Tackling Household Decision Making Inefficiencies for Young Women’s Skills Investment in Pakistan

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Abstract

We study household decisions to invest in young women’s digital skills through a short-term online training program in urban Pakistan. We randomized the split of a fixed, cash incentive for program completion between daughters and parents. We informed daughters about the incentive allocation, and cross-randomized whether parents received information about daughters’ incentive. Under information asymmetry, assigning the entire incentive to parents increased program completion by 95 percent compared to assigning it to daughters. Once parents learnt about daughters’ incentive, the incentive allocation had no impact on program completion. We reject the unitary household model in this parent-daughter context and provide evidence on bargaining behavior supporting the efficient collective model.

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I. Introduction

Supplemental educational programs are often cited as a way to simultaneously address deficiencies in school curriculum and boost human capital outcomes for students (Attanasio, Kugler and Meghir (2011); Blattman and Ralston (2015); Groh et al. (2016); Premand et al. (2016); Chakravarty et al. (2019); Bandiera et al. (2020)). Such supplemental programs might be particularly useful in contexts where students are identified as deficient in particular skills and when schools are unable to directly provide such remedial curriculum.

Economics research suggests two methods of encouraging students to complete extra educational courses: (1) paying incentives for completion and (2) providing different types of information. The targeting of such encouragement has traditionally gone solely to the parents or child. This is because when children are young, parents are considered decision makers (Becker (1991); Haveman and Wolfe (1995); Banerjee (2004); Bast and Walberg (2004); Cunha and Heckman (2009); Dizongross (2019)), and as children grow older, the children themselves become the decision makers (Akerlof and Kranton (2002); Dauphin et al. (2011); Hastings, Neilson and Zimmerman (2013); Dinkelman and Martinez A. (2014); Hastings et al. (2016); Fricke, Grogger and Steinmayr (2018)).

However, there might be age-ranges in which education becomes a joint decision between parents and the dependent children. Then, intra-household decision making would have crucial implications for optimal targeting of incentivized educational programs. In particular, if parents and children act as one unit, policy makers can target either party and achieve the same outcome. However, if they have different objectives and behave collectively, the choice of which party to incentivize and who to receive the information can profoundly impact the outcome.

This paper aims to understand the role of intra-household decision making for the incentive design to improve young women’s digital skills investment through an experiment in Lahore, Pakistan. We partner with the Punjab Skills Development Fund (PSDF), Pakistan’s largest skills development fund, and attempt to boost completion of free, digital skills training programs among young women. 91 percent of young adults in Pakistan lack basic digital skills, such as copying and pasting text (World Skills Clock (2023)). In the state of Punjab, only 42 female youth possess digital skills for every 100 males (UNICEF (2023)). In an attempt to close this skills gap, the government of Pakistan helps fund supplemental digital skills training programs.\(^1\) Such training programs often come with a sizable cash reward upon completion, but training providers report low participation and completion rates among females.\(^2\) We recruited 489 households across 8 girls-only government high schools and colleges in Lahore, and offered seats in short-term Coursera programs. Girls in our sample had low computer literacy, struggling with basic tasks such as cutting and pasting text. Upon completing the training, girls received a globally recognizable Coursera certificate, and their household received 3,000 PKR, equivalent to 7.5 percent of monthly household income in our

\(^{1}\)In April 2023, the Pakistan Senate passed a resolution to equip women and girls with digital skills and to fund digital empowerment programs for women through the Punjab Skills Development Fund (PSDF) (Pakistan Today (2023)).

\(^{2}\)PSDF offers an incentive of 5,000-6,000 PKR upon program completion for its longer-term Coursera trainings. Females account for 20.2 percent among those signed up for the program and only 8.9 percent of them complete the course.
We study the role of intra-household decision making by fixing the total household incentive and randomly varying (1) the split of incentive paid to parents, ranging from 0, 1500, to 3000 and (2) information on the full incentive to parents. This 3-by-2 design generates 6 treatment arms and the control arm is when parents receive neither incentive nor information. We track differences across two main outcomes: parental permission to girls’ program participation, and program completion. We also measure daughters’ drop-out decisions separately from parents’ actions as girls are asked to complete the program in their schools computer laboratory during a free-period. Once parents give permission for their daughters to enroll, they cannot force their daughters out of the program.\footnote{See Section III for more discussion on separability of actions. We find no evidence of parents withdrawing consent at endline.} We use differences in household-level completion to test the unitary household model and differences in parental consent and daughters’ drop-out to evaluate the collective household model.

We find a completion rate of 15.94 percentage points in the control arm and without information symmetry, each extra 1,500 PKR paid to parents increases completion by 7.47 percentage points. Our results also reveal that when parents are paid the entire incentive under information asymmetry, program completion improves by 15.17 percentage points, which represents a 95.17 percent increase. This violates the income-pooling test under the unitary model, which suggests that the distribution of income does not matter for household decisions. Hence, we reject the unitary model.

Once we impose information symmetry on the total household incentive, we find no effect of incentive targeting to parents: differences in household completion become small in magnitude (between 0.21 and 1.20 percentage points). Once we decompose the actions, we find no differences in either parental consent or daughters’ drop-out. This is suggestive evidence that under a collective household model where parents and daughters bargain on a future incentive, payment targeting by itself is not a friction on cooperative negotiation between parents and daughters.

This project contributes to multiple strands of literature. First, we contribute to a strand of literature on empirical evaluations of the collective household (see Donni and Molina (2018) for a comprehensive summary of empirical studies on the unitary and collective household model from 1988 to 2018). The majority of the cited papers focus on (1) intra-household bargaining between husband and wife, and (2) evaluate how intra-household decision making impacts cash transfer policies, where observed decisions are made after some payment is received. In particular, our study relates to a small body of experimental work on parent and children’s decision making regarding human capital investment under the unitary framework (Baird, McIntosh and Özler (2011), Bursztyn and Coffman (2012), Berry (2015), Bergman (2021), de Walque and Valente (Forthcoming)). Our core contribution is to evaluate both the unitary and collective models by decomposing household-level outcomes into separate actions by parents and daughters via a randomized control trial on payment targeting and information provision. Our study is also one of the first to use the efficient collective model to analyze how intra-household decision making impacts incentive design, where household decisions are made before payments.\footnote{See Section V for a full discussion on differences in bargaining environments.}
We also contribute to a strand of literature on intra-household decision making that evaluates the impact of information asymmetry between household members. Ashraf (2009), Castilla and Walker (2013), Ashraf, Field and Lee (2014), Ashraf et al. (2020), and Lowe and McKelway (2022) evaluate impacts of information asymmetry between spouses when it comes to differences in financial decisions, fertility decisions, consumption, and female labor force participation. Bursztyn and Coffman (2012), Bergman (2021), and de Walque and Valente (Forthcoming) study the impact of information provision on children’s school attendance by reducing parents’ monitoring costs. Of the above papers that are able to evaluate household decision making under information symmetry, ours is the only study that does not find evidence against cooperative bargaining.

Finally, we add to the growing literature on barriers to female labor force participation in developing countries. A number of studies (Dean and Jayachandran (2019); Bursztyn, González and Yanagizawa-Drott (2020); Heath and Tan (2020); McKelway (2021); Lowe and McKelway (2022)) examine the household constraints for women’s labor supply decisions. Related to these studies, we aim to show information asymmetry within the household can cause under-investment in young women’s income-generating skills, and thus exacerbate women’s ability to prepare themselves for future employment.

The rest of this paper is organized as follows. Section II generates testable hypotheses under the unitary model of the household and a collective model with pareto-efficient outcomes. Sections III and IV describe our experiment and results, mapped to the testable hypotheses. Section V discusses the implications and caveats of our results on household decision making between parents and children, and Section VI concludes.

II. Theoretical Framework

Suppose there is a skills training program that offers an incentive for successful completion. Daughters are targeted for the program, meaning they have full information about the incentive. Daughters cannot hide their decision to participate, however, they may choose to hide the incentive payment to fund private consumption. Parents must provide consent for their daughters to participate in the program and cannot easily monitor their child’s progress within the program.

Let $s$ be the total incentive in currency that is paid to one household. $s^p$ is the amount that is paid to the parent and $(s - s^p)$ is paid to the daughter. Daughters have full information on $s$ and $(s - s^p)$, while parents may not know about $s$ and therefore $(s - s^p)$.

Let $\theta \in [0, 1]$ be the proportion of daughters’ incentive $(s - s^p)$ that is revealed to the parents. The total incentive $s$ is fixed, but $s^p$ and $\theta$ may vary at the household level and represent differences in incentive payment and information targeting to parents.

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5In alternate settings, daughters may need to travel outside the house or use the shared home computer to attend the training sessions. Even though parental consent might be explicitly required by the training provider in these cases, daughters will still need to obtain parents’ approval for their participation.
A. Unitary household

The unitary model posits that households behave as a single individual with one payoff maximization problem. There is effectively one utility function $u^{hh}$ and one cost function $c^{hh}$\textsuperscript{6}. The household decision function is characterized as follows.

$$I_{\text{completes}}\{u^{hh} + s > c^{hh}\}$$

Program completion is efficient when benefits outweigh costs at the household level. Otherwise, parents and daughters will choose not to participate.

**Hypothesis 1:** Under the unitary model, completion does not vary with $s^p$ or $\theta$.

B. Collective household

In the collective household, parents and children have different preference and cost parameters $(u^p, c^p)$ and $(u^d, c^d)$ and they make decisions by solving separate payoff maximization problems. $u^p$ and $u^d$ are assumed to be stable, and embed all beliefs about the benefits of completing the training. $c^p$ and $c^d$ are also assumed to be stable, and embed all monetary, social, and effort costs of completing the training.

