

When Parents Are in the Room: Experimental Evidence on Job Information and Youth Job Search

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Abstract

Employment programs deliver job information to individual jobseekers, although employment decisions are often made within households. We study both margins in two separately randomized experiments covering 11,781 unemployed youth in rural Upper Egypt: what concrete, verified, tailored job information does on its own, and whether the household configuration in which the same information is delivered changes what youth do with it. Information moves behavior sharply. It raises the probability of taking a concrete step toward employment by 16.1 percentage points in the first experiment and 11.2 in the second, against control rates near a third, and men and women respond alike despite very different prior work histories. It produces no statistically detectable employment gain within seven to eight weeks. Conditional on the same information, parental co-presence has little detectable effect for sons. For daughters, father and mother co-presence generate short-run increases of about five percentage points in action on the offered opportunities; the two estimates are statistically indistinguishable, and their estimated trajectories subsequently diverge. Involving both parents has no detectable effect. Information appears sufficient to move youth from awareness to action but not to carry that action into employment over this horizon.

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1 Introduction

Employment programs in low- and middle-income countries are built around the individual jobseeker. Vacancies are advertised to a person, transport subsidies are paid to a person, and matching services enrol a person. Yet in much of the world young people live with their parents until marriage and often well beyond, and the decision to pursue a particular job is taken inside a household rather than by an isolated agent. The tension is sharpest for young women. In Egypt, female labor-force participation has fallen from roughly a quarter of working-age women in 2010 to about 15 percent in 2024, even as women’s educational attainment has risen (World Bank, 2024). Whether a young woman acts on a vacancy may depend not only on what she knows about it, but on what her parents know, and on whether her interest in the job is visible to them. Two questions follow, and this paper is built to answer both. What does concrete, verified job information do for unemployed youth who would otherwise have none? And conditional on receiving exactly the same information, does the household configuration in which it is delivered change what they do with it? The first asks how much of youth inaction reflects an information friction rather than the other constraints on getting a job; the second asks whether the individual jobseeker is the right unit at which to think about delivering employment services at all.

We answer both in two separately randomized experiments covering 11,781 unemployed youth aged 18–35 in 287 rural villages across six governorates of Upper Egypt. All participants were unemployed, expressed interest in finding work, and lived with both parents at enrolment. Project staff mapped vacancies at the village, district, and governorate level and assembled for each youth a tailored set of verified job leads naming the employer, the location, the required qualifications, the schedule, and the pay. Randomization was at the individual level within villages. Experiment 1 assigned youth to a control group that received no job information, to a youth-only arm that received the tailored leads privately, or to arms in which the same leads were delivered in a joint session with the father or with the mother. Experiment 2, conducted subsequently among a separate group of enrollees, assigned youth to a control group, a youth-only arm, or an arm in which the session included both parents. The service, its content, and the presence of the jobseeker are identical across the treated arms; what random assignment changes is who else is in the room. The design therefore yields two contrasts. Comparing the youth-only arm with the control group gives the effect of the information itself, and that contrast exists in both experiments, so the information result is estimated twice on separate samples. Comparing each parental arm with the youth-only arm of its

own experiment gives the effect of the household configuration of delivery, holding the information fixed. Because the two experiments were randomized separately, they are analyzed separately throughout.

Information moves behavior sharply. Seven to eight weeks after the session, in the only round where treated and control youth answered a common survey module, the share who had taken a concrete step toward employment is 16.1 percentage points higher in Experiment 1 and 11.2 points higher in Experiment 2, against control means of roughly a third, and the share looking or waiting without taking a concrete step falls by 12.5 and 7.1 points. Because the two experiments enrolled different youth under separate randomizations, the second estimate is a replication of the first rather than the same result reported twice. Employment does not follow: the estimated effect on the probability of currently working is 0.7 percentage points in Experiment 1 and 0.9 in Experiment 2, neither statistically detectable, with confidence intervals that exclude gains larger than about five points. Men and women respond to the information alike. The parental results, by contrast, are gender-specific. For sons we detect little response to co-presence in any arm or round. For daughters in Experiment 1, adding a father raises action on the offered opportunities at two weeks by 4.6 percentage points and adding a mother by 5.3 points, against a youth-only mean of 39 percent; neither survives adjustment for multiple testing within the family of hypotheses we designate, so we treat them as suggestive. The two arms are statistically indistinguishable at that point, and their estimated trajectories subsequently diverge, with the change in the gap between them significant and neither arm's effect individually distinguishable from zero by the final follow-up. In Experiment 2, involving both parents produces no detectable effect relative to youth-only delivery. No parental arm produces a statistically detectable employment gain inside the observed window.

These estimates speak first to the literature on search and information frictions, which establishes that relaxing such frictions changes how young people look for work. Transport subsidies extend the geographic range of search (Franklin, 2018), credible signals of worker skill improve both search strategies and firms' hiring decisions (Carranza et al., 2022), search assistance lowers the barriers associated with applications and employer contact (Abebe et al., 2021), and job fairs raise contact between workers and firms and shift beliefs (Abebe et al., 2025). More intensive packages combining training, capital, and labor-market support have produced longer-run employment gains (Alfonsi et al., 2020; Bandiera et al., 2025). Average employment effects are nonetheless modest and heterogeneous (McKenzie, 2017; Card et al., 2018), and a recurring pattern is movement in search without movement in jobs: matching assistance in Jordan did not raise employ-

ment because young people declined opportunities they judged unsuitable (Groh et al., 2015), a digital job platform in India raised reservation wages and reduced short-run employment (Kelley et al., 2024), and a job fair in Egypt raised attendance without a detectable employment effect (Crépon et al., 2021). What this paper adds is a case in which a short, low-intensity intervention, one session, no subsidy, no training, no change in the set of jobs available, raises concrete action by a sixth of the assigned sample, and does so twice, on separate enrollees under separate randomizations. That is a large behavioral response to information alone, and it shows that access to specific opportunity information materially affects search behavior in this setting. The same evidence bounds what information accomplishes. The action response does not carry into employment over two months, and the confidence intervals rule out employment gains larger than a few percentage points. The wedge between the two is sharper here than in designs where a weak action response leaves the employment null uninformative, although the experiment does not identify which of the later constraints prevents action from becoming employment.

Several constraints that could account for that wedge are well documented for women and lie outside the information margin. Egyptian jobseekers place substantial weight on non-wage attributes, and women are particularly sensitive to long commutes and value flexible schedules and on-site childcare (Feld et al., 2020); qualitative evidence shows them weighing pay, hours, informality, workplace safety, and social suitability (Barsoum, 2019). The private-sector jobs most available are often informal and insecure, so non-employment reflects the supply of acceptable work and not only awareness of vacancies (Barsoum, 2015; Krafft and Armas Montalvo, 2026). Mobility is a further barrier: better transport raised women’s employment in Lima (Martinez et al., 2020), commuting services raised their applications in urban Pakistan (Garlick et al., 2025), and women in Cairo respond more than men to lower travel costs (Christensen et al., 2023). Marriage and family formation shape women’s employment trajectories in Egypt (Assaad et al., 2022; Selwaness and Krafft, 2021), and norms constrain work through expectations about reputation and appropriate behavior (Jayachandran, 2021). Consistent with all of this, providing job-matching assistance and childcare subsidies to mothers in Cairo changed take-up but produced no appreciable change in employment (Caria et al., 2025). The same features bear on the second margin: where a daughter’s independent contact with an employer is costlier than a son’s, the household context in which an opportunity arrives may matter for her even where the information itself does not. That men and women in this sample respond to the information alike, despite the women arriving with a fraction of the men’s prior work experience, narrows what can be attributed to differential responsiveness to

information in accounting for the gender gap here.

Several of these constraints plausibly operate at least partly inside households, and households do not behave as single decision-makers: members hold different preferences and exert different influence over choices (Chiappori, 1992; Lundberg and Pollak, 1993; Udry, 1996), parents shape the preferences their children acquire (Bisin and Verdier, 2001; Fernández et al., 2004), and parents’ beliefs about their children can be wrong in ways that change household investment when corrected (Dizon-Ross, 2019). Crucially for the design question here, several experiments show that the configuration in which a service is delivered matters as much as its content. Varying spousal observability changes financial decisions in the Philippines (Ashraf, 2009), offering family-planning services jointly rather than privately changes contraceptive choices in Zambia (Ashraf et al., 2014), and randomizing which parent attends an otherwise identical child-health class in Uganda changes what the household does afterwards (Björkman Nyqvist and Jayachandran, 2017). Interventions that engage families around women’s work have changed parents’ information about nearby factory jobs (Makino, 2024), husbands’ beliefs about the acceptability of female employment (Bursztyn et al., 2020), and communication within the household (Kala and McKelway, 2025), with persuasion-based approaches finding no effect (Dean and Jayachandran, 2019). Each of these varies what a family member is given, told, or believes. Holding the employment service, its content, and its recipient fixed, and varying only which parent is present when the youth receives it, isolates a design parameter that programs already control but rarely consider. We are not aware of a previous employment experiment that varies this parameter while holding the service and its recipient fixed, or of a previous experimental comparison between delivering the same employment service with a father and with a mother. The parental margin complements the information result: holding the informational content fixed, the suggestive daughter estimates indicate that short-run responses can also depend on the household configuration in which the information is delivered. For work on household decision-making, the results indicate that co-presence can matter for daughters even though parents receive no separate treatment, and without requiring a specific bargaining interpretation. The design does not identify whether co-presence operates through information sharing, endorsement, coordination, or permission, and a set of exploratory tests spanning job attributes, education, baseline attitudes, and local marriage transfers does not isolate a coherent moderator. For program design, the lesson is not that parents should be included, but that who attends is an experimentally consequential choice that can be tested at low cost.

Rural Upper Egypt suits this question, and the features that make it suitable also mark the limits of the answer. Young people in these villages typically live with their

parents until marriage, parents are routinely consulted before major decisions, and the practical costs of a daughter contacting an employer or commuting to work are borne partly by the household, which makes the margin salient enough to detect. The sample is correspondingly specific: unemployed youth who wanted to work and lived with both parents, observed over seven to eight weeks, long enough to capture search behavior and interviews, too short for employment effects that operate through slower channels. The underlying question is not specific to Egypt. Wherever young people’s economic decisions are embedded in households, treating the jobseeker as the unit of intervention is an assumption rather than a fact, and it is one that can be tested by changing who is invited to the session.

2 Setting and Experimental Design

2.1 Setting and Sample

The study took place in 287 villages across six governorates of Upper Egypt: Beni Suef, Fayoum, Minya, Assiut, Sohag, and Qena. Of these villages, 227 appear in Experiment 1, 158 in Experiment 2, and 98 in both. The area is largely rural, with agricultural communities and limited formal employment. Female labor-force participation in Egypt is low and has been falling (World Bank, 2024), and the private-sector jobs most available to young people in this region are often informal, with weak contractual protection and limited social insurance, one reason non-employment here reflects the availability of acceptable work and not only awareness of vacancies (Barsoum, 2015; Krafft and Armas Montalvo, 2026).

Two features of the setting make the household the relevant unit for a delivery experiment. The first is co-residence: young people in this region typically live in the parental household until marriage and often afterwards, so the decision to pursue a vacancy is normally taken in a household where parents are present and informed. The second is that parents in this sample hold views that bear directly on whether a daughter can act on an opportunity. In the baseline parent modules, 83.3 percent of fathers and 83.8 percent of mothers said they would not agree to a daughter relocating for work, against 23.4 and 25.4 percent for sons. Among youth who had previously been offered a job they judged unsuitable, 8.2 percent of daughters cited their father’s refusal and 6.1 percent their mother’s, against 1.4 and 0.8 percent of sons, and 13.8 percent of daughters said the job was not socially acceptable against 6.2 percent of sons (Appendix Table C2). These are baseline descriptions of the environment into which job information arrives,

not treatment effects, and they do not establish that parental attitudes cause the employment gap. They do indicate that whether a parent knows about an opportunity, and whether the youth’s interest in it is visible to them, is unlikely to be irrelevant in this population.

Eligible youth were identified through door-to-door baseline surveys. Eligibility required that respondents be aged 18–35, currently unemployed, interested in finding work, resident in a targeted rural area of the study governorates, and living with both parents, with both parents present at the time of the intervention. The last criterion is required by the treatment itself, since the parental arms need both parents available at assignment.

The analytical population is constructed in two steps. The raw survey file contains 20,662 records across three mutually exclusive randomizations. Of these, 2,915 are married women assigned to a separate, simultaneous experiment on husband involvement; they carry their own randomization variable and are not part of this study, leaving 17,747 records belonging to Experiments 1 and 2. Excluding 5,966 pilot-phase records collected before August 1, 2024, for which adherence to the data collection protocol could not be verified, leaves the analytical population of 11,781 youth. Both are design exclusions rather than attrition, and the pilot exclusion falls almost entirely on Experiment 1; Experiment 2 began in September 2024 and loses no observations to it.

Table 2 summarizes the sample. It is 62.7 percent male, with a mean age of 23.9 years, and village size ranges from 1 to 292 eligible youth. The baseline instrument records the last educational stage a respondent reached or enrolled in rather than completed. On that measure women in this selected jobseeker sample are more educated than men: 42.1 percent of women reached university or above against 17.2 percent of men. They nonetheless arrive with far less exposure to work. Asked whether they had ever worked, 70.6 percent of men said yes against 29.1 percent of women. The forty-point gap in prior work experience is among the largest gender differences in the baseline data, and it cannot be attributed simply to lower educational attainment among the women in this sample. Because every participant was unemployed and expressed interest in working at enrolment, differences across arms cannot reflect differences in stated interest in work at entry.

2.2 Experimental Design

Youth were randomly assigned at the individual level, stratified by village, in two separately randomized experiments. Experiment 1 assigned 7,912 youth to a control group receiving no job information ($N = 2,028$), a youth-only arm receiving the tailored infor-

mation privately ($N = 1,978$), a father arm ($N = 1,973$), or a mother arm ($N = 1,933$). Experiment 2, fielded subsequently among a separate group of enrollees, assigned 3,869 youth to its own control group ($N = 1,294$), its own youth-only arm ($N = 1,283$), or a both-parents arm ($N = 1,292$) in which the session included the father and the mother together. Each experiment therefore carries its own control and youth-only groups; pooling across the two, 3,322 youth were assigned to control and 3,261 to youth-only delivery. Table 1 sets out the full allocation.

The design yields two estimands. The first is the effect of receiving tailored job information, identified by comparing the youth-only arm with the control group. The second is the effect of parental co-presence conditional on receiving the same service, identified by comparing each parental arm with the youth-only arm of its own experiment. Individual assignment within villages means both are identified from within-village variation.

Because the two experiments were randomized separately, they are estimated separately throughout and no specification pools observations across them. The father and mother arms are jointly randomized within Experiment 1, so the direct father–mother comparison is a valid experimental contrast. The both-parents arm exists only in Experiment 2 and is therefore compared only with that experiment’s own youth-only group; the design does not support a causal comparison of joint with single-parent involvement. Baseline characteristics are generally balanced across randomized arms within each experiment. Of the 55 arm-versus-control comparisons reported, eleven baseline variables crossed with three treatment contrasts in Experiment 1 and two in Experiment 2, one is significant at the 5 percent level and two more at the 10 percent level, which is what chance would produce. Randomization validity rests on the design rather than on these tests; Appendix Tables A1 and A2 report the full statistics.

