

## Soft Skills or Hard Cash?

### The impact of training and wage subsidy programs on female youth employment in Jordan<sup>\*</sup>

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#### Abstract

This study examines the impact of a randomized experiment in Jordan in which female community college graduates were assigned to receive a wage subsidy voucher, soft skills training, both, or nothing. The wage voucher led to a 40 percentage point increase in employment in the short-run, but the average effect is much smaller and no longer statistically significant after the voucher period has expired. Soft skills training has no average impact on employment. We elicit expectations of academics and development professionals and reveal that these findings are novel and unexpected.

**Keywords:** Wage subsidy, Soft skills, youth unemployment, Randomized Experiment, Impact Expectations, Displacement.

**JEL classification codes:** O12, O15, J08, J16.

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## 1. Introduction

Upon graduation from community college in Jordan, 93 percent of women say they want to work, yet only 25 percent are working two years later. This enormous gap between expectations and reality highlights the enormous challenge facing young women who want to work in the Middle East and is a stark example of the lack of employment opportunities for educated youth occurring worldwide.

Firms typically give two main reasons for reluctance towards hiring young women. First, they are often reluctant to hire youth because they lack work experience, require on-the-job training, and it may be costly to assess how good a worker they are. On top of this, they have doubts about how committed young women are to pursuing careers, and whether they are as flexible in working hours and travel as males. Secondly, firms often complain that formal schooling, at best, teaches only the technical skills workers need, but that many youth are lacking in the soft skills needed for success in the workplace – such as how to interact with customers, work in teams, act professionally, and even how to properly represent themselves in job interviews in the first place.

We conducted a randomized experiment in Jordan intended to overcome both these constraints and assist female community college graduates to find employment. Community college graduates are typical of many of the relatively educated unemployed in the Middle East, with relatively high unemployment rates and concerns expressed about the quality and relevance of their classroom training. The Jordan New Opportunities for Women (Jordan NOW) pilot randomly allocated almost the entire cohort of female community college graduates of the class of 2010 into four groups: (1) a voucher group that received wage subsidy vouchers, which could be redeemed by a graduate's employer for up to six months for an amount equal to the minimum wage, (2) a training group that that received an invitation to attend a 45-hour soft skills training course, (3) a treatment group that received both the wage subsidy vouchers and an invitation to training, and (4) the control group.

The interventions were launched at the end of September and beginning of October 2010, and follow-up surveys were conducted 6, 14, and 27 months later. The wage subsidies led to extremely large short-term gains in employment with a 25 percentage point increase in employment in Amman, the capital, and a particularly large 50 percent increase outside of the capital. However, by the final survey in January 2013, the overall employment gains had dissipated: there was no longer any statistically significant impact on employment in Amman while outside of Amman the treatment effect had shrunk

to 8.5 percentage points, and appears as if it may arise from displacement of the control group from jobs. Soft skills training had no short-term impact on employment. Despite the lack of employment impact, the soft skills training led to improvements in life outlook and a reduction in depression, which suggests the training may have benefits outside of the labor market.

Wage subsidies have long been used to help disadvantaged groups find jobs in developed countries. There have been several randomized experiments to measure their impacts in the U.S. (Burtless, 1985; Dubin and Rivers, 1993) that have found disappointing impacts, which the authors attribute to potential stigma effects. Several non-experimental studies have found some positive impacts (e.g. Katz, 1998), although an overview of different wage subsidy evaluations by Betcherman et al. (2004) concluded that such programs have largely not been effective in developed countries. An experiment in Canada which gave subsidies for full-time work to welfare recipients (not to their employers) did find positive short-term impacts on employment, but that a year and a half after the subsidy had ended, the effect had completely faded out (Card and Hyslop, 2005). Wage subsidy programs for youth have been used in a number of transition countries such as Poland and Slovakia, and there appears to be renewed policy interest in developing countries, with examples such as Morocco's *Idmaj* program and Tunisia's *SIVP* program<sup>1</sup>, and South Africa's much discussed Youth Wage Subsidy. Despite this policy enthusiasm, there is very little evidence on the effectiveness of such programs in developing countries with the one exception of a randomized experiment by Galasso et al. (2004) in Argentina. They found that wage vouchers given to the unemployed lead to a 6 percentage point increase in wage employment 18 months later although this impact largely occurred in informal and temporary jobs.

In these existing evaluations, the rather limited effects have in part come from low usage rates of the wage vouchers, preventing existing studies from seeing whether providing access to subsidized short-term employment can lead to lasting jobs. Since voucher take-up was relatively high in our study, we are able to examine this issue, as well as further contribute to the literature by providing evidence on the effectiveness of wage subsidies in a context where female skilled youth unemployment rates are very high.

