinic without permission	0.037	0.041*	0.021
	(0.026)	(0.024)	(0.043)
Visit natal family without permission	0.010	0.012	0.016
	(0.020)	(0.018)	(0.042)
Call natal family without permission	0.098***	0.112**	0.091
	(0.035)	(0.041)	(0.093)
Go out for entertainment without permission	0.005	0.013	0.051
	(0.022)	(0.023)	(0.051)
Control of assets Index	0.097	0.108	0.074
	(0.076)	(0.088)	(0.173)
Owns mobile phone	0.014	0.011	-0.024
	(0.034)	(0.036)	(0.048)
Own back account	0.073*	0.087*	0.088
	(0.039)	(0.045)	(0.079)
Keeps her own jewellery	0.002	0.004	0.013
	(0.022)	(0.028)	(0.064)

This table presents the results of regression of endline empowerment outcomes on treatment assignment after controlling for spillover. Column (1) reports the intent-to-treat effects from Section 4.6.1..All regressions control for individual and household level controls, as well as the value of the outcome variables at baseline. Standard errors are clustered at hamlet level. Number of observations for all regressions is 668. ***p<0.01, ** p<0.05, * p<0.1.

4.7. **Conclusion**

India accounts for a third of the adult illiterates in the world (UNESCO, 2020) and is critical for understanding the impact of adult literacy programmes. This study directly contributes to the small body of rigorous evaluation of adult literacy programmes. It evaluates the impact of TARA Akshar+ programme targeted at adult female in the state of Uttar Pradesh, the state with the highest illiteracy rate in India and low levels of female empowerment. Evaluating the literacy and numeracy impacts of the programme, Deshpande et al. (2017) find positive effects. In this Chapter, I study the impact of the programme beyond literacy, on a range of female empowerment outcomes – decision making role of the woman within the household, freedom of movement and control of financial assets.

I find a significant and substantial increase in woman's mobility or ability to leave the house without having to seek permission, and an increase in the likelihood of the woman having a personal bank account. Women in the literacy programme are 22 percent more likely to leave to house to go to the local shops and 17 percent more likely to leave the house to call their natal family, both activities without requiring permission. They are also 13 percent more likely to open a personal bank account. I find no evidence of an increase in decision-making power over daily household decisions such as those involving household purchases, cooking, and children's health and education.

Since the TA+ programme enabled women to move out of the house daily to assemble at a central location to attend these classes, this may explain the impacts I find on mobility indicators but not on decision-making variables. Literature on social networks suggests that peer support and companionship can help overcome mobility barriers (Anukriti et al., 2020) and encourage financial independence (Field et al., 2016; Banerjee et al., 2013). Alternatively, this change may be due to an increase in self-confidence and self-esteem as suggested by qualitative research (see Egbo, 2000 in Nigeria; Archer & Cottingham, 1997 in Bangladesh; Stromquist, 1997 in Brazil). The increased likelihood of owning a personal bank account, post the literacy programme, may be a more direct result of becoming literate and the women now being able to read,

fill out, and sign bank forms.

However, the null effects on woman's decision making power may be due to the strong patriarchal family and societal structures that exist in India, particularly in rural Uttar Pradesh. Other studies in Uttar Pradesh looking at the impact of formal education on female empowerment found similar results on decision making power of the woman within the household, while finding positive impacts on woman's mobility (Bloom et al., 2001). In a comparative study between women in Uttar Pradesh and women in a less patriarchal Indian state, Tamil Nadu, Jejeebhoy and Sathar (2001) find that while primary schooling leads to female empowerment in Tamil Nadu, such relationship is absent in their Uttar Pradesh sample. Since women's access to labour market in Uttar Pradesh is limited (Foster & Rosenzweig, 1996), it is unlikely that the programme could have increased women's bargaining and decision making power. Additionally, due to low availability of information sources in rural (Thomas et al., 1991) and restrictive gender norms (Andrabi et al., 2012) settings, women are unlikely to have been able to increase their knowledge on household decision making domains, such as children's health and education.

The chapter also studied a limited form of spillover where the control group woman lives at the same location as a treatment group woman. I find suggestive evidence of positive spillovers, but I have low power to detect significance. I hypothesise that these spillovers may be a 'role model' (Beamen et al., 2012) effect where the aspirations of the control group women shift due to observing the change in behaviour of the treatment group women. This is good news for policy as even women with limited agency (who are not permitted by their household to join the literacy programme) may be influenced by the positive externality of the programme being offered in their 127

Investigating the correlates of the programme take-up, I find high levels of take-up (self-reported) among the treatment group. 79 percent of the treatment group women take part in the programme and attended 77 percent of the instructional classes, on average. Women from backward and scheduled castes (the lower castes) were more likely to take up the literacy programme, indicative that such programmes encourage inclusivity of socially marginalised groups in India. However, such a programme may still miss out on the least empowered women, as the study finds that women who refused to participate in the programme (and in the randomisation process), are also

subject to restrictions on leaving the house freely.

Despite the TA+ being a rather short programme, spanning only 56 days of instruction, the results in this study are encouraging and suggest that adult literacy programmes targeted at women can be a cost-effective way of improving women's position in the society. While there are other large scale government interventions that focus on gender equity, each aims at a different aspect of empowerment. One such intervention, Conditional Cash Transfers (CCT) targeted at women has been shown to reduce inter-generational gender-based inequity in human capital investment within the household (for an overview, see Rawlings & Rubio, 2005; Fiszbein & Schady, 2009, Kabeer & Waddington, 2015). However, the effect of such transfers on women's own agency is mixed – while most document no effects on decision making in rural areas (Handa et al., 2009; de Brauw et al., 2014; Hidrobo et al., 2014), some find limited impacts (Attanasio & Lechene, 2002 for Oportunidaes) or substantial impacts in urban areas (de Brauw et al., 2014 for Bolsa Familia). Compared to this narrow focus and mixed findings of the impact of CCTs on female empowerment, coupled with the overwhelming evidence on increase in domestic abuse due to the increase in women's income (Schuler et al.,1998; Angelucci, 2008), adult literacy programmes seem a more attractive policy route to improve women's own agency.

As developing countries such as India continue to roll out adult literacy programmes with the additional goal of achieving gender equality, it is crucial to contextualise the design of these programme. As seen in this study and elsewhere (Jejeebhoy & Sathar, 2001), achieving female empowerment through literacy programmes in the patriarchal society of Uttar Pradesh may be different from achieving the same in less patriarchal societies. Literacy is a socially situated experience and the design of such programmes should bear in mind the implementation contexts. Given the impacts of adult literacy programmes on female empowerment, it is important to reflect how such programmes can be better designed to bolster these impacts – for instance, employing a woman to deliver the literacy instructional classes (see Beamen et al., 2012 on role-model effects).

In finding positive impacts of female literacy programme, this study opens up exciting avenues for the evaluation of such programmes. There is a need for evaluation projects to include a wider range of measures to capture the change in self-esteem, aspirations, beliefs about the self and gender roles to understand how literacy translates into female

empowerment. Moreover, we need to push the frontier of the measurement of female empowerment to find more reliable measures that are appropriate for different contexts (such as, experimental measures explored in Almas et al., 2018).

5. Conclusion to the thesis

The thesis consists of three empirical chapters investigating questions of key importance to policy making in developing countries such as India. Here, I summarise the findings of each chapter, discuss their limitations, and possible avenues for future research.

Chapter 2 (*Do preschools add 'value'? Evidence on achievement gaps from rural India*) studied the impact of attending a preschool before starting primary school on cognitive, early language and numeracy skills. It additionally studied the heterogeneity in value-added of preschools by their management type.

Employing a lagged-score Value Added Model (VAM), the study found a positive and significant test score premium of attending a preschool before starting primary school. The entire effect was driven by children who attend private preschools. Children who attend public preschools before starting primary school do not have a significant advantage over children who start primary school with no preschool experience. On the contrary, children who attend private preschools prior to starting primary school have a test score premium of 0.33 SD units when compared with children in primary school with no preschool exposure.

I conducted a series of robustness checks to assess if the lagged test score in the VAM is a sufficient proxy for child and parent motivation, and if the results are sensitive to test score construction or testing environment. I find the results remain qualitatively similar in magnitude and significant.

A descriptive study of the preschool quality by management type showed that private preschools have lower student-teacher ratios, longer hours of operation and a focus on formal instructional style of teaching. On the other hand, public preschools conduct more play-based activities.

In the backdrop of the new National Education Policy (Government of India, 2020) that emphasises the development of foundational literacy and numeracy in preschool years, this study provided evidence that the public preschools in India are a long way away from achieving the policy goal.

However, preschools should not narrowly focus on development of cognitive skills, early literacy and numeracy, as captured by the test score in this study. Empirical evidence shows that one of the main benefits of early childhood education lies in nurturing of a child's non-cognitive or socio-emotional skills (see Barnett, 1995, 2011, for a review). In this light, there is a need to supplement the findings of this study with outcome measures on non-cognitive skills. The play-based activities used in public preschools may nurture soft-skills, and it would be incorrect to conclude that they have no effect on child development based only on the results of this study.

Given that children start primary school at varying levels of learning, there is a need to push for more research on pedagogical innovations that deal with learning heterogeneity within the classroom, such as 'Teaching at the Right Level' developed by Pratham NGO (Banerjee et al., 2017; Banerji & Chavan, 2020).

Chapter 3, co-authored with Dr. Annemie Maertens and Dr. Christopher Ksoll, (*Intra-household Efficiency in Extended Family Household: Evidence from rural India*) studied the intra-household efficiency in the decision-making process in complex households. We define complex households as households where multiple generations and/or married siblings co-reside. We employed lab-in-the-field public goods experiment, which has been used extensively in experimental literature to study intra-household efficiency (Munro, 2015). The public goods experiment is designed to uncover inefficiency which arises due to concealing of personal resources instead of contributing them to the household, with potentially larger shared benefits.

Our study showed that spouses in extended households are less efficient than spouses in nuclear households, although in monetary terms the efficiency loss between spouses in extended households only amounted to 1.40 Rupees (approximately 3.5 percent of the daily wage at the time of the experiment). Within extended households, not all relationships were equally inefficient. Household members related by blood were less inefficient than members related by in-law status, the relationship between mother-in-law and daughter-in-law displaying the highest level of inefficiency. We further supplement the experimental results with survey data on primary decision makers and qualitative interviews. We found that these inefficiencies within extended households exist due to multiple decision-makers and fragmented decision-making power, and the limited ability of young married women to assert their preferences in extended

households.

The findings in this study raise concern over using the traditional intra-household model that defines a household as comprised of the married couple and their unmarried children, in developing countries. The decision-making process is different in complex households, making it vital to come up with more appropriate intra-household models and better targeted and designed household-based policies. To this end, a long-term panel data documenting changes in household structure will prove useful.

While a public goods experiment uncovers only a specific type of inefficiency, one which is related to hiding of extra income/resources, the qualitative interviews in our study also uncovered additional patterns and dimensions of inefficient behaviour, such as production inefficiencies (slacking off and other forms of free riding). Developing experiments or other empirical methods, perhaps building on Udry (1996), to unpack other dimensions of inefficiency within the extended households, is another fruitful avenue for future research.

Chapter 4 (*Female Adult Literacy Programme and Empowerment: Evidence from RCT in rural India*) studied the impact of an adult female literacy programme (set up as an RCT) on a range of female empowerment measures – decision-making power within the household, freedom of movement and control over assets.

The study found a significant and substantial increase in a woman's ability to leave the house without having to seek permission, and an increase in the likelihood of the woman having a personal bank account. There was no significant impact on decision-making power over daily household decisions. The increase in mobility could be due to the woman having to leave the house on a daily basis to attend the literacy classes. The increased likelihood of owning a personal bank account, however, is a more direct result of becoming literate and the women now able to read, fill out and sign bank forms.

The study found high levels of programme take-up (79 percent among the treatment group) and that women from lower castes were more likely to take up the programme. The study explored a particular form of spillover that arises when the control group woman lives with a treatment group woman. It found suggestive evidence on the presence of positive spillovers on measures of freedom of movement but not on having a personal bank account, although there was not enough power to test for significance.

This positive externality may be because of ‘role model’ effects; the control group women feel empowered by observing the increased freedom of movement exhibited by the treated women.

While finding positive impacts of the literacy programme, the study remains constrained in exploring the mechanisms due to unavailability of data. There is a need for literacy programme evaluation projects to include a wider range of measures to capture the changes in self-esteem, beliefs about the self and gender roles, and aspirations.

Additionally, while this study finds positive impacts on some measures of female empowerment as does another study based in India by Kandpal et al. (2012); a study by Banerji et al. (2017) finds no significant impacts of their maternal literacy programme on female empowerment measures. Given that this literature is sparsely populated, there is a need to generate more evidence in this area as national policies and international agendas continue to roll out adult literacy programmes. It may also be beneficial to push the frontier of the measurement of female empowerment (such as, experimental measures explored in Almas et al., 2018) to provide more robust outcome variables that are less prone to measurement errors.

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A. Appendix for Chapter 2 (Do preschools add 'value'? Evidence on achievement gaps from rural India)

A.1. School Readiness Inventory Test score construction

Table A.1 lists the 24 items administered to the children in Rounds 1 and 2. The classification of each item as per the competency has been provided by the developers of the tool (the World Bank in conjunction with Centre for Early Childhood Education and Development, New Delhi).