2.3 The Information Intervention

Before delivery, project staff mapped available vacancies at the village, district, and governorate levels, cataloguing positions by type, sector, suitability, and accessibility. Vacancies entered the pool only after staff contacted the employer and recorded the post’s title, location, schedule, pay, required qualifications, and a named contact with a telephone number, so the opportunities presented were live openings rather than advertisements of uncertain status. For each youth assigned to a treated arm, staff then selected up to three jobs using baseline information on geographic accessibility from the youth’s village, schedule compatibility with household responsibilities, and the match between the youth’s qualifications and job requirements. If the youth judged none of the initial three

suitable, staff offered two additional leads. For each opportunity the youth received the job title and description, the employer’s name and location, the required qualifications, the work schedule, and the compensation structure.

The leads were therefore concrete and actionable, and their content was substantial. Treated youth received 3.4 leads on average, four in five of which carried a listed salary, with a median listed monthly wage of 5,000 EGP for young men and 3,500 EGP for young women. Roughly six in ten of the leads offered to young women carried health and social insurance, against about eight in ten of those offered to young men, and 91.7 percent of women’s leads were inside their home governorate against 72.8 percent of men’s (Appendix Table C5). Because matching was tailored to each youth’s stated constraints, these differences reflect the matching process and the local opportunity structure rather than any experimental manipulation, and they are not treatment effects.

The baseline visit followed a fixed protocol. Enumerators first interviewed the youth alone, then the father and the mother separately about their attitudes toward their child’s employment. Only then, according to the randomized assignment, was the job information delivered, privately to the youth, or in a joint session of 20 to 30 minutes with the assigned parent or parents present. Co-presence is a narrow manipulation. In the parental arms the same enumerator presented the same leads to the same youth with the assigned parent seated in the session. Parents received no separate briefing, no training, no persuasion or encouragement message, no transfer, and no information the youth did not receive at the same moment. Nothing was withheld from parents of youth-only participants either: they were simply not present, and the youth was free to tell them afterwards. What randomization changes is whether the household learns about these specific opportunities at the same time as the jobseeker and in the same room. The matching procedure, the content of the leads, the enumerator protocol, and the presence of the youth are common to all treated arms.

2.4 Outcomes and Follow-Up Surveys

Follow-up surveys were conducted by phone approximately two weeks (FU1), four weeks (FU2), and seven to eight weeks (FU3) after the information session. Enumerators attempted each respondent up to seven or eight times before recording non-response. Response rates are high: 99.0, 95.7, and 89.1 percent across the three rounds for the youth-only and control sample, and 98.5, 95.0, and 88.6 percent for the sample of information-receiving arms. Attrition is slightly higher for women than men, about 11 against 9 percent between the first and final rounds. We find no detectable evidence of differential

attrition by treatment arm; Appendix Table B3 reports response rates and formal tests.

At every round respondents were asked what they had done to join the labor force since the previous contact. Treated youth were additionally asked about the specific opportunities they had been offered. Our two central outcomes are an indicator for *taking a concrete job-related step*, contacting an employer, requesting information, submitting a CV or application, attending an interview, being accepted, or starting work, and an indicator for *currently working*. A third outcome records *looking or waiting without a concrete step*: the respondent reports still looking for work, or waiting for a preferred opportunity, without having contacted an employer or otherwise acted. Because every youth in the sample expressed interest in finding work at enrolment, this third category describes behavior rather than a lack of interest in working.

The difference in what the two groups could be asked determines which comparisons each outcome supports, and this shapes the analysis in two ways. First, lead-specific outcomes are not observed for control youth, who were never presented with project leads; they are undefined for that group rather than zero. Treatment–control comparisons therefore use only outcomes measured comparably across arms. At the final follow-up both groups answered a common module in which taking a concrete step and currently working are distinct outcomes, and the information estimates we report come from that round. In the first two rounds a comparable treatment–control construction restricts the recorded actions to those meaningful for respondents who never received leads, and the surviving action codes then coincide with the employment codes, so the FU1–FU2 universal indicator is numerically identical to currently working and should be read as employment. This identity is a property of that restricted construction and does not hold at FU3. Second, all arms in the parental comparisons received leads and answered the same lead-specific questions, so those comparisons use the lead-specific action outcomes at FU1 and FU2 and the common module at FU3. Appendix E documents the construction of every outcome.

An additional follow-up was conducted approximately one week after FU3. It used a substantially different measurement approach, including retrospective questions about labor-market activity over the preceding three months rather than since the last contact, and is therefore excluded from the primary analysis. Appendix F documents the measurement differences and reports the previously estimated results from that round for transparency.

3 Conceptual Framework

The path from a vacancy to a job runs through three steps. A young person first becomes aware that a particular opportunity exists. Conditional on awareness, she decides whether to take a concrete action on it: to call the employer, submit an application, travel to an interview. Conditional on acting, the search process may or may not end in employment, which depends on whether the job clears her reservation conditions, whether she can reach it, and whether the employer hires her. The two treatments in this paper operate at different points in that sequence, and keeping the steps distinct is what allows a single experiment to speak to both.

Tailored job information operates on the first step and, through it, on the second. A youth who does not know which employers are hiring, what they pay, where they are, or what they require must form beliefs about the return to searching from whatever she can infer locally. If those beliefs are pessimistic enough, the expected return falls below the cost of acting and she does not search, even when suitable vacancies exist (Caria et al., 2024). Verified, specific leads raise the perceived return by removing precisely the uncertainty that depresses it: the employer is named, the wage is stated, the location is known, and a telephone number makes contact feasible. The prediction is an increase in the probability of taking a concrete action.

What information does not obviously change is the conditional probability that an action becomes an acceptable job. That probability is governed by the distribution of available work, by the cost of reaching it, and by employers' hiring decisions, none of which the intervention touches. In a setting where much of the accessible private-sector work is informal and insecure (Barsoum, 2015; Krafft and Armas Montalvo, 2026), a youth who acts on a lead may still decline what she finds or fail to be hired. The framework therefore admits a large treatment effect on action alongside little or no employment effect at a short horizon, and the distance between the two is informative in its own right rather than a sign that the first estimate is fragile. It shows that access to concrete opportunity information matters for the decision to act, while leaving open which of the later constraints prevents that action from becoming employment.

The second treatment operates somewhere else entirely. Conditional on receiving the same leads, described in the same words by the same enumerator, parental co-presence changes nothing about the factual information the youth holds. Parents in these arms receive no separate message, no training, no resources, and no instruction; they are present while the youth is told. If the youth's decision depended only on her own information and her own preferences, attendance would be irrelevant. What it changes is the household

information environment in which the decision is taken.

Under youth-only delivery the young person observes the opportunity privately. She may describe it to her parents afterwards, but she chooses whether, when, and how to do so, and her account is not verifiable. Under co-presence the parent observes the same opportunity at the same moment. The parent sees the specificity and credibility of what is offered rather than a second-hand summary; the parent also observes the youth's immediate reaction to it. And the youth knows that the parent has seen both. Private observation becomes joint observation, and the opportunity becomes common knowledge within the household in a way that a subsequent conversation cannot fully replicate.

Co-presence can affect behavior through several economically distinct channels, which this design cannot separate. It transmits information to the parent as a by-product, which matters if parents hold inaccurate beliefs about what local work involves and would revise them on seeing concrete details (Dizon-Ross, 2019; Makino, 2024). Independently of any belief revision, joint observation makes the opportunity common knowledge, which can change behavior even when nobody learns anything new, because each party knows what the other knows. It permits immediate coordination over a particular vacancy in households where parents and children agree that work is desirable but disagree about which jobs qualify. It creates the occasion for an endorsement, or for the conspicuous withholding of one, at the moment the choice is framed rather than later. It may alter what the youth expects a parent to permit or veto, whether or not any permission is actually sought. And by making the youth's interest visible to the household, it changes the private and social cost of acting on the opportunity, which matters where visible job search carries reputational consequences (Jayachandran, 2021). These channels predict similar things about a short-run increase in action. A design that varies only attendance cannot distinguish them, and we do not claim otherwise; the paper treats them as competing interpretations and reports in Section 6 what the available evidence can and cannot say about each.

Household economics supplies a benchmark for whether the identity of the attendee should matter at all. Under a unitary household in which information passes costlessly between members and the attendee's identity carries no independent implementation or credibility role, holding the service and the information fixed implies that father and mother attendance generate similar responses: the same opportunity reaching the same household produces the same decision (Becker, 1981). Experiment 1 randomizes the two arms jointly, so that implication can be tested directly.

The auxiliary assumptions govern what a rejection would mean. Attendance by different parents could produce different responses even under common household preferences:

if information passes imperfectly from the attending parent to the rest of the household, if the two parents differ in their capacity to act on what they learn, or if the identity of the person in the room changes how credible the opportunity appears. Equality of the two arms is a benchmark implication under those assumptions, not a test of whether the household is unitary. The same benchmark implies that a second parent should not reduce the response, but the both-parents arm belongs to Experiment 2 while the single-parent arms belong to Experiment 1, so joint attendance can be compared only with youth-only delivery inside its own experiment, and no comparison in this paper speaks to whether two parents differ from one.

A large literature gives reasons to expect departures from the unitary benchmark, since household members hold different preferences, control different resources, and exert different influence over decisions (Chiappori, 1992; Lundberg and Pollak, 1993; Udry, 1996). Applied to this margin, fathers and mothers could differ because they hold different preferences over a daughter's employment, because authority over mobility and over major decisions is distributed unequally between them, because they hold different beliefs about the safety, quality, or social acceptability of particular workplaces, because they differ in their capacity to absorb the household costs that a daughter's employment implies, because they pass on what they learn differently within the household, or because their presence conveys a different kind of endorsement. These are possibilities the framework admits rather than facts this paper establishes; the design identifies whether the two arms differ, not why. One is at least observable in the baseline data: fathers and mothers report restrictions on a daughter's mobility at nearly identical rates, so any difference between the arms is unlikely to be a simple difference in stated permissiveness. The design is not built to recover bargaining weights and we do not attempt to.

Gender enters this framework for a reason specific to the second treatment rather than the first. Knowing that a suitable job exists nearby is useful to a son and to a daughter alike, so there is little reason to expect the informational return to differ sharply by gender. The cost of acting on that knowledge is another matter. Where community norms make a young woman's independent contact with an employer more costly than a young man's, where parents exercise more control over daughters' mobility than sons', and where daughters arrive with far less prior exposure to work, the household context in which an opportunity is delivered plausibly matters more for daughters even when the information itself does not. The framework therefore generates a specific joint pattern: information effects of similar size for sons and daughters, little response to parental attendance among sons, and a larger response among daughters. That combination would be hard to interpret without separating the two treatments, and it is the reason the two

experiments are more informative together than either would be alone.

Timing follows from the same logic. If co-presence works mainly by lowering the immediate cost of acting, or by resolving at that moment what the youth expects her parents to permit, the response should appear early and may fade as the household’s information about the opportunity ages and as the youth accumulates her own experience of contacting employers. If instead the two parents differ in the kind of support their attendance implies, the initial responses may be similar while their paths diverge. The experiment can estimate whether the father and mother trajectories differ; it cannot identify which of these accounts produces the difference. The final round measures outcomes with a different instrument from the first two. The level of an arm’s effect either side of that boundary combines any change in behavior with the change in measurement, so the framework’s dynamic content is best expressed as a statement about the gap between the two arms, and about how that gap moves, rather than about either arm’s own level path.

These arguments map onto a small set of tests, each of which the paper reports. Information should raise the probability of taking a concrete action relative to control, and need not raise employment over a horizon of two months. Father and mother attendance can each be tested against youth-only delivery within Experiment 1, and against each other directly, since both arms are randomized there. Attendance effects may be larger for daughters than for sons, which can be examined both in the subgroups and in direct interactions. Both-parents attendance can be tested only against youth-only delivery within Experiment 2. Whether the initial responses evolve differently across the two single-parent arms can be assessed from the change in the gap between them. Finally, if any of the channels sketched above were the operative one, responses should vary with the attributes of the job on offer, with baseline household restrictions, with education, or with the local scale of customary marriage transfers; those comparisons are exploratory, were not specified in advance, and can at best narrow the set of interpretations rather than select among them. Appendix Table F1 sets out the correspondence between the arguments above and the analyses that follow; as Section 4 notes, none of them carries confirmatory status.

4 Empirical Strategy

Treatment is randomly assigned at the individual level within villages, so the estimating equations are straightforward. For the effect of information we compare the youth-only

arm with the control group within each experiment,

$$Y_{iv} = \alpha + \beta \text{YouthOnly}_i + \gamma_v + \varepsilon_{iv}, \quad (1)$$

where Y_{iv} is an outcome for youth i in village v and γ_v is a village fixed effect, so that β is identified from variation within villages and absorbs local labor-market conditions. For the effect of parental co-presence we restrict attention to the information-receiving arms and take youth-only delivery as the reference category. In Experiment 1,

$$Y_{iv} = \alpha + \beta_F \text{Father}_i + \beta_M \text{Mother}_i + \gamma_v + \varepsilon_{iv}, \quad (2)$$

and in Experiment 2,

$$Y_{iv} = \alpha + \beta_B \text{BothParents}_i + \gamma_v + \varepsilon_{iv}. \quad (3)$$

Every arm entering equations (2) and (3) received the same tailored service, so β_F , β_M , and β_B isolate the effect of who attended the session. Since the father and mother arms are jointly randomized, $\beta_M - \beta_F$ is a valid experimental contrast, tested directly within equation (2). The parameter β_B is identified only against youth-only delivery inside Experiment 2, and we draw no comparison between it and the single-parent arms. Each equation is estimated on a single experiment: because the two experiments drew separate enrollees under separate randomizations, no specification pools observations across them, and the arms of one experiment never serve as a counterfactual for the arms of the other. All estimates are intent-to-treat, so they recover the effect of being assigned to a delivery configuration rather than of attending a session with a parent who in fact stayed for it. Standard errors are heteroskedasticity-robust, the design-aligned choice for individually randomized treatment (Abadie et al., 2023); Appendix Table B1 reports the alternative we examine, clustering on the de-identified survey submission, and shows that the central estimates and their p -values are essentially unchanged under it.

Which outcome enters each equation follows from the measurement structure described in Section 2.4. Equation (1) uses outcomes measured comparably for treated and control youth, and the estimates we report come from the final follow-up, where all arms answered the common module. Equations (2) and (3) compare arms that all received leads, so they use the lead-specific action outcomes at the first two rounds and the common module at the final round.

To describe how the parental estimates evolve, we stack the three follow-up rounds for the same respondents and interact each treatment arm with the round, retaining village

fixed effects and clustering by youth. This yields the change in each arm’s effect between the first and the final follow-up, and the change in the gap between the two arms, which is the statistic we emphasize below. One caution applies to any comparison across the final boundary. The first two rounds ask about the specific leads and the final round uses the common module, so an arm’s own change in level between them combines a change in behavior with a change in instrument. The measurement change is common to the father and mother arms, which is why the change in the gap between them, rather than either arm’s level change, is the statistic we rely on; it is less exposed to the concern but not immune to it, and we return to this in Section 5.3.

Attrition is low and we find no detectable evidence that it varies with assignment: response rates exceed 88 percent at the final round in both analysis samples, and joint tests that the parental arm indicators are zero in the response equation do not approach conventional significance at any round (Appendix Table B3). Estimates use all respondents observed in the relevant round; the trajectory model is also reported on the balanced panel of daughters observed in all three rounds as a diagnostic.

