Intra-Household Incentive Design: An Experiment on Parent-Child Decision Dynamics in Pakistan

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Abstract

How should we design and target incentives for skills investment in young adults who live with their parents? We use models of intra-household decision making and a randomized control trial to study how policymakers can use cash incentives to boost completion of a digital skills training program for female youths in urban Pakistan. Fixing the household's total incentive for program completion, we cross vary (1) the payment split between parents and daughters and (2) whether parents receive information about the daughters' incentive. We find strong evidence against daughters as sole decision makers, as assigning the entire incentive to parents under information asymmetry increases program completion by 103% relative to assigning it to daughters. Once parents are given information about the daughter's portion of the incentive, payment allocation does not have a statistically significant impact on program completion. Our results suggest that in this parent-child context, incomplete information sharing is the main barrier to the optimal incentive targeting, instead of bargaining frictions on the future payment.

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I. Introduction

Cash transfers are a popular policy instrument for promoting human capital investment, particularly in low-income countries. These investment decisions often involve both parents and children, and thus, the policy effectiveness is contingent on targeting the main decision maker(s) within the households. Therefore, policymakers need to understand the dynamics of intra-household decision making and frictions that may arise. We study incentive design under intra-household decision making between parents and daughters still living in the household to evaluate how policymakers can use payment to increase digital skills investments for young women in Lahore, Pakistan. The government of Pakistan heavily subsidizes supplemental digital skills training programs for the youth, as 91 percent of young adults in Pakistan lack basic digital skills, such as copying and pasting text (World Skills Clock (2023)) and only 42 female youth possess digital skills for every 100 males in Punjab (UNICEF (2023)). While many trainings offer sizable cash rewards upon completion, training providers still report low female participation and completion rates.[1]

We partner with the Punjab Skills Development Fund (PSDF), which is Pakistan's largest skills development fund and an organization explicitly tasked to boost digital skills for young women by the national government.[2] We recruited 489 households across 8 girls-only, government high schools and colleges in Lahore, and offered seats in short-term training programs from Coursera. Since girls in our sample had low computer literacy at baseline, the program offered a selection of beginner-level online trainings, ranging from bookkeeping to digital marketing. Upon program completion, girls received a globally recognized Coursera certificate, and their household received 3,000 PKR, equivalent to 7.5 percent of the monthly household income in our sample.

To study incentive design under intra-household decision making, we fixed the total household reward at 3,000 PKR and cross varied (1) the split of incentive paid to parents (0 PKR, 1500 PKR, or 3000 PKR) and (2) whether parents learned about the total incentive size (partial or full information). This 3-by-2 design results in six experiment arms, and the control arm is the status-quo case when parents receive neither the incentive nor information about the incentive. When girls signed up for the program at school, they were randomly assigned to one of the six arms and received full information on the total household incentive and the incentive split. They were also made aware of what information their parents would receive from the training provider. We then contacted parents to request consent for their daughters' participation in this external training program and informed them about the incentive offer. After the training provider received the parental permission, students enrolled and attended the courses through computer labs on campus.

We track differences by incentive contract across two main outcomes: parental permission for participation, and program completion. We report daughters' drop-out decisions separately from parental consent, as girls were asked to complete the program in their school's computer laboratory

[1] For example, PSDF offers an incentive of 5,000-6,000 PKR upon program completion for the standard Coursera trainings. Administrative data from PSDF shows that females account for 20.2 percent among those signed up for the program and only 8.9 percent of them complete the course.

[2] 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)).

during a free-period. Parents cannot perfectly monitor girls' action at school and we find no evidence of parents forcing daughters to drop out at the endline.[3]

We find that under information asymmetry, allocating the entire incentive to parents instead of the daughter increases completion by 16.45 percentage points (pp), which represents a 103% increase from the control group's mean of 15.94 percent. We reject the unitary model of decision making between parents and daughters under the standard test of income-pooling.

Once we impose information symmetry on the total household incentive, there is no statistically significant difference in completion across who was allocated the reward. Differences in household completion are small in magnitude (0.78 to 3.09pp). Under the assumption that we are underpowered, we also find that our percentage estimates of differences in completion (6 to 10%) are also on the smaller end of measured household "inefficiencies" when compared to other papers that are powered to reject efficient household decision making.[4]

Taken together, our results from household completion rates under both partial and full information suggest two things. First, the decision to invest in digital skills for daughters is a joint household decision between parents and children, highlighting the importance for training providers and policymakers to account for intra-household decision making when designing payment rewards. Second, there is little evidence of non-cooperative bargaining frictions on payment between parents and daughters under full information, meaning that incomplete information sharing is the most significant barrier to optimal incentive targeting.

This project contributes to multiple strands of literature. Our key contribution is to experimentally evaluate the effectiveness of different incentive contracts through the lens of intra-household decision making. While many papers document impacts of information symmetry and payment targeting on spousal decisions regarding finance, fertility, consumption, production, and female labor force participation (Duflo (2000), Ashraf (2009), De Mel, McKenzie and Woodruff (2009), Castilla and Walker (2013), Ashraf, Field and Lee (2014), Roy et al. (2015), Haushofer and Shapiro (2016), Ashraf et al. (2020), and Lowe and McKelway (2022)), there are fewer empirical studies on intra-household dynamics between parents and children. Bursztyn and Coffman (2012), Bergman (2021), and De Walque and Valente (2023) study the impact of information provision on children's school attendance by reducing parents' monitoring costs. Baird, McIntosh and Özler (2011), Berry (2015), De Walque and Valente (2023) explore the effectiveness of payment targeting at parents versus children. We build on the previous work in two ways. First, we test the interactive effects of payment targeting and information symmetry, and second, we are the only study of the above papers that finds evidence in support of cooperative bargaining on payments.

Our work also adds 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 evaluate how intra-household decision making impacts cash transfer policies, where observed decisions are made **after** some payment is received. Our core contribution is to evaluate both the unitary and

[3]See Section III for more discussion on separability of actions.
[4]See Appendix Table D1 for the range of inefficiencies and citations.

collective models by decomposing household-level outcomes into separate actions by parents and daughters via a randomized control trial. 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.[5]

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. 73 percent of girls in our sample state that they believe women like them should only work in skilled, white-collar jobs, yet 47 percent of girls who do not end up participating in our training are not comfortable cutting and pasting text at endline. We document how information asymmetry within the household can cause under-investment in young women's essential income-generating skills, which may further exacerbate their ability to work.

The rest of this paper is organized as follows. Section II generates testable hypotheses on how different incentive contracts affect program completion under the unitary and the efficient collective household models. 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

To generate predictions on the relationship between different incentive contracts and program completion, we use two standard models of decision making: (1) the unitary household and (2) the collective household with efficient bargaining.

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,[6] 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 perfectly 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 θ may vary at the household level and represent differences in incentive targeting to parents and the extent of information asymmetry within the household.

[5] See Section V for a full discussion on differences in bargaining environments.

[6] In 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}.[7] 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 θ.

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[8] 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.