Table A.1. Description of test administered

Competency		Assessment activity	ITEM No.
Cognitive skills & concepts	Pre-number concept	Given pictures of four apple trees, children were asked to point to the one with the least and most apples.	1, 2
	Space Concept	Given two illustrations of children and houses, children were asked to point to the one in which the child was behind the house.	3
	Sequential thinking	Children were shown illustrations of water filling up a bucket and were asked to determine the correct sequence for the pictures.	4
	Classification	Children were asked to classify six creatures as either birds or animals.	5
	Number/object matching	Children were asked to match three numbers with pictures showing the same number of objects.	8,9,10
	Picture Identification	Children were asked to identify three different pictures.	11,12,13
	Pattern making	Children were asked to repeat and complete a pictorial pattern.	18,19
	Relative comparisons	Children were asked to point to a number (among 9, 3, 7, 8) that was less than the number 5.	24
Language skills & concepts	Following instructions	Children were asked to raise their hands. Next, the child was asked to pick up an object and bring it to someone.	6, 7
	Reading readiness, identifies beginning sound	Children were asked to identify the beginning sound of words and to match the two words with the same beginning sound.	14,15,16,17
	Sentence making	Children were asked to describe four photographs in complete sentences.	20, 21, 22, 23

I used Item Response Theory (IRT) to assess the performance of each of the 24 items in

uncovering the latent ability parameter. The terminology 'ability' used in IRT is not the same as inherent ability, but only used to mean the skill or trait that the test intends to measure. Based on the observed probability of answering an item correctly in the data, the IRT estimates the difficulty and discrimination parameters for each item and hence, the latent ability for each individual. IRT models have been extensively used in the education literature, for example, in the construction of test score in international assessments such as TIMSS and PISA.

I used both the one-parameter logistic (1-PL) model and two-parameter logistic (2-PL) model to assess the reliabilty the test score. The 2-PL model is given by the following functional form, also known as the Item Characteristic Curve (ICC) –

$$P_q(X_{iq} = 1|\theta_i) = \frac{1}{1 + exp[-1.7a_q(\theta_i - b_q)]}$$

Where the probability of an individual *i* with ability θ_i to correctly answer a question *q* is given by two parameters – the difficulty parameter b_q, and the discrimination parameter a_q. The difference between 2-PL model and 1-PL model is that 1-PL model assumes that the discrimination parameter is constant across items, that is, $a_q = a$.

The discrimination parameter measures how well an item differentiates between high and low ability individuals. A positive discrimination parameter implies that higher ability individuals have a higher probability of answering the item correctly. A negative discrimination parameter would imply that a higher ability individual has a lower probability of answering the item correctly. Thus, in assessing the validity of an item, one would like the discrimination parameter (*a*) to be positive and high. Holding the discrimination parameter as constant across all items, as in the 1-PL model, implies that all ICCs have the same slope.

The difficulty parameter tells us how difficult an item is. Ceniza and Cereno (2012) provide the interpretation of the values of the difficulty parameter (*b*): Very Easy = Less than -2, Easy = -0.50 to -2.00, Average = -0.49 to 0.49, Difficult = 0.50 to 2.00 and Very Difficult = Greater than 2.00.

Using maximum likelihood estimator, I retrieve the difficulty and discrimination

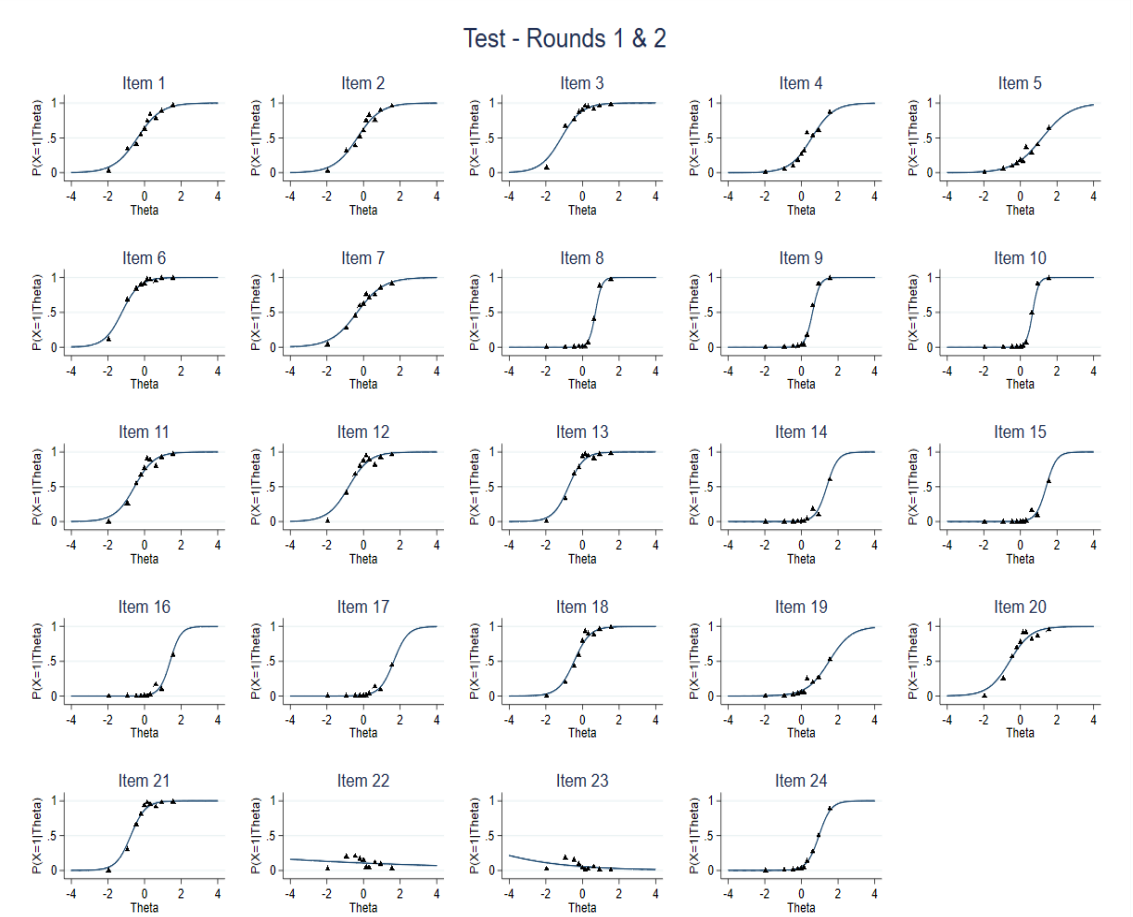
parameters for each of the 24 items on the test. I ran the IRT models on the combined Round 1 and 2 data. Table A.2 presents the results of these parameters from 2-PL and 1-PL model. First, the 2-PL model represents a better fit for the test as seen by the lower log likelihood value. However, even for the 1-PL model, the constant discrimination parameter is high and positive at 1.91. From the 2-PL model, I find that all values of discrimination parameter are positive. These results assure me that the test was reliable in differentiating between low and high ability children. Looking at the difficulty parameter, most items ranged from average to difficult levels. However, items 22 and 23 have very high values of difficulty parameter and low values on discrimination. Values higher than 3 on the difficulty parameter are mostly seen as suspicious and invalid. Hence, I drop items 22 and 23 from the test score construction.

Table A.2. Results of IRT 2 parameter and 1 parameter logistic model

	2-PL		1-PL	
Log likelihood	-157713		-165719	
Item No.	Discrimination	Difficulty	Discrimination	Difficulty
1	1.55	0.39	1.91	0.34
2	1.57	0.34	1.91	-0.30
3	1.78	-1.16	1.91	1.11
4	1.69	0.56	1.91	0.53
5	1.30	1.19	1.91	0.97
6	2.08	1.23	1.91	1.28
7	1.39	0.34	1.91	-0.27
8	5.76	0.71	1.91	0.96
9	5.00	0.61	1.91	0.79
10	6.14	0.67	1.91	0.90
11	1.86	0.55	1.91	0.54
12	1.76	0.81	1.91	0.77
13	2.37	0.76	1.91	0.85
14	3.31	1.38	1.91	1.74
15	3.60	1.41	1.91	1.82
16	3.46	1.40	1.91	1.79
17	2.62	1.66	1.91	1.92
18	2.30	0.45	1.91	0.51
19	1.64	1.52	1.91	1.42
20	1.72	0.58	1.91	0.55
21	2.50	0.75	1.91	0.86
22	0.12	17.79	1.91	1.64
23	0.38	7.37	1.91	2.08
24	3.04	0.96	1.91	1.16

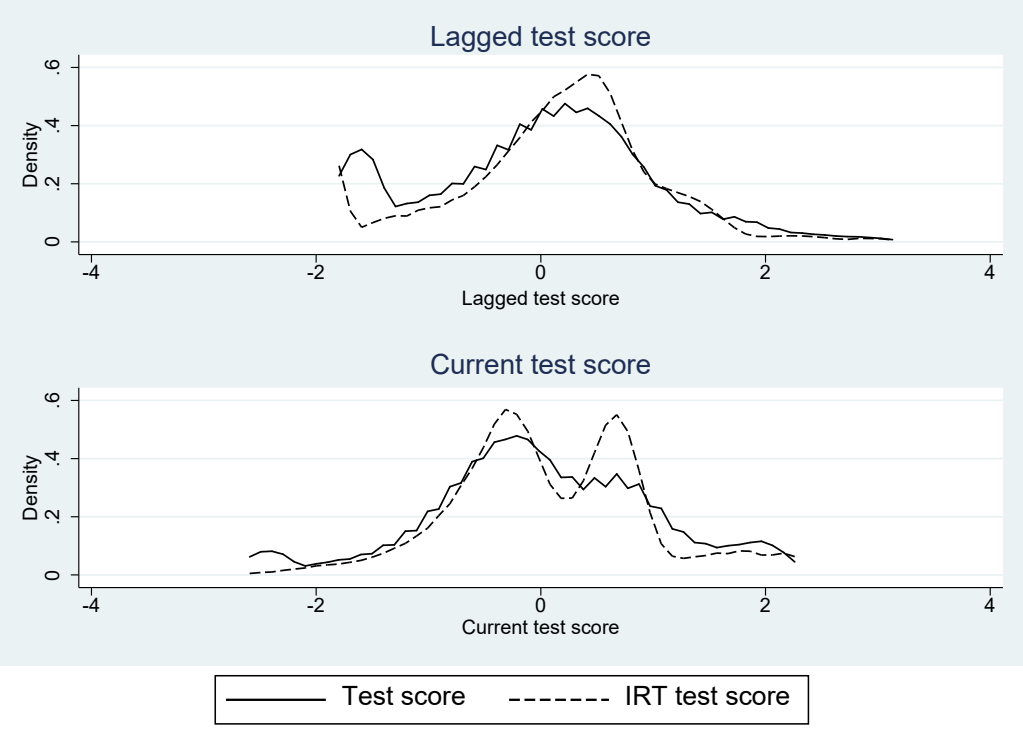
In Figure A.1, I graph the ICCs and the observed probability of answering an item correctly, to visually check the fit provided by the IRT 2-PL model. As is evident, suspiciously low proportion of children answered items 22 and 23 correctly, which leads to the IRT model predicting large values for the difficulty parameter.

Figure A.1. Item characteristics curves and observed probability



For simplicity, I used the standardised sum of scores over 22 items in my main analyses. In Figure A.2, I present the latent ability parameter using the IRT 2-PL model and how it compares with the standardised test scores used in the main paper. The latent ability parameter was also standardised to have a mean of 0 and standard deviation of 1. As we can see, the two distributions are similar. The current IRT score displays a bimodal tendency, and this is common when items are binary.

Figure A.2. Distribution of standardised test score and IRT estimated score



In Table A.3, I re-run the analysis from Table 2.9 using IRT constructed scores. The estimates using IRT scores are qualitatively similar to those using the standardised score in the main paper.