We use the methodology described in Browning and Chiappori (1998) to evaluate efficient (or cooperative) collective households which (1) makes no assumptions on the specific method of household bargaining so long as outcomes are pareto-optimal and (2) assumes that household members divide income to fund private consumption. Browning, Chiappori and Lechene (2006) clarifies the sequence of steps required to correctly analyze household behavior under the efficient collective framework. We proceed exactly as recommended.

First, we describe the one-period household decision where the incentive payment is made after the completion of a skills training program. Then, we characterize the separate payoff maximization problems for parents and children under pareto-optimal outcomes. The maximization problems will depend on parents and children’s ability to divide the incentive payment. We generate bounds on this sharing rule from parents’ and children’s problems which will make program completion incentive compatible for both parties. Finally, we analyze how the bounds change with differences in payment targeting and information symmetry to generate testable predictions of observable behavior.

\textsuperscript{6}There are two prominent theories that link individual preference parameters to household behavior consistent with the unitary model: Samuelson (1956) and Becker (1974, 1991). Samuelson (1956) explains the unitary model results when the household welfare function already incorporates each member’s preferences. Becker’s Rotten Kid Theorem (1974, 1991) finds the household may behave as a single unit when an altruistic ‘patriarch’ uses transfers to motivate the behavior of other members. The Rotten Kid Theorem hinges on a patriarch’s ability to monitor children’s behavior, which does not apply well in our setting.
B.1 Negotiation on a Future Payment

In order for households to receive \( s \), parents and daughters must both remain interested in the course. Parents cannot force their daughters to join or complete the training,\(^7\) and daughters cannot enroll in the training without permission from their parents. To reach an agreement, they must negotiate a sharing rule \((\lambda \in [0,1])\) to divide the payment. The actual value of \( \lambda \) is not the interest of this paper, as it will depend on factors like altruism or bargaining ability which are beyond the scope of this paper. Instead, we examine the feasible range for \( \lambda \) such that both parents and daughters remain interested in completing the course. The larger the feasible range of \( \lambda \), under pareto-optimality (and hence efficient or cooperative bargaining) the more likely households are to complete the program.

The feasible range of \( \lambda \) depends on the exogenous targeting of incentives and information. Below are two examples to illustrate feasible sets for two different households that contain sharing rules \( \lambda \) and \( \hat{\lambda} \).

\[
\begin{array}{c|c|c}
0 & \text{Feasible set for } \lambda & 1 \\
\hline
\lambda_{lb}(\theta, s^P) & \lambda_{ub}(\theta, s^P) \\
0 & \text{Feasible set for } \hat{\lambda} & 1 \\
\hline
\hat{\lambda}_{lb}(\hat{\theta}, \hat{s}^P) & \hat{\lambda}_{ub}(\hat{\theta}, \hat{s}^P)
\end{array}
\]

We characterize efficient household bargaining as the ability for parents and daughters to split the stipend so that benefits are greater than costs for both parties. If both parties can be satisfied, households complete the training and receive incentive \( s \). Otherwise, households do not complete the training and receive nothing. Outcomes are pareto optimal, because neither party can be made better off by investing in the training at the expense of the other.

B.2 Decision Processes

We assume the timeline of household bargaining is as follows.

\[
\begin{array}{c|c|c|c}
\text{Parent contacted} & \text{Parent consents} & \text{Completion} \\
\hline
\text{Girl receives offer} & \text{Bargaining } \lambda & \\
\end{array}
\]

At the beginning, girls receive their the skills training offer and have full information on \( s \) and \( s^P \). If they are still interested in participating, they can choose \( \theta \in [0,1] \) of their portion of the payment \((s - s^P)\) to reveal to parents and retain \((1 - \theta)(s - s^P)\) as the private return.

Next, parents are informed about their daughter’s interest in the program, and of \( s^P \) by the training provider. Parents also know about the proportion of the incentive that their daughters choose to

\(^7\)In the experimental setting, parents only learn about this program after daughters agree to participate. Since the program is held during school time, parents cannot force their daughters to attend the sessions. They can only nudge their daughters. As mentioned earlier, parents also cannot monitor progress very easily, as daughters are completing the trainings in school.
Hence, $\theta(s-s^p) + s^p$ is known with certainty by parents and this is the amount negotiated on.\(^8\)

Negotiation results in a choice of $\lambda \in [0,1]$, which is the proportion of the negotiated amount that is kept by parents. Post-negotiation, parents keep their share of the public return, $\lambda(\theta(s-s^p) + s^p)$, and daughters keep their share of the public return plus their private return, $(1-\lambda)(\theta(s-s^p) + s^p) + (1-\theta)(s-s^p)$.

Equations 1 and 2 are the two respective decision functions of the parents and daughters. We use these functions to generate predictions on observable actions between parents, daughters, and the household by deriving bounds on the sharing rule, $\lambda$.

\begin{equation}
I_p^{\text{consents}}\{u^p + \lambda(\theta(s-s^p) + s^p) > c^p\}
\end{equation}

\begin{equation}
I_d^{\text{completes}}\{u^d + (1-\theta)(s-s^p) + (1-\lambda)(\theta(s-s^p) + s^p) > c^d\}
\end{equation}

**Parents:** Rearrange Equation 1 to isolate $\lambda$ in the parents’ decision function.

\begin{equation}
I_p^{\text{consents}}\begin{cases}
\lambda > \frac{c^p - u^p}{\theta s + s^p (1-\theta)} & \text{if } \theta(s-s^p) + s^p > 0 \text{ Case 1} \\
u^p > c^p & \text{if } \theta s + s^p (1-\theta) = 0 \text{ Case 2}
\end{cases}
\end{equation}

We ignore Case 2, because payments do not enter the parents’ decision function and parents always give consent. Case 1 generates a lower bound on payment division which will make consent to daughters’ participation incentive compatible to parents.

\begin{equation}
\lambda > \frac{c^p - u^p}{\theta s + s^p (1-\theta)} = \lambda_{lb}
\end{equation}

**Hypothesis 2:** Under the efficient collective model, when parents have full information on the household incentive, parental consent does not vary with $s^p$.

**Proof:** Plug $\theta = 1$ into Equation 3. This result does not depend on $s^p$.

\[\lambda_{lb}|_{\theta=1} = \frac{c_m + c_s - u^p}{s}\]

**Daughters:** Rearrange Equation 2 to isolate $\lambda$ within the daughter’s decision function.

\begin{equation}
I_d^{\text{completes}}\begin{cases}
\lambda < \frac{u^d - c^d + s}{\theta (s-s^p) + s^p} & \text{if } \theta(s-s^p) + s^p > 0 \text{ Case 1} \\
u^d + s > c^d & \text{if } \theta(s-s^p) + s^p = 0 \text{ Case 2}
\end{cases}
\end{equation}

Again, we ignore Case 2, because the daughter’s decision does not depend on information or payment targeting and she will complete the program only if she captures the entire incentive. Hence, we derive an upper bound on payment division which will make program completion incentive compatible for the daughters.

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\(^8\)The parents portion of the incentive is negotiated on, because daughters in this context are not working outside of the household, so parents fund all of daughter’s consumption.
\[ \lambda < \frac{u^d - c^d + s}{\theta(s - s^p) + s^p} = \lambda_{ub} \]

**Hypothesis 3**: Under the efficient collective model, when parents have full information, the girls’ choice to complete or drop out does not vary with \( s^p \).

**Proof**: Plug \( \theta = 1 \) into Equation 4. This result does not depend on \( s^p \).

\[ \lambda_{ub|\theta=1} = \frac{u^d - c^d + s}{s} \]

**Remark 1**: Under the collective household framework, we do not observe the unconditional decision of the girls to drop out of the program. All daughter’s decisions are observed, conditional on parental consent. To test Hypothesis 3 on the data, we rely on the assumption that under information symmetry, Hypothesis 2 holds and parental consent does not vary with \( s^p \). Hence, once information symmetry is imposed, the girl’s drop-out decision conditional on parental consent is the same as the drop-out decision unconditional on parental consent.

**Households**: We combine Equations 3 and 4 to generate predictions on observed household completion.

\[ \lambda_{ub} - \lambda_{lb} = \frac{u^d + u^p + s - (c^d + c^p)}{\theta(s - s^p) + s^p} \]

**Hypothesis 4**: Under the efficient collective household, when parents have full information, completion does not vary with \( s^p \).

**Proof**: Plug \( \theta = 1 \) into Equation 5. This result does not depend on \( s^p \).

\[ \lambda_{ub} - \lambda_{lb|\theta=1} = \frac{u^d + u^p + s - (c^d + c^p)}{s} \]

**Remark 2**: Hypothesis 4 comes directly from Hypotheses 2 and 3. We report this because it allows us to confirm that our household-level results are robust to misspecification of the bargaining timeline.\(^9\)

**Remark 3**: The numerator in Equation 5, \( u^d + u^p + s - (c^d + c^p) \), can be interpreted as household preference parameters within a unitary household decision, assuming the household preference parameters are aggregated from individual preferences via simple addition. Note that if this is the correct aggregation method, then under full information, we are not able to distinguish the unitary household from the collective household with cooperative bargaining because in both models, households will obtain the pareto-optimal outcome. Hence, if we are unable to reject cooperative bargaining in the full information setting, we must rely on differences in payment targeting under...
information asymmetry to properly evaluate the unitary model.\textsuperscript{10} In particular, we test linearity in treatment effects of payment targeting and the income pooling property under the unitary model.

Table 1 summarize all testable hypotheses generated by the two models of the household. We can reject one framework if any of its hypotheses are rejected with a reasonable statistical test. See Appendix Table C2 for the list of every reasonable statistical test.