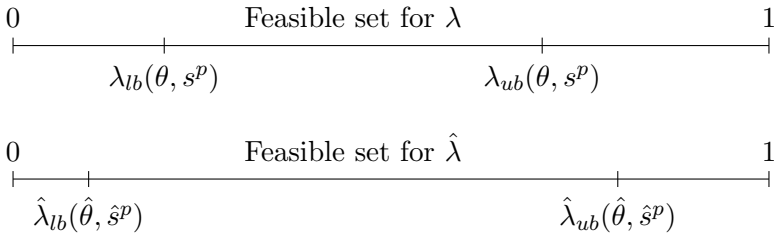
[7] There are two prominent theories that link individual preference parameters to household behavior consistent with the unitary model: Samuelson (1956) and Becker (1974, 1991). Samuelson (1956) explains a collective household may still behave like a unitary one when the household welfare function already incorporates each member's preferences. In our context, this maps to a setting where bargaining is unconditionally cooperative (see Subsection B). 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.

[8] Efficient collective households are often also referred to as households where bargaining is cooperative.

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, and daughters cannot enroll in the training without permission from their parents.[9, 10] To reach an agreement, they must negotiate a sharing rule ($\lambda \in [0, 1]$) to divide the payment. The actual value of λ is not the interest of this paper, as will depend on factors like altruism or bargaining ability which are beyond the scope of this study. Instead, we examine the feasible range for λ such that both parents and daughters remain interested in completing the course. The larger the feasible range of λ, under pareto-optimality (and hence efficient or cooperative bargaining) the more likely households are to complete the program.

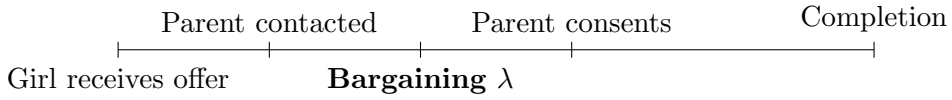
The feasible range of λ depends on the exogenous targeting of incentives and information. Below are two examples to illustrate feasible sets for two different households that contain sharing rules λ and $\hat{\lambda}$.



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.



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.

[9] 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.

[10] We require written parental consent for participation as PSDF previously attempted to offer digital skills trainings in schools without parental permission, and they received backlash from parents and school administrators.

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 reveal. Hence, $\theta(s - s^p) + s^p$ is known with certainty by parents and this is the amount negotiated on.[11] Negotiation results in a choice of $\lambda \in [0, 1]$, which is the proportion of the negotiated amount that is kept by parents. Post-negotiaton, 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, λ.

$$I^p_{\text{consents}}\{u^p + \lambda(\theta(s - s^p) + s^p) > c^p\} \tag{1}$$

$$I^d_{\text{completes}}\{u^d + (1 - \theta)(s - s^p) + (1 - \lambda)(\theta(s - s^p) + s^p) > c^d\} \tag{2}$$

Parents: Rearrange Equation 1 to isolate λ in the parents' decision function.

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

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.

$$\lambda > \frac{c^p - u^p}{\theta s + s^p(1 - \theta)} = \lambda_{lb} \tag{3}$$

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.

$$\lambda_{lb}|_{\theta=1} = \frac{c_p - u^p}{s}$$

Daughters: Rearrange Equation 2 to isolate λ within the daughter's decision function.

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

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

[11] The parents portion of the incentive is negotiated on, because daughters in this context are not working outside of the household. The majority of consumption by daughters is indirectly funded via income to parents.

compatible for the daughters.

$$\lambda < \frac{u^d - c^d + s}{\theta(s - s^p) + s^p} = \lambda_{ub} \tag{4}$$

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.

$$\lambda_{ub} - \lambda_{lb} = \frac{u^d + u^p + s - (c^d + c^p)}{\theta(s - s^p) + s^p} \tag{5}$$

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.[12]

Remark 3: Under full information, both the unitary model and the collective model predict that household completion does not vary with s^p. *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.*[13] In particular, we test the income pooling property under the unitary model.

[12] Our full sample of results on household bargaining is on the set of households in which daughter and parents both complete a survey. The timeline stated in Section IIB2 requires that bargaining happens after parents are contacted by the training provider. However, if bargaining happens before, it may be possible that we have selective attrition into the parents survey. See Appendix B for further discussion. We do not find much evidence of selective attrition in our sample.

[13] 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.

Remark 4: The collective model cannot predict ex-ante how completion will change with incentive allocation under partial information because the partial derivative of Equation 5 with respect to s^p depends on unobserved preference and cost parameters $\{u^d, c^d\}$ and $\{u^p, c^p\}$.

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 C1 for the list of every reasonable statistical test.

Table 1: Testable Hypotheses on s^p and θ

A. Unitary household
H1: Household completion does not vary with s^p or θ.
B. Efficient collective household
H2: Under full information, parental consent does not vary with s^p.
H3: Under full information, the girl's decision to complete or drop-out does not vary with s^p.
H4: Under full information, household completion does not vary with s^p.

Notes: This table presents testable hypotheses regarding the impact of payment targeting and information symmetry on the main outcomes, under the unitary and collective models respectively.

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.[14] 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

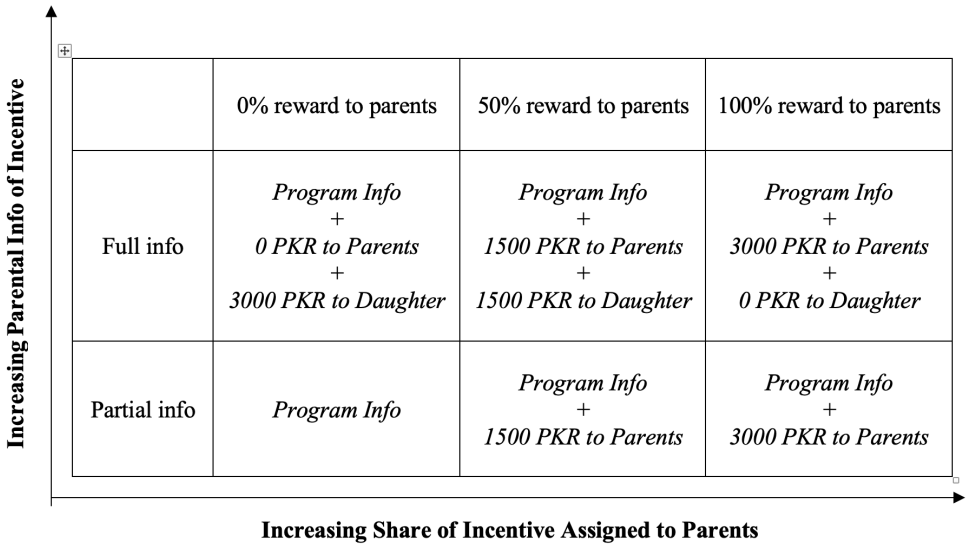
We map our experiment directly to the model in Section II by exogenously varying the split of the incentive to parents, s^p, and information on the full incentive amount to parents, θ. We select $s^p \in \{0, 1500, 3000\} = \{0\%, 50\%, 100\%\}$ and cross-randomize full information on the total household 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.[15] We refer to the group

[14]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.

[15]For example, girls assigned to {50%,Partial} were told: *"You will receive 1,500 rupees in cash, and your parents/legal*

Figure 1: Targeting Incentive and Information to Parents



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.