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<sup>1</sup> SIVP = Stage d'Initiation à la Vie Professionnelle. This is a subsidized internship where beneficiaries receive 150 TND monthly and employers have full coverage of social security and training costs. The subsidy targets recent university graduates who have been looking for a job for six months, a group especially affected by unemployment (Almeida et al. 2012).

There is even less empirical evidence on the effectiveness of soft skills training programs<sup>2</sup>, but youth employment programs in Latin America in particular have increasingly included a component that focuses on these skills (World Bank, 2010). For example, the *entra 21* program implemented in 18 Latin American countries includes a soft skills training component. Although there is no rigorous evaluation of this intervention, employers report that participants who took part in this program have greater ability to work in teams and take on responsibility than do their other employees (*entra 21*, 2009). The Dominican Republic's *Juventud y Empleo* program also teaches soft skills along with providing work experience, with early evidence from Ibarra et al. (2012) finding this increased the formality of work, but did not lead to any increase in overall employment in the short-run. Our paper provides the first experimental evidence on the effectiveness of soft skills training alone in a developing country context.

A common refrain following the presentation of experimental results or results from other rigorous impact evaluations is often that the findings “will surprise no one who had opened an economics textbook” (Scheiber, 2007). However, as Watts (2011) discusses, many findings in social science seem obvious once you know the answer, but the opposite conclusions could also often be easily justified *ex post*. A further contribution of this study is to provide a way to quantify the extent to which the findings are novel or unexpected by means of an audience expectations elicitation exercise. The study was presented in seminars to academics and development professionals and described online on a popular development economics blog; point expectations of the treatment impacts were elicited before the results were revealed. The results reveal considerable heterogeneity in expected outcomes from our interventions and that the short-term impact of the voucher was much larger than almost everyone participating expected, while the long-term impact of the voucher and the impact of the soft-skills training were somewhat lower than people expected. In addition to highlighting the unexpected nature of the findings in this study, we believe the approach pioneered here could be useful for other experimental studies to employ.

The remainder of this paper is structured as follows: Section 2 describes the context, our sample, and the details of the intervention; Section 3 the experimental design, data collection, and intervention take-up; Section 4 the midline and endline experimental impacts; Section 5 whether these impacts are in

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<sup>2</sup> There are several evaluations of intensified job placement assistance for the unemployed that have been carried out in several European countries (e.g. Graversen and van Ours, 2008; Hägglund, 2009; Behaghel et al, 2012). These studies typically include some help with interview techniques and CV preparation, which is a small part of our soft skills training courses, but not the emphasis on development of workplace soft skills such as time management, teamwork, and positive thinking that are the focus of our program and of the employment-oriented soft skills training programs in Latin America.

line with expectations or not; Section 6 then discusses possible explanations for these impacts; and Section 7 concludes.

## **2. Context, Sample, and the Intervention**

### **2.1 Labor Market Context**

Two striking features of labor markets throughout the Middle East are bulging youth populations with very high unemployment rates, particularly among relatively educated youth and low labor force participation rates among women. Jordan is typical in this regard. Youth aged 15-29 are the largest demographic group in the country, making up 30 percent of the total population. In 2011, the unemployment rate for 20-24 year old females was 47.6 percent compared to 23.1 percent for 20-24 year old males. Only 10.5 percent of 20-24 year old females are employed compared to 49.1 percent of 20-24 year old males. Women with post-secondary education are more likely to be unemployed than women with just secondary or primary education, which is largely because the latter do not participate in the labor force at all (Jordanian Department of Statistics, 2011).

Young women face twin constraints in accessing jobs. First, firms are often reluctant to hire youth, regardless of gender, since they lack job experience, are of untested quality, and may lack soft skills such as reliability, a strong work ethic, and knowledge of how to work and communicate effectively in a workplace (IYF, 2010). If hiring, training, and firing workers is costly, firms may be reluctant to take a chance on someone untested in the labor market. Second, young women face additional barriers because of gender. Employers often express clear preferences for male workers, based on the belief that women are less committed to their jobs and may leave if they get married or have children, that men are more flexible in work hours and ability to work over-time, and that women might experience more difficulties interacting with customers in some occupations due to culture (Jordanian Enterprise Survey, 2006; Peebles et al, 2007). When faced with these constraints and a lack of networks and role models who have obtained jobs, many young women may lack confidence to look for jobs in the first place.

### **2.2 Educational System Context and Focus on Public Community College Students**

Students in the Jordanian educational system go through a common core curriculum that ends with the tenth grade, followed by two years of specialization where students choose between an academic track (focusing on either sciences or arts) and a vocational track. Both tracks end upon the

completion of twelfth grade with a General Secondary Education Examination (the *Tawjihi*), which if passed concludes secondary education. Students who take the academic track in Arts or Science can then gain entrance to university provided they achieve a competitive score on the *Tawjihi*. Alternatively, those taking part in the Vocational track, those who get a low *Tawjihi* score, or those with limited financial means can enroll in a two-year community college – either as a terminal qualification with skills in a particular field, or as a second chance at university admissions (Kanaan and Hanania, 2009).