The study was not pre-registered and had no pre-analysis plan. For multiple-testing purposes we designate one primary family after the fact: the lead-specific measure of taking a concrete step at the first two follow-ups for daughters, crossed with the three parental arms, giving six hypotheses. Holm-adjusted p -values and Benjamini–Hochberg q -values for that family are reported in Appendix Table B2. Every other test reported in the paper, heterogeneity, moderators, and secondary outcomes, is exploratory, and we describe it as such.

5 Results

The two experiments identify distinct treatment margins. The first asks what tailored job information does for a young person who would otherwise not have it, and is identified by comparing the youth-only arm with a control group that received nothing. The second asks what changes when the same service is delivered in a different household configuration, and is identified by comparing each parental arm with youth-only delivery. The information comparison changes the youth’s information set; the parental comparison holds it fixed and changes the household configuration of delivery. Their estimates are therefore not competing measures of one effect, and the difference in their size, large and precise for information, smaller and weaker for co-presence, reflects the difference in what each treatment changes rather than a ranking of importance.

We report intent-to-treat estimates throughout. The information estimates come from

the final follow-up, the only round in which treated and control youth answered the same questions; the parental estimates use all three rounds, since every arm entering those comparisons received leads and answered the same lead-specific questions. Estimation samples are the analytical population of each experiment with non-missing outcomes at the relevant round, and all specifications include village fixed effects.

5.1 Information Provision

Tailored job information changes what young people do. Seven to eight weeks after the session, in the round where treated and control youth answered a common module, the share who had taken a concrete step toward employment is 16.1 percentage points higher in the youth-only arm than in the control group of Experiment 1, against a control mean of 37.8 percent, and 11.2 points higher in Experiment 2, against a control mean of 33.2 percent (Table 3). Both estimates are precisely determined, with standard errors of 1.7 and 2.1 points. Relative to the control means these are increases of roughly 43 and 34 percent in the share of youth taking a concrete step. Put differently, the treatment raised the share taking a concrete step by about 16 youth per 100 assigned in Experiment 1, after a single 20-to-30-minute session conveying information that already existed in the local labor market. The size is easier to interpret alongside what the treatment did not require: no subsidy, no training, no change in the set of jobs available, and no contact with employers on the youth’s behalf. Reports of looking or waiting without a concrete step fall by 12.5 points in Experiment 1 and 7.1 points in Experiment 2. Together, the estimates show more concrete action alongside less looking or waiting without action, rather than a general rise in reported activity. Because the two experiments enrolled different youth under separate randomizations, the second set of estimates is a replication of the first.

Employment does not follow. The estimated effect on the probability of currently working is 0.7 percentage points in Experiment 1 (SE 1.7) and 0.9 points in Experiment 2 (SE 2.0), against control means of 36.1 and 32.6 percent. Neither is statistically detectable, and the confidence intervals rule out gains larger than about five percentage points in either experiment. Two features of the setting make this less surprising than it might appear. The horizon is short, and the private-sector jobs available in these villages are frequently informal and insecure, so youth who act on a lead may still decline what they find or fail to be hired. The estimates do not establish that information has no employment effect; they establish that any effect within seven to eight weeks is small relative to the movement in concrete action. The large increase in concrete job-related action alongside the absence of a statistically detectable employment gain shows that pro-

viding information alone is insufficient to generate employment transitions over this short horizon. Youth act on the information, but those actions do not translate detectably into employment within seven to eight weeks. The experiment does not distinguish whether the remaining barriers arise from the suitability of the available work, the cost of reaching it, employer hiring decisions, the level of local labor demand, or other constraints later in the matching process.

Two further outcomes fill in the picture. Reports of not looking for work at all, and of being unable to work for family reasons, are both low at the final follow-up and do not respond detectably to the information treatment, so the movement we observe is a shift within the set of youth who were already looking for work rather than a change in who considers themselves available for work. This is consistent with the eligibility criteria, which required interest in finding work at enrolment.

The response to information is similar for men and women. At the final follow-up, the share taking a concrete step rises by 15.0 percentage points among men and 17.6 points among women in Experiment 1, and the direct information-by-daughter interaction is 0.016 ($p = 0.625$) in Experiment 1 and essentially zero ($p = 1.000$) in Experiment 2 (Table 6). We therefore do not detect differential responsiveness to this information treatment by gender. Women in this sample are more educated than men on the measure available, 42.1 against 17.2 percent reached or enrolled in university, but far less likely ever to have worked, 29.1 against 70.6 percent. That women respond to concrete job leads as strongly as men suggests that differential responsiveness to information of this kind is unlikely to explain the gap in their work histories. It does not identify what does, and it does not rule out gender differences in preferences, mobility, household constraints, or employer behavior. Appendix Tables A3 and A4 report the information estimates separately by gender.

This result stands in contrast to the parental one that follows. Men and women in these villages respond to concrete job information in much the same way; when the same information is delivered with a parent in the room, the responses stop looking alike. The two sets of estimates are therefore more informative together than either would be alone, because the contrast suggests that the gender difference in the experimental responses is more apparent in the household configuration of delivery than in responsiveness to the information itself. The parental estimates are suggestive and the interaction tests exploratory, so the contrast is indicative rather than decisive.

5.2 Parental Co-Presence

Pooling sons and daughters within each experiment, parental co-presence has effects close to zero: the Experiment 1 estimates are +0.6 points for the father arm ($p = 0.73$) and +0.2 points for the mother arm ($p = 0.90$), and the Experiment 2 estimate is -0.9 points ($p = 0.66$) (Appendix Table A5). That average conceals a difference between sons and daughters.

For daughters in Experiment 1, adding a parent to the session raises action on the offered opportunities in the short run. Two weeks after delivery, the father arm is 4.6 percentage points above youth-only delivery ($p = 0.093$) and the mother arm is 5.3 points above ($p = 0.059$), against a youth-only mean of 38.8 percent (Table 4). Relative to that base the estimates correspond to increases of roughly 12 to 14 percent in the share of daughters taking a concrete step. The informational content is held fixed across these arms, so the estimates identify the effect of changing the household configuration of delivery. Both are estimated with standard errors near 2.8 points, so effects below about five points cannot be distinguished from zero at conventional levels, and neither individually survives the Holm or Benjamini–Hochberg adjustment within the designated six-hypothesis family (Appendix Table B2). We therefore describe them as suggestive throughout. A test of equality between the two arms is not rejected at the first follow-up ($p = 0.807$) or the second ($p = 0.544$), so at the horizons where the effects are largest the design provides no evidence that it matters which parent attends.

The response appears on the margins one would expect if daughters are acting on the specific opportunities discussed in the session. The concrete-step indicator is the union of three rungs, and disaggregating it is informative about what the headline outcome represents. At the first follow-up, 26.2 percent of youth-only daughters had contacted an employer or submitted a CV, 15.3 percent had reached an interview or been rejected or declined after learning details, and 4.5 percent had been accepted or started work. The parental estimates are positive on the first two rungs, +2.7 and +2.5 points in the father arm, +3.2 and +3.8 points in the mother arm, the last of these the only rung estimate reaching the 10 percent level, and slightly negative on the third, at -0.5 and -1.2 points (Appendix Table A7). The movement is therefore concentrated in initiating contact and progressing to an interview rather than in converting to a start, which is consistent with a short-horizon response but is not decisive: the individual rungs are imprecise, they are not mutually exclusive at the youth level, and the strong rung has a base of under five percent, so it has little room to move. Both estimates are unchanged in magnitude, and their p -values barely move, when standard errors are clustered on the

survey submission rather than computed as heteroskedasticity-robust at the individual level: the father estimate stays at 4.6 points ($p = 0.099$ against 0.093) and the mother estimate at 5.3 points ($p = 0.063$ against 0.059), so neither reaches the 5 percent level under either choice (Appendix Table B1).

For sons, we detect little evidence that parental co-presence changes behavior. Across the nine parental-arm-by-round estimates reported for sons, each of the three arms at each of the three follow-ups, no coefficient is statistically significant at conventional levels, and the largest in absolute value is 2.4 points against a standard error of 2.7 (Appendix Table A6). Disaggregating by action intensity leaves the same picture, with no son estimate reaching the 10 percent level on any rung (Appendix Table A7). The direct treatment-by-daughter interactions point in the same direction as the subgroup estimates and are stronger for the mother arm: father $\times$ daughter is 0.056 ($p = 0.091$) and mother $\times$ daughter is 0.072 ($p = 0.033$) (Table 6). These interaction tests are exploratory and are not adjusted for multiple testing, so they corroborate the subgroup pattern rather than establish it independently.

In Experiment 2, involving both parents produces no detectable effect. The estimate for daughters at the first follow-up is -0.1 percentage points ($p = 0.969$, $N = 1,035$), with a confidence interval running from roughly -6 to $+6$ points, and the estimates at the two later rounds are similarly small. The data are therefore consistent with modest effects of either sign but not with a large one. Because the both-parents arm was randomized in a separate experiment, the comparison is with youth-only delivery inside Experiment 2, and nothing here speaks to whether involving two parents differs from involving one. The null does limit a simple interpretation in which involving family members in employment-service delivery is generally sufficient to change daughters' responses.

5.3 Dynamics

The two single-parent arms start together and then separate. Table 5 reports the direct tests and Figure 1 plots the estimates with confidence intervals. In a model that stacks the three follow-ups for the same respondents and interacts each arm with the round, the mother-arm effect declines by 10.8 percentage points between the first and the final follow-up ($p = 0.004$), while the father-arm estimate shows no statistically detectable comparable decline (-2.6 points, $p = 0.481$). The gap between the two arms moves from 0.7 points at two weeks to -7.3 points at seven to eight weeks ($p = 0.015$), and the change in that gap is -8.2 points ($p = 0.029$). A joint test on all arm-by-round terms is rejected at $p = 0.022$. Restricting to the balanced panel of 1,725 daughters observed in

all three rounds, the pattern is preserved and the joint test remains at the conventional threshold ($p = 0.050$).

Figure 1 makes the pattern visible. The two arms' estimates sit almost on top of each other at two weeks, with overlapping confidence intervals, and separate by the final round, with the mother-arm estimate falling below zero and the father-arm estimate remaining slightly above it. Both intervals include zero at the final round.

Two cautions govern the interpretation. First, neither arm's effect is individually distinguishable from zero at the final follow-up: the father-arm estimate is +3.7 points ($p = 0.220$) and the mother-arm estimate is -3.6 points ($p = 0.225$). What the data identify is a divergence between two trajectories, not a lasting effect of paternal involvement. Second, the final round used a common survey module while the first two used lead-specific questions. The change applies to both arms, so the between-arm gap, and especially the change in that gap, is less vulnerable to this concern than either arm's own level change, though not immune to it, and we therefore do not read either arm's level at the final follow-up as a clean continuation of its earlier estimate. We find no evidence that differential attrition explains the pattern, joint tests that all parental arm dummies are zero in the response equation yield p -values of 0.819, 0.570, and 0.221 across the three rounds, and little indication that changes in outcome variance do, since round-specific outcome variances remain in a narrow range across arms and rounds (Appendix Tables B3 and B4).

Two interpretations are consistent with the delayed divergence, and they carry different implications. Under the first, the initial responses are genuinely alike, as the equality tests at two and four weeks indicate, and what differs is how quickly each fades, so that the divergence reflects the durability of whatever co-presence supplies rather than its initial strength. Under the second, the arms differ from the start in ways the first-round estimates are too imprecise to detect, and the later rounds simply make an existing difference visible. Our estimates cannot separate these, and the change in outcome measurement at the final round is a further reason for caution. What the data do show is that the two arms' estimated paths differ over the horizon we observe: a design that involved whichever parent was available, treating fathers and mothers as substitutes, would obscure a difference that is statistically detectable by the final round. That is a fact about the treatment, not about which parent is preferable, since neither arm's effect is distinguishable from zero at that point.

5.4 Employment

Neither experimental margin produces a statistically detectable employment effect within the window we observe, and the two nulls carry different weight. The information estimates reported above are precise, 0.7 and 0.9 percentage points against standard errors under two points, so they rule out gains larger than about five points and constitute a finding rather than an absence of one. The parental estimates are far less precise. For daughters in Experiment 1 the estimated effects on currently working are -0.7 , -1.6 , and $+1.2$ percentage points for the father arm across the three rounds, and -0.2 , $+0.3$, and $+0.6$ points for the mother arm; in Experiment 2 the both-parents estimates are -0.7 , $+0.8$, and -1.4 points (Table 4). Against a youth-only employment rate of 17.0 percent at the final follow-up, the confidence intervals admit effects of roughly five to six points in either direction, so the estimates are informative about large effects and uninformative about small ones. The daughter subsamples are about a third of each experiment, and this limited precision, rather than a tightly estimated zero, is what the employment results reflect.

Both experiments therefore show movement in early labor-market behavior, concrete action in the case of information, action on specific opportunities among daughters in the case of parental co-presence, without a detectable change in employment inside seven to eight weeks. The wedge is established with confidence for information and only suggested for co-presence.

Three features of the setting bear on how much this should surprise. The horizon is short: a youth who contacts an employer in the fortnight after the session may still be waiting on an interview outcome when the final round is fielded, and the action-intensity pattern above is consistent with movement concentrated before the hiring decision. The employment measure counts any paid work, including informal and temporary jobs, so it is not that only formal placements would register. And the opportunities themselves are constrained: as the setting section notes, the leads on offer are frequently informal and, for young women, less likely to carry insurance, so a youth who acts may still decline what she finds or not be hired. None of this is testable here. The estimates show a large, precisely estimated behavioral response alongside a small, imprecisely estimated employment response. Other job-search experiments report the same combination where the available jobs are thought not to clear workers' reservation conditions (Groh et al., 2015; Kelley et al., 2024; Crépon et al., 2021), though our design cannot confirm that this is the reason here.

6 Additional Evidence

Two questions remain from the results: why parental co-presence appears to shift daughters' short-run behavior, and why the two single-parent arms diverge. We ran a set of exploratory tests looking for a moderator that would organize the pattern. None does. We report them because the absence of a coherent moderator is itself part of what the paper can say, and because each test is informative about a specific story even when it cannot adjudicate among them. All of these analyses are exploratory and none is adjusted for multiple testing; Appendix C reports them in full.

We first examine whether the response varies with the characteristics of the jobs on offer. If co-presence works by resolving disagreement about which jobs are acceptable, its effect should depend on the attributes of the opportunity being discussed. This is the dimension on which a disagreement story is most naturally tested here, because the leads differ substantially in pay and formality, and because the baseline instrument elicited minimum acceptable salaries separately from the youth and from each parent, so household members could and did disagree about what counts as an acceptable job. Interacting the parental arms with the salary attached to the offered opportunity, and separately with whether it carries social insurance, produces no detectable gradient in either case (Appendix Table C9). The subgroups defined by explicit salary and mobility mismatch between parent and youth are too small to be informative. The absence of a gradient tells against a story in which co-presence works mainly by reconciling stated job criteria; it does not rule that story out, since the relevant disagreement may concern attributes we do not measure.

We next examine heterogeneity by baseline household restrictions. If co-presence relaxes a constraint that parents would otherwise impose, the effect should be larger where that constraint binds. Two baseline measures speak to this. Splitting daughters by whether they cited any family or social barrier at baseline, the estimates are small in both groups and the high-barrier cell is small: 358 daughters at the first follow-up against 1,031 in the low-barrier group, with standard errors around three to five points (Appendix Table C4). A separate check using baseline parental approval of a daughter's relocation for work finds the effect concentrated among daughters whose parents had approved, but it rests on 51 and 64 treated daughters in the non-approval cells and was not pre-specified. Neither is powered to distinguish a permission story from the alternatives, and we do not use either to do so.