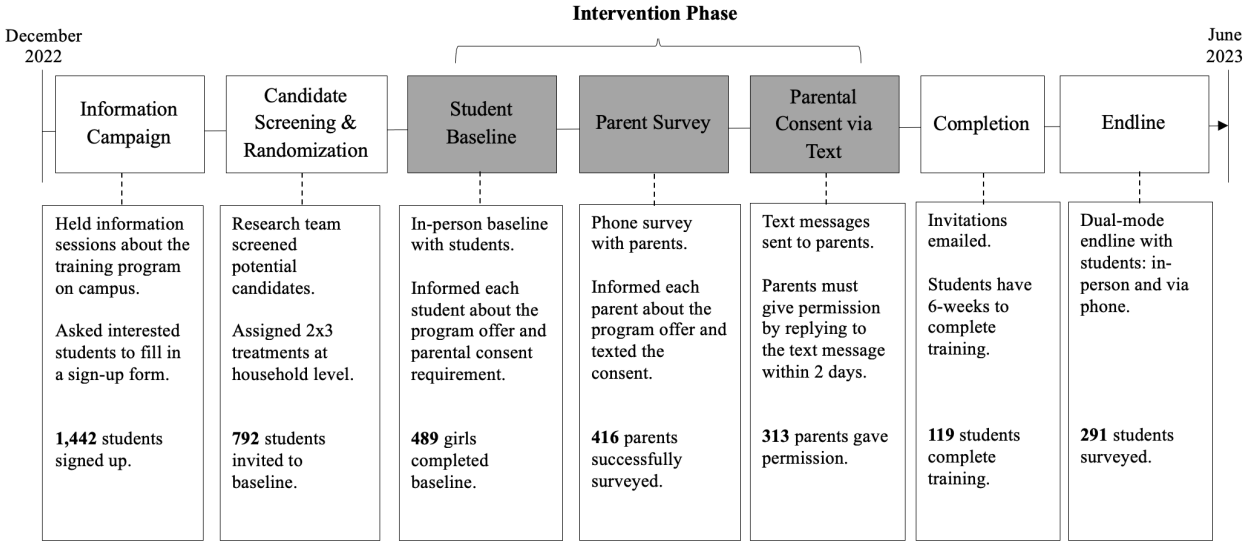
Given the program capacity of 300 seats, we screened candidates based on their responses to the attention check and open-ended questions.[16] 792 students passed the screening and were invited to the baseline survey. These students were randomized into contract groups at the household level.[17]

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 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."*

[16] 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.

[17] The randomization was stratified on school ID and a dummy variable for computer use above average. These

Figure 2: Experiment timeline



Notes: This figure reports the timeline of the fieldwork and the sample size at each stage.

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.[18] 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

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.

[18] 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.

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.[19] We asked the parents to text 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.[20]

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.[21] 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.[22] 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

[19] The text message to parents is in Urdu and reads as follows: *"[student's name] has been given priority for a spot to participate in the xxx training program. [Description of the program offer] Text 1 to accept the offer. Text 0 to decline. Please respond to us within 2 days, otherwise this offer will expire."*

[20] We 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.

[21] Anecdotal 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.

[22] According 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

	Full	Partial Info			Full Info			
	Sample	0%	50%	100%	0%	50%	100%	P-Value
N	489	69	85	90	82	69	94	
				Panel A: Personal Characteristics				
High School Student	0.225	0.275	0.212	0.278	0.171	0.217	0.202	0.531
	(0.418)	(0.450)	(0.411)	(0.450)	(0.379)	(0.415)	(0.404)	
Plans to Work	0.978	0.971	0.988	0.978	0.976	0.971	0.979	0.965
	(0.148)	(0.169)	(0.108)	(0.148)	(0.155)	(0.169)	(0.145)	
Only Skilled Jobs Appropriate for Women	0.734	0.783	0.718	0.656	0.756	0.812	0.713	0.265
	(0.442)	(0.415)	(0.453)	(0.478)	(0.432)	(0.394)	(0.455)	
Own a Personal Smart Phone	0.108	0.116	0.059	0.078	0.195	0.087	0.117	0.143
	(0.311)	(0.323)	(0.237)	(0.269)	(0.399)	(0.284)	(0.323)	
				Panel B: Household Characteristics				
Household Size (in Members)	6.427	6.304	6.600	6.544	6.244	6.565	6.309	0.679
	(1.812)	(1.428)	(2.178)	(1.831)	(1.495)	(2.193)	(1.633)	
Monthly Income (PKR in Thousands)	41.620	42.392	41.582	39.379	42.656	40.250	43.334	0.657
	(17.735)	(17.723)	(17.353)	(17.520)	(20.501)	(16.668)	(16.594)	
Father Works	0.890	0.797	0.906	0.922	0.939	0.841	0.904	0.111
	(0.314)	(0.405)	(0.294)	(0.269)	(0.241)	(0.369)	(0.296)	
Mother Works	0.135	0.188	0.106	0.122	0.159	0.130	0.117	0.742
	(0.342)	(0.394)	(0.310)	(0.329)	(0.367)	(0.339)	(0.323)	
Father with HS Degree or Above	0.681	0.754	0.671	0.633	0.646	0.652	0.734	0.439
	(0.467)	(0.434)	(0.473)	(0.485)	(0.481)	(0.480)	(0.444)	
Mother with HS Degree or Above	0.556	0.551	0.506	0.578	0.549	0.580	0.574	0.932
	(0.497)	(0.501)	(0.503)	(0.497)	(0.501)	(0.497)	(0.497)	
Has Computer at Home	0.689	0.594	0.765	0.722	0.683	0.681	0.670	0.324
	(0.463)	(0.495)	(0.427)	(0.450)	(0.468)	(0.469)	(0.473)	

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 = 416). We replace missing values of household monthly income with the average household income at the respondent's school.

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 A1 documents the reasons why girls who were given consent dropped out of the program. Not a single student reported being discouraged 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

The specification for all outcomes is:

$$(6) \quad y_i = \beta_0 + \beta_1\{50\%, \text{Partial}\}_i + \beta_2\{100\%, \text{Partial}\}_i + \beta_3\{0\%, \text{Full}\}_i + \beta_4\{50\%, \text{Full}\}_i + \beta_5\{100\%, \text{Full}\}_i + \varepsilon_i$$

y_i is the outcome of interest for household i, and the omitted group is $\{0\%, \text{Partial}\}$, which is the control group. We report bootstrapped confidence intervals and p-values. All regressions and statistical tests map directly to the hypotheses presented in Table 1 and are fully written out in Appendix Table C1. Appendix Tables C2 and C3 report all relevant regressions, bootstrapped p-values, and Fisher's exact p-values robust to relevant sample restrictions and the exclusion of covariates.

A. Effects on Program Completion

Figure 3 shows household completion rates across treatment groups for all households that had a girl's baseline survey.[23] 15.94 percent of students completed the program in the control group. We find that treating parents with either payments or information about payments pushed completion rates up by at least 7.32pp and at most 16.45pp. The completion rate is the highest at 32.39 percent when parents are assigned the entire payment under partial information. We highlight two distinct patterns.

First, we observe very little differences in completion rates for households where parents were given full information. Remark 3 from Section II suggests that under full information, we cannot distinguish the unitary household from an efficient collective one as both models generate the same prediction for completion rates. Hence, to evaluate the unitary model, we need to restrict our analysis to the partial information treatment.