Table A.3. Preschool VAM estimates using IRT scores

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
Lagged - IRT score	0	0	1	1	0.253***	0.208***
					(0.017)	(0.016)
Preschool	0.643***	0.510***	0.164	0.146	0.522***	0.434***
	(0.088)	(0.091)	(0.103)	(0.104)	(0.085)	(0.089)
Primary school	0.610***	0.589***	0.202*	0.215**	0.507***	0.511***
	(0.092)	(0.095)	(0.108)	(0.109)	(0.089)	(0.092)
Preschool and school	0.812***	0.729***		0.325***	0.688***	0.645***
	(0.090)	(0.093)		(0.107)	(0.087)	(0.091)
Female		-0.095***		0.017		-0.072***
		(0.019)		(0.024)		(0.018)
Age in months		0.026***		0.000		0.020***
		(0.003)		(0.004)		(0.003)
Years of education - Father		0.014***		0.007**		0.012***
		(0.003)		(0.003)		(0.003)
Years of education - Mother		0.023***		0.004		0.019***
		(0.003)		(0.003)		(0.003)
Both parents work outside of home		-0.102***		-0.005		-0.082***
		(0.033)		(0.040)		(0.031)
Muslim (Base category: Hindu)		-0.143***		-0.019		-0.117**
		(0.046)		(0.065)		(0.046)
Scheduled caste		-0.173***		-0.004		-0.138***
		(0.043)		(0.056)		(0.041)
Scheduled tribe		-0.171***		0.102		-0.114**
		(0.060)		(0.072)		(0.057)
Backward castes		-0.060*		0.080*		-0.031
		(0.035)		(0.045)		(0.034)
Wealth index		0.034**		0.013		0.030*
		(0.015)		(0.020)		(0.015)
Ownership of durables index		0.072***		0.018		0.060***
		(0.014)		(0.019)		(0.014)
HH has children's reading material		0.041		-0.052		0.021
		(0.027)		(0.036)		(0.026)
HH has toys/games for child		0.045*		-0.020		0.032
		(0.027)		(0.036)		(0.026)
Constant	0.130	-1.412***	0.599***	0.517*	0.249***	-1.011***
	(0.086)	(0.232)	(0.100)	(0.277)	(0.083)	(0.220)
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,124	8,124	8,124	8,124	8,124	8,124
R-squared	0.231	0.288	0.224	0.227	0.286	0.323

All specifications control for village fixed effects. Standard errors are clustered at the village level. The variables of interest are preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), primary school (attending primary schools in Rounds 1 and 2 with no preschool exposure), attending preschool before starting primary school. The base category is not enrolled. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

In Table A.4, I re-run the analysis from Table 2.10 using IRT constructed scores. The estimates using IRT scores are qualitatively similar to those using the standardised score in the main paper.

Table A.4. Private preschool VAM estimates using IRT scores

	(1)	(2)	(3)	(4)	(5)	(6)
	Contemporaneous VAM		Perfect persistence VAM		Lagged score VAM	
Lagged - IRT score	0	0	1	1	0.207***	0.184***
					(0.016)	(0.016)
Private preschool	0.912***	0.771***	0.295***	0.297***	0.784***	0.684***
	(0.086)	(0.089)	(0.105)	(0.106)	(0.084)	(0.088)
Public preschool	0.122	0.132	-0.065	-0.060	0.083	0.097
	(0.086)	(0.089)	(0.104)	(0.105)	(0.084)	(0.087)
Primary school	0.656***	0.627***	0.215**	0.229**	0.565***	0.554***
	(0.090)	(0.092)	(0.108)	(0.108)	(0.087)	(0.090)
Private preschool and school	1.109***	0.991***	0.296***	0.305***	0.940***	0.865***
	(0.094)	(0.098)	(0.113)	(0.113)	(0.090)	(0.094)
Public preschool and school	0.656***	0.625***	0.341***	0.356***	0.591***	0.576***
	(0.088)	(0.090)	(0.107)	(0.108)	(0.087)	(0.089)
Female		-0.058***		0.034		-0.042**
		(0.019)		(0.024)		(0.018)
Age in months		0.021***		-0.002		0.017***
		(0.003)		(0.004)		(0.003)
Years of education - Father		0.010***		0.005		0.009***
		(0.003)		(0.003)		(0.003)
Years of education - Mother		0.018***		0.002		0.015***
		(0.003)		(0.003)		(0.003)
Both parents work outside of home		-0.076**		0.007		-0.061**
		(0.031)		(0.040)		(0.030)
Muslim (Base category: Hindu)		-0.086*		0.009		-0.068
		(0.045)		(0.065)		(0.045)
Scheduled caste		-0.080*		0.032		-0.059
		(0.042)		(0.057)		(0.041)
Scheduled tribe		-0.101*		0.139*		-0.057
		(0.057)		(0.072)		(0.055)
Backward castes		-0.031		0.093**		-0.008
		(0.033)		(0.045)		(0.032)
Wealth index		0.006		-0.000		0.005
		(0.015)		(0.019)		(0.014)
Ownership of durables index		0.051***		0.006		0.043***
		(0.014)		(0.019)		(0.014)
HH has children's reading material		0.026		-0.055		0.012
		(0.027)		(0.037)		(0.026)
HH has toys/games for child		0.022		-0.031		0.012
		(0.026)		(0.036)		(0.025)
Constant	0.216***	-1.126***	0.632***	0.665**	0.302***	-0.796***
	(0.081)	(0.222)	(0.100)	(0.274)	(0.080)	(0.212)
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,124	8,124	8,124	8,124	8,124	8,124
R-squared	0.306	0.330	0.233	0.235	0.341	0.357
Private preschool=School F-stat	30.81***	10.44***	2.190	1.520	24.27***	8.982***
Public preschool=School F-stat	92.32***	84.06***	21.57***	22.63***	84.83***	78.55***
Private preschool=Public preschool F-stat	354.1***	230.3***	65.28***	61.89***	304.3***	208.5***
Private preschool and school=School F-stat	58.53***	37.80***	1.439	1.277	43.81***	29.51***
Public preschool and school=School F-stat	2.09e-06	0.00126	3.779*	3.893**	0.286	0.212

All specifications control for village fixed effects. Standard errors are clustered at the village level. The variables of interest are private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), primary school (attending primary schools in Rounds 1 and 2 with no preschool exposure), attending private preschool before starting primary school, and attending public preschool before starting primary school. The base category is not enrolled. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

A.2. **VAM excluding 'not enrolled'**

Since only 100 children are not enrolled and are all located in Rajasthan, I re-run the lagged score VAM excluding the not enrolled children and use children who attended primary school with no exposure to preschool as the base category. In Table A.5, I first report the lagged score VAM estimates on the full sample without controls (Column 1) and with controls (Column 2). In Columns 3 and 4, I report the lagged score VAM estimates on sample excluding the 100 not enrolled children. The coefficients on the variables of interest change marginally (by approximately 0.001 SD unit) and remain qualitatively similar.

Similarly, in Table A.6, I report the lagged score VAM estimates by management type excluding the not enrolled children. Columns 1 and 2 run the same lagged score VAM as reported in Table 2.10. The only difference is that I use primary school with no preschool exposure as the base category, instead of not enrolled. Columns 3 and 4 report the estimates on the sample excluding the 100 not enrolled children. The coefficients on the variables of interest change marginally and remain significant.

Table A.5. Preschool VAM estimates excluding never enrolled

	(1) Current Score	(2) Current Score	(3) Current Score	(4) Current Score
Lagged - Standardised score	0.276***	0.225***	0.275***	0.224***
	(0.019)	(0.017)	(0.019)	(0.017)
Not enrolled	-0.520***	-0.526***		
(Base category: Primary school only)	(0.083)	(0.086)		
Preschool	0.009	-0.088**	0.012	-0.087**
(Base category: Primary school only)	(0.046)	(0.043)	(0.046)	(0.043)
Preschool and school	0.190***	0.140***	0.190***	0.139***
(Base category: Primary school only)	(0.048)	(0.046)	(0.048)	(0.046)
Female		-0.080***		-0.081***
		(0.019)		(0.019)
Age in months		0.021***		0.021***
		(0.003)		(0.003)
Years of education - Father		0.012***		0.013***
		(0.003)		(0.003)
Years of education - Mother		0.020***		0.019***
		(0.003)		(0.003)
Both parents work outside of home		-0.080**		-0.081**
		(0.036)		(0.036)
Muslim (Base category: Hindu)		-0.126***		-0.133***
		(0.047)		(0.048)
Scheduled caste		-0.159***		-0.162***
		(0.043)		(0.043)
Scheduled tribe		-0.122**		-0.127**
		(0.057)		(0.058)
Backward castes		-0.040		-0.045
		(0.035)		(0.035)
Wealth index		0.034**		0.032**
		(0.016)		(0.016)
Ownership of durables index		0.068***		0.072***
		(0.015)		(0.015)
HH has children's reading material		0.023		0.023
		(0.027)		(0.028)
HH has toys/games for child		0.026		0.025
		(0.027)		(0.027)
Constant	-0.043	-1.306***	-0.041	-1.311***
	(0.040)	(0.226)	(0.040)	(0.227)
Village fixed effects	Yes	Yes	Yes	Yes
Sample	Full	Full	Excluding not enrolled	Excluding not enrolled
Observations	8,124	8,124	8,024	8,024
R-squared	0.312	0.348	0.304	0.342
Preschool=Mixed F-stat	25.38***	44.21***	24.65***	43.54***

All specifications control for village fixed effects. Standard errors are clustered at the village level. The variables of interest are not enrolled, preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), attending preschool before starting primary school. The base category is attending primary school with no preschool exposure. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

Table A.6. Private preschool VAM estimates excluding not enrolled

	(1) Current Score	(2) Current Score	(3) Current Score	(4) Current Score
Lagged - Standardised score	0.224***	0.198***	0.224***	0.197***
	(0.018)	(0.017)	(0.018)	(0.017)
Not enrolled	-0.581***	-0.571***		
(Base category: Primary school only)	(0.081)	(0.084)		
Private preschool	0.225***	0.129***	0.228***	0.130***
(Base category: Primary school only)	(0.045)	(0.043)	(0.045)	(0.043)
Public preschool	-0.513***	-0.488***	-0.510***	-0.486***
(Base category: Primary school only)	(0.052)	(0.051)	(0.052)	(0.051)
Private preschool and school	0.400***	0.332***	0.400***	0.331***
(Base category: Primary school only)	(0.058)	(0.059)	(0.058)	(0.059)
Public preschool and school	0.024	0.019	0.024	0.019
(Base category: Primary school only)	(0.048)	(0.047)	(0.048)	(0.047)
Female		-0.048***		-0.049***
		(0.019)		(0.019)
Age in months		0.017***		0.017***
		(0.003)		(0.003)
Years of education - Father		0.009***		0.010***
		(0.003)		(0.003)
Years of education - Mother		0.015***		0.015***
		(0.003)		(0.003)
Both parents work outside of home		-0.058*		-0.059*
		(0.035)		(0.035)
Muslim (Base category: Hindu)		-0.075		-0.082*
		(0.046)		(0.047)
Scheduled caste		-0.076*		-0.079*
		(0.042)		(0.042)
Scheduled tribe		-0.062		-0.067
		(0.055)		(0.055)
Backward castes		-0.017		-0.021
		(0.034)		(0.034)
Wealth index		0.008		0.006
		(0.015)		(0.015)
Ownership of durables index		0.050***		0.053***
		(0.014)		(0.015)
HH has children's reading material		0.013		0.012
		(0.027)		(0.028)
HH has toys/games for child		0.005		0.004
		(0.026)		(0.026)
Constant	0.073*	-1.035***	0.076**	-1.039***
	(0.038)	(0.220)	(0.038)	(0.221)
Village fixed effects	Yes	Yes	Yes	Yes
Sample	Full	Full	Excluding not enrolled	Excluding not enrolled
Observations	8,124	8,124	8,024	8,024
R-squared	0.367	0.382	0.360	0.376
Private preschool=Public preschool F-stat	317.8***	210.8***	316.7***	208.5***

All specifications control for village fixed effects. Standard errors are clustered at the village level. The variables of interest are not enrolled, private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), attending private preschool before starting primary school, and attending public preschool before starting primary school. The base category is primary school with no preschool exposure. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

A.3. **Preschool survey**

The data presented in Section 2.5.3 comes from the preschool survey conducted in Round 1. Not all preschools would have been surveyed, depending on whether these were open at the time of the visit and granted access to the investigators to conduct a survey. Private preschools may be more inclined to not grant such access, and the ones that did, could very well be 'better' quality. Additionally, if a preschool was located outside the village, the facility would not have been surveyed. This is more likely to be a private preschool which would be located outside a village in order to cater to the catchment area of several nearby villages.

In Table A.7, I present the information on number of preschools surveyed by state. As suspected, on average, the study surveyed three public preschools per village and only one private preschool per village. In Assam, on average, four public preschools were surveyed per village, the highest among the three states. This is expected as the current funding guidelines for North-eastern states (of which Assam is one) is that the Central government would contribute to 90 percent of the construction and operational costs[66]. Compare this to the guideline for Rajasthan and Andhra Pradesh where the Central government contributes to 75 percent of the construction cost and 60 percent of the operational cost.

While, one would assume that the number of private facilities to be lower than public facilities in each village, there is an element of bias introduced by the survey itself. For instance, the data shows that one village in Assam and four villages in Rajasthan had no public preschool. This cannot be true as the household survey clearly indicates that children in these village were going to a public preschool. Additionally, the government mandate is to have at least one public preschool in an area of 800 children under the age of six years, or a 'mini' public preschool in an area of 150-300 children under the age of six years[67].

Second, according to the preschool survey, 10 villages in Rajasthan, 68 villages in

[66] www.icds-wcd.nic.in/icds.aspx
[67] www.icds-wcd.nic.in/icds.aspx

Assam, and 64 villages in Andhra Pradesh have no private preschool. However, from the household survey, I find that in all the 10 villages in Rajasthan, in 45 out of the 68 village in Assam, and in 58 out of the 64 villages in Andhra Pradesh, children are enrolled in private preschools. Thus, the preschool survey was neither a census of the preschool facilities in the village, nor representative of these facilities.