A third set of tests considers education and family background. If the constraint on daughters' action is tighter in less-educated households, education should moderate

the effect. Across daughters' own education, mothers' education, and fathers' education, we find no systematic evidence of moderation (Appendix Table C3). No interaction is distinguishable from zero once the full set is considered. The subgroup point estimates for more-educated daughters are larger, but the interactions, which are the relevant test, do not support an education gradient.

We then examine whether responses vary with local marriage-transfer norms. If the constraint reflects the cost of daughters' visible search in the local marriage market, the local scale of customary marriage transfers should moderate the effect. We match the study villages to an independent village survey covering 229 of them, which records two customary transfers made by the prospective groom's side at marriage: the Mahr, a monetary transfer promised or paid by the groom to the bride, and the Shabka, the customary gift of gold jewelry, recorded in the source as the average value of marriage gold. Mahr is a precise null moderator of both parental effects, with interaction estimates of -0.010 for the father arm and $+0.014$ for the mother arm per 100,000 EGP and confidence intervals that exclude economically meaningful gradients; the null holds under logarithmic and above-median specifications, after dropping villages with extreme values, and under village-clustered inference (Appendix Table C6). The Shabka and the combined Mahr-and-Shabka measure yield one movement, a both-parents interaction in Experiment 2. We treat it as exploratory: it does not survive the designated multiple-testing adjustment, it does not replicate in the larger Experiment 1 sample, its magnitude moves substantially when single governorates are dropped, and the below- and above-median Shabka categories are each concentrated in a few governorates, so the interaction is not separable from governorate-level differences (Appendix Tables C7 and C8).

Two further checks bear on what the session changes: whether co-presence alters how youth judge the jobs on offer, and whether it alters how often families are cited as an obstacle. Parental presence does not detectably alter how youth assess the suitability of the offered jobs: no coefficient in the lead-level suitability analysis approaches conventional significance, the smallest p -value being 0.21. We find no detectable evidence that parental co-presence changes youth assessments of job suitability. Nor does co-presence change how often daughters give family prevention or guardian refusal as the reason for inaction. That outcome is rare, the youth-only rate is zero at the first follow-up and 2.3 percent by the last, and the parental estimates are within a percentage point of zero at every round (Appendix Table C1). Its base rate is too low to support inference, so this is a statement about what the data can show rather than evidence against a permission story.

Finally, we examine whether the response extends beyond the opportunities presented.

In the appendix specifications that use the general engagement measure rather than the offered leads, parental co-presence does not detectably raise daughters' broader labor-market activity. The detectable response appears in action on the specific opportunities discussed in the session rather than in the broader measure. That narrows the set of stories consistent with the data, it is hard to reconcile with co-presence producing a general change in how daughters approach the labor market, without selecting among the stories that remain.

Taken as a set, the additional evidence does not identify a mechanism. Every channel sketched in Section 3, information reaching the parent, joint observation and common knowledge, coordination over a particular vacancy, endorsement, expectations about permission, and the cost to the daughter of acting visibly, remains consistent with what we observe, and we present none of them as established. What the exercises do indicate is narrower and still useful: the detectable response is confined to the opportunities discussed, it does not track the job attributes or the household and village characteristics that the most natural mechanical stories would predict, and it is not accompanied by a detectable change in how families are cited as an obstacle.

7 Discussion and Conclusion

Two experiments in rural Upper Egypt show that tailored job information substantially raises the share of young people taking a concrete step toward employment and that, within seven to eight weeks, none of that increase shows up as employment. The gap is large: a 16 percentage point rise in the share taking a concrete step against an employment estimate of less than one point, with a confidence interval that excludes gains of more than a few points. The result is consistent with a growing body of evidence that search and matching interventions move intermediate behavior more readily than jobs (Groh et al., 2015; Kelley et al., 2024; Caria et al., 2025), and it narrows the set of plausible explanations. The information treatment substantially increased concrete action, but the additional actions did not translate into detectable employment gains within the observed horizon. The results therefore point to constraints beyond information provision in the transition from action to employment.. Job suitability, mobility, hiring frictions, and labor demand are all candidates, and the evidence already cited makes each plausible: young people in Jordan turned down matches they judged unsuitable (Groh et al., 2015), Egyptian women weigh commutes and schedules heavily when evaluating work (Feld et al., 2020), and the jobs most available in this region are informal and insecure (Barsoum, 2015; Krafft and Armas Montalvo, 2026). The experiment does not distinguish among them,

and the short horizon means we cannot rule out employment effects that materialize later.

A common reading of information interventions is that they relax a friction which, once relaxed, allows matches to form. Providing concrete information here substantially changes job-related behavior, the response is large, precise, and replicated across two independent randomizations, yet no statistically detectable employment gain follows within two months. The evidence therefore indicates that information alone is insufficient to generate employment transitions over this horizon, while leaving open which additional constraints limit the translation of search into work. That is a more informative starting point for design than a null information effect would have been, and it suggests that pairing information with instruments acting further along the hiring process is the more promising question. It also cautions against reading intermediate outcomes as leading indicators: a program evaluated at two weeks on concrete action would have looked highly successful here. the intervention substantially changed the transition from awareness to action while producing no detectable change in employment over the observed horizon. Two features make that reading unusually clean. The action response is estimated twice, on separate enrollees under separate randomizations, so it does not rest on one sample. And it is large and precise rather than marginal, which means the employment null is informative: in designs where the behavioral response is itself weak, an employment null cannot distinguish a downstream constraint from an intervention that simply did little.

The second experimental question concerns a different margin, and the parental results speak to it independently of the first. Holding the service and its recipient fixed, changing who else is in the room shifts what daughters do with the information in the short run, by roughly five percentage points in both single-parent arms, with no detectable response among sons. These estimates are suggestive rather than decisive: they do not individually survive adjustment for multiple testing, and the daughter subsamples are roughly a third of each experiment, which limits precision. The more precisely estimated evidence concerns the difference between the two arms' trajectories. Father and mother co-presence produce statistically indistinguishable effects at two weeks, and their estimated trajectories subsequently diverge significantly, with neither arm's effect distinguishable from zero at the final follow-up. The data therefore support a statement about how the two arms differ from each other, not a statement that either parent's involvement has a lasting effect.

The two treatments also differ in how they interact with gender, and the contrast is more informative than either result alone. Women respond to the information itself about as strongly as men do: the estimates by gender are similar and the direct interactions are close to zero. Given that the women in this sample have reached higher educational stages than the men while being far less likely ever to have worked, a differential response to job

information would have been an obvious candidate explanation for that divergence, and we do not find one. What does differ by gender is the response to the household configuration in which the information arrives, which is detectable for daughters and not for sons.

One reading is that community norms make a daughter’s independent contact with an employer costlier than a son’s, so that a parent’s presence changes the private cost of acting; parents may also learn something about the specific opportunity, coordinate with the daughter over which jobs are acceptable, or signal approval. We cannot distinguish these accounts, and the exploratory tests in Section 6 do not favor any of them. Nor should the pattern be read as explaining the gender gap in employment: the parental estimates are suggestive, the interaction tests are exploratory, and the experiment measures short-run action rather than employment. The defensible statement is narrower. The household context of employment decisions may matter for how young women respond to job opportunities, even though the experiment does not identify the underlying constraint, and it appears to matter more for them than for young men in the same villages.

For household economics, the finding is narrow but clean. Most evidence that households do not act as single decision-makers comes from manipulating what members control or know: resources are assigned to one member rather than another, information is given privately or jointly, or communication is structured. The manipulation here is smaller than any of these. The young person receives the same service in every treated arm; parents are given no information, no resources, and no instruction; and the only thing that varies is whether a parent is in the room while the youth is told about jobs that already exist. That a randomized change of this size moves daughters’ short-run behavior at all is the substantive point, because it identifies a delivery margin that existing designs do not isolate. Studies that vary spousal observability (Ashraf, 2009), joint versus private access to a service (Ashraf et al., 2014), or which parent receives an intervention (Björkman Nyqvist and Jayachandran, 2017) all change what a household member is given; this design changes only who witnesses the delivery.

The corresponding limit is equally clear. Because co-presence bundles information transmission, common knowledge, coordination, endorsement, and perceived acceptability into a single treatment, a positive effect cannot be attributed to any one of them, and we do not attempt to estimate bargaining weights or to test a specific model of household allocation. The exercises in Section 6 were designed to find a moderator that would favor one interpretation, and none does. The household configuration of delivery is an experimentally relevant object, and identifying why will require a design that varies the channels separately, for example by manipulating what parents are told independently of whether they attend.

Placing the result against the closest experiments clarifies what is new and what is not. Four studies engage families around women’s work, and each varies something this design holds fixed. Makino (2024) gives parents of unmarried young women information about nearby factory employment and improves their stated attitudes; the treatment changes what parents know before the daughter acts, and the outcome is the parents’ views rather than the daughter’s search. Bursztyn et al. (2020) corrects husbands’ beliefs about how other men regard female employment, which raises wives’ applications and interviews; the object manipulated is a belief about the community, not information about a specific job. Kala and McKelway (2025) structures communication inside the household and raises women’s labor supply, changing the process by which the household talks rather than the setting in which an opportunity is presented. Dean and Jayachandran (2019) uses videos and facilitated conversations to make family members more supportive and finds no effect on attitudes or employment. Outside labor markets, Björkman Nyqvist and Jayachandran (2017) randomize which parent attends an identical child-health class, which is the closest analogue to the margin here, and Ashraf (2009); Ashraf et al. (2014) vary whether a spouse can observe or jointly access a service. Against this set, the present design is distinctive in three respects: the service is a concrete, individually tailored employment product rather than a message or a norm; the young jobseeker is present in every arm, so nothing is delegated to a family member; and the only randomized difference is attendance. The design isolates the delivery margin cleanly but does not distinguish the mechanisms through which parental presence operates: because parents receive nothing of their own, we cannot identify what their presence supplies, whereas Makino can attribute a change to information and Bursztyn and coauthors to belief correction.

The paper’s contribution to the design of employment programs is correspondingly specific. It is not that parents should be included in employment services: the evidence does not establish which parent to involve, whether the short-run response persists, or whether involving parents would be cost-effective at scale, and in the second experiment involving both parents produced no detectable effect at all. It is that the household configuration of service delivery is itself a design parameter with measurable consequences, and one that programs already control at essentially no cost. Where employment decisions are embedded in households, who attends is a treatment, and it can be tested as such.

Several limitations bound these conclusions. The two parental designs are separate experiments, so no causal comparison between single-parent and both-parent involvement is available. The initial daughter estimates are suggestive and do not individually survive the designated multiplicity adjustment. The final follow-up used a different outcome instrument from the first two rounds, which is why we emphasize the change in the

between-arm gap rather than level changes across that boundary. The horizon is short, and the employment measure captures any paid work, including informal and temporary work. The sample is unemployed youth living with both parents in rural Upper Egypt who wanted to work, so the results speak to a population in which household involvement in economic decisions is the norm rather than the exception. And the mechanism behind the parental response remains unidentified.

The broader lesson is about the unit at which employment interventions are designed and evaluated. Programs treat the jobseeker as the decision-maker, and for the information itself that assumption appears adequate here: young men and young women respond to concrete job leads in much the same way. But for daughters, changing nothing about the service except who was present when it arrived changed what they did with it. Moving early engagement, however, is not the same as moving employment. Understanding which downstream constraints prevent increased search from becoming work, and whether household involvement can be structured to help clear them, is where this line of work should go next.

Table 1. Experimental Design

Arm	Receives job information	Present at delivery	<i>N</i>
<i>Panel A. Experiment 1</i>			
Control	No	,	2,028
Youth only	Yes	Youth	1,978
Youth + father	Yes	Youth, father	1,973
Youth + mother	Yes	Youth, mother	1,933
<i>Total</i>			<i>7,912</i>
<i>Panel B. Experiment 2</i>			
Control	No	,	1,294
Youth only	Yes	Youth	1,283
Youth + both parents	Yes	Youth, father, mother	1,292
<i>Total</i>			<i>3,869</i>
Analytical population			11,781

Notes: Randomization was at the individual level, stratified by village, in two separately randomized experiments drawing separate enrollees; the analytical population is 11,781 youth. The tailored job information, the matching procedure, and the presence of the youth are identical across the treated arms of an experiment, so each parental arm differs from youth-only delivery only in who else attended the session. The effect of information is identified by youth only versus control within an experiment; the effect of parental co-presence by each parental arm versus youth only within its own experiment. Because the experiments were randomized separately, the both-parents arm is compared only with the youth-only arm of Experiment 2, and no estimate pools observations across experiments. Each experiment has its own control and youth-only groups, reported within its panel; pooling across experiments the study contains 3,322 control and 3,261 youth-only assignments.

Table 2. Sample Characteristics

	Men (1)	Women (2)
<i>Panel A. Baseline characteristics</i>		
Age (mean, years)	24.7	22.4
Currently married (%)	29.6	1.5
Reached at least secondary stage (%)	79.3	89.8
Reached or enrolled in university (%)	17.2	42.1
Ever worked (%)	70.6	29.1
Observations	7,384	4,397
<i>Panel B. Treatment assignment, both experiments</i>		
	N	Share (%)
Control	3,322	28.2
Youth only	3,261	27.7
Youth + father (Experiment 1)	1,973	16.7
Youth + mother (Experiment 1)	1,933	16.4
Youth + both parents (Experiment 2)	1,292	11.0
Total	11,781	100.0

Notes: Unemployed youth aged 18–35 who were interested in finding work and living with both parents at enrolment, in 287 villages across six governorates of Upper Egypt. The sample is 62.7 percent male with a mean age of 23.9 years. Education records the last stage reached or enrolled in, not necessarily completed; “ever worked” records whether the respondent had ever done any work before the baseline survey. The analytical population of 11,781 is obtained from 20,662 raw records by removing 2,915 married women assigned to a separate husband-involvement experiment and 5,966 pilot-phase records collected before August 1, 2024; Appendix Table F2 reports the sample flow. Follow-up response rates appear in Appendix Table B3.