Table 1: Testable Hypotheses on $s^p$ and $\theta$

<table>
<thead>
<tr>
<th>Hypothesis</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>A. Unitary household</strong></td>
<td></td>
</tr>
<tr>
<td>H1:</td>
<td>Household completion does not vary with $s^p$ or $\theta$.</td>
</tr>
<tr>
<td><strong>B. Efficient collective household</strong></td>
<td></td>
</tr>
<tr>
<td>H2:</td>
<td>Under full information, parental consent does not vary with $s^p$.</td>
</tr>
<tr>
<td>H3:</td>
<td>Under full information, the girl’s decision to complete or drop-out does not vary with $s^p$.</td>
</tr>
<tr>
<td>H4:</td>
<td>Under full information, household completion does not vary with $s^p$.</td>
</tr>
</tbody>
</table>

III. Experimental Design

Through PSDF, we offered the first supplemental digital skills training programs within 8 women’s government schools in Lahore. Given the low digital literacy in our sample, we selected 6 beginner-level, Coursera courses for students to choose from.\textsuperscript{11} PSDF usually provides an incentive of 5,000-6,000 PKR to trainees conditional on completing a standard, four to six-month online training program. To encourage participation in our beginner-level, 2-month program, we offered 3,000 PKR to households upon course completion.

A. Description of the Experiment

We map our experiment directly to the model in Section II by exogenously varying the split of the incentive to parents, $s^p$, and information on the full incentive amount to parents, $\theta$. We select $s^p \in \{0, 1500, 3000\} = \{0\%, 50\%, 100\%\}$ and cross-randomize full information on the total household incentive to parents, allowing $\theta \in \{\text{Partial, Full}\}$. This results in 6 contract groups. Figure 1 summarizes the six contract groups, along with the information given to parents.

All daughters have full information on the total household incentive and the incentive split. To ensure full information transparency on girls’ side when they made the decision to join the program, we also told the girls exactly what information their parents would receive.\textsuperscript{12} We refer to the group

\textsuperscript{10}See Samuelson (1956), Becker (1974, 1991) or footnote 6 for more examples of situations in which a collective household may behave in ways that are indistinguishable from the unitary household.

\textsuperscript{11}The courses are: (1) Better Business Writing in English, (2) Fundamentals of Graphic Design, (3) Foundations of Digital Marketing & E-commerce, (4) Intuit Bookkeeping, (5) Work Smarter with Microsoft Excel, and (6) How to Create a Website. These courses differ from the Coursera programs offered by PSDF. Our selected courses take on average a total of 15 hours over 4 weeks to complete and are designed to help students acquire basic digital skills for more advanced courses. The typical Coursera programs at PSDF last 4-6 months and are intended to make trainees job-ready after finishing the course.

\textsuperscript{12}For example, girls assigned to \{50\%, Partial\} were told: “You will receive 1,500 rupees in cash, and your parents/legal guardian will receive 1,500 rupees via mobile payment at the end of the program. Your parents/legal guardian will not be informed of your share of the payment.” The girls assigned to \{50\%, Full\} were told: “You will receive
Figure 1: Targeting Incentive and Information to Parents

<table>
<thead>
<tr>
<th>Increasing Parental Information</th>
<th>0% reward to parents</th>
<th>50% reward to parents</th>
<th>100% reward to parents</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full info</td>
<td>Program Info + 0 PKR to Parents + 3000 PKR to Daughter</td>
<td>Program Info + 1500 PKR to Parents</td>
<td>Program Info + 3000 PKR to Parents</td>
</tr>
<tr>
<td>Partial info</td>
<td>Program Info + 1500 PKR to Parents</td>
<td>Program Info + 3000 PKR to Parents</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Incentive and information targeting to parents by treatment arms. Daughters always have complete information about the program, the incentive, the allocation rule, and the information shared by the research team with their parents.

where parents are not paid and not given full information on the household incentive as the control group ($\{s^p, \theta\} = \{0, \text{Partial}\}$). This is to mimic the status-quo in terms of program targeting, where girls are targeted with the program information and the incentive for task-completion, and girls have total control over information sharing on the entire incentive $s$.

B. Experiment Timeline

Figure 2 summarizes the timeline of the experiment and documents how many students and parents participated in each part of the research.

In December 2022, we held information sessions with PSDF at the selected high schools and colleges. To impose some effort on the girls to sign up, we asked attendees who were interested in enrolling in the program to complete a paper form, where they were quizzed on the information session and had to write about why they wanted to participate in the trainings. 1,442 students completed the form following our information sessions.

Given the program capacity of 300 seats, we screened candidates based on their responses to the attention check and open-ended questions. 1,792 students passed the screening and were invited to the baseline survey. These students were randomized into contract groups at the household level. 1,500 rupees in cash, and your parents/legal guardian will receive 1,500 rupees via mobile payment at the end of the program. We will inform your parents/legal guardian that you were selected to receive 1,500 rupees.”

We told students to fill in the sign-up form as completely as possible and that their response would be used for screening purposes. The full screening criteria includes: having complete personal identifiable information (e.g. full name, student number), personal and parent’s contact information, consistent program choices, correct answers to all attention check questions, and answered at least one open-ended question about motivation for joining the program.

The randomization was stratified on school ID and a dummy variable for computer use above average. These information were collected through the sign-up form. If there were siblings who signed up for the program, only one sibling was randomly selected to complete baseline.
The baseline survey took place in schools and had two components. The first part was the questionnaire and the second part was where students were notified of their contract group. Enumerators informed them that the training provider would need to contact their parents for permission to participate in the training. To ensure that daughters believed the research team would actually split the total household incentive between girls and parents, we informed girls that upon program completion they would receive their portion, $s - s_p$, in cash and that parents would receive $s_p$ via mobile money transfer. Of the 489 girls who completed the baseline survey, only one girl reported that she was no longer interested in the program upon hearing her contract offer.

Following the baseline survey, we attempted to phone the parents of 488 girls who were still interested in the program. We asked parents to answer a few survey questions, and then provided the relevant information on the course and the incentive payment should their daughter complete the training program.\textsuperscript{15} Parents were informed that they would need to provide permission for their daughters to participate in the trainings, because they were being held by an external organization that was entering government schools and providing students with payment and certificates for completion. Parents were told that they would receive a text message to the same phone number, and that this text would contain the same information on the payments for completion that they were given over the phone. Because our program was oversubscribed, we told parents they would have 2 days to text us their permission, otherwise this offer would expire and their daughters would either be placed on a wait-list or not allowed to participate.\textsuperscript{16}

\textsuperscript{15} For example, parents from households assigned to \{50\%, Partial\} were told: “We used a lottery to determine the reward for [student’s name]. Congratulations, you as the legal guardian will receive 1,500 rupees via mobile payment if [student’s name] is selected and successfully completes this free program. The reward will be disbursed after the program finishes.” Parents in households assigned to \{50\%, Full\} were told: “We used a lottery to determine the reward for [student’s name]. Congratulations, your household has won 3,000 rupees which we will give 1,500 rupees to [student’s name] in cash and send the remaining 1,500 rupees to you via mobile payment if [student’s name] is selected and completes the program successfully. The reward will be disbursed after the program finishes.” If parents had any questions about the course or about PSDF, they were directed to a hot-line that was set up by PSDF.

\textsuperscript{16} The text message to parents is in Urdu and reads as follows: “[student’s name] has been given priority for a spot
us their consent in this way for two reasons. First, we needed to give some time for parents and daughters to negotiate. Second, we were worried that requesting consent over the phone would lead to experimenter demand bias. We successfully surveyed and texted 416 parents. Of the parents surveyed, 313 texted us back within 2 days granting permission to their daughters to participate in the trainings.

Students who received permission to participate were then contacted during school to enroll into the PSDF Coursera platform. They were requested to complete the digital skills training courses in the computer lab of their school, during the government mandated daily free-period. Once enrolled in the program, students needed to watch the online tutorials and receive satisfactory marks on all of the required assignments. The girls were informed that they would have 6 weeks to complete the course, and PSDF tracked student progress through the Coursera platform. Girls who successfully completed the trainings were invited to a certificate ceremony hosted by PSDF, where incentives for completion were dispersed to daughters via cash and sent to parents via mobile money. 119 girls completed the course.

After PSDF closed the Coursera platform for all students, we invited the 489 girls who filled out the baseline survey to complete the endline survey, which was designed to help us understand why girls dropped out of the program and to measure digital skills through a self-assessment test and a short quiz. 291 students completed the endline in person or via phone.

C. Sample Characteristics and Main Outcomes

Table 2 presents baseline characteristics for all 489 households and shows that the characteristics are balanced across treatment arms. Our full sample has an average household size of 6 people with a monthly household income of 41,620 PKR, which is about the average in urban Lahore in 2020. 97.8 percent of girls report interest in participating in the labor force after graduation. 68.9 percent of the girls report having a computer at home, but only 10.8 percent have their own smartphone.

Our main outcomes of interest are parental consent and program completion, which were collected from the text messages and the PSDF Coursera platform. We use the fact that the girls complete the trainings in school to decompose the household completion outcome into actions by the parents and daughters. Once a parent provides consent, we attribute any decision to drop-out of the program as the choice of the daughter. Appendix Figure A2 documents the reasons why girls who were given consent dropped out of the program. Not a single student reported being discouraged

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17We conducted a back-check survey on 65% of households who completed the parents survey but did not text us back. We find evidence that parents who did not text us back seemed to not be interested. See Appendix B2 for further discussion.

18Anecdotal evidence suggests that students usually take the free-period for leisure. In the baseline survey, most students also anticipated that the program would take up their leisure time.