Table A.7. Preschools surveyed per village

	(1) Rajasthan	(2) Assam	(3) Andhra Pradesh	(4) Total
Average number of preschools surveyed per village	4.136	4.406	3.163	3.902
	[1.627]	[2.499]	[1.266]	[1.930]
Average number of public preschool surveyed per village	2.272	3.990	2.745	2.983
	[1.021]	[2.231]	[1.169]	[1.711]
Average number of private preschool surveyed per village	1.864	0.417	0.418	0.919
	[1.221]	[0.706]	[0.608]	[1.127]
Village has at least 1 public preschool surveyed	0.961	0.990	1.000	0.983
	[0.194]	[0.102]	[0.000]	[0.129]
Village has at least 1 private preschool surveyed	0.903	0.323	0.357	0.535
	[0.298]	[0.470]	[0.482]	[0.500]
Number of villages	100	100	100	300

A.4. **Lagged test score proxy for motivation**

Table A.8. Regression of lagged test score on controls, child motivation and parent's motivation

	(1) Lagged score	(2) Lagged score	(3) Lagged score	(4) Lagged score
Reads story to child	0.118***	0.071*	0.062	0.029
	(0.040)	(0.043)	(0.038)	(0.041)
Helps with learning tasks	0.293***	0.186***	0.183***	0.106***
	(0.032)	(0.035)	(0.031)	(0.035)
Talk to staff about child's learning progress		0.108***		0.068**
		(0.032)		(0.031)
Wants child to read/write		0.100***		0.095***
		(0.032)		(0.031)
Child talks about preschool always		0.193***		0.165***
		(0.040)		(0.040)
Child talks about preschool sometimes		0.120***		0.108***
		(0.031)		(0.030)
Child likes going to preschool		0.153***		0.141***
		(0.032)		(0.031)
Female			-0.105***	-0.097***
			(0.020)	(0.021)
Age in months			0.024***	0.023***
			(0.003)	(0.003)
Years of education - Father			0.005**	0.005*
			(0.002)	(0.003)
Years of education - Mother			0.015***	0.014***
			(0.003)	(0.003)
Both parents work outside of home			-0.087**	-0.080**
			(0.037)	(0.037)
Muslim (Base category: Hindu)			-0.129***	-0.118**
			(0.048)	(0.053)
Scheduled caste			-0.167***	-0.172***
			(0.050)	(0.054)
Scheduled tribe			-0.256***	-0.222***
			(0.059)	(0.063)
Backward castes			-0.137***	-0.128***
			(0.041)	(0.044)
Wealth index			0.023	0.022
			(0.017)	(0.017)
Ownership of durables index			0.056***	0.052***
			(0.014)	(0.016)
HH has children's reading material			0.069**	0.025
			(0.031)	(0.034)
HH has toys/games for child			0.054*	0.036
			(0.032)	(0.034)
Constant	-0.200***	-0.398***	-1.584***	-1.663***
	(0.018)	(0.032)	(0.221)	(0.231)
Sample	Full	Preschool	Full	Preschool
Controls	No	No	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Observations	8,124	7,263	8,124	7,263
R-squared	0.290	0.300	0.318	0.324

All specifications control for village fixed effects. Standard errors are clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

A.5. **Results for sub-sample without mixed (preschool and primary school) category – only non-switchers**

Table A.9. Lagged score VAM estimates excluding mixed category sub-sample

	(1) Current score	(2) Current score	(3) Current score	(4) Current score
Lagged - Standardised score	0.266***	0.212***	0.213***	0.186***
	(0.019)	(0.018)	(0.019)	(0.018)
Preschool	0.524***	0.435***		
	(0.082)	(0.086)		
Primary school	0.482***	0.495***	0.549***	0.542***
	(0.087)	(0.091)	(0.085)	(0.088)
Private preschool			0.810***	0.704***
			(0.082)	(0.085)
Public preschool			0.073	0.089
			(0.079)	(0.083)
Female		-0.083***		-0.049**
		(0.021)		(0.021)
Age in months		0.020***		0.017***
		(0.004)		(0.004)
Years of education - Father		0.015***		0.011***
		(0.003)		(0.003)
Years of education - Mother		0.018***		0.014***
		(0.003)		(0.003)
Both parents work outside of home		-0.079*		-0.058
		(0.040)		(0.039)
Muslim (Base category: Hindu)		-0.120**		-0.061
		(0.058)		(0.058)
Scheduled caste		-0.183***		-0.100**
		(0.049)		(0.049)
Scheduled tribe		-0.080		-0.011
		(0.066)		(0.063)
Backward castes		-0.050		-0.025
		(0.039)		(0.038)
Wealth index		0.039**		0.009
		(0.017)		(0.016)
Ownership of durables index		0.071***		0.047***
		(0.017)		(0.017)
HH has children's reading material		0.022		0.020
		(0.031)		(0.031)
HH has toys/games for child		0.048		0.028
		(0.030)		(0.029)
Constant	-0.544***	-1.775***	-0.474***	-1.552***
	(0.079)	(0.260)	(0.076)	(0.249)
Controls added	No	Yes	No	Yes
Sample	No mixed	No mixed	No mixed	No mixed
Village fixed effects	Yes	Yes	Yes	Yes
Observations	6,263	6,263	6,263	6,263
R-squared	0.341	0.380	0.401	0.417
Preschool=School F-stat	0.729	1.748		
Private preschool=School F-stat			28.45***	12.04***
Public preschool=School F-stat			74.52***	70.30***
Private preschool=Public preschool F-stat			300.5***	193.3***

This table reports the results of Table 2.9 and Table 2.10 excluding the children who switch from preschool to primary school between Rounds 1 and 2. All specifications control for village fixed effects. Standard errors are clustered at the village level. The variables of interest are private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), and primary school with no preschool exposure. The base category is not enrolled. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

A.6. **A discussion of choice**

I run a multinomial logit model on the choice of educational participation. The categories of participation are – never enrolled, going to a primary school only, going to a public preschool only, going to a private preschool only, going to a public preschool with primary school, and going to a private preschool with primary school. The model includes full set of child and household level controls available for the entire sample. It includes state dummies (base category being Rajasthan) to capture the difference in educational norms and trends by state (see Section 2.3.3).

In Table A.10, I report the probability of selecting a participation category for each covariate, instead of the log odds ratio or relative risk ratio. Both, odds ratio and relative risk ratio, are conditional on the base category, making interpretations between categories difficult. For direct comparisons, I present the average marginal effect of covariates on each participation category.

The results confirm the patterns that emerged from the descriptive statistics reported in Section 2.3.5. Girls, Muslims, and children from socially disadvantaged groups (lower caste categories) are less likely to attend a private preschool. Older children are more likely to be in primary school or to have switched from preschool to primary school. Older children are also more likely to be in a public preschool.

Parent's education, wealth index and consumer durable index are positively associated with private preschool attendance. If both parents are employed outside the household, the child is more likely to attend a public preschool and less likely to attend a private preschool.

Households having reading material at home is negatively associated with public preschool attendance and positively with primary school attendance. Parents are also more likely to help the child with learning tasks at home if the child attends private preschool.

Children in Assam and Andhra Pradesh are less likely to attend primary school than children in Rajasthan. Children in Assam are more likely to attend public preschools,

while in Andhra Pradesh, they are more likely to attend private preschools.

Table A.10. Average marginal effects on educational participation estimated from multinomial logit model

	(1) Never enrolled	(2) Primary school	(3) Public preschool	(4) Private preschool	(5) Public preschool and school	(6) Private preschool and school
Female	0.008	0.012*	0.025***	-0.059***	0.022***	-0.007
	(0.006)	(0.006)	(0.009)	(0.011)	(0.008)	(0.004)
Age in months	-0.001	0.002***	-0.009***	-0.001	0.005***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years of education - Father	-0.001**	0.000	-0.003***	0.006***	-0.003***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years of education - Mother	0.002*	-0.002	-0.008***	0.012***	-0.004***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Both parents work outside of home	0.002	0.000	0.042***	-0.064***	0.026**	-0.005
	(0.011)	(0.011)	(0.012)	(0.015)	(0.010)	(0.011)
Muslim (Base category: Hindu)	0.010	0.025***	0.095***	-0.118***	0.026***	-0.037***
	(0.007)	(0.008)	(0.010)	(0.009)	(0.009)	(0.009)
Scheduled caste	-0.009	0.033***	0.091***	-0.132***	0.055***	-0.039***
	(0.010)	(0.010)	(0.014)	(0.014)	(0.015)	(0.009)
Scheduled tribe	-0.009	0.031**	0.070***	-0.101***	0.018	-0.010
	(0.011)	(0.012)	(0.017)	(0.015)	(0.018)	(0.009)
Backward castes	0.004	0.002	0.032***	-0.024**	0.006	-0.021***
	(0.008)	(0.007)	(0.012)	(0.010)	(0.013)	(0.006)
Wealth index	0.003	-0.018***	-0.039***	0.060***	-0.011**	0.005
	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.003)
Ownership of durables index	-0.003	-0.011***	-0.009	0.031***	0.002	-0.011***
	(0.003)	(0.004)	(0.006)	(0.006)	(0.005)	(0.003)
HH has children's reading material	-0.028***	0.037***	-0.029***	-0.004	-0.011	0.035***
	(0.008)	(0.008)	(0.010)	(0.011)	(0.009)	(0.008)
HH has toys/games for child	0.009	0.010	-0.050***	0.006	0.010	0.014***
	(0.007)	(0.007)	(0.010)	(0.010)	(0.009)	(0.005)
Reads story to child	0.001	-0.024**	0.013	-0.004	0.018*	-0.004
	(0.009)	(0.010)	(0.012)	(0.012)	(0.010)	(0.007)
Helps with learning tasks	-0.021**	-0.015**	-0.036***	0.043***	-0.021**	0.049***
	(0.009)	(0.006)	(0.010)	(0.011)	(0.010)	(0.007)
Assam	0.031***	-0.158***	0.577***	-0.194***	-0.103***	-0.153***
	(0.010)	(0.009)	(0.010)	(0.011)	(0.009)	(0.009)
Andhra Pradesh	-0.007	-0.087***	0.002	0.078***	0.106***	-0.092***
	(0.011)	(0.013)	(0.011)	(0.020)	(0.015)	(0.012)
Observations	9,121	9,121	9,121	9,121	9,121	9,121

The table reports the marginal effects post running a multinomial logistic regression on the educational participation categories. Standard errors were bootstrapped and clustered at the village level. The child's age is in months at the time of testing in Round 2. Both parents work outside of home is a dummy variable which is 0 when either one of the parent stays at home. The base category for scheduled caste, scheduled tribe and backward castes is general caste. The wealth index comprises of household building material, having a toilet, piped water, electricity and using higher grade fuel for cooking. The durables index comprises of ownership of TV, fan, fridge, cycle, scooter, phone.*** p<0.01, ** p<0.05, * p<0.1

A.7. **Robustness check – excluding zeroes and not enrolled**

Table A.11. VAM estimates excluding children scoring zero in Round 1 and not enrolled

	(3) Current Score	(4) Current Score	(7) Current Score	(8) Current Score
Lagged - Standardised score	0.224***	0.239***	0.197***	0.209***
	(0.017)	(0.019)	(0.017)	(0.019)
Private preschool			0.130***	0.140***
(Base category: Primary school only)			(0.043)	(0.048)
Public preschool			-0.486***	-0.477***
(Base category: Primary school only)			(0.051)	(0.056)
Private preschool and school			0.331***	0.330***
(Base category: Primary school only)			(0.059)	(0.062)
Public preschool and school			0.019	0.020
(Base category: Primary school only)			(0.047)	(0.051)
Preschool	-0.087**	-0.066		
(Base category: Primary school only)	(0.043)	(0.047)		
Preschool and school	0.139***	0.148***		
(Base category: Primary school only)	(0.046)	(0.050)		
Sample	Excluding not enrolled	Excluding not enrolled and children scoring zero on lagged score	Excluding not enrolled	Excluding not enrolled and children scoring zero on lagged score
Controls added	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Observations	8,024	7,091	8,024	7,091
R-squared	0.342	0.342	0.376	0.378

This table presents the results of Table 2.9 (Column 1) and Table 2.10 (Column 3) for sub-sample of children excluding not enrolled. In Columns 2 and 4, it re-runs the same specifications for the sub-sample of children excluding children who scored 0 on the tests in Round 1. All specifications control for village fixed effects and child and household level controls as in Table 2. 9. Standard errors are clustered at the village level. The variables of interest are private preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), public preschool (attending preschool in Rounds 1 and 2 and not yet started primary school), attending private preschool before starting primary school, and attending public preschool before starting primary school. The base category is primary school with no preschool exposure. .*** p<0.01, ** p<0.05, * p<0.1

B. Appendix for Chapter 3 (Intra-household Efficiency in Extended Family Households: Evidence from rural India)

Table B.1. Characteristics of omitted and experiment extended household sample

	(1) Omitted Sample	(2) Experiment Sample	(3) Total	(4) t-test
Selected illiterate woman's current age	41.510	38.71	40.154	2.8
	[10.229]	[10.616]	[10.389]	
Selected woman's husband's age	44.881	41.68	42.903	3.201
	[10.402]	[10.360]	[10.206]	
Number of years married	27.500	23.47	25.220	4.03
	[11.331]	[11.94]	[11.642]	
Husband's education in years	6.024	6.83	6.592	-0.806
	[4.937]	[4.689]	[4.720]	
Backward caste	0.347	0.38	0.386	-0.033
	[0.481]	[0.487]	[0.488]	
Scheduled Caste	0.388	0.44	0.423	-0.052
	[0.492]	[0.498]	[0.495]	
PPIscore	27.714	24.98	25.702	2.734
	[10.368]	[10.95]	[10.793]	
Number of household members	8.490	9.838	9.249	-1.348
	[3.916]	[4.352]	[3.970]	
Number of adult male household members	2.673	2.865	2.826	-0.192
	[1.231]	[1.404]	[1.370]	
Number of adult female household members	2.490	2.708	2.664	-0.218
	[0.960]	[1.087]	[1.064]	
Number of migrant members	1.939	1.630	1.693	0.309
	[2.096]	[2.098]	[2.097]	
Number of adult male migrant members	1.184	1.203	1.199	-0.019
	[1.014]	[1.191]	[1.155]	
Number of adult female migrant members	0.224	0.167	0.178	0.058
	[0.468]	[0.461]	[0.462]	
Sample size	37	204	241	