Second, in the partial information group, completion increases with the share of incentive targeted to parents. This pattern contradicts Hypothesis 1, as completion should not vary with s^p under the unitary model. We conduct a classic test of income pooling, which is to compare household completion where 100% of payment is allocated to either the student or the parents ($\beta_2 = 0$).[24] When parents are paid instead of daughters, we find that completion rates differ by 16.45pp and is statistically significant at the 1% level. This difference represents a 103% increase in probability of the household receiving 3,000 PKR.

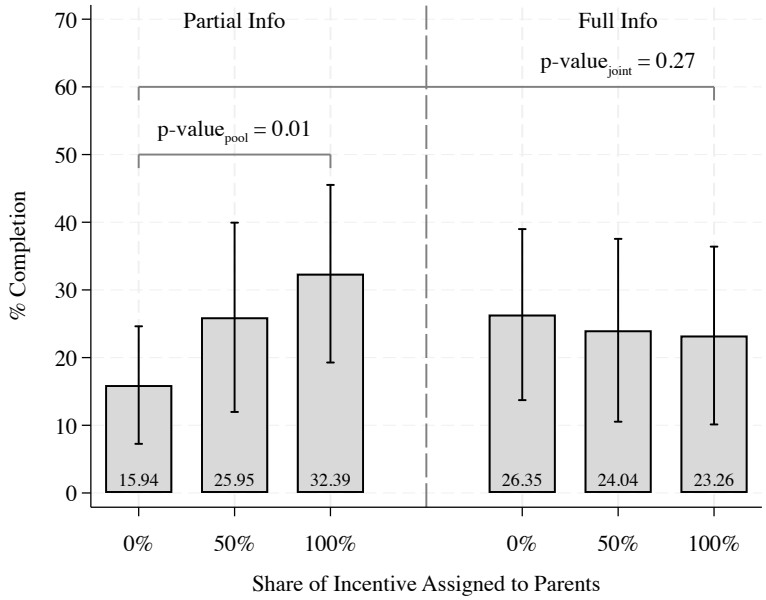
B. Effects on Parental Consent and Girl's Drop-out

We now restrict our analysis to households that have both a daughter's and parent's baseline survey. This allows us to evaluate whether changes in household completion were driven by parent's refusing

[23] We also report all household level results in Appendix Table C2 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.

[24] Appendix Table C2 reports p-values for two other statistical tests that would contradict H1 under the unitary household.

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



Notes: We report completion rates across the treatments in the full analysis sample ($N = 489$), derived from a regression of program completion on treatment indicators with stratum fixed effects and controls for whether the student is in high school, in the last year of school, household size, working status of the father and mother, high school graduation status of the father and mother, if there is a computer at home, if the student has their own cell phone, and if the student plans to work. Bootstrapped 95% confidence intervals are reported, along with a bootstrapped p-value from a joint test of null effects for all treatments and a bootstrapped p-value of a two-sided test on income pooling ($\beta_2 = 0$). Bootstrap results are generated from 10,000 draws.

consent or daughters dropping out.

Hypotheses 2, 3 and 4 from Table 1 state that under information symmetry on the full payment amount, 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 ($\beta_3 = \beta_4 = \beta_5$).

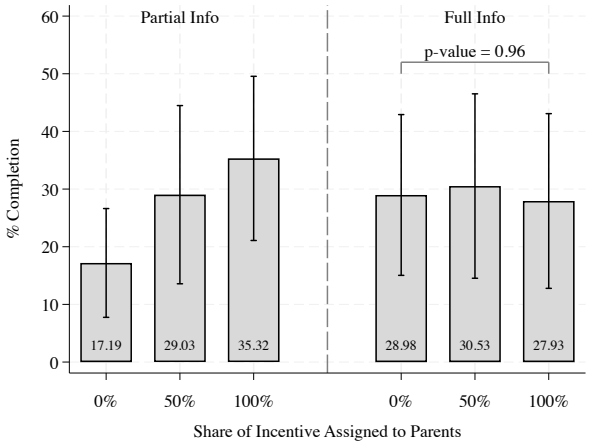
Panel A of Figure 4 shows that within the sample of households where we have both a parent and student survey, completion rates are remarkably similar under full information. Panels B and C break down household completion by evaluating parental consent and girl's drop-out. Bootstrapped p-values for joint tests of equality on payment targeting under full information are 0.82, 0.88, and 0.96 for H2, H3, and H4 of Table 1. Fisher's exact p-values are reported in Appendix Table C3 and are essentially identical to their bootstrapped counterparts. Appendix Figure D1 plots the full distributions of bootstrapped group means under full information.

Our results seem consistent with the efficient collective household model of decision making between parents and daughters, and we are unable to reject pareto-efficient bargaining on payments under full information. Differences in our point estimates across payment targeting under full information, adjusted from percentage points to percentages, range from (6 to 10%). Assuming we are underpowered due to sample size, we compare our percentage-adjusted estimates with those in other

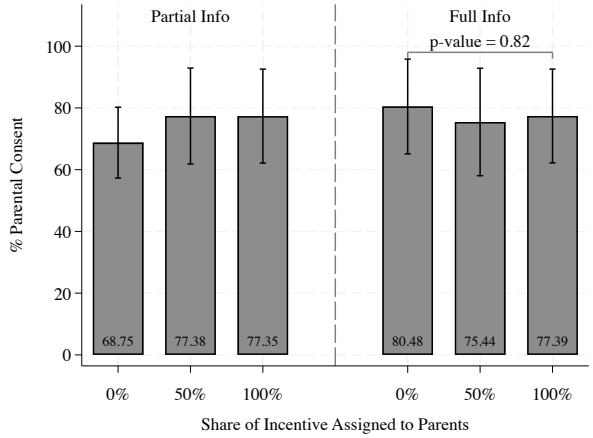
papers powered to reject pareto-efficient decision making and we find that our estimates are on the smaller end. See Appendix Table D1 for the range of estimates and citations, and see Munro (2018) for a review of intra-household experiments and sample sizes.

Figure 4: Household Bargaining Outcomes

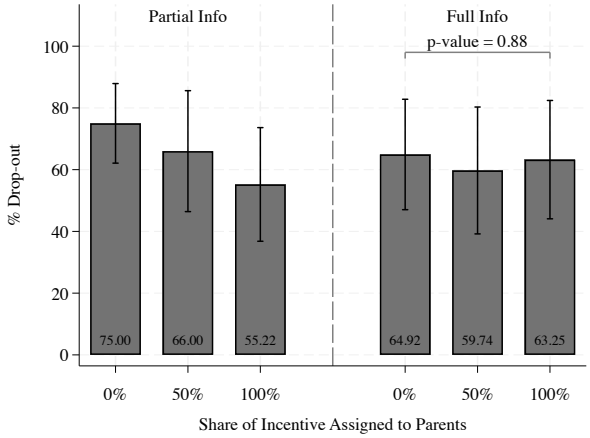
Panel A. Program Completion *(Parent Survey Sample)*



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



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



Notes: Program completion, parental consent, and girls' drop-out across the six treatments, derived from a regression of the outcomes on treatment indicators with stratum fixed effects and controls for whether the student is in high school, in the last year of school, household size, working status of the father and mother, high school graduation status of the father and mother, if there is a computer at home, if the student has their own cell phone, and if the student plans to work. Panels A and B report completion and parental consent among households that answered the parent survey ($N = 416$). Panel C reports girls' drop-out conditional on parental consent ($N = 313$). Bootstrapped 95% confidence intervals are reported, along with bootstrapped p-values from the joint test on equality of treatment effects under full information. Bootstrap results are generated from 10,000 draws.