19According to Pakistan Social and Living Standards Measurement Survey 2019-2020, the average monthly household income in urban Lahore is 45,900 thousand PKR. However, Pakistan has experienced high inflation rates from 9.7 percent in 2020 to 19.9 percent in 2022 (World Bank (2022)), so we expect the average monthly household income in 2022 to be higher than the 2020 statistics.
### Table 2: Summary Statistics and Balance Check

<table>
<thead>
<tr>
<th></th>
<th>Full Sample</th>
<th>Partial Info</th>
<th>Full Info</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0%</td>
<td>50%</td>
<td>100%</td>
<td>0%</td>
<td>50%</td>
<td>100%</td>
<td>P-Value</td>
</tr>
<tr>
<td>N</td>
<td>489</td>
<td>69</td>
<td>85</td>
<td>90</td>
<td>82</td>
<td>69</td>
<td>94</td>
</tr>
</tbody>
</table>

#### Panel A: Personal Characteristics

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Full</th>
<th>Partial</th>
<th>Full</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>High School Student</td>
<td>0.225</td>
<td>0.275</td>
<td>0.212</td>
<td>0.171</td>
<td>0.217</td>
<td>0.202</td>
<td>0.531</td>
</tr>
<tr>
<td>(0.418)</td>
<td>(0.450)</td>
<td>(0.411)</td>
<td>(0.450)</td>
<td>(0.379)</td>
<td>(0.415)</td>
<td>(0.404)</td>
<td></td>
</tr>
<tr>
<td>Plans to Work</td>
<td>0.978</td>
<td>0.971</td>
<td>0.988</td>
<td>0.976</td>
<td>0.971</td>
<td>0.979</td>
<td>0.965</td>
</tr>
<tr>
<td>(0.148)</td>
<td>(0.169)</td>
<td>(0.148)</td>
<td>(0.148)</td>
<td>(0.155)</td>
<td>(0.169)</td>
<td>(0.145)</td>
<td></td>
</tr>
<tr>
<td>Only Skilled Jobs Appropriate for Women</td>
<td>0.734</td>
<td>0.783</td>
<td>0.718</td>
<td>0.756</td>
<td>0.812</td>
<td>0.713</td>
<td>0.265</td>
</tr>
<tr>
<td>(0.442)</td>
<td>(0.415)</td>
<td>(0.453)</td>
<td>(0.478)</td>
<td>(0.432)</td>
<td>(0.394)</td>
<td>(0.455)</td>
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</tr>
<tr>
<td>Own a Personal Smart Phone</td>
<td>0.108</td>
<td>0.116</td>
<td>0.059</td>
<td>0.195</td>
<td>0.087</td>
<td>0.117</td>
<td>0.143</td>
</tr>
<tr>
<td>(0.311)</td>
<td>(0.323)</td>
<td>(0.237)</td>
<td>(0.269)</td>
<td>(0.399)</td>
<td>(0.284)</td>
<td>(0.323)</td>
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</table>

#### Panel B: Household Characteristics

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Full</th>
<th>Partial</th>
<th>Full</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Household Size (in Members)</td>
<td>6.427</td>
<td>6.304</td>
<td>6.600</td>
<td>6.244</td>
<td>6.565</td>
<td>6.309</td>
<td>0.679</td>
</tr>
<tr>
<td>(1.812)</td>
<td>(1.428)</td>
<td>(2.178)</td>
<td>(1.831)</td>
<td>(1.495)</td>
<td>(2.193)</td>
<td>(1.633)</td>
<td></td>
</tr>
<tr>
<td>Monthly Income (PKR in Thousands)</td>
<td>41.620</td>
<td>42.392</td>
<td>41.582</td>
<td>42.656</td>
<td>40.250</td>
<td>43.334</td>
<td>0.657</td>
</tr>
<tr>
<td>Father Works</td>
<td>0.896</td>
<td>0.797</td>
<td>0.906</td>
<td>0.939</td>
<td>0.841</td>
<td>0.904</td>
<td>0.111</td>
</tr>
<tr>
<td>(0.314)</td>
<td>(0.405)</td>
<td>(0.294)</td>
<td>(0.209)</td>
<td>(0.241)</td>
<td>(0.360)</td>
<td>(0.296)</td>
<td></td>
</tr>
<tr>
<td>Mother Works</td>
<td>0.135</td>
<td>0.188</td>
<td>0.106</td>
<td>0.159</td>
<td>0.130</td>
<td>0.117</td>
<td>0.742</td>
</tr>
<tr>
<td>(0.342)</td>
<td>(0.394)</td>
<td>(0.310)</td>
<td>(0.329)</td>
<td>(0.367)</td>
<td>(0.359)</td>
<td>(0.323)</td>
<td></td>
</tr>
<tr>
<td>Father with HS Degree or Above</td>
<td>0.681</td>
<td>0.754</td>
<td>0.671</td>
<td>0.646</td>
<td>0.652</td>
<td>0.734</td>
<td>0.439</td>
</tr>
<tr>
<td>(0.467)</td>
<td>(0.434)</td>
<td>(0.473)</td>
<td>(0.485)</td>
<td>(0.481)</td>
<td>(0.480)</td>
<td>(0.444)</td>
<td></td>
</tr>
<tr>
<td>Mother with HS Degree or Above</td>
<td>0.556</td>
<td>0.551</td>
<td>0.506</td>
<td>0.549</td>
<td>0.580</td>
<td>0.574</td>
<td>0.932</td>
</tr>
<tr>
<td>(0.497)</td>
<td>(0.501)</td>
<td>(0.503)</td>
<td>(0.497)</td>
<td>(0.501)</td>
<td>(0.497)</td>
<td>(0.497)</td>
<td></td>
</tr>
<tr>
<td>Has Computer at Home</td>
<td>0.689</td>
<td>0.594</td>
<td>0.765</td>
<td>0.683</td>
<td>0.681</td>
<td>0.670</td>
<td>0.324</td>
</tr>
<tr>
<td>(0.463)</td>
<td>(0.495)</td>
<td>(0.427)</td>
<td>(0.450)</td>
<td>(0.468)</td>
<td>(0.409)</td>
<td>(0.473)</td>
<td></td>
</tr>
</tbody>
</table>

**Notes:** This table reports baseline summary statistics and balance of personal and household characteristics, and p-values from a joint test of equality of means. Standard deviations reported in parentheses. Summary statistics on household monthly income are from the parent survey (sample size = 414). We replace missing values of household monthly income with the average household income at the respondent’s school.

by a parent or other member in the household.

To evaluate the unitary model of the household and the overall impacts of the payment and information targeting on completion, we use household-level completion outcomes on the full sample of 489 households that have a student baseline survey. To evaluate whether household bargaining is inefficient (or non-cooperative) under information symmetry, we restrict our sample to the 416 households that have a completed student baseline and parent survey. See Appendix B for a detailed discussion on why this sample restriction is necessary, and how we find no evidence of selective attrition into the parent survey.

### IV. Results

Our main specification for all outcomes is:

\[
y_i = \beta_0 + \beta_11_{\{50\%\, \text{Partial}\}}i + \beta_21_{\{100\%\, \text{Partial}\}}i + \beta_31_{\{0\%\, \text{Full}\}}i + \beta_41_{\{50\%\, \text{Full}\}}i + \beta_51_{\{100\%\, \text{Full}\}}i + \epsilon_i
\]
\( y_i \) is the outcome of interest for household \( i \), and the omitted group is \{0\%, Partial\}, which is the control group. We estimate robust standard errors throughout. All statistical tests of the unitary and collective household model map directly to the hypotheses presented in Table 1, and results presented in this section do not include covariates. See Appendix Table C2 for the full set of feasible regressions, along with a discussion of power and covariates. Appendix C shows that all regressions are robust to relevant sample restrictions and the inclusion of covariates.

A. Testing the Unitary Model: Effects on Program Completion

Panel A of Figure 3 shows that household completion rates across treatment groups vary within the full analysis sample.\(^{20}\) 15.94 percent of students completed the program in the control group. We find that treating parents with either payments or information about the payment pushed completion rates up by at least 7.25 percentage points and at most 15.17 percentage points. We highlight two takeaways from Panel A. (1) We are not powered to detect differences across all treatment arms with our sample size, as a joint test of equality \((\beta_1 = \beta_2 = \beta_3 = \beta_4 = \beta_5 = 0)\) yields a p-value of 0.35. The joint test of equality represents the purest test of the unitary household, tested against all alternate models of the household unconditional on bargaining environment. Remark 3 from Section II suggests that under certain scenarios, it may be impossible to distinguish the unitary household from an efficient collective one. In our context, this would occur under full information. (2) We see very little differences in completion rates for households where parents were given full information. (1) and (2) imply that to properly evaluate the unitary model, we need to restrict our analysis to the partial information treatment group. Panels B and C of Figure 3 report statistical tests of the unitary model that are restricted to the partial information group.

First, we use the following specification to test linearity in \( s^p \) within the partial information group:

\[
y_i = \gamma_0 + \gamma_1 s^p_i + \varepsilon_i
\]

Panel B of Figure 3 shows that under information asymmetry, an extra 1,500 PKR\(^{21}\) targeted to parents leads to a 7.47 percentage point increase in probability of completion.

Second, we conduct a classic test of income pooling, which is to compare household completion where 100\% of payment is targeted to either the student or the parents. From Specification 6, that simply means testing \( \beta_2 = 0 \). Panel C of Figure 3 shows that targeting 100\% of the payment to parents under information asymmetry leads to a 15.17 percentage point increase in probability of completing the skills program, a 95.17 percent increase.\(^{22}\)

---

\(^{20}\)We also report all household level results in Appendix Table C3 restricted to the sample where we were able to successfully complete the parents survey and deliver information on \( s^p \) and \( s \). The full sample and the parent survey sample represent intention to treat estimates and treatment on the treated estimates, respectively. For the sake of evaluating the unitary model of the household, estimates from the full sample of students who completed baseline is the most robust and policy relevant.

\(^{21}\)The most recent statistic estimates 1,500 PKR is about 34 USD in purchasing power (World Bank (2020)), however this figure has likely changed due to inflation in Pakistan that began in 2022.

\(^{22}\)Increases in household completion within the partial group jump by large magnitudes with extra payment to parents. While this may be noise due to small sample bias, we note that differences across completion within the full information are remarkably small (between 0.21 and 1.20 percentage points).
These two statistical tests on differences in household completion within under information asymmetry provide evidence against the unitary model of decision making between parents and teenage girls.