Table 3. Effects of Information Provision at the Final Follow-Up

	Experiment 1		Experiment 2	
	Control mean (1)	Youth only (2)	Control mean (3)	Youth only (4)
Took a concrete step	0.378	0.161*** (0.017)	0.332	0.112*** (0.021)
Looking/waiting without a concrete step	0.492	−0.125*** (0.017)	0.549	−0.071*** (0.022)
Currently working	0.361	0.007 (0.017)	0.326	0.009 (0.020)
Observations	3,574		2,290	

Notes: OLS estimates of youth-only delivery relative to control at the final follow-up, seven to eight weeks after the session and the only round in which all arms answered a common survey module. Each regression is restricted to the indicated experiment and includes village fixed effects; heteroskedasticity-robust standard errors in parentheses. Men and women are pooled; Appendix Tables A3 and A4 report the results separately by gender. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. Parental Co-Presence and Daughters' Job Search

	2 weeks (1)	4 weeks (2)	7–8 weeks (3)
<i>Panel A. Experiment 1: father and mother co-presence</i>			
<i>Took a concrete step</i>			
Father vs. youth only	0.046* (0.027)	0.024 (0.028)	0.037 (0.030)
Mother vs. youth only	0.053* (0.028)	0.042 (0.029)	−0.036 (0.030)
Mother − father	0.007 (0.028)	0.018 (0.029)	−0.073** (0.030)
Youth-only mean	0.388	0.341	0.362
<i>Currently working</i>			
Father vs. youth only	−0.007 (0.009)	−0.016 (0.016)	0.012 (0.024)
Mother vs. youth only	−0.002 (0.010)	0.003 (0.017)	0.006 (0.024)
Youth-only mean	0.027	0.088	0.170
Observations	2,033	1,957	1,804
<i>Panel B. Experiment 2: both parents</i>			
Took a concrete step	−0.001 (0.031)	0.003 (0.032)	0.004 (0.032)
Youth-only mean	0.315	0.288	0.276
Currently working	−0.007 (0.010)	0.008 (0.018)	−0.014 (0.027)
Youth-only mean	0.025	0.055	0.168
Observations	1,035	977	942

Notes: OLS estimates for daughters, with youth-only delivery as the omitted category, so each coefficient compares joint delivery of the same tailored information with private delivery. Panel A is estimated within Experiment 1 and Panel B within Experiment 2; the two experiments were randomized separately and are never pooled, and the both-parents arm is not comparable with the single-parent arms. All arms in these comparisons received job leads and answered the same lead-specific questions at the first two follow-ups; the final follow-up uses the common module. Regressions include village fixed effects; heteroskedasticity-robust standard errors in parentheses. The mother-minus-father row is a linear test within the same regression. Neither first-round parental estimate survives the multiple-testing adjustment of Appendix Table B2. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. Direct Tests of the Father and Mother Trajectories

	Estimate (1)	SE (2)	<i>p</i> -value (3)
Mother – father, 2 weeks	0.007	0.028	0.807
Mother – father, 7–8 weeks	–0.073	0.030	0.015
Change in the gap, 2 to 7–8 weeks	–0.082	0.038	0.029
Mother, change 2 to 7–8 weeks	–0.108	0.038	0.004
Father, change 2 to 7–8 weeks	–0.026	0.037	0.481
Joint test, all arm-by-round terms			0.022

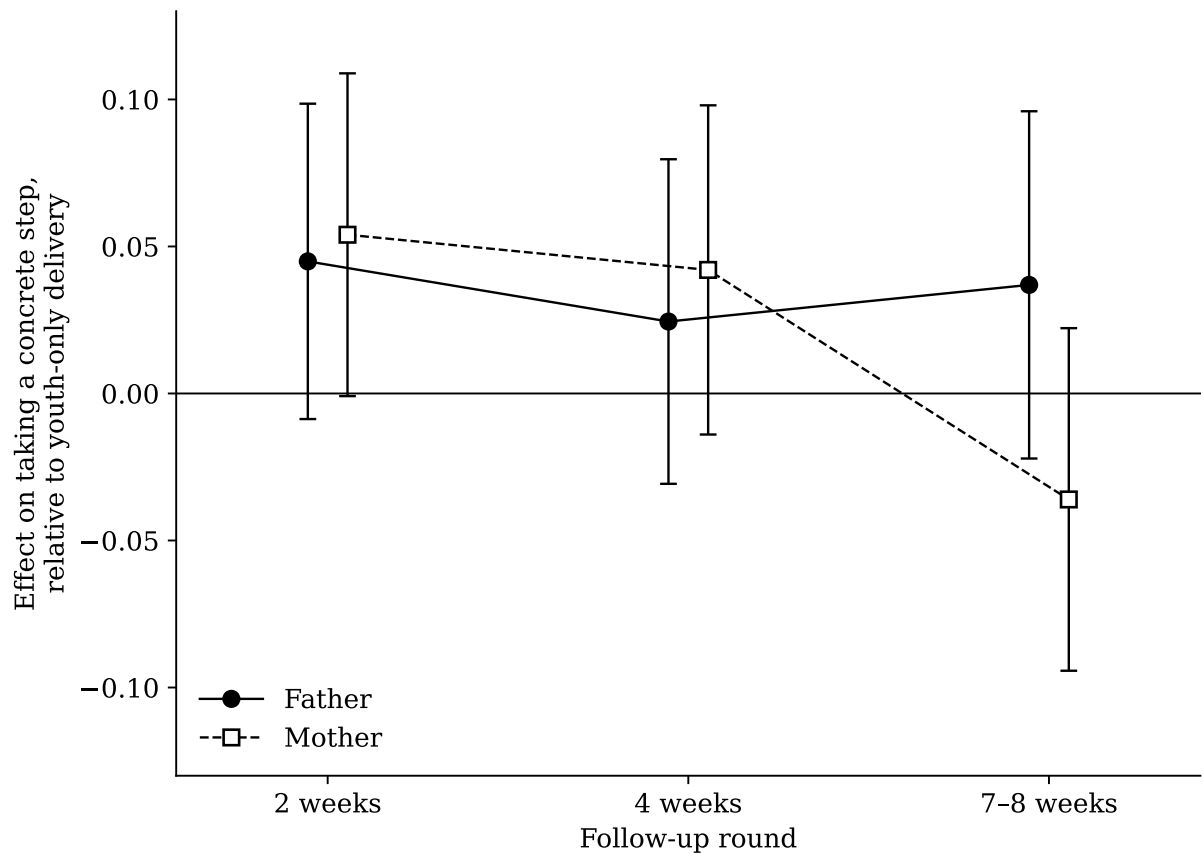
Notes: Experiment 1 daughters. The first two rows are linear tests within the round-specific regressions of Table 4. The remaining rows come from a single model that stacks the three follow-ups for the same respondents and interacts the treatment arm with the round, with village fixed effects and standard errors clustered by youth. The first two follow-ups use lead-specific questions and the final follow-up uses the common module; because that change applies to both arms, the change in the between-arm gap is less vulnerable to it than either arm’s own level change. In the balanced panel of 1,725 daughters observed in all three rounds the joint test yields $p = 0.050$.

Table 6. Treatment Effects by Gender: Direct Interaction Tests

Experiment	Interaction	Estimate (1)	SE (2)	<i>p</i> -value (3)
1	Father $\times$ daughter, 2 weeks	0.056	0.033	0.091
1	Mother $\times$ daughter, 2 weeks	0.072	0.034	0.033
2	Both parents $\times$ daughter, 2 weeks	0.030	0.039	0.452
1	Information $\times$ daughter, 7–8 weeks	0.016	0.034	0.625
2	Information $\times$ daughter, 7–8 weeks	0.000	0.040	1.000

Notes: Treatment-by-daughter interactions from experiment-specific regressions with village fixed effects and heteroskedasticity-robust standard errors. Parental interactions use the lead-specific concrete-step outcome at the first follow-up; information interactions use the common-module concrete-step outcome at the final follow-up. These tests are exploratory and are not adjusted for multiple testing.

Figure 1. Father and Mother Effects on Daughters' Action Across Follow-Ups



Notes: Experiment 1 daughters. Points are the father-arm and mother-arm estimates relative to youth-only delivery, with 95 percent confidence intervals, from round-specific regressions with village fixed effects and heteroskedasticity-robust standard errors. The change in the mother-minus-father gap between the first and final round is -8.2 percentage points ($p = 0.029$). Neither arm's effect is individually distinguishable from zero at the final round. The final round uses the common survey module; see Section 5.3.

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Appendix A Additional Experimental Results

This appendix reports baseline balance by experiment, the information results disaggregated by gender, and the parental estimates for the pooled and male samples that the main text summarizes.

Table A1. Baseline Balance: Experiment 1

<i>Panel A: Experiment 1 (each arm vs. its own Control)</i>				
	Control mean (SD)	Youth Only	Father	Mother
Age (years)	24.10 (4.81)	-0.037 (0.151)	0.138 (0.152)	0.129 (0.152)
Male	0.644 (0.479)	0.001 (0.015)	0.003 (0.015)	0.022 (0.015)
Currently married	0.207 (0.405)	0.005 (0.013)	0.012 (0.013)	0.012 (0.013)
Primary / preparatory	0.151 (0.359)	-0.021* (0.011)	0.008 (0.011)	-0.011 (0.011)
Secondary / upper intermediate	0.563 (0.496)	0.023 (0.016)	-0.006 (0.016)	0.005 (0.016)
Reached or enrolled in university	0.261 (0.439)	-0.002 (0.014)	-0.009 (0.014)	-0.001 (0.014)
Ever worked any work before	0.609 (0.488)	-0.010 (0.015)	0.018 (0.015)	0.012 (0.015)
Ever received training	0.172 (0.378)	0.003 (0.012)	-0.001 (0.012)	0.001 (0.012)
Months searching for a job	8.93 (10.86)	-0.397 (0.332)	-0.311 (0.333)	-0.064 (0.338)
Father has rejected job offers	0.049 (0.216)	-0.007 (0.011)	-0.013 (0.010)	-0.013 (0.011)
Mother has rejected job offers	0.034 (0.181)	0.000 (0.009)	-0.008 (0.009)	-0.003 (0.009)

Notes: Arms compared against Experiment 1's own Control ($N=7,912$; parental-attitude rows $N=3,218$). Village fixed effects; robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2. Baseline Balance: Experiment 2

	Control mean (SD)	Youth Only	Both Parents
Age (years)	23.16 (4.43)	0.147 (0.173)	0.132 (0.174)
Male	0.582 (0.493)	-0.005 (0.018)	-0.004 (0.018)
Currently married	0.139 (0.346)	0.020 (0.013)	0.012 (0.013)
Primary / preparatory	0.151 (0.358)	-0.022 (0.014)	-0.011 (0.014)
Secondary / upper intermediate	0.574 (0.495)	0.002 (0.019)	-0.013 (0.019)
Reached or enrolled in university	0.255 (0.436)	0.013 (0.016)	0.016 (0.017)
Ever worked any work before	0.447 (0.497)	-0.009 (0.018)	-0.004 (0.018)
Ever received training	0.100 (0.300)	0.028** (0.012)	0.020* (0.011)
Months searching for a job	9.17 (10.42)	-0.418 (0.379)	-0.045 (0.393)
Father has rejected job offers	0.089 (0.285)	-0.019 (0.023)	0.008 (0.024)
Mother has rejected job offers	0.060 (0.238)	-0.004 (0.019)	0.002 (0.019)

Notes: Arms compared against Experiment 2's own Control ($N=3,869$; parental-attitude rows $N=1,064$). The two experiments were randomized separately and are never pooled. Of 55 arm-versus-control comparisons across both panels (11 baseline variables crossed with three treatment contrasts in Experiment 1 and two in Experiment 2), three are significant at the 10 percent level, one of which is also significant at the 5 percent level, consistent with chance; the only joint test below 0.05 is ever-trained in Experiment 2 ($p=0.042$). Village fixed effects; robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3. Information Provision at the Final Follow-Up: Men

Outcome	Control mean	Youth Only effect	SE
<i>Panel A: Experiment 1</i>			
Concrete step	0.474	0.150***	(0.022)
Looking/waiting, no concrete step	0.430	-0.125***	(0.021)
Working	0.452	0.014	(0.022)
<i>N</i>		2,335	
<i>Panel B: Experiment 2</i>			
Concrete step	0.448	0.118***	(0.029)
Looking/waiting, no concrete step	0.463	-0.081***	(0.029)
Working	0.441	0.011	(0.029)
<i>N</i>		1,344	

Notes: OLS estimates of Youth Only relative to Control at FU3, the only follow-up in which all arms answered a common module; lead-specific FU1–FU2 outcomes cannot support this contrast and are not used. Estimation sample: males in the indicated experiment. Village fixed effects; robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4. Information Provision at the Final Follow-Up: Women

Outcome	Control mean	Youth Only effect	SE
<i>Panel A: Experiment 1</i>			
Concrete step	0.197	0.176***	(0.028)
Looking/waiting, no concrete step	0.607	-0.123***	(0.030)
Working	0.189	-0.008	(0.024)
<i>N</i>		1,239	
<i>Panel B: Experiment 2</i>			
Concrete step	0.168	0.140***	(0.029)
Looking/waiting, no concrete step	0.671	-0.094***	(0.034)
Working	0.164	0.036	(0.026)
<i>N</i>		946	

Notes: OLS estimates of Youth Only relative to Control at FU3, the only follow-up in which all arms answered a common module; lead-specific FU1–FU2 outcomes cannot support this contrast and are not used. Estimation sample: females in the indicated experiment. Village fixed effects; robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5. Parental Co-Presence, Sons and Daughters Pooled

	FU1: 2 weeks	FU2: 4 weeks	FU3: 7–8 weeks
<i>Panel A: Experiment 1 (reference: Youth Only)</i>			
Father vs. Youth Only	0.005 (0.016)	0.018 (0.017)	0.007 (0.017)
Mother vs. Youth Only	0.002 (0.016)	0.002 (0.017)	-0.019 (0.017)
Youth-only mean	0.441	0.467	0.538
<i>N</i>	5,830	5,669	5,218
<i>Panel B: Experiment 2 (reference: Youth Only)</i>			
Both Parents vs. Youth Only	-0.009 (0.020)	0.012 (0.021)	0.001 (0.021)
Youth-only mean	0.397	0.409	0.440
<i>N</i>	2,479	2,365	2,276

Notes: OLS estimates of Concrete step for the pooled-sex estimation sample of each experiment. The two experiments were randomized separately and are estimated separately; Both Parents is never compared with Father or Mother. Village fixed effects; robust standard errors in parentheses. FU1 uses the complete-outcome analytical flag. FU1–FU2 outcomes are lead-specific; FU3 uses the common module, so within-arm levels are not comparable across that boundary. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. Parental Co-Presence: Sons

	FU1: 2 weeks	FU2: 4 weeks	FU3: 7–8 weeks
<i>Panel A: Experiment 1 (reference: Youth Only)</i>			
Father vs. Youth Only	-0.010 (0.020)	0.009 (0.021)	-0.006 (0.021)
Mother vs. Youth Only	-0.019 (0.020)	-0.023 (0.021)	-0.008 (0.021)
Youth-only mean	0.469	0.535	0.631
<i>N</i>	3,797	3,712	3,414
<i>Panel B: Experiment 2 (reference: Youth Only)</i>			
Both Parents vs. Youth Only	-0.024 (0.027)	0.019 (0.028)	0.008 (0.029)
Youth-only mean	0.455	0.495	0.555
<i>N</i>	1,444	1,388	1,334

Notes: OLS estimates of Concrete step for the son subsample of each experiment. The two experiments were randomized separately and are estimated separately; Both Parents is never compared with Father or Mother. Village fixed effects; robust standard errors in parentheses. FU1 uses the complete-outcome analytical flag. FU1–FU2 outcomes are lead-specific; FU3 uses the common module, so within-arm levels are not comparable across that boundary. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table A7. Parental Co-Presence by Action Intensity

Outcome (FU1)	Experiment 1			Experiment 2	
	YO mean	Father	Mother	YO mean	Both Parents
<i>Panel A: Daughters</i>					
Took a concrete step	0.388	0.046* (0.027)	0.053* (0.028)	0.315	-0.001 (0.031)
Simple action	0.262	0.027 (0.025)	0.032 (0.025)	0.188	0.018 (0.026)
Medium action	0.153	0.025 (0.021)	0.038* (0.022)	0.139	-0.013 (0.023)
Strong action	0.045	-0.005 (0.011)	-0.012 (0.012)	0.031	-0.003 (0.011)
<i>Panel B: Sons</i>					
Took a concrete step	0.469	-0.010 (0.020)	-0.019 (0.020)	0.455	-0.024 (0.027)
Simple action	0.287	-0.002 (0.018)	-0.004 (0.018)	0.176	-0.018 (0.020)
Medium action	0.132	0.004 (0.014)	-0.008 (0.014)	0.100	-0.011 (0.016)
Strong action	0.111	0.005 (0.013)	-0.010 (0.013)	0.210	0.007 (0.022)

Notes: FU1 outcomes on the action ladder: simple (contacted employer or submitted a CV), medium (interviewed, rejected, or declined after learning details), strong (accepted, started work, or found work independently); Concrete step is their union and the rungs are not mutually exclusive at the youth level. Estimation samples: daughters $N=2,033$ and sons $N=3,797$ in Experiment 1; daughters $N=1,035$ and sons $N=1,444$ in Experiment 2, all with the FU1 analytical flag. Village fixed effects; robust standard errors. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Appendix B Robustness, Inference, and Attrition

This appendix reports the sensitivity of the central estimates to alternative inference and specification choices, the multiple-testing adjustments for the designated family, and the follow-up response and attrition tests.