This table presents the mean and standard deviation (in parenthesis) of the omitted sample for extended households where no public goods experiment could be conducted, the experiment sample for extended households where at least one experiment was conducted, and the full extended household sample. Column 4 presents the difference between omitted and experiment sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.2. Characteristics of omitted and experiment nuclear household sample

	(1) Omitted Sample	(2) Experiment Sample	(3) Total	(4) t-test
Selected illiterate woman's current age	37.036	38.76	37.623	-1.724
	[8.513]	[8.768]	[8.774]	
Selected woman's husband's age	39.589	41.95	40.404	-2.361
	[8.800]	[8.874]	[9.247]	
Number of years married	21.778	23.333	22.445	-1.555
	[9.326]	[8.902]	[9.164]	
Husband's education in years	6.356	4.6	5.654	1.756*
	[4.620]	[4.492]	[4.674]	
Backward caste	0.329	0.313	0.322	0.016
	[0.473]	[0.467]	[0.469]	
Scheduled Caste	0.565	0.642	0.599	-0.077
	[0.499]	[0.483]	[0.492]	
PPIscore	24.153	25.55	24.841	-1.397
	[10.203]	[10.43]	[10.253]	
Number of household members	5.212	5.030	5.132	0.182
	[1.582]	[1.714]	[1.638]	
Number of adult male household members	1.212	1.343	1.270	-0.132
	[0.709]	[0.641]	[0.681]	
Number of adult female household members	1.176	1.239	1.204	-0.062
	[0.467]	[0.495]	[0.479]	
Number of migrant members	0.776	0.463	0.638	0.314***
	[0.679]	[0.659]	[0.686]	
Number of adult male migrant members	0.706	0.403	0.572	0.303***
	[0.633]	[0.605]	[0.637]	
Number of adult female migrant members	0.000	0.000	0.000	N/A
	[0.000]	[0.000]	[0.000]	
Sample size	90	62	152	

This table presents the mean and standard deviation (in parenthesis) of the omitted sample for nuclear households where no public goods experiment could be conducted, the experiment sample for nuclear households where at least one experiment was conducted, and the full extended household sample. Column 4 presents the difference between omitted and experiment sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration. The number of adult female migrants is 0 as in a nuclear household, the selected woman would be the only adult female. Her unmarried daughter could also be an adult member, but given the low rate of migration among unmarried women, it is unlikely that this member would be a migrant.

Table B.3. Reason for not playing spousal experiment

	Extended	Nuclear	Overall
Spousal experiment implemented	111	62	173
Spousal experiment not implemented	130	90	220
Reasons for no spousal experiment			
Selected woman widowed	17	19	36
Husband is migrant labour	68	58	126

This table reports the reason inferred from household survey for not playing the spousal experiment in a household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.4. Reason for not playing non-spousal experiments

	Baseline extended household sample	Sample where the experiment was actually conducted	Eligible participant does not exist	Eligible participant is a migrant
Woman and another male	241	86	47	50
Woman and another female	241	124	41	8
Husband and another male	241	38	62	90
Husband and another female	241	52	54	76
The other male and other female	241	63	65	50
Total	1205	363	269	274

This table reports the reason inferred from household survey for not playing the non-spousal experiment in extended households. An eligible participant does not exist if 1) the husband is dead and/or 2) there is no adult married male member and/or 3) there is no adult married female member. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.5. Descriptive statistics for full baseline sample by household structure

	(1) Total	(2) Extended	(3) Nuclear	(4) t-test
Selected woman's current age	39.237	40.154	37.623	2.531**
	[9.918]	[10.389]	[8.774]	
Selected woman's husband's age	42.003	42.903	40.404	2.498**
	[9.970]	[10.206]	[9.247]	
Number of years married	24.232	25.220	22.445	2.775**
	[10.913]	[11.642]	[9.164]	
Husband's education in years	6.209	6.592	5.654	0.937*
	[4.723]	[4.720]	[4.674]	
Backward caste	0.363	0.386	0.322	0.064
	[0.481]	[0.488]	[0.469]	
Scheduled Caste	0.49	0.423	0.599	-0.175***
	[0.501]	[0.495]	[0.492]	
PPI score	25.368	25.702	24.841	0.861
	[10.581]	[10.793]	[10.253]	
Number of household members	7.656	9.249	5.132	4.117***
	[3.836]	[3.970]	[1.638]	
Sample size	393	241	152	

This table presents the mean and standard deviation (in parenthesis) of the full baseline sample (including where no experiments were conducted) by household structure. Column 4 presents the difference between extended and nuclear sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Figure B.1. Distribution of contribution to common account by experiment type

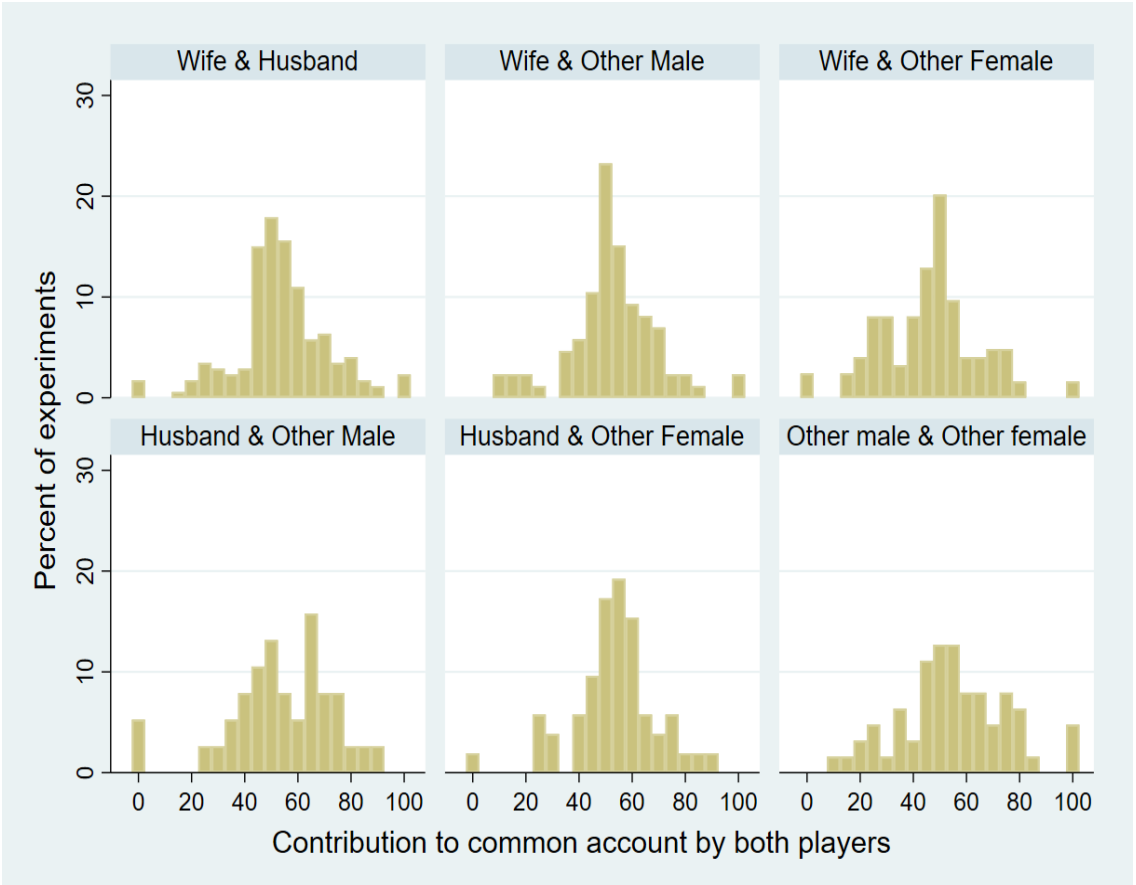


Figure B.2. Distribution of contribution in spousal experiment by household structure

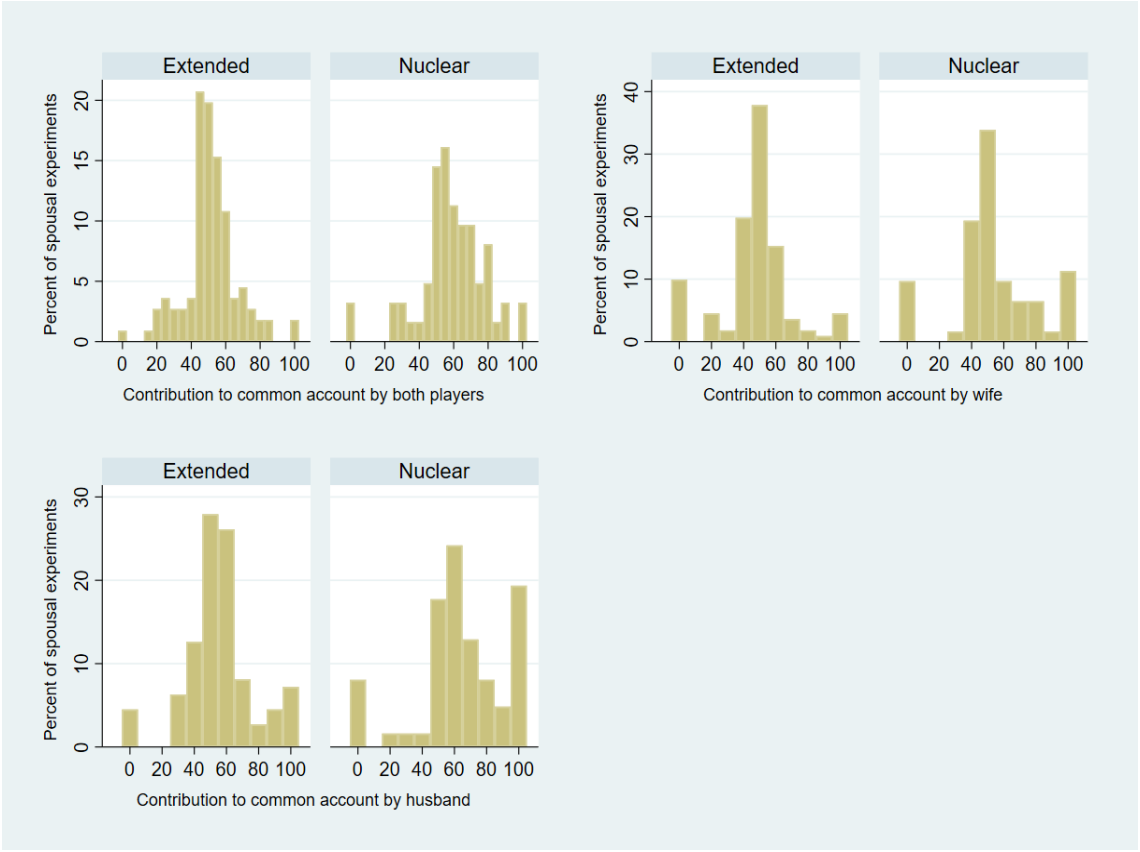


Table B.6. Correlation between participants' contributions

VARIABLES	(1) Player 1 contribution to common account	(2) Player 1 contribution to common account	(3) Player 1 contribution to common account	(4) Player 1 contribution to common account	(5) Player 1 contribution to common account
Player 2 contribution to common account	0.056	0.118**	0.010	0.144**	0.058
	(0.121)	(0.060)	(0.058)	(0.063)	(0.067)
Nuclear	0.522				
Nuclear==0 if extended	(1.308)				
Nuclear*Player2 contribution	0.015				
	(0.201)				
Constant	4.361***	4.500***	5.054***	4.516***	4.944***
	(0.649)	(0.305)	(0.311)	(0.336)	(0.349)
Standard errors	Clustered at HH level	Clustered at HH level	HH fixed effects	Clustered at HH level	HH fixed effects
Analysis Type	Only Spousal games	All games within extended family	All games within extended family	Games in extended family excluding spousal game	Games in extended family excluding spousal game
Observations	173	474	474	363	363
R-squared	0.022	0.017	0.515	0.027	0.497

This table reports the correlation between the two experiment participants' contributions to common account. No controls other than those mentioned in the table were added. *** p<0.01, ** p<0.05, * p<0.1

Table B.7. Descriptive statistics for spousal experiment sample by household structure