**Figure 3: Program Completion Rates (Full Sample)**

**Panel A. Joint Test of Equality**

<table>
<thead>
<tr>
<th>Partial Info</th>
<th>Full Info</th>
</tr>
</thead>
<tbody>
<tr>
<td>15.94</td>
<td>24.39</td>
</tr>
<tr>
<td>25.88</td>
<td>23.19</td>
</tr>
<tr>
<td>31.11</td>
<td>23.40</td>
</tr>
</tbody>
</table>

p-value = 0.35

**Panel B. Linearity Test (Partial Information)**

\[ \gamma_1 = 7.47 \text{pp} \]

p-value = 0.03

**Panel C. Income Pooling Test (Partial Information)**

\[ \beta_2 = 15.17 \text{pp} \]

p-value = 0.02

Notes: Panel A reports program completion rates across the treatments in the full analysis sample (N = 489), derived from a regression of program completion on treatment indicators with robust standard errors. The p-value from a joint test of null effects for all treatments is reported. 95% confidence intervals reported. Panel B restricts the analysis to the partial information group (N = 244). It reports results from a regression of program completion on payment amount with robust standard errors. The p-value from a two-sided test of null effects is reported. Panel C tests the income pooling hypothesis with the \{0\%, Partial\} and \{100\%, Partial\} groups (N = 159). The p-value from a two-sided test of null effects is reported as well as 95% confidence intervals.

**B. Testing the Efficient Collective Model: Effects on Parental Consent and Girl’s Drop-out**

We now evaluate inefficiencies on household bargaining by breaking down parental consent and girl’s drop-out decisions within a collective household model. Hypotheses 2, 3 and 4 from Table
state that under information symmetry on the full payment amount \((\theta = 1)\) parental consent, girl’s drop-out, and therefore household completion should not vary across payment targeting. We test these by regressing the relevant outcomes from Specification 6 and conducting a joint test of equality on payment targeting under full information \((\beta_3 = \beta_4 = \beta_5)\).

Panel A of Figure 4 shows that within the sample of households where we have both a parent and student survey, completion rates are remarkably similar under full information. Panels B and C break down household completion by evaluating parental consent and girl’s drop-out. P-values for joint tests of equality on payment targeting under full information are 0.89, 0.91, and 0.96 for H2, H3, and H4 of Table 1. Hence, we find the bargaining outcomes consistent with the efficient collective model.

V. Discussion of results

A. Implications for Incentive Design

We reject the unitary model of household decision making between parents and their teenage daughters. This means that should a policy-maker or program-designer want to incentivize completion of supplemental educational programs for students still living with their parents, they should consider how payment and information on such payments are targeted to both students and parents. We also find that once information symmetry on payment is imposed between parents and girls, all differences in household completion rate from payment targeting vanishes.

Our results have interesting implications for incentive design around supplemental educational programs for teenagers. Often times, policy-makers design incentives in a way that trade-off completion and externalities of the incentives themselves. These externalities include things like the costs of delivering payments or bargaining frictions. Our findings suggest that if information symmetry can be imposed, this trade-off may not exist. That is, policy-maker can simply design payment targeting in ways that focus on minimizing externalities of the payments while obtaining similar completion rates. Otherwise, our results show that under information asymmetry, household action is the highest when parents are paid the entire incentive. Appendix D1 has a more detailed discussion of the reduced form results on completion within the partial information group.

We highlight two discussion points around the full information group. First, we note that parents lack the ability to monitor the girls once they begin the training program. Several studies (Bursztyn and Coffman (2012), Bergman (2021), de Walque and Valente (Forthcoming)) find evidence that

23 Appendix Table C2 reports the sample size needed to detect differences at our current estimates per group (which are small), and we would need between 7,500 and 24,000 households in these three treatment arms to reject pareto-optimal decision making under full information. For a household level intervention on intra-household bargaining and incentive payments conditional on action, 7,500 households is a very large number. Most intra-household experiments have a sample size of a few hundreds and one of the largest experiments is Miller and A. Mushfiq (2011) with 2,228 households in Bangladesh. See Munro (2018) for a review of intra-household experiments.

24 The model in section II does not provide exact predictions for the partial information group because the effect of \(s^p\) on completion depends on unobserved utility and cost parameters. The way the girl’s drop-out variable is constructed is conditional on parental consent, meaning that drop-out rates within the partial information group is subject to selection bias. Thus, our results on the partial information group must be interpreted in a purely reduced form way.
**Figure 4:** Household Bargaining Outcomes

**Panel A. Program Completion (Parent Survey Sample)**

![Graph showing program completion for partial and full info](image)

<table>
<thead>
<tr>
<th>Partial Info</th>
<th>Full Info</th>
</tr>
</thead>
<tbody>
<tr>
<td>17.19</td>
<td>27.78</td>
</tr>
<tr>
<td>30.14</td>
<td>29.63</td>
</tr>
<tr>
<td>35.44</td>
<td>29.73</td>
</tr>
</tbody>
</table>

*p-value = 0.96*

**Panel B. Parental Consent (Parent Survey Sample)**

![Graph showing parental consent for partial and full info](image)

<table>
<thead>
<tr>
<th>Partial Info</th>
<th>Full Info</th>
</tr>
</thead>
<tbody>
<tr>
<td>68.75</td>
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<td>76.71</td>
<td>74.07</td>
</tr>
<tr>
<td>75.95</td>
<td>77.03</td>
</tr>
</tbody>
</table>

*p-value = 0.89*

**Panel C. Girls’ Drop-out (Parental Consent Sample)**

![Graph showing girls' drop-out for partial and full info](image)

<table>
<thead>
<tr>
<th>Partial Info</th>
<th>Full Info</th>
</tr>
</thead>
<tbody>
<tr>
<td>75.00</td>
<td>64.29</td>
</tr>
<tr>
<td>62.50</td>
<td>60.00</td>
</tr>
<tr>
<td>53.33</td>
<td>61.40</td>
</tr>
</tbody>
</table>

*p-value = 0.91*

**Notes:** Program completion, parental consent, and girls’ drop-out across the six treatments, derived from a regression of the outcome of interest on treatment indicators with robust standard errors. Panels A and B report completion and parental consent among households that answered the parent survey (N = 416), respectively. Panel C reports girls’ drop-out conditional on parental consent (N = 313). 95% confidence intervals reported. P-value from the joint test on equality of treatment effects under Full Info.

randomizing the ability for parents to monitor the actions of their children pushes the household towards non-cooperative bargaining. It is possible that had we introduced monitoring, completion would have increased with payment targeting to parents even under full information.

Second, we note that negotiation outcomes are conditional on the ability to split the incentive according to the sharing rule. Any frictions on the ability to split the stipend will impact completion outcomes through payment targeting, even under full information. If parents do not believe that daughters will comply with the sharing rule, targeting more money to parents should improve parental consent. If daughters do not believe that parents will comply with the sharing rule, then we should see more girls drop-out as we target more money to parents. We find no evidence of
frictions on the ability to split the payment under full information. \footnote{See Appendix D2 for a more detailed derivation.} If parents and daughters are negotiating under biased beliefs, such beliefs will update outside the scope of our experiment.

### B. Can We Generalize Our Results to Different Bargaining Settings?

Our research design is meant to inform incentive design under intra-household decision making when bargaining happens before a single, future payment that is made conditional on an observed action (ex-ante bargaining). While this setup generalizes to many other incentivized programs, we use this subsection to discuss main considerations for incentive design when abstracting away from this bargaining setting.

We start by considering the one-time payment that comes before an observed household action. This type of scenario is common when a policy-maker is trying to incentivize household spending using conditional cash transfers (CCT). Household members negotiate how to spend the money after receiving the CCT (ex-post bargaining). The key distinction between the ex-ante and the ex-post bargaining environments is the friction on ability to split the incentives. Frictions in the ex-ante bargaining case depend entirely on beliefs around the other party’s compliance to the agreed sharing rule. In the ex-post case, frictions may also arise in actual ability to transfer money between agents. This friction on transferring money between agents is often studied in the spouse context where agents have separate savings methods or bank accounts. In our parent-child context, we speculate this friction is less relevant than in the husband-wife context, as girls in our sample receive all money for private consumption from family.

We end by considering the multi-period bargaining context. This scenario is most analogous to working outside of the household (Lowe and McKelway (2022)), or longer incentivized educational programs where payments are made conditional on attendance every period (Baird, McIntosh and Özler (2011), Bursztyn and Coffman (2012), de Walque and Valente (Forthcoming)). Beliefs around the other party’s compliance to the sharing rule should reflect the true friction as agents update their priors by the start of every period. Hence, the actual ability to transfer incentives or income becomes highly relevant for outcomes such as labor supply and program take-up. Results in this multi-period scenario likely converge to the ex-post bargaining outcomes.

### C. Research Design and Program Evaluation

The main strength of our research design is that we are able to credibly decompose the actions of parents and daughters in a scenario where payment is conditional on a joint household action. This is difficult to accomplish, because typically, only household outcomes are observable. There are very few settings in which one-time parental consent is credible. If trainings were held outside of school hours or if they were held at home, parents might implicitly withdraw consent by refusing to drive daughters to the training or by restricting their daughter’s access to the computer at home. As mentioned in Section III, within our program design, after parents gave us their permission, it was very difficult for parents to monitor whether their daughters were participating in the training or not. To withdraw their permission, they would need to either contact the research team or pull their
daughter out of school, and Appendix Figure A2 shows that conditional on parental consent, not one girl reported parents discouraging them from finishing the training at school. It is possible to reject pareto-efficient outcomes with only household outcomes, but it is not possible to convincingly say there is no extra inefficiency in household bargaining induced by payment targeting under full information without being able to decompose the actions of parents and daughters.