Table B1. Inference Sensitivity for Daughters' First-Round Concrete step

Experiment	Contrast and inference	Estimate	SE	p -value
1	Father, individual robust	0.046	0.027	0.093
1	Father, submission clustered	0.046	0.028	0.099
1	Mother, individual robust	0.053	0.028	0.059
1	Mother, submission clustered	0.053	0.028	0.063
2	Both Parents, individual robust	−0.001	0.031	0.969
2	Both Parents, submission clustered	−0.001	0.031	0.970

Notes: All regressions include village fixed effects. The primary covariance estimator is heteroskedasticity-robust at the individual level. The sensitivity clusters by de-identified SurveyCTO submission.

Table B2. Multiple-Testing Adjustments, Designated Family

Family	Hypothesis	Raw p	Holm p	Bonferroni p	BH q
Experiment 1	Father, FU1	0.093	0.278	0.371	0.186
	Mother, FU1	0.059	0.236	0.236	0.186
	Father, FU2	0.385	0.385	1.000	0.385
	Mother, FU2	0.141	0.282	0.564	0.188
Experiment 2	Both Parents, FU1	0.969	1.000	1.000	0.969
	Both Parents, FU2	0.923	1.000	1.000	0.969

Notes: Experiment 1 contains four tests: Father and Mother at FU1 and FU2. Experiment 2 contains two tests: Both Parents at FU1 and FU2. The families and adjustments are calculated in the production code. BH denotes the Benjamini-Hochberg false-discovery-rate adjustment.

Table B3. Follow-Up Response and Differential Attrition

	YO response rate	Father (p)	Mother (p)	Both, Exp. 2 (p)	Joint p, Exp. 1
<i>Daughters</i>					
FU1	0.994	0.001 (0.869)	-0.005 (0.331)	-0.004 (0.754)	0.474
FU2	0.956	-0.013 (0.295)	-0.001 (0.967)	-0.028 (0.173)	0.467
FU3	0.866	0.006 (0.762)	0.032 (0.095)	0.036 (0.107)	0.200
<i>Sons</i>					
FU1	0.995	-0.003 (0.427)	0.000 (0.871)	-0.005 (0.595)	0.597
FU2	0.966	0.006 (0.411)	0.011 (0.127)	-0.008 (0.584)	0.311
FU3	0.897	-0.012 (0.338)	-0.008 (0.514)	0.016 (0.346)	0.617

Notes: The outcome is an indicator for having a recorded follow-up outcome. Father and Mother effects are within Experiment 1; Both Parents is within Experiment 2. No arm shows differential attrition at conventional levels. Village fixed effects; robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4. Control-Group Outcome Levels Across Follow-Ups

	Concrete step	Looking/waiting, no concrete step	Working	<i>N</i>
<i>Experiment 1 control group</i>				
FU1	0.090	0.666	0.090	2,024
FU2	0.179	0.708	0.179	1,983
FU3	0.378	0.492	0.361	1,822
<i>Experiment 2 control group</i>				
FU1	0.118	0.799	0.118	1,283
FU2	0.180	0.770	0.176	1,230
FU3	0.332	0.549	0.326	1,168

Notes: Raw means for control respondents, who answered a general job-search question at every follow-up. At FU1 and FU2 the control instrument records employment inside the general question, so Concrete step and Working coincide by construction in this group; at FU3 the common module separates them. These are within-group descriptives, not treatment contrasts. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C Additional Evidence and Heterogeneity

This appendix reports the exploratory analyses summarized in Section 6. None of them is pre-specified, and none identifies a mechanism.

Relocation-approval heterogeneity. A heterogeneity check uses the baseline item on parental approval of a daughter’s relocation for work. Pooling the father and mother arms within Experiment 1, a specification used only for this check, the first-round effect on taking a concrete step is concentrated among daughters whose parents reported baseline approval, and the interaction with a non-approval indicator is negative. The estimate is exploratory: it was not pre-specified, it rests on very small non-approval cells in the treated arms (51 daughters for the father item and 64 for the mother item), and the relocation item is one specific dimension of parental permission that need not generalize to permission for local job search. It does not identify a permission mechanism, and it does not show that the intervention operates only in already-permissive households.

Marriage transfers. Village-level marriage-payment measures are matched to the study villages from an independent survey covering 229 of them. The customary marriage payment (Mahr) is a precise null moderator of both parental effects. The Shabka (value of marriage gold) and the combined Mahr-and-Shabka measure are exploratory: the only movement is a both-parents interaction in Experiment 2 that does not survive the designated multiple-testing adjustment, rests on thin geographic support, and does not replicate in the larger Experiment 1.

Table C1. Effects on the Unable-Due-to-Family Outcome

	FU1	FU2	FU3
<i>Panel A: Experiment 1, daughters</i>			
Father vs. Youth Only	-0.0002 (0.0004)	0.0024 (0.0057)	0.0053 (0.0089)
Mother vs. Youth Only	0.0027 (0.0020)	0.0092 (0.0063)	0.0067 (0.0091)
Youth-only mean	0.0000	0.0075	0.0231
<i>N</i>	2,033	1,957	1,804
<i>Panel B: Experiment 2, daughters</i>			
Both Parents vs. Youth Only	0.0067 (0.0069)	-0.0072 (0.0057)	-0.0005 (0.0106)
Youth-only mean	0.0116	0.0081	0.0216
<i>N</i>	1,035	977	942

Notes: Daughter subsamples of each experiment; the outcome equals one when the recorded reason was family prevention or guardian refusal. Base rates are very low, so precision is limited. Village fixed effects; robust standard errors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C2. Baseline Perceived Parental Barriers by Gender

	Daughters	Sons	Difference	<i>p</i> -value
Panel A: Youth-Reported Barriers (Q206)				
Father rejected job (Q206A)	0.082	0.014	0.068	<0.001
Mother rejected job (Q206B)	0.061	0.008	0.053	<0.001
Job not socially acceptable (Q206C)	0.138	0.062	0.076	<0.001
Working outside house unsuitable (Q206E)	0.018	0.003	0.015	<0.001
Combined barrier index (sum of above)	0.299	0.087	0.212	<0.001
Any barrier cited (index > 0)	0.238	0.081	0.157	<0.001
Panel B: Parent-Reported Restrictions (Q407/Q507)				
Father refuses child's relocation for work	0.833	0.234	0.599	<0.001
Mother refuses child's relocation for work	0.838	0.254	0.584	<0.001

Notes: Panel A reports youth-reported reasons for rejecting a previously offered job (Q206, baseline Section 2). The sample is restricted to youth who reported being offered at least one unsuitable job ($N = 2,487$ daughters and $N = 2,326$ sons; approximately 45% and 36% of the full sample). Panel B reports parent-reported attitudes from the analytical sample: the father module (Q407, $N = 10,456$) and mother module (Q507, $N = 11,490$). All p -values are from two-sample t -tests. The gender gap is large and statistically significant across all measures: for example, 8.2% of daughters versus 1.4% of sons cite father rejection, and 83% of fathers and 84% of mothers refuse daughters' relocation for work versus 23–25% for sons.

Table C3. Heterogeneity by Education

	Father $\times$ Educ.	Mother $\times$ Educ.	p , M-F	Both $\times$ Educ. (Exp. 2)	N (Exp. 1)
Daughter reached more than high school	0.016 (0.054)	0.022 (0.056)	0.905	0.013 (0.834)	2033
Mother holds any qualification	0.040 (0.055)	-0.045 (0.056)	0.136	0.012 (0.851)	1994
Father holds any qualification	0.055 (0.065)	0.122* (0.067)	0.320	-0.051 (0.443)	1741

Notes: Interaction coefficients on Concrete step at FU1 for the daughter subsample; Experiment 1 estimates Father and Mother jointly against Youth Only, Experiment 2 estimates Both Parents separately (Both column shows the coefficient with its p -value beneath). Education is the highest stage reached or enrolled in, from q6__b; parents' education comes from the household roster, so Ns fall where the roster lacks an entry. No interaction is distinguishable from zero: there is no systematic education heterogeneity. Village fixed effects; robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C4. Parental Co-Presence by Baseline Perceived Family Barrier

	<i>Daughters</i>				<i>Sons</i>			
	Low barrier		High barrier		Low barrier		High barrier	
	FU1	FU2	FU1	FU2	FU1	FU2	FU1	FU2
Father vs. Youth only	−0.012 (0.015)	0.010 (0.029)	0.021 (0.028)	−0.023 (0.052)	0.010 (0.022)	0.003 (0.036)	0.017 (0.250)	−0.175 (0.267)
Mother vs. Youth only	0.005 (0.016)	0.001 (0.028)	−0.002 (0.019)	0.021 (0.057)	−0.030 (0.022)	−0.018 (0.035)	−0.041 (0.088)	−0.067 (0.274)
Both vs. Youth only	0.028 (0.027)	−0.023 (0.043)	0.047 (0.041)	0.070 (0.079)	0.061 (0.044)	−0.005 (0.056)	−0.014 (0.035)	−0.017 (0.074)
Youth-only mean	0.024	0.084	0.021	0.036	0.113	0.306	0.127	0.296
<i>N</i>	1,031	993	358	340	1,528	1,491	137	132

Notes: OLS estimates with village fixed effects and robust standard errors. The outcome is the lead-specific concrete-step measure used in the main parental analysis (main text Table 4). The sample is restricted to youth with non-missing Q206 data (those who reported being offered at least one unsuitable job at baseline). “High barrier” denotes youth who cited at least one family or social barrier at baseline (father rejection, mother rejection, job not socially acceptable, or working outside the house unsuitable; Q206 options A, B, C, or E); “Low barrier” denotes youth who cited none. Among daughters, 26% are classified as high-barrier versus 8% of sons (Table C2). No interaction is statistically distinguishable from zero, and the high-barrier subgroups contain relatively few treated daughters ($N = 358$ daughters and $N = 137$ sons). Youth Only is the omitted category. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Father- and mother-specific barriers: Separate specifications using only father rejection (Q206A; $N = 129$ high-barrier daughters) or only mother rejection (Q206B; $N = 98$) produce zero-variation coefficients at FU1 because of small cell sizes and are not reported. At FU2, point estimates are positive but imprecise (for example, Both +0.165, SE = 0.125 for father-rejected daughters). These results are consistent with the combined specification but lack power to support conclusions.

Table C5. Characteristics of Tailored Job Leads by Gender

	Young men	Young women	Difference (Women – Men)
Mean number of job leads	3.39	3.45	0.07
Leads with a listed numeric salary (%)	90.7	80.0	–10.7
Median listed salary (EGP)	5,000	3,500	–1,500
Mean listed working hours	9.40	8.08	–1.32
Leads with health insurance (%)	79.8	58.4	–21.4
Leads with social insurance (%)	83.6	61.1	–22.4
Leads in the youth’s home governorate (%)	72.8	91.7	18.9
Leads outside the youth’s home governorate (%)	16.0	4.1	–12.0

Notes: Descriptive statistics for the 8,459 youth assigned to an information-treatment arm: 5,324 young men and 3,135 young women. Job leads are the opportunities recorded in the first through fifth job-lead fields. Listed-salary and working-hours statistics use leads with a numeric value recorded. Insurance shares use leads with a recorded insurance status. Location categories are derived from the recorded job-location text; leads listing multiple locations are not classified as either within or outside the youth’s home governorate. The table is descriptive and does not report experimental treatment effects.

Table C6. Marriage Transfers: Mahr Does Not Moderate the Parental Effects

Specification	Father arm			Mother arm			<i>N</i>
	Est.	(SE)	<i>p</i>	Est.	(SE)	<i>p</i>	
Mahr, per 100,000 EGP	−0.010	(0.045)	0.829	0.014	(0.044)	0.743	1,890
Log Mahr	0.001	(0.090)	0.992	0.024	(0.088)	0.782	1,890
Above-median Mahr village	−0.067	(0.065)	0.303	0.024	(0.067)	0.724	1,890
Per 100,000 EGP, extreme villages dropped	0.011	(0.058)	0.845	0.040	(0.058)	0.496	1,862
Per 100,000 EGP, village-clustered SE	−0.010	(0.036)	0.785	0.014	(0.042)	0.734	1,890

Notes: Experiment 1 daughters. Each row is the interaction between the parental arm and the village-level Mahr measure in a regression of action on the offered opportunities at the first follow-up, with village fixed effects. Mahr is the monetary marriage transfer promised or paid by the prospective groom to the bride, measured as a village average from an independent survey covering 229 study villages. The first row is the specification reported in the text. Standard errors are heteroskedasticity-robust except where noted. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table C7. Marriage Transfers: The Shabka Result Is Exploratory and Geographically Fragile

Specification	Estimate	(SE)	p	N
<i>Panel A: Experiment 1 (daughters)</i>				
Father arm $\times$ Shabka	0.085	(0.078)	0.273	1,890
Mother arm $\times$ Shabka	0.052	(0.081)	0.518	1,890
<i>Panel B: Experiment 2 (daughters)</i>				
Both-parents arm $\times$ Shabka	0.296	(0.124)	0.017	869
<i>Panel C: Experiment 2, dropping one governorate at a time</i>				
Excluding governorate 22	0.314	(0.142)	0.028	459
Excluding governorate 23	0.237	(0.138)	0.086	819
Excluding governorate 24	0.522	(0.192)	0.007	730
Excluding governorate 25	0.319	(0.127)	0.012	762
Excluding governorate 26	0.253	(0.127)	0.047	739
Excluding governorate 27	0.280	(0.127)	0.028	836

Notes: Daughters. Each row is the interaction between the parental arm and the village-level Shabka measure, per 100,000 EGP, in a regression of action on the offered opportunities at the first follow-up with village fixed effects. The Shabka is the customary gift of gold jewelry from the prospective groom to the bride; the source records the average value of marriage gold at the village level. The Experiment 2 interaction in Panel B does not survive the designated multiple-testing adjustment (minimum $q = 0.156$ across the nine-test family in Table B2), it does not replicate in the larger Experiment 1 sample of Panel A, and Panel C shows that its magnitude moves substantially when single governorates are excluded. Table C8 documents the thin geographic support. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Table C8. Geographic Support of the Shabka Measure, Experiment 2 Daughters

Governorate	Shabka category	Daughters
Beni Suef	Above median	441
Fayoum	Below median	50
Minya	Above median	136
Minya	Below median	3
Assiut	Below median	107
Sohag	Above median	107
Sohag	Below median	23
Qena	Above median	33

Notes: Experiment 2 daughters with a matched village Shabka value, by governorate and by whether the governorate falls below or above the median village Shabka. Because the below-median and above-median categories are each concentrated in a small number of governorates, the Experiment 2 interaction in Table C7 is identified from limited geographic variation and is not separable from governorate-level differences.