	(1) Total Mean/SD	N	(2) Extended Mean/SD	N	(3) Nuclear Mean/SD	N	(4) t-test
Wife's current age	40.514	173	41.045	111	38.76	62	2.285
	[10.137]		[11.028]		[8.768]		
Husband's current age	43.427	164	44.038	104	41.95	60	2.088
	[10.118]		[10.842]		[8.874]		
Number of years married	25.433	171	26.312	109	23.333	62	2.979
	[11.354]		[12.552]		[8.902]		
Husband's Education in years	5.762	164	6.452	104	4.6	60	1.852**
	[4.906]		[5.014]		[4.492]		
Backward Caste	0.364	173	0.378	111	0.313	62	0.065
	[0.483]		[0.487]		[0.467]		
Scheduled Caste	0.526	173	0.477	111	0.642	62	-0.165*
	[0.501]		[0.502]		[0.483]		
PPI Score	25.721	172	25.818	110	25.55	62	0.268
	[11.128]		[11.357]		[10.43]		
Number of household members	8.197	173	9.928	111	5.030	62	4.898***
	[4.320]		[4.362]		[1.714]		

This table presents the mean and standard deviation (in parenthesis) of the experiment sample where the spousal experiment was conducted, by household structure. Column 4 presents the difference between extended and nuclear sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.8. Descriptive statistics for spousal experiment regression sample by household structure

	(1) Total	(2) Extended	(3) Nuclear	(4) t-test
Wife's current age	40.484	41.000	39.610	1.390
	[9.840]	[10.560]	[8.495]	
Husband's current age	43.333	43.930	42.322	1.608
	[10.103]	[10.808]	[8.776]	
Number of years married	25.428	26.380	23.814	2.566
	[11.147]	[12.199]	[8.959]	
Husband's Education in years	5.774	6.540	4.475	2.065**
	[4.920]	[5.022]	[4.489]	
Backward Caste	0.358	0.380	0.322	0.058
	[0.481]	[0.488]	[0.471]	
Scheduled Caste	0.522	0.460	0.627	-0.167**
	[0.501]	[0.501]	[0.488]	
PPI Score	26.182	26.100	26.322	-0.222
	[11.249]	[11.742]	[10.458]	
Number of household members	8.151	10.010	5.000	5.010***
	[4.358]	[4.380]	[1.712]	
Sample size	159	100	59	

This table presents the mean and standard deviation (in parenthesis) of the spousal experiment regression sample reported in **Error! Reference source not found.**, by household structure. Column 4 presents the difference between extended and nuclear sample and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. PPI score is Progress out of Poverty Index ranging from 0 to 100. Base category for Backward Caste and Scheduled Caste is General category. Number of household members includes migrant labour in the household. Migrant member in the survey was defined as any member who has been away from the household for at least 6 months but intends to return. It may not capture shorter-term migration.

Table B.9. Order or repetition effects in experiments employed within extended households

	Total contribution (1)
Both players male	0.017
(0 = players are not both male; 1 = both players are male)	(0.060)
Both players different gender	0.056**
(0 = both players not different gender; 1 = both players are different gender)	(0.028)
Blood relatives	0.079***
(0 = players are not related by blood; 1 = players are related by blood)	(0.024)
Same generation	-0.021
(0 = players belong to different generations; 1 = players are from same generation)	(0.027)
Spouses	0.036
(0 = players are not married to each other; 1 = players are married to each other)	(0.032)
Player 1 second game	0.032
(0 = game is not player 1's 2nd game; 1 = is 2nd game)	(0.024)
Player 1 third game	0.031
(0 = game is not player 1's 3rd game; 1 = is 3rd game)	(0.039)
Player 2 second game	-0.021
(0 = game is not player 2's 2nd game; 1 = is 2nd game)	(0.028)
Player 2 third game	0.022
(0 = game is not player 2's 3rd game; 1 = is 3rd game)	(0.048)
Constant	0.441***
	(0.025)
HH fixed effects	Yes
Number of games	474
R-squared	0.058

This table reports the results of the regression mapping the contribution to the common account in all experiments (including the spousal experiment) played in the extended households, after controlling for the order in which the game for played for each participant. Contributions are measured in proportions. Total contribution is the contribution by both players to the common account. *** p<0.01, ** p<0.05, * p<0.1

Table B.10. Primary decision maker by household type

	Nuclear		Extended				
				No parents-in-law	Only mother-in-law	Only father-in-law	Both parents-in-law
Decisions	Primary decision maker	% (1)	Primary decision maker	% (2)	% (3)	% (4)	% (5)
HH chores: cooking, shopping	Husband	45	Father-in-law			40	47
	Wife	51	Mother-in-law		6		13
			Husband	31	26	17	13
			Wife	65	64	40	22
	Others	4	Others	4	4	4	6
Children: child illness, school enrollment and attendance	Husband	49	Father-in-law			22	38
	Wife	50	Mother-in-law		3		4
			Husband	35	34	21	16
			Wife	58	54	48	31
	Others	1	Others	7	9	9	12
Number of respondents		62		84	38	19	63

Note: This table reports results from an analysis of survey data recording who is the primary decision maker for specific situations in nuclear and extended households. The 'wife' is the selected illiterate woman and all relationships are with respect to this 'wife'. This 'wife' could herself be a mother-in-law which would not be captured in the table here. The 'others' category in nuclear household is a child of the married couple who may or may not be 18 years of age. The 'others' category in extended households can be a child of the married couple, a brother-in-law, a sister-in-law or a daughter-in-law of the selected illiterate woman. The percentage reported is the average over three decision making subcategories (for household chores - cooking, local purchases, and shopping outside the village; for decisions related to respondent's children - health, enrolment, and attendance).

Table B.11.Qualitative study nuclear household summary

	Interview 1	Interview 2	Interview 3
Village type	Poorer	Wealthier	Wealthier
Number of children	2	4	7
Role in the household	Wife	Wife	Wife
Migrant household members	Yes, husband	Yes, husband	No
Livelihood of household	Small store, remittances	Remittances, sell milk from cow	Making and selling of spice mixtures
Views on extended family households	Emotional support, public goods and joint assets	Prefers nuclear family as extended family is characterized by conflict	Emotional support, risk-sharing
Mention of inefficiency?	No	No	No
Sources of inefficiency	Not relevant	Not relevant	Not relevant

Table B.12.Qualitative study extended household summary

	Interview 4	Interview 5	Interview 6	Interview 7	Interview 8
Village type	Poorer	Poorer	Wealthier	Poorer	Poorer
Number of children	1	6	2	1	5
Role in the household	Daughter-in-law	Mother-in-law	Mother-in-law	Daughter-in-law	Daughter-in-law
Migrant household members (in relation to respondent)	No, but migrant brother-in-law	No	Yes, eldest son	Yes, husband	No
Livelihood of household	Farming, sell milk from cow	Farming, casual labor, driver, shoemaking	Remittances, petrol stand	Farming, remittances	Farming, carpet weaving, sell milk from cow
Views on extended families	Emotional support, public goods, specialization	Risk-sharing, public goods	No reflections offered	Risk-sharing	Public goods, specialization
Mention of inefficiency?	Yes: free-riding brothers-in-law and mother-in-law	No	Yes: free-riding mother-in-law	Yes: hiding remittances, free-riding brothers-in-law and mother-in-law, low effort from daughter-in-law	Yes: free-riding brothers-in-law and mother-in-law
Sources of inefficiency	Uncontractable effort with social norms of equal share Invisible effort, unequal power		Invisible effort, unequal power	Invisible effort, unequal power	Uncontractable effort with social norms of equal share Invisible effort, unequal power

C. Appendix for Chapter 4 (Female Adult Literacy Programme and Empowerment: Evidence from RCT in rural India)

Table C.1. Baseline characteristics by treatment assignment for full sample (including those who attrit)

	(1) Total		(2) Control		(3) Treatment		(4) t-test
	N	Mean	N	Mean	N	Mean	(2)-(3)
Nuclear Household	725	0.330	349	0.344	376	0.316	0.027
		[0.470]		[0.476]		[0.466]	
Backward caste	725	0.472	349	0.464	376	0.479	-0.015
		[0.500]		[0.499]		[0.500]	
Scheduled caste	725	0.488	349	0.496	376	0.481	0.014
		[0.500]		[0.501]		[0.500]	
Progress out of Poverty Index	725	22.690	349	22.513	376	22.854	-0.341
		[9.475]		[9.679]		[9.291]	
Number of adult HH members	725	5.639	349	5.670	376	5.609	0.061
		[4.220]		[4.378]		[4.073]	
Woman's age in years	725	35.068	349	34.934	376	35.191	-0.257
		[8.501]		[8.150]		[8.824]	
Woman in paid labour	725	0.063	349	0.060	376	0.066	-0.006
		[0.244]		[0.238]		[0.249]	
Forward Digit Span score	720	5.603	346	5.526	374	5.674	-0.148
		[1.504]		[1.521]		[1.486]	
Number of children 4-18	725	2.461	349	2.433	376	2.487	-0.054
		[1.706]		[1.712]		[1.702]	
Woman has no child 4-18	725	0.153	349	0.166	376	0.141	0.025
		[0.360]		[0.373]		[0.348]	
Baseline value of outcomes							
Decision Index	725	0.029	349	-0.026	376	0.080	-0.106
		[0.998]		[1.001]		[0.993]	
Decision Index excluding child decisions	725	0.065	349	-0.010	376	0.136	-0.146*
		[1.010]		[1.003]		[1.013]	
Cooking	725	0.632	349	0.605	376	0.657	-0.052
		[0.483]		[0.490]		[0.475]	
Purchases at local shop	725	0.512	349	0.476	376	0.545	-0.070*
		[0.500]		[0.500]		[0.499]	
Purchases outside village	725	0.469	349	0.433	376	0.503	-0.070*
		[0.499]		[0.496]		[0.501]	
Child's Illness	725	0.550	349	0.536	376	0.564	-0.028
		[0.498]		[0.499]		[0.497]	
Child's enrolment	725	0.521	349	0.521	376	0.521	0.000
		[0.500]		[0.500]		[0.500]	
Child's attendance	725	0.521	349	0.501	376	0.540	-0.038
		[0.500]		[0.501]		[0.499]	

	(1) Total		(2) Control		(3) Treatment		(4) t-test
	N	Mean	N	Mean	N	Mean	(2)-(3)
Mobility Index	725	-0.009	349	0.001	376	-0.018	0.019
		[0.985]		[1.003]		[0.969]	
Leave house without permission	725	0.166	349	0.181	376	0.152	0.029
		[0.372]		[0.385]		[0.359]	
Go to local shop without permission	725	0.421	349	0.421	376	0.420	0.001
		[0.494]		[0.494]		[0.494]	
Go to shop outside the village without permission	725	0.218	349	0.223	376	0.213	0.011
		[0.413]		[0.417]		[0.410]	
Visit health clinic without permission	725	0.193	349	0.206	376	0.181	0.025
		[0.395]		[0.405]		[0.385]	
Visit natal family without permission	725	0.141	349	0.138	376	0.144	-0.006
		[0.348]		[0.345]		[0.351]	
Call natal family without permission	725	0.716	349	0.699	376	0.731	-0.032
		[0.451]		[0.459]		[0.444]	
Go out for entertainment without permission	725	0.193	349	0.195	376	0.191	0.003
		[0.395]		[0.397]		[0.394]	
Control of assets Index	725	-0.006	349	-0.018	376	0.006	-0.024
		[1.025]		[1.011]		[1.038]	
Owns mobile phone	725	0.407	349	0.410	376	0.404	0.005
		[0.492]		[0.492]		[0.491]	
Own back account	725	0.407	349	0.387	376	0.426	-0.039
		[0.492]		[0.488]		[0.495]	
Keeps own jewellery	725	0.714	349	0.719	376	0.710	0.009
		[0.452]		[0.450]		[0.454]	

This table presents the mean and standard deviation (in parenthesis) of the overall sample randomised the sample of women who were assigned to control and the sample of women who were assigned to treatment. Column 4 presents the difference between control and treatment groups and reports the results of the t-test of difference. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category. Forward Digit Span Test is a subtest of both of Wechsler Adult Intelligence Scale (WAIS) and the Wechsler Memory Scale (WMS) with a maximum score of 16.Progress out of Poverty Score ranges from 1 to 100 and is a composite index of education level of household head, household's main occupation, cooking fuel, ownership of durables such as cupboard, vehicle, TV, VCR/DVD/VCD player, sewing machine, thermoware.