Although our choice of digital trainings via Coursera during school free-periods allows us to evaluate the impacts of incentive targeting via intra-household decision making, we do not find compelling impacts of the program on students’ computer literacy. Appendix Figure A1 reveal minimal effects on digital skills measured by both a self-assessment test of confidence and a short quiz. Appendix Figure A2 shows the main reasons for why the girls dropped out of the program, conditional on parental consent. The majority of students dropped out due to reported technical difficulties with the online course and assignment submission, suggesting that their digital skills were too low for the basic digital trainings offered via Coursera.

The research team and PSDF both agree that respondents would need a longer, more advanced training to boost their labor market prospects. We believe that this sample of middle-class government school girls in Urban Pakistan is a relevant sample to skill because of their intention to work in white collar jobs and their deficiencies in digital skills. On the one hand, while 97.75 percent of girls in our sample planned to participate in the labor force, 73.42 percent reported in Table 2 that they felt only appropriate for women like them to work in skilled, white collar jobs. On the other hand, Appendix Table A1 shows that among girls who did not enroll in the program, 47.67 percent reported not feeling confident with cutting and pasting text. While the use of Coursera via school laptops or personal devices during free-period is a scalable solution to boost digital skills, we conclude from the program evaluation that offering self-guided Coursera courses is not the right method of boosting digital skills for this sample.26

VI. Conclusion

In many contexts, educational decisions for teenagers are made jointly with parents. Our study sheds light on how households make decisions regarding a short-term supplemental educational skills training and how policy makers might design incentive targeting to boost completion.

We reject the unitary model of household decision making between parents and young adult daughters in Pakistan and we document that inefficiencies in household negotiation on incentive payments come through information asymmetry and not through payment targeting. Our results suggest that when young women faced household opposition for investing in their income-generating skills, a policy-maker can boost take-up and completion by targeting parents with payments or information. If the policy-maker can impose information symmetry on payment, payment targeting does not impact completion. If the program designer cannot impose information symmetry, payments should be targeted towards parents to maximize household completion.

26We believe the girls need a program that is guided by an in-person teacher, because they seemed to lack the most basic digital skills to complete the beginner-level Coursera courses.
We think that studying household bargaining between parents and young adults and identifying other frictions to decision making, especially in the context of longer-term human capital investments and labor force participation, is a promising area for future research. Future work might also explore how family dynamics affect bargaining between different household members (e.g., parents and sons, the role of extended family members). Finally, identifying a scalable solution that can successfully boost basic digital literacy skills for middle-income teenage girls in Pakistan is an important area of research, as these girls are some of the least likely to participate in the labor force due to social stigma around unskilled work coupled with basic skills gaps (Jayachandran (2021)).
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Appendix A. Endline Results

Appendix Figure A1: Endline Computer Scores

Panel A. Self-Assessment Test

Panel B. Quiz

Notes: This figure shows students’ computer skills measured at the endline. We conducted a self-assessment test for 171 students who participated in the in-person endline. Panel A reports average scores from the self-assessment test by program status. Not enrolled refers to students from the baseline sample who did not have the parental consent and those who had the consent but did not enroll (N = 86). Enrolled, not completed refers to students who started the course but did not complete after 6 weeks (N = 26). Completed refers to students who completed the course in time (N = 59). Panel B reports average scores from a short quiz conducted for all 291 students who participated in the endline in person or over phone. Among 291 students, 164 never enrolled in the program, 43 enrolled but did not complete, and 84 completed the program.

Appendix Figure A2: Reasons for Drop-out

Notes: This figure shows the top reasons for program drop-out given by 113 students during the endline survey. Technical issue means that students encountered some technical difficulties when enrolling in the online course or submitting their assignment on the Coursera webpage. Too busy means that students were pre-occupied with other responsibilities. No computer lab access means that students couldn’t use computers at school to attend the course. Forgot to enroll means that students forgot to attend the enrollment session or the training sessions at school. Cannot follow course means that students found the course difficult to follow.
### Appendix Table A1: Self-Reported (Non)Confidence in Computer Tasks

<table>
<thead>
<tr>
<th>Task Description</th>
<th>Not Enrolled</th>
<th>Enrolled, Not Completed</th>
<th>Enrolled, Completed</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Word Processing</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Cut and paste text within a document</td>
<td>47.67</td>
<td>30.77</td>
<td>22.03</td>
</tr>
<tr>
<td>Change font size, style, and color</td>
<td>46.51</td>
<td>34.62</td>
<td>22.03</td>
</tr>
<tr>
<td>Create bulleted or numbered lists</td>
<td>55.81</td>
<td>42.31</td>
<td>37.29</td>
</tr>
<tr>
<td>Create a hyperlink</td>
<td>83.72</td>
<td>76.92</td>
<td>69.49</td>
</tr>
<tr>
<td><strong>Excel Spreadsheets</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Create a new spreadsheet</td>
<td>59.30</td>
<td>53.85</td>
<td>42.37</td>
</tr>
<tr>
<td>Use math functions such as sum and mean</td>
<td>53.49</td>
<td>38.46</td>
<td>35.59</td>
</tr>
<tr>
<td>Create a graph and adjust the properties</td>
<td>60.47</td>
<td>65.38</td>
<td>52.54</td>
</tr>
<tr>
<td><strong>Email</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Open and print an email attachment</td>
<td>54.65</td>
<td>46.15</td>
<td>37.29</td>
</tr>
<tr>
<td>Use cc and bcc to manage email recipients</td>
<td>87.21</td>
<td>76.92</td>
<td>79.66</td>
</tr>
<tr>
<td>Use Calendar to assist in time management</td>
<td>55.81</td>
<td>50.00</td>
<td>50.85</td>
</tr>
<tr>
<td><strong>Internet</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Use a search engine like google</td>
<td>36.05</td>
<td>30.77</td>
<td>28.81</td>
</tr>
<tr>
<td>Create a blog</td>
<td>76.74</td>
<td>61.54</td>
<td>67.80</td>
</tr>
<tr>
<td>Create a website</td>
<td>82.56</td>
<td>76.92</td>
<td>81.36</td>
</tr>
<tr>
<td>Insert an audio file, image, video, and podcast onto a website</td>
<td>69.77</td>
<td>53.85</td>
<td>49.15</td>
</tr>
<tr>
<td><strong>File Management</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Save a file and locate that file</td>
<td>55.81</td>
<td>38.46</td>
<td>32.20</td>
</tr>
<tr>
<td>Search and find a missing file</td>
<td>52.33</td>
<td>34.62</td>
<td>35.59</td>
</tr>
<tr>
<td>Create new folders</td>
<td>38.37</td>
<td>30.77</td>
<td>20.34</td>
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<tr>
<td><strong>Media Files</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Create and edit an audio recording</td>
<td>36.05</td>
<td>38.46</td>
<td>32.20</td>
</tr>
<tr>
<td>Create and edit a video recording</td>
<td>37.21</td>
<td>38.46</td>
<td>28.81</td>
</tr>
<tr>
<td>Upload and download a video from a website</td>
<td>29.07</td>
<td>46.15</td>
<td>20.34</td>
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<tr>
<td>Observations</td>
<td>86</td>
<td>26</td>
<td>59</td>
</tr>
</tbody>
</table>

**Notes:** This table reports people self-reported non-confidence in completing various computer tasks on their own. The self-reported measures were collected through the in-person endline survey of 171 students. The original measure was on a scale of 1 to 5. 1 means I have never done this before, 2 I could do this with help, 3 I can do this alone, but might make some mistakes, 4 I can do this alone with confidence, and 5 I can teach others to do this task. A dummy variable was generated which would take the value of 1 if the original scale was less than 4 and 0 if the scale was greater than 3.
Appendix B. Non-reponses from parents

B1. Attrition on the parents survey

Our main results suggest that the household operates under a collective framework where no inefficiencies arise due to payment targeting under full information. Evaluation of the inefficiency was dependent on our outcomes measuring differences in parental consent and daughters dropping out across contract groups. It is not sufficient to comment on a lack of inefficiency without clean-null on results on the actions of both the parents and daughters (Hypotheses 2 and 3 of Table 1). We are only able to request parental consent from parents who agree to participate in the research, and we are only able to observe daughters dropping out after we receive a permission text from parents. For the research team to measure both of these outcomes, parents need to complete the parents survey. Hence, our results on efficiency may be sensitive to selective attrition on the parents survey. This selective attrition might occur if bargaining and decision of $\lambda$ occurs before the parents are contacted.

There is a reasonable interpretation of household completion under the full sample of girls who filled out baseline. Any household whose parents were not successfully contacted did not complete the training, and therefore were not paid. Therefore, we can easily report hypothesis tests on household completion (Hypotheses 1 and 4 from Table 1), robust to attrition on the parents survey. Differences across groups in this full sample are interpreted as intent-to-treat estimates.

It is also possible to interpret all parents who did not complete the parents survey as refusing consent. However, it is much more difficult to interpret the daughters drop-out decision under attrition. Girls cannot drop out if their parents have never been texted. Therefore, instead of trying to argue that Hypotheses 2 and 3 from Table 1 are robust to attrition within the main results, we proceed by showing that there is little evidence of selective attrition on the parents survey.

Argument 1: We find very little attrition as a whole, meaning any differences across group may be subject to finite sample bias.

First, we have high completion rates on the parents survey. Figure 2 shows that 416 parents completed the parent survey, of the 488 girls who completed baseline and were still interested in the training after hearing about their contract. That is a completion rate of 85% on an un-incentivized phone survey.

We should be extremely hesitant in interpreting differential attrition by contract group due to small sample bias as there are only 72 households who did not complete the parents survey and there are 6 contract groups. Appendix Table C1 shows that of the number of households who did not complete the parents survey per group ranges from 4 to 20. Only 20 households have parents answer the phone and explicitly refuse the survey. Refusals by contract group range from 2 to 7 households.

Argument 2: Despite the small sample, we find no interpretable patterns in the data suggesting selective attrition.
We do not have parent surveys on 52 households because (1) phone numbers were disconnected or incorrect or (2) no one answered the phone after 5 attempts. While it is possible that girls gave us incorrect phone numbers, we find it unlikely, as it is common within Pakistan to use prepaid SIM cards which expire, meaning that parent phone numbers are subject to change.