Table C9. Job Attributes Do Not Explain the Parental Effects

Interaction	Estimate	(SE)	<i>p</i>
<i>Panel A: Offered salary (per 1,000 EGP)</i>			
Father arm $\times$ salary	−0.000	(0.012)	0.988
Mother arm $\times$ salary	0.019	(0.012)	0.115
<i>Panel B: Whether the offered job carries social insurance</i>			
Father arm $\times$ lead offers insurance	−0.021	(0.025)	0.394
Mother arm $\times$ lead offers insurance	−0.047	(0.025)	0.065

Notes: Experiment 1 daughters. Panel A interacts the parental arm with the salary attached to the offered opportunity, at the youth-by-lead level for leads classified as suitable ($N = 2,442$ daughter-lead observations). Panel B interacts the parental arm with an indicator for whether the offered job carries social insurance. If parental co-presence worked by changing which jobs the household regarded as acceptable, the response should vary with these attributes; it does not vary detectably with either. $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix D Spillovers

Because treatment was randomized at the individual level within villages, each village contains both treated and control individuals, creating a natural channel for information spillover. If treated youth share the job leads they received with control peers, then control-group outcomes would be inflated and the intent-to-treat estimates would be attenuated downward. The analysis below tests for such spillover using five complementary approaches drawn from Miguel and Kremer (2004), Angelucci and De Giorgi (2009), and Crépon et al. (2013). *Identification strategy.* “Universal” outcomes, taking a concrete job-related step, reporting not looking for work, or being employed, can in principle be affected by information spillover, since control youth might act on information passed along by treated peers. “Lead-specific” outcomes, actions taken on the particular job leads offered during the intervention, are not observed for control youth, who were never presented with those leads. If treatment saturation predicts control youth’s universal outcomes, that pattern would be consistent with information spillover; if it does not, we find no such indication. Lead-specific outcomes cannot serve as a comparison here because they are not observed for control youth. *Village-level variables.* For each village v , we compute: (i) the number of treated individuals N_v^T ; (ii) the treatment share $s_v = N_v^T/N_v$; (iii) the leave-one-out share $s_{-i,v}$; and (iv) arm-specific and gender-specific counts. Because these regressors are village-level, we cannot include village fixed effects; instead, we control for village size and individual baseline covariates with standard errors clustered at the village level. *Results.* Table B4 documents the raw pattern: control-group action rates rise substantially across follow-ups. The treatment share coefficient is statistically zero in all specifications: $\hat{\beta} = -0.077$ ($p = 0.23$) for the full control sample and $\hat{\beta} = 0.007$ ($p = 0.90$) for control daughters. There is no temporal escalation (the coefficient does not grow across waves), no saturation–treatment interaction ($p = 0.87$ for daughters), and no same-gender peer spillover ($p = 0.93$ for treated daughters on control daughters). Lead-specific outcomes are not observed for control individuals, who were never presented with project leads; consistent with this, no control record carries a lead-specific action code. Control-group action rises over follow-ups (from 9.0 to 37.8 percent in Experiment 1; Table B4), but this increase is not systematically related to measured village treatment saturation. We therefore find no detectable evidence that the rise is driven by within-village treatment spillovers. While this cannot establish that the stable unit treatment value assumption (SUTVA) holds, the results provide no indication that within-village contamination materially affects the ITT estimates.

Appendix E Outcome Construction and Measurement

This appendix documents how each outcome is built from the follow-up response codes and which comparisons each supports.

Table E1. Which Comparisons Are Valid at Each Follow-Up

Comparison		Follow-up	Use in paper	Reason
Father/Mother Youth Only	vs.	FU1–FU2	Yes	All groups received leads and answered the same job-specific questions.
Both Parents Only	vs. Youth	FU1–FU2	Yes	Both groups received leads and answered the same job-specific questions.
Youth Only	vs. Control	FU1–FU2	No	Treated youth answered job-specific questions; Control answered a general question.
All experimental trasts	con-	FU3	Yes	The module was administered in the same form to all arms.

Notes: The restriction is based on the survey instruments and the construction code. It is not based on estimated effect sizes.

Table E2. Follow-Up Action Response Codes and Tier Classification

Code	Description	Action Tier	FU1/2	FU3
<i>Simple Actions (codes 2, 3, 10)</i>				
2	Called the employer; told the job is available	Simple	✓	✓
3	Submitted CV; awaiting interview	Simple	✓	✓
10	Job phone number did not respond / is closed	Simple	✓	✓
<i>Medium Actions (codes 4, 9, 11)</i>				
4	Interview was held; awaiting result	Medium	✓	✓
9	Applied for the job and was rejected	Medium	✓	✓
11	After learning job details, decided not suitable	Medium	✓	✓
<i>Strong Actions (codes 5, 6, 7, 8, 15)</i>				
5	Was accepted; waiting to start	Strong	✓	,
6	Was accepted and started working	Strong	✓	✓
7	After being accepted, was not able to start	Strong	✓	✓
8	After being accepted, was prevented from starting	Strong	✓	,
15	Applied independently and got a job	Strong	✓	✓
<i>No Action</i>				
1	Did not call the employer	No Action	✓	✓
12	Still looking for work	No Action	✓	✓
13	Respondent number closed / does not respond	No Action ^a	✓	✓
14	Not looking for work now	No Action	✓	✓
21	Desire for a government job	No Action	,	✓
22	Refusal by guardian (father / husband / parent)	No Action	✓	✓
23	Change of mind due to family circumstances	No Action	✓	✓
24	Change of mind due to military service	No Action	✓	✓
25	Change of mind due to illness / accident	No Action	✓	✓
96	Other (specify)	No Action	✓	✓

Notes: Response codes from the follow-up phone surveys. At FU1 and FU2, each respondent was asked about up to five specific job leads; one code was recorded per lead. At FU3, a single summary code was recorded. Code 21 (desire for government job) appears only in FU3. Codes 5 and 8 appear only in FU1/2. “,” indicates the code is not available in that wave. Tier classification follows the production outcome-construction code: Simple = {2, 3, 10}, Medium = {4, 9, 11}, Strong = {5, 6, 7, 8, 15}, identical at every follow-up; “Concrete step” is the union of the three tiers. ^aCode 13 is used for attrition identification and excluded from outcome variable construction. *Conservative construction* (used only for treatment–control comparisons on the FU1–FU2 instruments): only codes 6, 15, 12, 14, 22, and 23 are retained, with all other codes set to missing before variable construction. The parental-arm analysis uses the full code set above.

Table E3. Outcome Variable Definitions

Variable	Description	Construction	Source	Waves
Treatment Assignment				
treatment_all	Unified treatment arm	0 = Control, 2 = Youth+Father, 3 = Youth+Mother, 4 = Youth+Both parents	1 = Youth-only, rand_1, rand_4	,
Baseline Covariates				
age	Age in years	Direct from survey	q5__	BL
Gender	Sex (1 = male, 2 = female)	Direct from survey	q4__	BL
male	Male indicator	= 1 if Gender = 1	q4__	BL
married	Currently married	= 1 if marital_status = 1	q6__	BL
secondary	Secondary education	= 1 if Education = 3	q6__b	BL
university	University education	= 1 if Education = 4	q6__b	BL
worked_before	Ever worked	= 1 if yes	q101__	BL
trained_before	Received skills training	= 1 if yes	q105__	BL
Universal Outcomes (all arms including control)				
any_action_taken_{1,2,3}	Any labor market action	= 1 if any of simple, medium, or strong action; = 0 if surveyed but no action; missing if not surveyed	Job codes	1,2,3
not_interested_{1,2,3}	Not interested in working	= 1 if any job response ∈ {1, 14}	Job codes	1,2,3
unable_family_{1,2,3}	Unable due to family constraints	= 1 if any job response ∈ {22, 23}	Job codes	1,2,3
passive_search_{1,2,3}	Looking or waiting without a concrete step	= 1 if any job response ∈ {1, 12, 21, 96}	Job codes	1,2,3
working_{1,2,3}	Currently working	= 1 if any job response ∈ {5, 6, 15}	Job codes	1,2,3
active_search_{1,2,3}	Any search action or currently working	= 1 if any job response ∈ {2–10, 15}	Job codes	1,2,3
positive_search_{1,2,3}	Any concrete step toward employment	= 1 if any job response ∈ {2–11, 15}	Job codes	1,2,3
Lead-Specific Outcomes (information-receiving arms only; not observed for Control)				
simple_action_{1,2}	Took simple action on a lead	= 1 if any job response ∈ {2, 3, 10}	Job codes	1,2
medium_action_{1,2}	Took medium action on a lead	= 1 if any job response ∈ {4, 9, 11}	Job codes	1,2
strong_action_{1,2}	Took strong action on a lead	= 1 if any job response ∈ {5, 6, 7, 8, 15}	Job codes	1,2
simple_any3, medium_any3, strong_any3	Action tiers at FU3	Same code sets as the FU1/2 tiers, applied to the single FU3 summary code	Job codes	3
FU4 Outcomes (retrospective, 3-month follow-up)				
any_action_taken4	Any labor market action (FU4)	= 1 if searching, searched in 3 months, >0 days/week, >0 jobs contacted, >0 interviews, or >0 jobs rejected; = 0 if surveyed but none	q1–q7	4
working_4	Currently working	= 1 if Last_followup_q1 = 1	q1	4
searching_to_work	Currently searching for work	= 1 if Last_followup_q2 = 1	q2	4
simple_action_4	Low-intensity search	= 1 if searching 1–2 days/week	q4	4
medium_action_4	Moderate-intensity search	= 1 if searching 3–5 days/week	q4	4
strong_action_4	High-intensity search	= 1 if searching 6–7 days/week	q4	4

Notes: “Job codes” refers to the follow-up action response codes listed in Table E2. “BL” denotes baseline survey variables. For FU1/2, each outcome is constructed using the row maximum across the five job-lead responses (i.e., = 1 if *any* of the five leads matches the relevant codes). For FU3, outcomes are based on a single summary response code. For FU4, outcomes are based on retrospective questions about the preceding three months. All variables are set to missing for respondents not surveyed in the relevant wave. In the conservative construction, used only for treatment–control comparisons on the FU1–FU2 instruments, only codes 6, 15, 12, 14, 22, and 23 are retained before variable construction; the parental-arm analysis uses the full code set.

Appendix F Design and Implementation Details

This appendix maps the arguments of the conceptual framework onto the analyses reported in the paper, documents the sample flow, and sets out the measurement differences behind the exclusion of the fourth follow-up.

The additional fourth follow-up. An additional follow-up was conducted approximately one week after the third round. It asked about current employment, current search, and, retrospectively, labor-market activity over the preceding three months: whether the respondent had searched, days per week spent searching, numbers of jobs sought, interviews obtained, and applications rejected.

This round differs from the first three in what it measures, not only in when it was collected. The analytical outcomes at FU1 and FU2 record actions taken on the specific job leads presented at the household visit, and the FU3 outcome records what the respondent had done since the previous contact, each coded from the single action-code frame documented in Appendix E. The fourth round instead combines six search-intensity items over a three-month recall window into a broad indicator of whether the respondent searched at all. The two constructions do not measure the same object, and their levels are not on a common scale. About 0.81 of respondents are classified as having some search activity under the fourth-round construction, well above the shares recorded for taking a concrete step under the FU1–FU3 measures in Tables 3 and 4; the difference reflects the broader three-month search concept, which counts any search during the recall window rather than a concrete step taken on the opportunities presented, and should not be read as a change in behavior between rounds. The fourth-round intensity categories are likewise defined by days searched per week, which is unrelated to the action tiers used at FU1–FU3.

Because the construct and the reference period both differ, the fourth round is excluded from all primary estimates and is not treated as a fourth point on the FU1–FU3 trajectory. Two further considerations reinforced that decision at the time: the round followed the third by only about a week, and the research team had concerns about the comparability and quality of the responses collected under the retrospective frame. The exclusion rests on the measurement differences, which are visible in the instrument and in the outcome construction, and does not depend on what the round’s estimates show.

Having established that, we report the previously estimated results from the round for transparency in Table F3. They show no clear parental treatment effects. Because the round measures a different construct, these estimates are presented as a diagnostic

exercise and not as a continuation of the main trajectory. The available historical specifications also differ from the paper’s final design: they pool men and women, pool arms across the two experiments in a single regression, and cluster standard errors at the village level. The existing outputs did not estimate the daughters-only mother-versus-father contrast for this round, and we do not introduce a new specification for a round that is excluded from the primary analysis because its outcome is not comparable.

Independence of the reported third-round estimates. All outcomes reported in the paper at FU3 are constructed exclusively from the third-round action variable (`Follow_3`, named `jobs_actions3` in the cleaning code). None uses `Last_followup_q*` or any variable constructed from the additional retrospective follow-up.

Table F3. Fourth-Follow-Up Diagnostic Estimates Using the Retrospective Outcome Construction

	Any search activity (1)	Currently working (2)
<i>Panel A. Pooled sexes, arms pooled across experiments, relative to youth only</i>		
Father	0.011 (0.015)	−0.003 (0.015)
Mother	0.015 (0.014)	0.002 (0.014)
Both parents	−0.022 (0.015)	0.019 (0.020)
Observations	7,494	7,494
<i>Panel B. Daughters, relative to control</i>		
Youth only	—	−0.002 (0.015)
Father	—	0.004 (0.019)
Mother	—	0.004 (0.019)
Both parents	—	−0.014 (0.021)
Observations		3,863

Notes: The fourth follow-up is excluded from the primary analysis. It uses a different outcome construction based on a retrospective three-month reference period, described above, and its outcomes are therefore not comparable with the FU1–FU3 measures reported in the paper. These estimates are shown only for transparency and should not be interpreted as a fourth point on the FU1–FU3 trajectory. Coefficients are transcribed from the historical estimation outputs; those specifications do not exactly match the final specifications used in the paper. Panel A pools men and women and pools arms across the two experiments in a single regression with village fixed effects and standard errors clustered at the village level, comparing each parental arm with youth-only delivery. Panel B reports the daughters’ subsample with each arm compared against the control group. Column 1 is the broad retrospective search indicator; column 2 is current employment. “—” indicates that the historical output does not report that cell. No coefficient in either panel is statistically significant at conventional levels; the historical outputs report significance markers rather than exact p -values, so none are reproduced here. The daughters-only mother-versus-father contrast was not estimated in these outputs and is not introduced here.

Questionnaire. The baseline questionnaire is available in the supplementary materials.

Table F1. Arguments in the Conceptual Framework and the Corresponding Tests

Argument	What it implies for the data	Where it is tested
Information raises the perceived return to search	Concrete action rises relative to control	Table 3
Information does not change the conditional probability that an action becomes an acceptable job	Employment need not rise over a two-month horizon	Table 3; Section 5.4
Co-presence changes the household information environment, not the youth's information	Each parental arm may differ from youth-only delivery	Table 4, Panel A
Unitary benchmark, with costless within-household information transmission and no attendee-specific implementation or credibility role	Father and mother attendance generate similar responses within Experiment 1	Table 4; Table 5
Departures from that benchmark: parents differ in preferences, authority, beliefs, implementation roles, what they pass on within the household, or the endorsement their presence conveys	Father and mother attendance generates different responses, immediately or over time	Table 5; Figure 1
The cost of acting differs by gender while the informational return does not	Similar information effects by gender; attendance effects larger for daughters than sons	Tables 3 and 6; Appendix Tables A3, A4, A6
Joint attendance, tested only within its own experiment	Both-parents attendance may differ from youth-only delivery in Experiment 2	Table 4, Panel B
If a specific channel were operative, the response should track the relevant margin	Responses vary with job attributes, baseline household restrictions, education, or local marriage transfers	Section 6; Appendix C

Notes: The table records the correspondence between the arguments developed in Section 3 and the analyses reported in the paper. It is not a pre-analysis plan: the study was not pre-registered and had no pre-analysis plan, and none of these tests carries confirmatory status. The first five rows correspond to randomized contrasts; the gender row combines randomized subgroup estimates with interaction tests that are exploratory and unadjusted for multiple testing; the final row is exploratory throughout. The unitary row states a benchmark implication that holds under the auxiliary assumptions named in it; similar father and mother responses are consistent with that benchmark but do not establish that the household is unitary, and different responses do not by themselves establish that it is not. Because the single-parent arms belong to Experiment 1 and the both-parents arm to Experiment 2, no row implies a comparison of joint with single-parent attendance, and none is estimated.