V. Discussion of results

A. *Why does completion increase with s^p under partial information?*

Remark 4 of Section II highlights that a standard collective model with no assumptions on preferences and cost structures $\{u^d, c^d\}$ and $\{u^p, c^p\}$ cannot generate predictions on completion rates

across s^p under partial information. Experimental results show that under partial information, completion rates actually increase in s^p. We consider two explanations for why completion rates increase when parents are allocated larger shares of the incentive.

Explanation 1: Parents are able to influence completion through reminders.

Post-program focus groups with a small number of treated households reveal some parental nudging to encourage daughters' program completion through reminders and discussions with relatives.

Following the timeline of household bargaining in Section IIB2, after parents give consent, they can nudge daughters to complete the program so that the household receives the 3,000 PKR. We assume that it is cost-less for parents to nudge daughters and they exert more influence on daughters actions when the public return $(\theta(s - s^p) + s^p)$ is higher. By completing the program, daughters get indirect utility v^d from complying to parents and v^d is scaled by a function of the public return, denoted by $\tau(\theta(s - s^p) + s^p)$. Under full information, the total indirect utility $\tau(s)v^d$ does not vary by s^p. Under partial information, the indirect utility $\tau(\theta(s - s^p) + s^p)v^d$ is increasing in s^p.

We rewrite Equation 2 of the girl's decision function for program completion to include the indirect utility term.

$$I^d_{\text{completes}}\{u^d + \tau(\theta(s - s^p) + s^p)v^d + (1 - \theta)(s - s^p) + (1 - \lambda)(\theta(s - s^p) + s^p) > c^d\} \tag{7}$$

We generate a new upper bound on payment sharing by isolating λ from Equation 7.

$$\lambda < \frac{u^d + \tau(\theta(s - s^p) + s^p)v^d + s - c^d}{\theta(s - s^p) + s^p} = \lambda'_{ub} \tag{8}$$

We combine λ_{lb} of Equation 3 from the parent's problem and λ'_{ub} of Equation 8 from the daughter's problem to generate the new support of λ that will incentivize the household to complete the program.

$$\lambda'_{ub} - \lambda_{lb} = \frac{u^d + u^p + \tau(\theta(s - s^p) + s^p)v^d + s - (c^d + c^p)}{\theta(s - s^p) + s^p} \tag{9}$$

Under partial information ($\theta \in [0, 1]$), the effect of incentive allocation s^p on completion is ambiguous because both the numerator and the denominator in Equation 9 are increasing in s^p. When a greater amount of payment is assigned to parents, they exert a stronger influence on daughters actions to ensure the household can attain the reward. Consequently, daughters gain indirect utility by completing the program but lose own monetary incentive to finish it. Our experimental results imply that the positive effect of parental nudging dominates the negative effect of daughters losing incentive for completion.

Under full information ($\theta = 1$), this model with parental nudging generates the same predictions as the original model.

This adjusted model fits our experimental results on household completion very well, however, we are unable to directly test hypotheses on girls' drop-out decisions under partial information in our

data because we only observe their decisions conditional on parental consent. See Remark 1 of Section II.

Explanation 2: Payment to parents reveals information.

It is possible that the act of paying parents acts as information on the true value of s. In this case we rewrite, $\theta(s^p) \in [0,1]$ as a function that is increasing in s^p. Under this explanation, the support of $\lambda|_{\theta\in[0,1]}$ will converge to that of $\lambda|_{\theta=1}$ as we increase payment to parents.

This explanation is also consistent with our experimental results, as we cannot detect a statistically significant difference in completion between $\{100\%, \text{Partial}\}$ and all treatment groups $\{s^p, \text{Full}\}$.

B. Implications for Incentive Design

We reject the unitary model of household decision making between parents and their daughters, but also find that once information symmetry on payment is imposed between parents and girls, differences in household completion rate from payment targeting are minimal.

Our results have interesting implications for incentive design around skills training programs for young adults still living in the household. Often times, policymakers design incentives in a way that trade-off training completion with externalities of the payment itself. 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, policymakers can simply design payment targeting in ways that focus on minimizing externalities of the payments while obtaining similar completion rates. Otherwise, if information symmetry cannot be easily imposed, results from our context show that household completion is the highest when parents are paid the entire incentive.

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 (2023)) find evidence that 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. See Appendix E for a more detailed derivation.

C. 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 policymaker 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 (2023)). 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.

D. Research Design and Program Evaluation

One contribution of our research design is that we decompose the actions of parents and daughters in a scenario where payment is conditional on a joint household action. This has been difficult to do, because typically, only household-level outcomes are observable to an econometrician. 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 perfectly 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 A1 shows that conditional on parental consent, not one girl reported parents discouraging them from finishing the training at school.

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 are not able to properly evaluate impacts of Coursera as an effective tool for digital skills literacy. This is because we did not enroll enough households to include a pure control group.

Appendix A documents non-causal effects of the training. Basic digital skills within our sample are very low, as Appendix Table A1 shows that among girls who did not enroll in the program, 47.67 percent are not confident cutting and pasting text. Likewise, 73.42 percent of girls in our baseline survey felt that it was only appropriate for women like them to work in skilled, white collar jobs. Such jobs are guaranteed to require basic knowledge of computer skills. We document improvements in self-reported confidence with essential computer tasks, providing hope that girls who participated are more likely to continue acquiring basic digital skills on their own, without need for official trainings.

VI. Conclusion

In many contexts, educational decisions for teenagers are made jointly with parents. Our study sheds light on how policymakers can leverage the dynamics of intrahousehold decision-making to effectively incentivize young women's skills investment.

Our findings reveal that targeting parents with payments for program completion or information about payments can improve program take-up and completion. Hence, we reject the unitary household model between parents and young adult daughters. Moreover, we document that inefficiencies in household negotiation on incentive payments come through information asymmetry and not through payment targeting. When young women face household opposition for investing in their income-generating skills, our results have several implications for the optimal incentive design. If the policymaker 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.