Table C.2. Intent-to-treat effects on decision-making (no controls)

	(1) Decision Index	(2) Decision Index excl child	(3) Cooking	(4) Purchases at local shop	(5) Purchases outside village	(6) Child's Illness	(7) Child's enrolment	(8) Child's attendance
Treatment	-0.011	-0.019	-0.018	-0.003	-0.005	-0.006	-0.012	0.017
	(0.078)	(0.091)	(0.044)	(0.045)	(0.043)	(0.042)	(0.038)	(0.026)
Constant	0.000	0.000	0.566***	0.508***	0.431***	0.498***	0.391***	0.450***
	(0.044)	(0.051)	(0.027)	(0.028)	(0.024)	(0.028)	(0.024)	(0.025)
Controls	No	No	No	No	No	No	No	No
Observations	672	672	672	672	672	672	672	672
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

This table presents the results of regression of decision-making power on treatment assignment with no controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.3. Intent-to-treat effects on decision-making (controlling for only baseline value of outcome)

	(1) Decision Index	(2) Decision Index excl child	(3) Cooking	(4) Purchases at local shop	(5) Purchases outside village	(6) Child's Illness	(7) Child's enrolment	(8) Child's attendance
Treatment	-0.048	-0.075	-0.033	-0.026	-0.030	-0.011	-0.007	0.011
	(0.079)	(0.092)	(0.042)	(0.046)	(0.044)	(0.042)	(0.037)	(0.026)
Outcome at baseline	0.399***	0.367***	0.268***	0.326***	0.338***	0.288***	0.261***	0.263***
	(0.049)	(0.047)	(0.055)	(0.044)	(0.039)	(0.035)	(0.035)	(0.046)
Constant	0.000	0.000	0.402***	0.352***	0.283***	0.341***	0.251***	0.312***
	(0.043)	(0.047)	(0.044)	(0.034)	(0.028)	(0.034)	(0.028)	(0.039)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Observations	672	672	672	672	672	672	672	672
R-squared	0.153	0.135	0.067	0.106	0.116	0.082	0.072	0.070

This table presents the results of regression of decision-making power on treatment assignment with controlling only for the baseline value of the outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.4. Intent-to-treat effects on decision-making (controlling for only characteristics)

	(1) Decision Index	(2) Decision Index excl child	(3) Cooking	(4) Purchases at local shop	(5) Purchases outside village	(6) Child's Illness	(7) Child's enrolment	(8) Child's attendance
Treatment	-0.023	-0.027	-0.023	-0.005	-0.008	-0.007	-0.017	0.005
	(0.066)	(0.070)	(0.034)	(0.035)	(0.037)	(0.038)	(0.038)	(0.024)
Nuclear household	0.428***	0.418***	0.172**	0.198**	0.205***	0.229***	0.104**	0.127***
	(0.131)	(0.148)	(0.067)	(0.072)	(0.072)	(0.059)	(0.047)	(0.041)
Backward caste	-0.142	-0.091	-0.031	-0.035	-0.059	-0.030	-0.079	-0.107**
	(0.132)	(0.128)	(0.066)	(0.070)	(0.068)	(0.089)	(0.068)	(0.051)
Scheduled caste	-0.092	0.013	0.017	0.028	-0.026	-0.038	-0.065	-0.137**
	(0.135)	(0.132)	(0.068)	(0.073)	(0.072)	(0.088)	(0.073)	(0.055)
Progress out of Poverty Index	-0.000	0.002	0.001	0.000	0.001	0.001	-0.002	-0.002
	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Number of adult HH members	-0.013	-0.016	-0.008	-0.008	-0.006	0.002	-0.004	-0.007
	(0.021)	(0.023)	(0.010)	(0.011)	(0.011)	(0.009)	(0.007)	(0.007)
Woman's age in years	0.030***	0.036***	0.017***	0.017***	0.016***	0.007***	0.007***	0.008***
	(0.004)	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Woman in paid labour	0.333**	0.315**	0.096	0.173**	0.164*	0.176**	0.068	0.129
	(0.160)	(0.149)	(0.072)	(0.071)	(0.082)	(0.067)	(0.079)	(0.091)
Forward Digit Span score	0.032	0.025	0.010	0.012	0.013	0.013	0.012	0.017
	(0.023)	(0.022)	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)
Number of children 4-18	0.061**	-0.000	0.006	-0.002	-0.004	0.048***	0.042***	0.058***
	(0.029)	(0.027)	(0.015)	(0.013)	(0.012)	(0.016)	(0.012)	(0.013)
Constant	-1.348***	-1.486***	-0.138	-0.180	-0.220	-0.038	0.058	0.105
	(0.290)	(0.310)	(0.177)	(0.145)	(0.142)	(0.175)	(0.119)	(0.116)
Controls	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman
Observations	668	668	668	668	668	668	668	668
R-squared	0.137	0.155	0.126	0.142	0.132	0.098	0.063	0.104

This table presents the results of regression of decision-making power on treatment assignment controlling for household and woman level characteristics but no control for the baseline outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.5. Intent-to-treat effects on mobility (no controls)

	(1) Mobility Index	(2) Leave house	(3) Go to local shop	(4) Go to shop outside the village	(5) Visit health clinic	(6) Visit natal family	(7) Call natal family	(8) Go out for entertainment
Treatment	0.134*	0.002	0.090***	0.027	0.035	0.013	0.109***	0.006
	(0.073)	(0.021)	(0.032)	(0.026)	(0.031)	(0.020)	(0.036)	(0.025)
Constant	0.000	0.128***	0.333***	0.141***	0.159***	0.092***	0.572***	0.101***
	(0.065)	(0.017)	(0.040)	(0.020)	(0.021)	(0.017)	(0.037)	(0.017)
Controls	No	No	No	No	No	No	No	No
Observations	672	672	672	672	672	672	672	672
R-squared	0.004	0.000	0.009	0.001	0.002	0.000	0.013	0.000

This table presents the results of regression of mobility on treatment assignment with no controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.6. Intent-to-treat effects on mobility (controlling for only baseline value of outcome)

VARIABLES	(1) Mobility Index	(2) Leave house	(3) Go to local shop	(4) Go to shop outside the village	(5) Visit health clinic	(6) Visit natal family	(7) Call natal family	(8) Go out for entertainment
Treatment	0.134*	0.006	0.084**	0.030	0.041	0.007	0.107***	0.006
	(0.071)	(0.022)	(0.031)	(0.025)	(0.031)	(0.019)	(0.036)	(0.025)
Outcome at baseline	0.426***	0.177***	0.254***	0.313***	0.277***	0.335***	0.089**	0.079**
	(0.056)	(0.062)	(0.043)	(0.052)	(0.039)	(0.058)	(0.040)	(0.037)
Constant	0.000	0.097***	0.228***	0.072***	0.100***	0.047***	0.509***	0.085***
	(0.059)	(0.016)	(0.036)	(0.015)	(0.019)	(0.015)	(0.048)	(0.018)
Controls	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline
Observations	672	672	672	672	672	672	672	672
R-squared	0.174	0.039	0.076	0.128	0.087	0.155	0.020	0.011

This table presents the results of regression of mobility on treatment assignment with controlling only for the baseline value of the outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.7. Intent-to-treat effects on mobility (controlling for only characteristics)

	(1) Mobility Index	(2) Leave house	(3) Go to local shop	(4) Go to shop outside the village	(5) Visit health clinic	(6) Visit natal family	(7) Call natal family	(8) Go out for entertainment
Treatment	0.121*	0.001	0.077***	0.025	0.032	0.014	0.100***	0.005
	(0.063)	(0.021)	(0.026)	(0.026)	(0.025)	(0.020)	(0.034)	(0.022)
Nuclear household	0.224*	0.106***	0.080	0.102**	0.139***	0.047	-0.119***	0.005
	(0.124)	(0.035)	(0.055)	(0.041)	(0.036)	(0.032)	(0.042)	(0.038)
Backward caste	0.057	0.043	0.076	-0.039	0.017	-0.018	0.041	0.010
	(0.110)	(0.046)	(0.091)	(0.052)	(0.054)	(0.053)	(0.111)	(0.067)
Scheduled caste	0.032	0.037	0.083	-0.036	0.004	-0.019	-0.013	0.018
	(0.134)	(0.057)	(0.092)	(0.058)	(0.059)	(0.050)	(0.115)	(0.078)
Progress out of Poverty Index	-0.014***	-0.005**	-0.005*	-0.004**	-0.004**	-0.004***	-0.001	-0.001
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)
Number of adult HH members	-0.010	0.002	-0.016***	-0.001	0.001	0.004	-0.012***	-0.003
	(0.012)	(0.003)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)
Woman's age in years	0.028***	0.008***	0.018***	0.011***	0.010***	0.001	-0.003	0.006***
	(0.005)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
Woman in paid labour	0.823***	0.143**	0.119**	0.256***	0.260***	0.262***	0.200***	0.182***
	(0.170)	(0.065)	(0.054)	(0.067)	(0.056)	(0.071)	(0.066)	(0.047)
Forward Digit Span score	0.004	-0.005	0.006	0.001	0.008	-0.003	0.019	-0.009
	(0.024)	(0.007)	(0.014)	(0.009)	(0.009)	(0.006)	(0.011)	(0.008)
Number of children 4-18	-0.053*	-0.019**	-0.015	-0.014	-0.017*	-0.025**	0.026*	-0.016*
	(0.028)	(0.009)	(0.011)	(0.010)	(0.009)	(0.010)	(0.014)	(0.008)
Constant	-0.670***	-0.068	-0.198	-0.119	-0.182	0.207**	0.615***	-0.014
	(0.240)	(0.095)	(0.132)	(0.107)	(0.109)	(0.090)	(0.151)	(0.110)
Controls	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman	HH and Woman
Observations	668	668	668	668	668	668	668	668
R-squared	0.123	0.081	0.134	0.118	0.116	0.072	0.051	0.061

This table presents the results of regression of mobility on treatment assignment controlling for household and woman level characteristics but no control for the baseline outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.8. Intent-to-treat effects on control of assets (no controls)

	(1) Control of assets Index	(2) Owns mobile phone	(3) Own back account	(4) Keeps own jewellery
Treatment	0.119	0.019	0.092*	0.004
	(0.077)	(0.039)	(0.046)	(0.023)
Constant	-0.000	0.422***	0.575***	0.865***
	(0.100)	(0.032)	(0.044)	(0.028)
Controls	No	No	No	No
Observations	672	672	672	672
R-squared	0.004	0.000	0.009	0.000

This table presents the results of regression of control of assets on treatment assignment with no controls. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.9. Intent-to-treat effects on control of assets (controlling for only baseline value of outcome)

	(1) Control of assets Index	(2) Owns mobile phone	(3) Own back account	(4) Keeps own jewellery
Treatment	0.109	0.018	0.078*	0.006
	(0.078)	(0.034)	(0.043)	(0.024)
Outcome at baseline	0.348***	0.424***	0.310***	0.106***
	(0.041)	(0.035)	(0.034)	(0.037)
Constant	-0.000	0.248***	0.454***	0.788***
	(0.081)	(0.032)	(0.039)	(0.040)
Controls	Baseline	Baseline	Baseline	Baseline
Observations	672	672	672	672
R-squared	0.136	0.177	0.108	0.020

This table presents the results of regression of control of assets on treatment assignment with controlling only for the baseline value of the outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.10. Intent-to-treat effects on control of assets (controlling for only characteristics)

	(1) Control of assets Index	(2) Owns mobile phone	(3) Own back account	(4) Keeps own jewellery
Treatment	0.048	0.001	0.064	-0.013
	(0.086)	(0.033)	(0.043)	(0.028)
Nuclear household	0.252**	0.058	0.047	0.098**
	(0.111)	(0.054)	(0.041)	(0.038)
Backward caste	-0.009	-0.014	-0.029	0.024
	(0.196)	(0.094)	(0.101)	(0.085)
Scheduled caste	-0.179	-0.147	-0.066	0.026
	(0.216)	(0.089)	(0.109)	(0.088)
Progress out of Poverty Index	0.006	0.004**	-0.000	0.001
	(0.005)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.011	-0.011*	0.000	-0.000
	(0.013)	(0.006)	(0.006)	(0.005)
Woman's age in years	0.028***	-0.004	0.012***	0.013***
	(0.007)	(0.002)	(0.002)	(0.003)
Woman in paid labour	-0.020	0.120**	-0.125**	-0.010
	(0.132)	(0.046)	(0.051)	(0.072)
Forward Digit Span score	0.058**	0.029***	0.029*	-0.001
	(0.026)	(0.009)	(0.015)	(0.009)
Number of children 4-18	0.110***	0.033**	0.032**	0.029**
	(0.036)	(0.013)	(0.015)	(0.012)
Constant	-1.753***	0.294*	-0.090	0.227*
	(0.363)	(0.154)	(0.169)	(0.121)
Controls	HH and Woman	HH and Woman	HH and Woman	HH and Woman
Observations	720	720	720	720
R-squared	0.104	0.050	0.082	0.111

This table presents the results of regression of control of assets on treatment assignment controlling for household and woman level characteristics but no control for the baseline outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.11.Variation in compliance by hamlet

Hamlet	Compliance among treatment group (%)	Compliance among control group (%)
1	93	73
2	100	86
3	100	64
4	75	64
5	67	87
6	100	67
7	90	86
8	57	73
9	92	38
10	75	83
11	89	77
12	58	100
13	82	92
14	88	85
15	63	67
16	83	78
17	91	81
18	82	60
19	82	78
20	77	92
21	85	100
22	91	90
23	58	89
24	67	100
25	56	100
26	100	91
27	73	100
28	70	89

Table C.12.Correlates of programme take-up with hamlet fixed effects

	(1) Participation in treatment	(2) Participation in control
Nuclear household	-0.064	0.057
	(0.055)	(0.059)
Backward caste	0.338**	0.190**
	(0.164)	(0.096)
Scheduled caste	0.405**	0.276**
	(0.181)	(0.125)
Progress out of Poverty Index	0.004	0.001
	(0.003)	(0.003)
Number of adult HH members	-0.012*	0.002
	(0.006)	(0.006)
Woman's age in years	0.005*	0.001
	(0.003)	(0.003)
Woman in paid labour	-0.139	-0.036
	(0.113)	(0.103)
Forward Digit Span score	0.015	-0.016
	(0.016)	(0.015)
Number of children 4-18	0.033**	0.014
	(0.015)	(0.014)
Decision Index at baseline	0.017	0.024
	(0.028)	(0.027)
Mobility Index at baseline	0.013	-0.028
	(0.021)	(0.027)
Control of assets Index at baseline	-0.010	0.007
	(0.025)	(0.026)
Constant	0.083	-0.063
	(0.229)	(0.183)
Fixed effects	Hamlet	Hamlet
Observations	344	324
R-squared	0.184	0.140

This table presents the results of regression of program take up within treatment and control groups on observables and hamlet fixed effects. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. Base category for Backward Caste and Scheduled Caste is General category.