Table F2. Sample Flow

	<i>N</i>
Raw survey records, all three randomizations	20,662
Less: married women in the separate husband-involvement experiment	−2,915
Records belonging to Experiments 1 and 2	17,747
Less: pilot-phase records collected before August 1, 2024	−5,966
Analytical population	11,781

Notes: Both deductions are design exclusions rather than attrition. The three randomizations are mutually exclusive. The pilot exclusion falls almost entirely on Experiment 1; Experiment 2 began in September 2024 and loses no observations to it. The analytical population spans 287 villages: 227 in Experiment 1, 158 in Experiment 2, and 98 in both.

Appendix G Survey Instruments

This appendix reproduces, in English translation, the survey instruments used in the study: the baseline registration instrument administered to youth, the father and mother attitude modules embedded in the same visit, the job presentation form used during the information session, and the follow-up phone forms. All baseline covariates and parental-attitude measures are constructed from the baseline items, and all outcomes are constructed from the follow-up forms reproduced in Section G.6. Items belonging to the additional fourth follow-up are marked there and are excluded from every reported estimate, for the reasons given in Appendix F.

The following is the English translation of the baseline registration questionnaire administered to youth aged 18–35 in six governorates of Upper Egypt (Fayoum, Beni Suef, Minya, Assiut, Sohag, and Qena). The survey was conducted by Gawab Studies Company on behalf of the Sawiris Foundation for Social Development. *Introduction read to respondent:*

Good morning / good evening. My name is _____ and I work with Gawab Studies Company. We are conducting a scientific study for the Sawiris Foundation about young men and women aged 18 to 35 who are looking for work. We try to identify the jobs that are suitable for them and those that they desire and are looking for. We also help companies, factories, and business owners find young men and women who are suitable for the jobs they need. This information will help us develop the services we provide to youth and help them obtain suitable work, especially young women and people with disabilities. The interview will take about 25 minutes, and any information you provide will remain confidential by law and will not be used for purposes other than scientific research. Your participation is voluntary, and you can choose not to answer any question.

Section 1: Background Characteristics of the Young Person

Q100 Is (name) home now?

01 Yes, currently available (son/daughter)

02 Yes, currently available (wife)

03 No, not available

Q101 Did you ever work before? (1 = Yes, 2 = No → skip to Q105)

Q102 Can you tell me all the ways and things you did to find this job?

A I asked at companies

B Called companies

C Follow up on advertisements in newspapers

D Follow up on online advertisements

E Through friends or relatives

X Other (specify)

Q103 What jobs have you worked before? (*Record up to 3 professions*)

Q104 What are the reasons that made you leave your job / want to leave your job?

A Reduction in employment

B Expiration of the contract

C Family circumstances

D Unsuitable working conditions

E Salary is low

F Corona crisis

X Other (specify)

Q105 Have you received any training to develop your skills? (1 = Yes, 2 = No → skip to Q107)

Q106 What are these trainings? *(Record up to 3)*

Q107 Would you like to take these/other trainings? (1 = Yes, 2 = No → skip to Q201)

Q108 What field(s) would you like to receive training in? *(Record up to 3)*

Q109 If you have to pay for training, will you participate? (1 = Yes, 2 = No → skip to Q111)

Q110 How much can you pay to get these trainings? *(Amount in Egyptian pounds)*

Q111 How many weeks do you think these trainings will take? *(Number of weeks)*

Q112 How many days a week can you spend in these trainings? (1–6 days/week)

Q113 During this training period, can you afford your personal training expenses?
(1 = Yes, 2 = No)

Q114 Does your father agree that you move outside the village to get this training?
(1 = Yes, 2 = No)

Q115 Does your mother agree that you move outside the village to get this training?
(1 = Yes, 2 = No)

Section 2: Unemployment and Looking for Work

Q201 How long have you been looking for a job? *(Months; if < 1 month, record zero)*

Q202 Can you tell me how you are looking for a job? Tell me all the ways and things you do.

A I asked at companies

B Called companies

C Follow up on advertisements in newspapers

D Follow up on online advertisements

E Through friends or relatives

X Other (specify)

Q203 How much time do you spend each week looking for a job? (*Number of days per week*)

Q204 How many times have you been offered jobs that were not suitable and you rejected them?

1 Once

2 More than once

3 Not once → skip to Q207

Q205 What jobs were offered that were not suitable? (*Open-ended*)

Q206 What are the reasons that made you think the job(s) are not suitable?

A Father's rejection

B Mother's rejection

C The job is not socially acceptable

D The pay is low

E It will not be useful for me to work outside the house

F The place is far away / cannot work outside the village

G Does not fit my qualifications

H Does not fit my experience

I My children are young and I do not know where to take them

J If I work, it will be difficult to find a chance for a passport

X Other (specify)

Q207 Do you see job opportunities suitable for you around you? (1 = Yes, 2 = No → skip to Q210)

Q208 What are these opportunities? (*Open-ended*)

Q209 Why do you think it is appropriate?

- A Father agrees
- B Mother agrees
- C Work inside the village
- D The salary is reasonable
- E Good dealing because we are from the same place
- F This is the job we know how to do
- G Business hours are convenient
- X Other (specify)

Section 3: Needs and Aspirations

Q301 What type of work are you looking for?

- A A job that suits my educational qualifications
- B A job suitable for my age
- C A job based on my previous experience
- X Other (specify)

Q302 Would you prefer a government job, a private job, or your own project?

- 1 Government work
- 2 Private job
- 3 Working for others
- 4 Non-profit organizations
- 5 Civil society organizations
- 6 Any work is fine
- 96 Other (specify)

Q302A What is the appropriate workplace? (1 = Inside governorate, 2 = Outside governorate)

Q303 Do you think your education qualifies you for a suitable job? (1 = Yes, 2 = No)

Q304 What salary do you think your education qualifies you for?

1 Less than 2000 EGP

2 2000–3000

3 3000–4000

4 4000–6000

5 More than 6000

Q305 What is the appropriate number of working hours? (*Hours*)

Q306 Do you accept a job opportunity without: health insurance / social insurance / overtime / transportation / nurseries / meals? (*Yes/No for each; minimum and maximum acceptable salary*)

Q307 What transportation time (round trip) to the workplace is acceptable? (*Minutes*)

Q308 What do you want to achieve in the next five years?

1 Make money and build my future

2 Make money for marriage

3 Help my family with household expenses

4 Build a house to live in

96 Other (specify)

Q309 Have you thought or sought to travel for work? (*Yes/No for each:*) Outside the village / Outside the markaz / Outside the governorate / Outside Egypt

Q310 Have you thought about traveling through illegal immigration? (1 = Yes, 2 = No
→ skip to Q312)

Q311 Why did you think of traveling this way?

A Unemployment

- B Low wages
- C Low economic and living standards
- D Achieve my dream and become rich
- X Other (specify)

Q312 Have you thought about obtaining funding for a private project? (1 = Yes, 2 = No → skip to Q401)

Q313 What type of financing? (A = Loan, B = Grant, C = Interest-bearing loan, D = Interest-free loan, X = Other)

Q314 Why this type of financing?

- A No interest rate
- B Interest rate is small
- C This is what is available
- D Procedures are simple
- E No guarantor required
- F Previous experience with relative or friend
- X Other (specify)

Q315 Have you taken actual steps to obtain financing? (1 = Yes, 2 = No → skip to Q401)

Q316 What happened?

- A Preparing required papers
- B Submitted application, waiting for approval
- C Preparing the place for the project
- D Application rejected (no guarantees)
- E Application rejected (place not suitable)
- X Other (specify)

Section 4: Father's Attitudes

Q400 Is your father here?

- 1 Available
- 2 Traveling → skip to Section 5
- 3 Deceased → skip to Section 5
- 4 Divorced/separated and residing elsewhere → skip to Section 5

Q401 Do you accept a job for your son/daughter without: health insurance / social insurance / overtime / transportation / nurseries / meals? *(Yes/No for each; minimum and maximum acceptable salary)*

Q402 What is the appropriate workplace for your son/daughter?

- A At home
- B In the village
- C In markaz
- D In the governorate
- E Anywhere
- X Other (specify)

Q403 Would you prefer your son/daughter to have a government job, private job, or own project?

- 1 Government work
- 2 Private job
- 3 Working for others
- 6 Other (specify)

Q404 Why do you prefer this job for your son/daughter?

- A Monthly salary
- B With time, will be promoted and salary increases

- C So that child will not have to work for anyone
- D Child cannot manage own business
- E When you help others, God will honor you
- F This is the work available in the country
- G Customs and traditions prevent girls from going out to work outside the home
- H I do not want to exhaust my children in transportation to work
- I Life's circumstances are difficult, making me accept any job
- X Other (specify)

Q405 What salary do you think your child's education qualifies them for? (*Same 5-category scale*)

Q406 What number of daily working hours is appropriate for your son/daughter? (*Hours*)

Q407 If a job was available in another markaz/governorate, would you agree for your child to reside at the workplace and visit during holidays? (1 = Yes, 2 = No)

Q408 If a job was available for your son on the condition that you agreed to let your daughter work, would you accept? (1 = Yes, 2 = No)

Section 5: Mother's Attitudes

Q500 Is your mother here?

- 1 Available
- 2 Traveling → skip
- 3 Deceased → skip
- 4 Divorced/separated and residing elsewhere → skip
- 5 N/A (respondent is a wife) → skip

Q501–Q508 *Mirror of Q401–Q408, addressed to the mother.*

Job Presentation Form

Enumerator instruction: Display the available jobs to the young person (accompanied by father/mother or husband). Based on their stated desires, present up to five matched job opportunities:

	First Job	Second Job	Third Job	Fourth Job	Fifth Job
Location					
Job title					
Salary					
Working hours					
No. of work hours					
Start of work					
Health insurance					
Social insurance					
Acceptable?	1 = Yes / 2 = No	1 / 2	1 / 2	1 / 2	1 / 2
Contact person					
Phone number					

Q600 Why are all jobs not suitable?

- 1 The distance is far
- 2 The salary is low
- 3 Far from my specialty
- 4 I have no experience in it
- 5 I want to start a private project

If no jobs match, enumerator offers to display the respondent's data on employment websites (e.g., Shaghlni). Respondent is thanked and informed that the research team may contact them again.

Section 6: Follow-Up Phone Forms

The surviving follow-up workbook contains separate forms for the first, second, and third follow-ups, translated below. The first and second forms are identical in structure: after the identifying block, the enumerator records the details of each job lead presented at the household visit, up to five leads, and for each lead asks whether the youth considered it suitable and what action was taken. Two closing questions apply to respondents for whom no lead was suitable and to the control group, which was presented with no leads. The third form drops the lead-by-lead structure and asks a single common question of every arm, which is the only round in which treated and control youth answer the same item. The workbook stores that common-module question and the retrospective items associated with the additional fourth follow-up on the same final worksheet, which is a data-capture layout rather than a printed field script. The project team confirms that the retrospective items were administered in a separate follow-up approximately one week later; the workbook does not preserve realized interview-date fields, so the archival file cannot itself establish which items were asked at which contact. The retrospective items measure a different construct and are excluded from all reported estimates, for the reasons given in Appendix F.

Table G1. Follow-Up 1 and Follow-Up 2 Phone Form

Block	Items recorded
Identification	Governorate; district; village; enumerator name; form number; household head's telephone; name of the jobseeker
Follow-up record	Date of the follow-up call
Job lead 1–5 (repeated per lead)	Job code; job title; location; salary; working hours; start date; health insurance available; social insurance available; contact person; contact telephone
Per lead, screening	“Was the job suitable?” If not suitable, move to the next job.
Per lead, if suitable	“What did you do about the job you said was suitable?” (action code)
If no lead suitable	“What are the reasons none of the jobs was suitable?” (reason code)
Control group	“What have you done since we were last at your home?” (action code)

Notes: English translation of the first- and second-round field forms (“Follow-up Form 1” and “Follow-up Form 2” in the field workbook). Seventy fields per respondent. The lead-specific action question is asked only of youth who received job leads, which is why lead-specific outcomes are not observed for the control group. Action and reason codes are listed in Table G3.

Table G2. Follow-Up 3 Phone Form

Block	Items recorded
Identification	Governorate; district; village; enumerator name; form number; household head's telephone; name of the jobseeker
Follow-up record	Date of the follow-up call
Common module (asked of all arms)	"What have you done about work since we last contacted you?" (action code); "Are you currently working?"; "Are you currently looking for work again?"
Retrospective items (additional follow-up; excluded)	Whether the respondent searched for any job in the last three months; average days per week spent searching; number of jobs searched for; number of those with an interview; number of applications rejected; average number of times the jobs presented at the household visit were contacted (not asked of the control group)

Notes: English translation of the third-round field form ("Follow-up Form 3"). The common module is the source of the universal outcomes used in the information comparison. The retrospective three-month block corresponds to the additional follow-up conducted approximately one week later; it uses a different reference period and outcome construction and is excluded from all reported estimates (Appendix F).

Table G3. Follow-Up Response Codes as Printed on the Field Form

Action codes (per lead, and common module)	Reasons no job was suitable
1 = Did not call	1 = Salary insufficient
2 = Called; they said the vacancy was filled	2 = Jobs do not match my qualifications or experience
3 = Submitted CV, awaiting an interview	3 = Location unsuitable / far from the village or district
4 = Interview held, awaiting the result	4 = Working hours unsuitable
5 = Accepted; preparing the remaining papers	5 = The firm or workplace is unsuitable
6 = Accepted and started working	6 = The jobs offer no scope for advancement
7 = After acceptance, the salary was unsatisfactory	7 = The type of work does not match my interests
8 = After acceptance, the hours were unsatisfactory	8 = Prefers a government job
9 = Applied and was rejected	9 = Wants to start an own business
10 = The job's number did not answer / was closed	10 = Started working after the enumerator's visit
11 = After learning the details, not interested	11 = Changed mind, family circumstances or military service
12 = Still looking for work	96 = Other (specify)
13 = Respondent's number closed / no answer / wrong	
14 = Not looking for work	
15 = Applied independently and started working	
21 = Prefers a government job	
22 = Refusal by the guardian (father / husband / family)	
23 = Changed mind, family circumstances	
24 = Changed mind, military service or study	
25 = Changed mind, illness, accident, or death	
26 = Wishes to work abroad	
96 = Other (specify)	

Notes: English translation of the code sheet distributed with the follow-up form. These are the raw codes from which every outcome in the paper is constructed; the mapping from codes to analysis variables is given in Appendix E. Code 13 identifies non-response and is excluded from outcome construction.