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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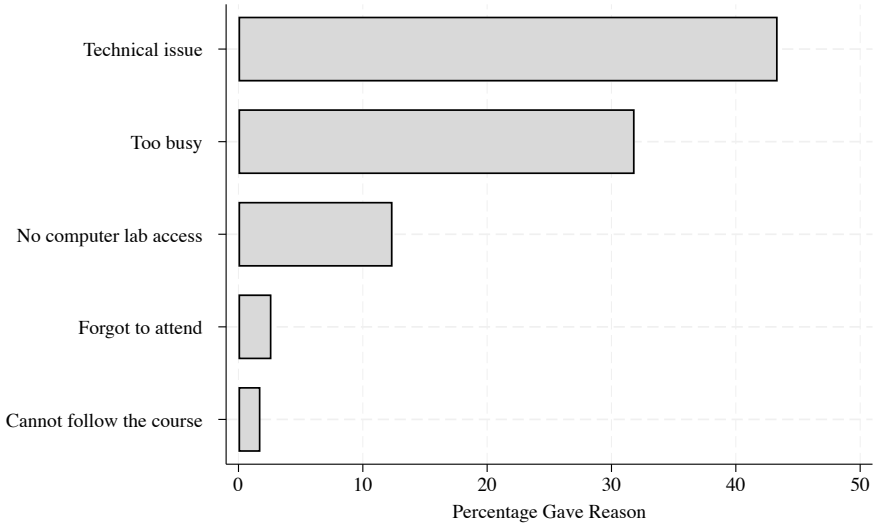
Appendix A. Endline Results

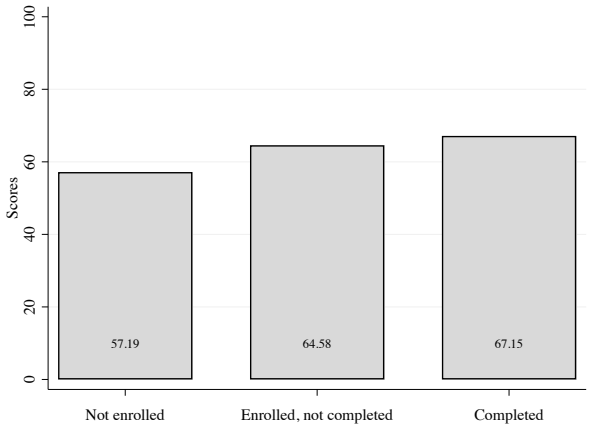
Appendix Figure A1: Reasons for Drop-out



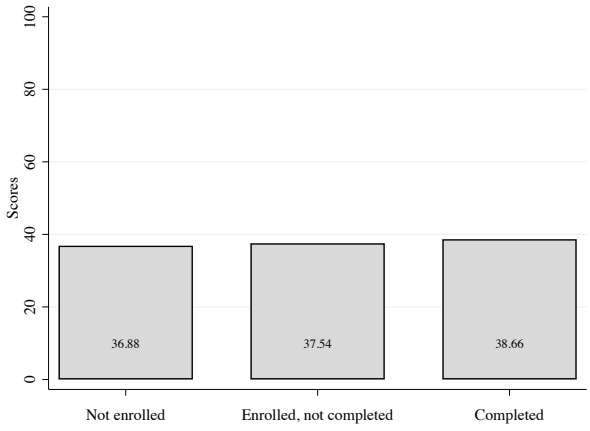
Notes: This figure shows the top reasons for program drop-out given by 113 students during the endline survey. *Technical issue* means that students encountered some technical difficulties when enrolling in the online course or submitting their assignment on the Coursera webpage. *Too busy* means that 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 Figure A2: Endline Computer Scores

Panel A. Self-Assessment Test

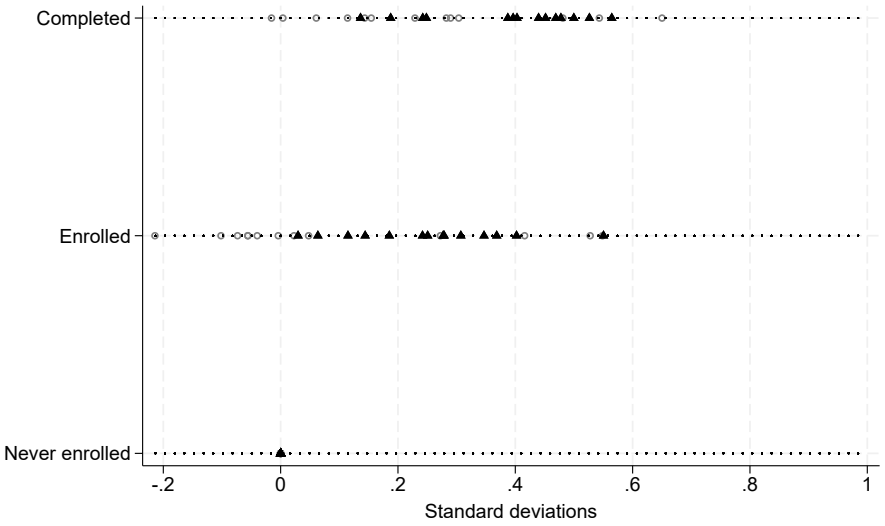


Panel B. Quiz



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 A3: Essential vs non-essential digital skills at endline



Notes: This figure shows differences in self-reported digital literacy skills by 171 students during the endline survey. Responses are converted to standard deviations, within each treatment arm, relative to the students who never enrolled in the training. Triangles represent essential digital skills that are absolutely required for any type of white collar work. Circles represent intermediate or more niche digital skills that may not be necessary for every type of white collar job. Students report their level of confidence from 1 to 5. 1 is "I have never done this before". 2 is "I could do this with help". 3 is "I can do this alone, but might make some mistakes". 4 is "I can do this alone with confidence". 5 is "I can teach others to do this task". From Appendix Table A1, essential skills are categorized as "Cut and paste text within a document", "Change font size, style, and color", "Create bulleted or numbered lists", "Create a new spreadsheet", "Use math functions such as sum and mean", "Open and print an email attachment", "Use cc and bcc to manage email recipients", "Use Calendar to assist in time management", "Use a search engine like google", "Save a file and locate that file", "Search and find a missing file", "Create new folders", correctly choosing a browser from the list, and correctly identifying a computer folder. All other tasks from the computer test and Appendix Table A1 are deemed intermediate digital skills.

Appendix Table A1: Self-Reported ability to complete computer tasks

	Not comfortable with this task = 1		
	Not Enrolled	Enrolled, Not Completed	Completed
	(1)	(2)	(3)
Word Processing			
Cut and paste text within a document	47.67	30.77	22.03
Change font size, style, and color	46.51	34.62	22.03
Create bulleted or numbered lists	55.81	42.31	37.29
Create a hyperlink	83.72	76.92	69.49
Excel Spreadsheets			
Create a new spreadsheet	59.30	53.85	42.37
Use math functions such as sum and mean	53.49	38.46	35.59
Create a graph and adjust the properties	60.47	65.38	52.54
Email			
Open and print an email attachment	54.65	46.15	37.29
Use cc and bcc to manage email recipients	87.21	76.92	79.66
Use Calendar to assist in time management	55.81	50.00	50.85
Internet			
Use a search engine like google	36.05	30.77	28.81
Create a blog	76.74	61.54	67.80
Create a website	82.56	76.92	81.36
Insert an audio file, image, video, and podcast onto a website	69.77	53.85	49.15
File Management			
Save a file and locate that file	55.81	38.46	32.20
Search and find a missing file	52.33	34.62	35.59
Create new folders	38.37	30.77	20.34
Media Files			
Create and edit an audio recording	36.05	38.46	32.20
Create and edit a video recording	37.21	38.46	28.81
Upload and download a video from a website	29.07	46.15	20.34
Observations	86	26	59

Notes: This table reports people self-reported ability to complete various computer tasks. 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. A lower number is interpreted as having more digital skills.