Table C.13.IV regression results on all empowerment outcomes by controls used

	Local Average Treatment Effect of TA+ programme		
Endline outcomes	No controls	Only baseline outcome as control	Only HH and woman controls
Decision Index	-0.018	-0.080	-0.039
	(0.126)	(0.126)	(0.106)
Decision Index excluding child decisions	-0.032	-0.125	-0.045
	(0.147)	(0.148)	(0.113)
Cooking	-0.029	-0.056	-0.039
	(0.071)	(0.068)	(0.054)
Purchases at local shop	-0.005	-0.044	-0.008
	(0.073)	(0.074)	(0.057)
Purchases outside village	-0.008	-0.049	-0.014
	(0.070)	(0.070)	(0.059)
Child's Illness	-0.009	-0.018	-0.012
	(0.068)	(0.068)	(0.062)
Child's enrolment	-0.019	-0.012	-0.029
	(0.061)	(0.059)	(0.061)
Child's attendance	0.028	0.017	0.009
	(0.042)	(0.042)	(0.039)
Mobility Index	0.220*	0.220*	0.200*
	(0.120)	(0.114)	(0.103)
Leave house without permission	0.003	0.009	0.002
	(0.033)	(0.035)	(0.035)
Go to local shop without permission	0.148***	0.139***	0.128***
	(0.052)	(0.051)	(0.042)
Go to shop outside the village without permission	0.045	0.050	0.041
	(0.042)	(0.041)	(0.042)
Visit health clinic without permission	0.058	0.068	0.054
	(0.050)	(0.050)	(0.040)
Visit natal family without permission	0.021	0.012	0.023
	(0.033)	(0.031)	(0.033)
Call natal family without permission	0.180***	0.176***	0.166***
	(0.058)	(0.057)	(0.055)
Go out for entertainment without permission	0.010	0.011	0.008
	(0.040)	(0.041)	(0.036)
Control of assets Index	0.195	0.180	0.161
	(0.124)	(0.124)	(0.121)
Owns mobile phone	0.031	0.029	0.020
	(0.063)	(0.055)	(0.060)
Own back account	0.151**	0.129*	0.136**
	(0.072)	(0.069)	(0.066)
Keeps own jewellery	0.007	0.010	0.002
	(0.038)	(0.039)	(0.035)

This table presents the results of IV regression of all outcome variables on self-reported participation. The first column reports the dependent variable. The second column reports the LATE of participation in an IV regression with no controls. The third column reports the LATE of participation in an IV regression with only baseline value of outcome variable as control. The last column reports the LATE of participation in a IV regression controlling for household and woman level characteristics but no control for the baseline outcome variable. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1. The number of observations is 668.

Table C.14.First stage results for Table 4.9 IV regression on decision-making power

Instrumenting for self-reported participation	IV regression dependent variable: Decision Index	Decision Index excl child	Cooking	Purchases at local shop	Purchases outside village	Child's Illness	Child's enrolment	Child's attendance
Treatment	0.600***	0.598***	0.599***	0.599***	0.600***	0.602***	0.602***	0.602***
	(0.029)	(0.029)	(0.029)	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)
Nuclear household	0.010	0.008	0.006	0.011	0.013	0.014	0.017	0.015
	(0.038)	(0.036)	(0.037)	(0.036)	(0.037)	(0.039)	(0.038)	(0.038)
Backward caste	0.243*	0.241*	0.243*	0.240*	0.240*	0.244*	0.239*	0.240*
	(0.138)	(0.137)	(0.137)	(0.138)	(0.137)	(0.136)	(0.138)	(0.140)
Scheduled caste	0.304**	0.302**	0.303**	0.304**	0.303**	0.308**	0.305**	0.303**
	(0.135)	(0.135)	(0.135)	(0.135)	(0.135)	(0.134)	(0.135)	(0.136)
Progress out of Poverty Index	0.003	0.002	0.002	0.003	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.005	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Woman's age in years	0.003	0.002	0.002	0.003	0.003	0.003	0.003*	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Woman in paid labour	-0.133*	-0.135*	-0.131*	-0.134*	-0.133*	-0.130*	-0.128*	-0.131*
	(0.068)	(0.068)	(0.068)	(0.068)	(0.068)	(0.068)	(0.069)	(0.069)
Forward Digit Span score	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Number of children 4-18	0.023**	0.024**	0.024**	0.025**	0.025**	0.024**	0.025***	0.022**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)
Outcome at baseline	0.022	0.026	0.064*	0.039	0.032	0.026	0.004	0.039
	(0.016)	(0.017)	(0.034)	(0.031)	(0.031)	(0.035)	(0.029)	(0.028)
Constant	-0.233	-0.224	-0.264	-0.257	-0.258	-0.271	-0.271	-0.263
	(0.204)	(0.205)	(0.204)	(0.202)	(0.203)	(0.202)	(0.202)	(0.204)
Observations	668	668	668	668	668	668	668	668
R-squared	0.395	0.395	0.396	0.395	0.394	0.394	0.393	0.395
F-stat	58.56	59.45	59.02	55.39	60.96	55.96	59.72	55.88

This table reports the results of first stage regression of self-reported participation on treatment assignment, household and woman characteristics and baseline value of outcome variable. This regression is used to estimate the second stage IV estimate of the effect of treatment on decision-making power among the compliers. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1.

Table C.15.First stage results for Table 4.10 IV regression on mobility

	IV regression dependent variable							
Instrumenting for self-reported participation	Mobility Index	Leave house	Go to local shop	Go to shop outside the village	Visit health clinic	Visit natal family	Call natal family	Go out for entertainment
Treatment	0.602***	0.602***	0.602***	0.602***	0.602***	0.602***	0.603***	0.602***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)	(0.030)
Nuclear household	0.020	0.018	0.017	0.019	0.018	0.018	0.018	0.018
	(0.037)	(0.038)	(0.037)	(0.037)	(0.038)	(0.038)	(0.037)	(0.038)
Backward caste	0.242*	0.240*	0.239*	0.239*	0.239*	0.239*	0.243*	0.241*
	(0.137)	(0.137)	(0.137)	(0.137)	(0.138)	(0.138)	(0.136)	(0.138)
Scheduled caste	0.307**	0.305**	0.304**	0.305**	0.305**	0.305**	0.306**	0.306**
	(0.134)	(0.135)	(0.134)	(0.135)	(0.135)	(0.135)	(0.131)	(0.135)
Progress out of Poverty Index	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Woman's age in years	0.004*	0.003*	0.003*	0.003*	0.003*	0.003*	0.003	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Woman in paid labour	-0.124*	-0.127*	-0.127*	-0.126*	-0.127*	-0.129*	-0.119	-0.126*
	(0.070)	(0.070)	(0.068)	(0.070)	(0.069)	(0.072)	(0.071)	(0.069)
Forward Digit Span score	-0.004	-0.004	-0.004	-0.004	-0.004	-0.004	-0.003	-0.004
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Number of children 4-18	0.026**	0.026**	0.026**	0.026**	0.026**	0.026**	0.027**	0.026**
	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)
Outcome at baseline	-0.009	-0.007	0.006	-0.010	-0.003	0.006	-0.065*	-0.013
	(0.014)	(0.049)	(0.034)	(0.033)	(0.036)	(0.051)	(0.033)	(0.038)
Constant	-0.280	-0.270	-0.270	-0.273	-0.272	-0.272	-0.234	-0.274
	(0.201)	(0.202)	(0.202)	(0.202)	(0.204)	(0.205)	(0.201)	(0.203)
Observations	668	668	668	668	668	668	668	668
R-squared	0.394	0.393	0.393	0.393	0.393	0.393	0.397	0.394
F-stat	60.62	62.43	53.54	55.89	53.64	53.99	64.68	56.65

This table reports the results of first stage regression of self-reported participation on treatment assignment, household and woman characteristics and baseline value of outcome variable. This regression is used to estimate the second stage IV estimate of the effect of treatment on mobility among the compliers. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1.

Table C.16.First stage results for Table 4.11 IV regression on control of assets

Instrumenting for self-reported participation	IV regression dependent variable Control of assets Index	Owns mobile phone	Own back account	Keeps own jewellery
Treatment	0.602***	0.602***	0.601***	0.602***
	(0.030)	(0.030)	(0.030)	(0.030)
Nuclear household	0.018	0.018	0.018	0.018
	(0.038)	(0.038)	(0.037)	(0.038)
Backward caste	0.239*	0.238*	0.236*	0.239*
	(0.138)	(0.137)	(0.137)	(0.138)
Scheduled caste	0.305**	0.303**	0.301**	0.305**
	(0.135)	(0.135)	(0.135)	(0.136)
Progress out of Poverty Index	0.003	0.003	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Number of adult HH members	-0.005	-0.005	-0.005	-0.005
	(0.005)	(0.005)	(0.005)	(0.005)
Woman's age in years	0.003*	0.003*	0.003	0.004*
	(0.002)	(0.002)	(0.002)	(0.002)
Woman in paid labour	-0.127*	-0.125*	-0.130*	-0.128*
	(0.069)	(0.068)	(0.069)	(0.070)
Forward Digit Span score	-0.004	-0.004	-0.004	-0.003
	(0.010)	(0.010)	(0.010)	(0.010)
Number of children 4-18	0.026**	0.026**	0.025**	0.026**
	(0.010)	(0.010)	(0.010)	(0.010)
Outcome at baseline	-0.001	-0.011	0.026	-0.018
	(0.016)	(0.031)	(0.035)	(0.042)
Constant	-0.272	-0.266	-0.258	-0.268
	(0.205)	(0.201)	(0.205)	(0.205)
Observations	668	668	668	668
R-squared	0.393	0.394	0.394	0.394
F-stat	54.48	55.20	54.66	53.34

This table reports the results of first stage regression of self-reported participation on treatment assignment, household and woman characteristics and baseline value of outcome variable. This regression is used to estimate the second stage IV estimate of the effect of treatment on control of assets among the compliers. Standard errors are clustered at hamlet level. ***p<0.01, ** p<0.05, * p<0.1.

Table C.17.LATE correcting for attrition using Lee(2009) bounds

Endline outcomes	Participation	Lower Bound	Upper Bound
Decision Index	-0.078	-0.106	-0.040
	(0.115)	(0.118)	(0.136)
Decision Index excluding child decisions	-0.101	-0.134	-0.077
	(0.125)	(0.134)	(0.132)
Cooking	-0.053	-0.068	-0.040
	(0.056)	(0.058)	(0.060)
Purchases at local shop	-0.030	-0.044	-0.016
	(0.061)	(0.063)	(0.067)
Purchases outside village	-0.039	-0.049	-0.021
	(0.063)	(0.067)	(0.071)
Child's Illness	-0.018	-0.032	-0.002
	(0.064)	(0.065)	(0.073)
Child's enrolment	-0.021	-0.033	-0.002
	(0.060)	(0.060)	(0.064)
Child's attendance	0.005	-0.009	0.022
	(0.040)	(0.042)	(0.046)
Mobility Index	0.210**	0.176*	0.299***
	(0.100)	(0.099)	(0.099)
Leave house without permission	0.008	0.004	0.037
	(0.036)	(0.036)	(0.032)
Go to local shop without permission	0.124***	0.112**	0.145***
	(0.043)	(0.044)	(0.044)
Go to shop outside the village without permission	0.048	0.044	0.077*
	(0.041)	(0.041)	(0.043)
Visit health clinic without permission	0.062	0.057	0.087**
	(0.041)	(0.041)	(0.040)
Visit natal family without permission	0.017	0.011	0.045
	(0.032)	(0.035)	(0.029)
Call natal family without permission	0.163***	0.140***	0.181***
	(0.055)	(0.053)	(0.061)
Go out for entertainment without permission	0.009	0.005	0.039
	(0.036)	(0.040)	(0.038)
Control of assets Index	0.161	0.089	0.199*
	(0.123)	(0.113)	(0.120)
Owns mobile phone	0.024	0.016	0.040
	(0.055)	(0.059)	(0.054)
Own back account	0.121*	0.104	0.135**
	(0.064)	(0.070)	(0.057)
Keeps own jewellery	0.004	-0.026	0.007
	(0.036)	(0.036)	(0.035)

This table presents the results of IV regression of endline empowerment outcomes on participation after trimming the sample using the method described in Lee(2009). Column (1) reports IV results from Section 4.6.3.All regressions control for individual and household level controls, as well as the value of the outcome variables at baseline. Standard errors for Columns (2) and (3) were bootstrapped with 250 repetitions and clustered at hamlet level. The number of observations for bounds regressions is 661, after trimming 7 observations. ***p<0.01, ** p<0.05, * p<0.1.