We may be worried that households who did not answer the phone after 5 attempts effectively refused the survey. This would be the case if refusal was correlated with number of attempts. While we may have small-sample bias within refusals, Appendix Figure B1 shows no clear pattern between refusals and number of call attempts.

Appendix Table B1: Reason for non-response on parents survey

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<th>Full information</th>
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<tr>
<td></td>
<td>0%</td>
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<tr>
<td>Refused</td>
<td>(1)</td>
</tr>
<tr>
<td>No answer</td>
<td>(2)</td>
</tr>
<tr>
<td>Other</td>
<td>(3)</td>
</tr>
<tr>
<td>Total</td>
<td>(4)</td>
</tr>
</tbody>
</table>

Appendix Figure B1: No relationship between probability of refusal and number of attempts

B2. Non-response on permission texts

Our results on parental consent for daughters to participate in the skills training takes the entire set of parents who were texted. This is all 416 parents. We consider only responses of parents who texted back saying “Yes” as having given consent. We count parents who texted back saying “No” or parents who did not text us back within 2 days as having not given permission.
We feel strongly that parents who did not text us back were not interested in the program. First, all parents were texted on the same phone number where they willingly completed an un-incentivized phone survey with an enumerator. They were explicitly told to expect a text from us and given instructions on the requirements to respond within 2 days. Parents who did not receive a text could have tried calling back the enumerators to communicate with the research team that they did not receive a text.

Second, we randomly conducted a back-check survey of households who completed the parents survey and did not text us back. This was to confirm that our enumerators were actually completing the calls, and our messages were not being automatically filtered. Appendix Table Appendix B B2 shows that of the 67 households audited, only 1 parent did not recall receiving a call. Of the parents audited, 64% recall receiving our text message. Of the households who did not receive our text, we see no difference by contract group.

**Appendix Table B2:** Reason for non-response on parents survey

<table>
<thead>
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<th>Full information</th>
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<tbody>
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<td>0%</td>
<td>0%</td>
</tr>
<tr>
<td>50%</td>
<td>50%</td>
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<tr>
<td>100%</td>
<td>100%</td>
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</table>

<table>
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<th>(3)</th>
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<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Did not receive call</td>
<td>0</td>
<td>1</td>
<td>0</td>
<td>0</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>Did not receive text</td>
<td>4</td>
<td>4</td>
<td>3</td>
<td>4</td>
<td>3</td>
<td>6</td>
</tr>
</tbody>
</table>
Appendix C. Alternate specifications

C1. Covariates

Results in Section IV are generated from regressions that do not include covariates. Subsection C2 shows that our results are robust to including or excluding covariates, but we feel that showing the results by pure randomization is the most econometrically robust.

While we stratified on school and self-reported computer use before baseline, it was unclear before the experiment whether these variables were going to improve power on our main outcomes, which were parental consent and completion rates. The stratification was meant to improve power and decrease noise in detecting differences in improved computer skills, or for conducting heterogeneous treatment effects on digital literacy skills. Appendix Appendix A shows no differences in endline computer outcomes.

It is also ambiguous whether including covariates within the regressions should increase or decrease noise in our estimates due to the sample size. This is because before adding covariates, we already divide our main analysis samples (489, 416, 313 households) into 6 treatment groups. For illustrative purposes, Appendix Table C1 summarizes the number of households per treatment group by our three main samples for analyses. Panel C shows that cross cutting groups by only our strata results in very small groups. While we obviously are not interested in the coefficients on fully-interacted regressions, this exercise illustrates that adding stratum fixed effects generates variance weighted estimates by using variances and means from very small groups. It is unclear whether this would result in more noise on the main coefficients of interest, or less noise. Including other baseline covariates is a similar exercise in cross-cutting and re-aggregating our treatment groups by variance and mean.

Appendix Tables C3 and C4 show that adding in stratum fixed effects and baseline covariates have an ambiguous effect on the power of our main statistical tests of interest. Hence, while we find that our results are robust to covariates, we choose to report estimates that are generated from regressions without covariates.
**Appendix Table C1:** Number of households per group

<table>
<thead>
<tr>
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<th>Partial information</th>
<th>Full information</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0%</td>
<td>50%</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td><strong>A. Randomization sample</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Invited to baseline</td>
<td>132</td>
<td>132</td>
</tr>
<tr>
<td><strong>B. Analysis sample</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Has daughters baseline</td>
<td>69</td>
<td>85</td>
</tr>
<tr>
<td>Has daughter and parent survey</td>
<td>64</td>
<td>73</td>
</tr>
<tr>
<td>Parent gave consent</td>
<td>44</td>
<td>56</td>
</tr>
<tr>
<td><strong>C. Analysis sample, cross cut by 16 strata</strong></td>
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<td></td>
</tr>
<tr>
<td>Has daughters baseline</td>
<td>4.31</td>
<td>5.31</td>
</tr>
<tr>
<td>Has daughter and parent survey</td>
<td>4.00</td>
<td>4.56</td>
</tr>
<tr>
<td>Parent gave consent</td>
<td>2.75</td>
<td>3.50</td>
</tr>
</tbody>
</table>
C2. Results on all statistical tests

Appendix Table C2: All statistical tests, mapped to models of the household.

A. Unitary household

H1: Household completion is the same across \( \theta \) or \( s^p \).

Joint test across all 6 groups, which makes no assumptions on collective household model.

(1) \( y_h = \beta_0 + \beta_1\{50, \text{Partial}\}_h + \beta_2\{50, \text{Partial}\}_h + \beta_3\{0, \text{Full}\}_h + \beta_4\{50, \text{Full}\}_h + \beta_5\{100, \text{Full}\}_h + \varepsilon_h \)

\( H_0 : \beta_1 = \ldots = \beta_5 = 0 \) \hspace{1em} underpowered \hspace{1em} \( p \in [0.138, 0.258] \)

\( H_a : \) violation of one equality

Test of linearity on payment targeting under information asymmetry*.

(2) \( y_h = \gamma_0 + \gamma_1 s^p_h + \varepsilon_h \) if \( \{s^p, \text{Partial}\} = 1 \)

\( H_0 : \gamma_1 = 0 \) \hspace{1em} powered \hspace{1em} \( p \in [0.014, 0.032] \)

\( H_a : \gamma_1 \neq 0 \)

Classic test of income pooling under information asymmetry*.

(3) \( y_h = \delta_0 + \delta_1\{100, \text{Partial}\}_h + \varepsilon_h \) if \( \{0, \text{Partial}\} \) or \( \{100, \text{Partial}\} = 1 \)

\( H_0 : \delta_1 = 0 \) \hspace{1em} powered \hspace{1em} \( p \in [0.012, 0.032] \)

\( H_a : \delta_1 \neq 0 \)

Test of information symmetry**

(4) \( y_h = \eta_0 + \eta_1\{\text{Pooled, Full}\}_h + \varepsilon_h \) if \( \{0, \text{Partial}\} \) or \( \{s^p, \text{Full}\} = 1 \)

\( H_0 : \eta_1 = 0 \) \hspace{1em} marginal \hspace{1em} \( p \in [0.038, 0.157] \)

\( H_a : \eta_1 \neq 0 \)

B. Efficient collective household

H2: Under full information, parental consent does not vary with \( s^p \).

(5) \( y_p = \beta_0 + \beta_1\{50, \text{Partial}\}_h + \beta_2\{50, \text{Partial}\}_h + \beta_3\{0, \text{Full}\}_h + \beta_4\{50, \text{Full}\}_h + \beta_5\{100, \text{Full}\}_h + \varepsilon_p \)

\( H_0 : \beta_3 = \beta_4 = \beta_5 \) \hspace{1em} underpowered \hspace{1em} \( p \in [0.886, 0.889] \)

\( H_a : \) violation of one equality

Needs 7,520 in \( \{s^p, \text{Full}\} \)

H3: Under full information, the girl’s decision to drop-out does not vary with \( s^p \).

(6) \( y_d = \beta_0 + \beta_1\{50, \text{Partial}\}_h + \beta_2\{50, \text{Partial}\}_h + \beta_3\{0, \text{Full}\}_h + \beta_4\{50, \text{Full}\}_h + \beta_5\{100, \text{Full}\}_h + \varepsilon_d \)

\( H_0 : \beta_3 = \beta_4 = \beta_5 \) \hspace{1em} underpowered \hspace{1em} \( p \in [0.763, 0.903] \)

\( H_a : \) violation of one equality

Needs 8,000 in \( \{s^p, \text{Full}\} \)

H4: Under full information, household completion does not vary with \( s^p \).

(7) \( y_h = \beta_0 + \beta_1\{50, \text{Partial}\}_h + \beta_2\{50, \text{Partial}\}_h + \beta_3\{0, \text{Full}\}_h + \beta_4\{50, \text{Full}\}_h + \beta_5\{100, \text{Full}\}_h + \varepsilon_h \)

\( H_0 : \beta_3 = \beta_4 = \beta_5 \) \hspace{1em} underpowered \hspace{1em} \( p \in [0.763, 0.903] \)

\( H_a : \) violation of one equality

Needs 24,960 in \( \{s^p, \text{Full}\} \)

Notes: *See Section II, Remark 3 for discussion around the efficient collective household model and why we must evaluate the unitary model under information asymmetry. ** See Section II Subsection B for discussion of why under the efficient collective household, we want to pool all treatment groups with full information.
Appendix Figure C1: Household completion, robust to sample restriction and covariates. Regressions (1), (2) and (7) of Appendix Table C2

Panel A. No covariates (Full Sample)

Panel B. No covariates (Parent Survey Sample)

Panel C. Has covariates (Full Sample)

Panel D. Has covariates (Parent Survey Sample)

Notes:
Appendix Figure C2: Household completion under the standard test of income pooling, robust to sample restriction and covariates. Regression (3) of Appendix Table C2.