Appendix Table A2: Attrition by program choice

Program Choice	Baseline	Enrolled	Completed	In-person endline
	(1)	(2)	(3)	(4)
Better business writing in English	97	39	28	32
Fundamentals of graphic design	175	66	36	62
Foundations of digital marketing & e-commerce	85	38	22	28
Intuit bookkeeping	7	2	2	2
Work smarter with Microsoft excel	90	34	24	32
How to create a website	35	17	7	15
Total	489	196	119	171

Notes: This table reports the number of students at each stage of the study by program choice.

Appendix B. Non-reponses from parents

B1. Attrition on the parents survey

One of the main objectives of our experiment design was to look for evidence against efficient bargaining between parents and daughters. Tests against efficiency depend on both differences in parental consent and girls' drop-out. To properly measure both of these outcomes, we can only consider households where we have both a completed parent's survey, as we are only able to request parental permission from parents who complete the parent survey.

While it is possible to interpret all parents who did not complete the parents survey as refusing consent, it is impossible to interpret the daughters drop-out decision under parent survey attrition. Girls cannot drop out if their parents have never been texted. 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 B1 shows that of the number of households who did not complete the parents survey per group ranges from 5 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. We do not observe any pattern of refusals by contract groups.

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

	Partial information			Full information		
	0%	50%	100%	0%	50%	100%
	(1)	(2)	(3)	(4)	(5)	(6)
Refused	2	5	2	2	2	7
No answer	2	6	7	5	10	11
Other	1	1	2	3	3	2
Total	5	12	11	10	15	20

Notes: This table shows parents' reasons for non-response on the phone survey by information treatment (partial or full) and payment targeted to parents (0%, 50%, or 100% of total incentive). The total sample size is 72.

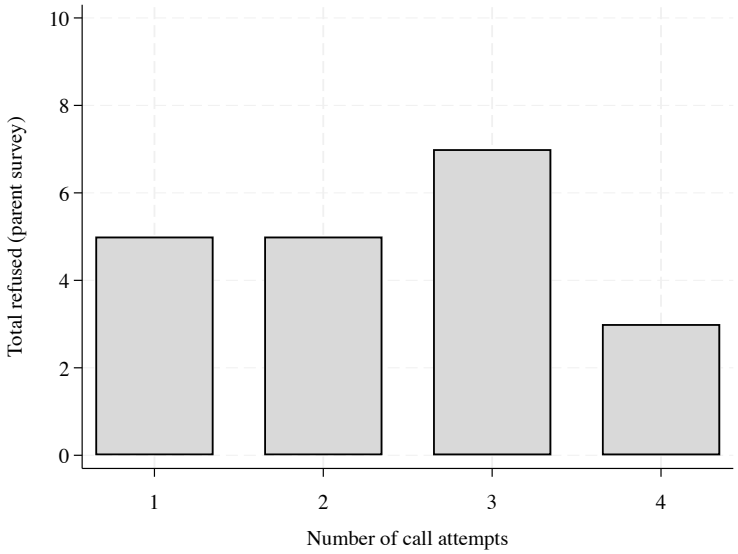
Argument 2: Despite the small sample, we find no interpretable patterns in the data suggesting selective attrition.

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



Notes: This figure shows correlation between the number of call attempts and the number of refusals ($N = 20$).

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 permission texts

	Partial information			Full information		
	0%	50%	100%	0%	50%	100%
	(1)	(2)	(3)	(4)	(5)	(6)
Did not receive call	0	1	0	0	0	0
Did not receive text	4	4	3	4	3	6

Notes: This table shows parents' reasons for non-response on permission texts by information treatment (partial or full) and payment targeted to parents (0%, 50%, or 100% of total incentive).

Appendix C. Alternate specifications

Appendix Table C1: All statistical tests, mapped to models of the household.

A. Unitary household

H1: Household completion is the same across θ or s^p.

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

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

$H_0 : \beta_1 = ... = \beta_5 = 0$	underpowered	$p \in [0.138, 0.258]$
H_a : violation of one equality		Needs 910 households

Classic test of income pooling under information asymmetry*.

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

$H_0 : \delta_1 = 0$	powered	$p \in [0.012, 0.032]$
$H_a : \delta_1 \neq 0$		

Test of information symmetry**

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

$H_0 : \eta_1 = 0$	marginal	$p \in [0.038, 0.157]$
$H_a : \eta_1 \neq 0$		Needs 588 total in $\{0, \text{Partial}\}$ and $\{s^p, \text{Full}\}$

B. Efficient collective household

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

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

$H_0 : \beta_3 = \beta_4 = \beta_5$	underpowered	$p \in [0.886, 0.889]$
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.

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

$H_0 : \beta_3 = \beta_4 = \beta_5$	underpowered	$p \in [0.763, 0.903]$
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.

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

$H_0 : \beta_3 = \beta_4 = \beta_5$	underpowered	$p \in [0.763, 0.903]$
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 Table C2: Unitary household tests, robust to covariates

Dependent variable:	Completed (=1)			
Sample:	Full Sample		Parent Surveyed	
	(1)	(2)	(3)	(4)
50%, Partial (β_1)	0.099	0.10	0.13*	0.12
	[0.065]	[0.071]	[0.073]	[0.079]
100%, Partial (β_2)	0.15**	0.16**	0.18**	0.18**
	[0.066]	[0.067]	[0.072]	[0.073]
0%, Full (β_3)	0.084	0.10	0.11	0.12*
	[0.065]	[0.064]	[0.072]	[0.071]
50%, Full (β_4)	0.072	0.081	0.12	0.13
	[0.067]	[0.069]	[0.079]	[0.082]
100%, Full (β_5)	0.075	0.073	0.13*	0.11
	[0.062]	[0.067]	[0.071]	[0.077]
p-values: Appendix Table C1				
(1) Joint test				
Boostrap	0.35	0.27	0.20	0.23
Fisher's exact	0.37	0.78	0.21	0.31
(2) Income pooling				
Boostrap	0.02	0.01	0.01	0.01
Fisher's exact	0.03	0.02	0.01	0.01
(3) Full information				
Boostrap	0.14	0.14	0.04	0.06
Fisher's exact	0.14	0.13	0.04	0.05
Strata FE		×		×
Baseline covariates		×		×
Control mean	0.16	0.16	0.16	0.16
Households (N)	489	489	416	416

Notes: This table shows the effect of payment targeting and information symmetry on household completion in the full analysis sample ($N = 489$) and the parent survey sample ($N = 416$). Columns (1) and (2) report results from Regression 1 of Appendix Table C1. Columns (2) and (4) include stratum fixed effects and baseline covariates for whether the student is in high school, in the last year of school, household size, working status of the father and mother, high school graduation status of the father and mother, if there is a computer at home, if the student has their own cell phone, and if the student plans to work. The second panel reports p-values from a joint test of null effect across all treatment groups, the income pooling test, and the information symmetry test from panel A of Appendix Table C1. Bootstrap results are generated from 10,000 draws. Fisher's exact p-values are generated from 10,000 re-randomized treatment groups with stratification.

Appendix Table C3: Efficient collective household, robust to covariates

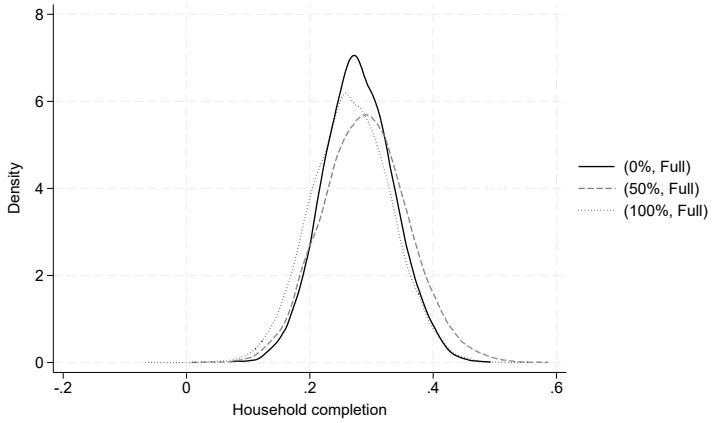
Dependent variable:	Completed (=1)		Parent consented (=1)		Girl's drop-out (=1)	
Sample:	Parent Surveyed				Parent Consented	
	(1)	(2)	(3)	(4)	(5)	(6)
50%, Partial (β_1)	0.13*	0.12	0.080	0.086	-0.12	-0.090
	[0.073]	[0.079]	[0.077]	[0.079]	[0.093]	[0.100]
100%, Partial (β_2)	0.18**	0.18**	0.072	0.086	-0.22**	-0.20**
	[0.072]	[0.073]	[0.076]	[0.078]	[0.092]	[0.094]
0%, Full (β_3)	0.11	0.12*	0.090	0.12	-0.11	-0.10
	[0.072]	[0.071]	[0.076]	[0.078]	[0.093]	[0.091]
50%, Full (β_4)	0.12	0.13	0.053	0.067	-0.15	-0.15
	[0.079]	[0.082]	[0.084]	[0.089]	[0.10]	[0.10]
100%, Full (β_5)	0.13*	0.11	0.083	0.086	-0.14	-0.12
	[0.071]	[0.077]	[0.077]	[0.078]	[0.092]	[0.098]
p-values: $\beta_3 = \beta_4 = \beta_5$						
Bootstrap	0.96	0.96	0.89	0.82	0.90	0.88
Fisher's exact	0.96	0.95	0.88	0.82	0.91	0.87
Strata FE		×		×		×
Baseline covariates		×		×		×
Control mean	0.16	0.16	0.69	0.69	0.75	0.75
Households (N)	416	416	416	416	313	313

Notes: This table shows the effect of payment targeting and information symmetry on household completion and parental consent in the parent survey sample ($N = 416$) as well as girls' drop-out in the parental consent sample ($N = 313$). Columns (1), (3), and (5) report results from Regressions 4, 5, and 6 of Appendix Table C1, respectively. Columns (2), (4), and (6) include stratum fixed effects and baseline covariates for whether the student is in high school, in the last year of school, household size, working status of the father and mother, high school graduation status of the father and mother, if there is a computer at home, if the student has their own cell phone, and if the student plans to work. Bootstrapped standard errors in parentheses. The second panel reports p-values from a joint test of equality in the full information group, derived from Panel B of Appendix Table C1. Bootstrap results are generated from 10,000 draws. Fisher's exact p-values are generated from 10,000 re-randomized treatment groups with stratification.

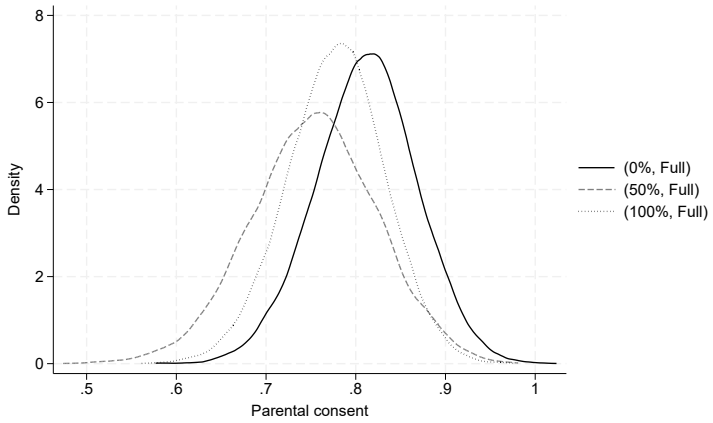
Appendix D. Null effect under full information

Appendix Figure D1: Boostrapped distribution of group means under full information

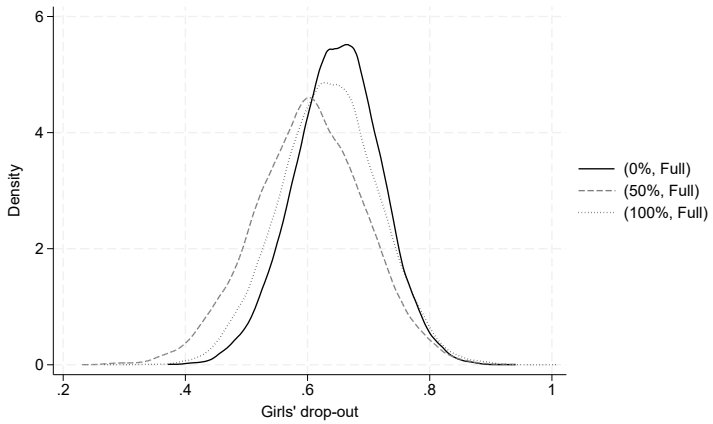
Panel A. Household completion *(N=416)*



Panel B. Parental consent *(N=416)*



Panel C. Girls' drop-out *(N=313)*



Notes: We generate distributions of the probability of household completion, parental consent, and girls' drop-out using 10,000 bootstrapped draws. The regression specification is Regression (1) from Appendix Table C1 and includes stratum fixed effects and baseline covariates for whether the student is in high school, in the last year of school, household size, working status of the father and mother, high school graduation status of the father and mother, if there is a computer at home, if the student has their own cell phone, and if the student plans to work.

Appendix Table D1: Effect sizes of inefficiencies under the collective household

Paper	Evaluation	Inefficiency size
Udry (1996)	Husband/Wife	6%
Choukhmane, Goodman and O'Dea (2023)	Husband/Wife	24%
Lowe and McKelway (2022)	Husband/Wife	50%
Schaner (2015)	Husband/Wife	52%
Bursztyn and Coffman (2012)	Parent/Child	6%
Bergman (2021)	Parent/Child	25%
Baird, McIntosh and Özler (2011)	Parent/Child	43%
De Walque and Valente (2023)	Parent/Child	54-75%

Notes: This table reports quantified effect sizes of household inefficiency in relevant economics papers. This list is by no means exhaustive.

Appendix E. Transfer frictions in the collective household

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 α^d, α^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.

$$I^p_{\text{consent}}\{u^p + \lambda(\theta\alpha^d(s - s^p) + s^p) > c^p\}$$

Rearranging inside the indicator function:

$$\lambda > \frac{c^p - u^p}{\theta\alpha^d(s - s^p) + s^p} = \lambda_{lb}$$

Holding θ 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.