**Panel A.** No covariates (*Full Sample*)

- δ₁ = 15.2pp (p = 0.023)

**Panel B.** No covariates (*Parent Survey Sample*)

- δ₁ = 18.3pp (p = 0.012)

**Panel C.** Has covariates (*Full Sample*)

- δ₁ = 14.2pp (p = 0.032)

**Panel D.** Has covariates (*Parent Survey Sample*)

- δ₁ = 15.9pp (p = 0.026)

*Notes:*
Appendix Figure C3: Household completion comparing control to groups with full information. Regression (4) of Appendix Table C2.

Panel A. No covariates (Full Sample)

Panel B. No covariates (Parent Survey Sample)

Panel C. Has covariates (Full Sample)

Panel D. Has covariates (Parent Survey Sample)

Notes:
Appendix Figure C4: Parental consent and girl’s drop-out, robust to covariates. Regression (5) and (6) of Appendix Table C2.

Panel A. No covariates (Parent Survey Sample)  
Panel B. Has covariates (Parent Survey Sample)  
Panel C. No covariates (Parental Consent Sample)  
Panel D. Has covariates (Parental Consent Sample)

Notes:
# Appendix Table C3: Unitary household tests, robust to covariates

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<tr>
<td></td>
<td>Full Sample</td>
<td>Parent Survey Sample</td>
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</tr>
<tr>
<td></td>
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### A. Linearity in the Partial Group

**Extra 1,500 PKR to parents**

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<td>0.071**</td>
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### B. Full Regression

**50%, Partial (β₁)**

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**100%, Partial (β₂)**

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<td>0.16**</td>
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<td>[0.072]</td>
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**0%, Full (β₃)**

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<tr>
<td></td>
<td>0.084</td>
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<td>0.10</td>
<td>0.11</td>
<td>0.098</td>
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**50%, Full (β₄)**

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<td>0.072</td>
<td>0.066</td>
<td>0.081</td>
<td>0.12</td>
<td>0.13*</td>
<td>0.13*</td>
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<tr>
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<td>[0.068]</td>
<td>[0.067]</td>
<td>[0.069]</td>
<td>[0.079]</td>
<td>[0.078]</td>
<td>[0.080]</td>
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</table>

**100%, Full (β₅)**

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<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
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<td>[0.064]</td>
<td>[0.067]</td>
<td>[0.072]</td>
<td>[0.074]</td>
<td>[0.076]</td>
</tr>
</tbody>
</table>

### Appendix Table C2 p-values:

(1) Joint test 0.35 0.37 0.26 0.20 0.28 0.22
(2) Linearity 0.03 0.03 0.03 0.01 0.01 0.01
(3) Income pooling 0.02 0.02 0.01 0.01 0.02 0.01
(4) Full information 0.14 0.14 0.14 0.04 0.04 0.04

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<tr>
<td>Control mean</td>
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<td>0.16</td>
<td>0.16</td>
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<tr>
<td>Households (N)</td>
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<td>489</td>
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<table>
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<tr>
<td>Strata FE</td>
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<tr>
<td>Baseline covariates</td>
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### Appendix Table C4: Efficient collective household, robust to covariates

<table>
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<tr>
<th>Sample:</th>
<th>Completed (=1)</th>
<th>Parental consent (=1)</th>
<th>Girl’s drop-out (=1)</th>
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</thead>
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<tr>
<td></td>
<td>Parent Survey Sample</td>
<td>Parental Consent Sample</td>
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</tr>
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<td>50%, Partial ($\beta_1$)</td>
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<td>0.10</td>
<td>0.12</td>
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<tr>
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<td>[0.072]</td>
<td>[0.073]</td>
<td>[0.077]</td>
</tr>
<tr>
<td>100%, Partial ($\beta_2$)</td>
<td>0.18**</td>
<td>0.16**</td>
<td>0.18**</td>
</tr>
<tr>
<td></td>
<td>[0.072]</td>
<td>[0.070]</td>
<td>[0.072]</td>
</tr>
<tr>
<td>0%, Full ($\beta_3$)</td>
<td>0.11</td>
<td>0.098</td>
<td>0.12*</td>
</tr>
<tr>
<td></td>
<td>[0.071]</td>
<td>[0.066]</td>
<td>[0.070]</td>
</tr>
<tr>
<td>50%, Full ($\beta_4$)</td>
<td>0.12</td>
<td>0.13*</td>
<td>0.13*</td>
</tr>
<tr>
<td></td>
<td>[0.079]</td>
<td>[0.078]</td>
<td>[0.080]</td>
</tr>
<tr>
<td>100%, Full ($\beta_5$)</td>
<td>0.13*</td>
<td>0.11</td>
<td>0.11</td>
</tr>
<tr>
<td></td>
<td>[0.072]</td>
<td>[0.074]</td>
<td>[0.076]</td>
</tr>
</tbody>
</table>

**Joint test p-value**

| $\beta_1 = \beta_4 = \beta_5$ | 0.96 | 0.92 | 0.96 | 0.89 | 0.89 | 0.82 | 0.91 | 0.76 | 0.88 |

**Strata FE**

$x$ $x$ $x$ $x$ $x$ $x$

**Baseline covariates**

$x$ $x$ $x$ $x$

**Control mean**

| 0.16 | 0.16 | 0.16 | 0.69 | 0.69 | 0.69 | 0.75 | 0.75 | 0.75 |

**Households (N)**

| 416 | 416 | 416 | 416 | 416 | 416 | 313 | 313 | 313 |
Appendix D. Further Discussion and Alternative Models

D1. Discussion of Reduced Form Results in Partial Information Group

Reduced form results in Figure 3 show that completion is increasing in $s^p$ within the partial information group. We were unable to generate predictions on the partial information group before data collection, because the partial derivative of Equation 5 with respect to $s^p$ depends on unobserved preference and cost parameters $\{u^d, c^d\}$ and $\{u^p, c^p\}$. Although we are not able to structurally estimate the parameters, we offer two possible explanations for these results.

\[
\frac{\partial \lambda_{ub} - \lambda_{lb}}{\partial s^p} = -\frac{(1 - \theta)(u^d + u^p + s - c^d - c^p)}{(\theta(s - s^p) + s^p)^2}
\]

Case 1: Corner Solution to Daughters’ Problem. When $u^d + u^p + s - c^d - c^p > 0$, Equation 8 suggests the feasible range for the sharing rule $\lambda$ is shrinking with $s^p$. However, there might exist a corner solution to daughters’ problem where $s^p$ is irrelevant for daughters’ decision to complete the training. Recall daughters’ problem from Section II Equation 2.

\[
I_{\text{completes}} = \left\{ u^d + (1 - \theta)(s - s^p) + (1 - \lambda)(\theta(s - s^p) + s^p) > c^d \right\}
\]

Since $(1 - \theta)(s - s^p)$ and $(1 - \lambda)(\theta(s - s^p) + s^p)$ are both non-negative, daughters are willing to complete the training, regardless of $s^p$, as long as $u^d > c^d$. Hence, $\lambda_{ub}$ is bounded at one and the feasible range for $\lambda$ is

\[
\lambda_{ub} - \lambda_{lb} = 1 - \frac{c^p - u^p}{\theta s + s^p(1 - \theta)}
\]

which is expanding in $s^p$ under partial information.

$\lambda_{ub} = 1$ also implies that girls’ decision to drop-out should not vary with $s^p$ in the partial information group. However, since girls’ drop-out was only measured conditional on parental consent, we cannot test this prediction in the data.

Case 2: Misperceived Benefits and Costs of Training. If $u^d + u^p + s - c^d - c^p < 0$, it is not optimal for the household to complete the training. Then why did we observe many households participating in this program?

Households might not know the true benefits and costs of training ex-ante, so their decisions were based on beliefs of $\{u^d, c^d\}$ and $\{u^p, c^p\}$ instead. Once the program started, household members might update their prior. If households on average underestimated the costs and overestimated the gains, then the ex-post costs might exceed the benefits and we would observe completion increasing in $s^p$.

We suspect both mechanisms to be at play for the following reasons. First, since the program was offered to school girls for the first time, students and their parents might not have much knowledge...
on the benefits and costs. Second, informal focus groups reveal that some students had unrealistic expectations ex-ante for their career prospect after completing the program. This might result in the corner solution to daughters’ problem. Lastly, the endline data suggests that students might have underestimated the time commitment to the program and how their levels of digital skills would impact their ability to complete the online courses.

D2. Alternative Model: Transfer Frictions

We consider an alternative model with frictions on the ability to split the stipend according to the sharing rule. Suppose that $\alpha_d, \alpha_p \in [0,1]$ represent daughters’ and parents’ beliefs about the other agent’s credibility of complying to the sharing rule ex-post. Recall that in the friction-less environment, $s_p + \theta(s - s_p)$ is the public return that daughters and parents negotiate on. Given $\alpha_d, \alpha_p$, the available amount for negotiation from parents’ perspective is $s_p + \theta \alpha_d(s - s_p)$ and that for daughters is $\alpha_p s_p + \theta(s - s_p)$. Hence, the new parents’ problem is as follows.

\[ I_{\text{consent}}^p \{ u_p + \lambda (\alpha_p(s - s_p) + s_p) > c_p \} \]

Rearranging inside the indicator function:

\[ \lambda > \frac{c_p - u_p}{\theta \alpha_d(s - s_p) + s_p} = \lambda_{lb} \]

Holding $\theta$ constant, the lower-bound is decreasing in $s_p$. Hence, this model predicts that in the full information group, parental consent should be increasing in $s_p$. Since girls’ drop-out decisions and completion are conditional on parental consent which varies with $s_p$, we cannot derive direct test for these two outcomes using our data.

Our results find no difference in parental consent in the full information groups and hence we reject the model with frictions on parental beliefs on transfer frictions.