Analysis of labor force survey data from 2007 shows that, among 25 to 30 year olds in Jordan, 67 percent of females and 72 percent of males have at most secondary education. Amongst the third of women this age with higher levels of education, one-third has a diploma from a community college, and two-thirds have a university degree. There are 14 public community colleges in Jordan, with total enrollment of approximately 11,895 students (7,072 female, 4,823 male) in 2007/08. These public community colleges have significantly lower tuition than universities and private community colleges.<sup>3</sup> However, as in much of the Middle East, there are concerns about both the quality and usefulness of some of the training being offered, leading to concerns about the employability of community college graduates, particularly in a labor market with limited jobs in which they may be outcompeted by university students. This context led the Government of Jordan to request assistance from the World Bank in conducting a pilot program to try to increase employment of female graduates.

To conduct this pilot, we chose the 8 public community colleges with the largest female enrolment numbers, together comprising over 85 percent of all female public community college enrolment. They consist of four colleges in Central Jordan (Amman University College, Princess Alia University College, Al-Salt College, Zarqa University College) and four located in Northern and Southern Jordan (Al-Huson University College for Engineering, Irbid University College, Ajloun University College, and Al-Karak University College). For ease of exposition, since Amman is the capital of Jordan and two of the four colleges in Central Jordan are located within the city of Amman while the other two are located within a 45-minute drive of Amman, we will herein refer to Central Jordan as inside Amman and Northern and Southern Jordan as outside Amman.

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<sup>3</sup> There were approximately 14,500 students enrolled in 22 private community colleges in 2007/08 according to Government statistics.

Baseline surveys were conducted in July 2010 for all second-year students in these colleges who were going to sit for their final exams, giving data on 1755 female students.<sup>4</sup> In August 2010, the baseline data was merged with administrative data on examination results, which revealed that 1395 had passed their examinations. We randomly selected 1350 of these graduates to be our experimental sample.

The typical graduate is 20 to 22 years old, is unmarried, and has never worked before. Only 13.8 percent were married at baseline, and 16.3 percent have ever worked. Only 7 percent of these young women's mothers are currently employed, whereas 57 percent of their fathers are currently employed. At the time of the baseline survey, which took place only weeks before final examination results were available, only 8 percent of students had already found a full-time job for work after graduation.

Table 1 shows the main courses of study undertaken by the experimental sample at the overall program of study level, as well as at the level of specialization. We see the majority of students are taking courses in administration and finance (43%), which covers specializations such as accounting, electronic administration, and management information systems; courses in medical assistance (24%), which covers mainly nursing and pharmacy specializations; and educational programs (10%), which covers those aiming to be teachers.

### **2.3 Pilot Interventions**

We piloted two policy interventions, each designed to overcome some of the barriers to firms hiring young female graduates: wage subsidy vouchers and soft skills training. Wage subsidies have a long history of use by policy makers as part of their active labor market policies to generate employment for the disadvantaged (e.g. Kaldor, 1936, Layard and Nickell, 1980, and Katz, 1998). It is argued that short-term subsidies may have long-term effects by raising the productivity of youth through work (Bell et al., 1999), and may encourage employers to take a chance on hiring inexperienced, untested workers (World Bank, 2006). Pallais (2013) shows via an experiment in an online jobs marketplace that simply giving a job to an inexperienced worker helps improve their subsequent employment outcome. There is also now growing evidence that non-cognitive or soft skills are important for employment and a range of other life outcomes (e.g. Bowles et al, 2001; Heckman et al., 2006; Heckman and Kautz, 2012). Interventions that aim to teach soft skills may enhance employment prospects by giving youth better

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<sup>4</sup> Some students take longer than 2 years to graduate, and some drop out before sitting their final examinations, so this 1755 students represents about half of the female students enrolled in the second year.

skills and confidence for looking for jobs and by making them more productive in their first months in the job by reducing the amount of time firms need to spend training them on the basics of working in a business environment.

Our pilot program was marketed to participants as the Jordan New Opportunities for Women (Jordan NOW) pilot. The details of our interventions are as follows:

#### *Wage Subsidy Vouchers*

Graduates receiving this intervention were given a voucher that they could present to firms while searching for jobs. The voucher had the graduate's name on it, was non-transferable, and worth 150 JD (USD 210), an amount equivalent to the minimum wage, per month for a maximum of six months. To be eligible to use the voucher a firm had to provide proof of registration, have a bank account to receive payment in, and provide an offer letter with the graduate's name and specification of work duties. The wage agreed for employment was also required to be at least the minimum wage of 150 JD per month. We did not require registration of workers in the social security system for eligibility, so employers were subject to the existing law on this, which in principle requires workers to be registered after three months with the firm. After the start of employment, both the firm and graduate were required to confirm their employment with the program administrator each month and periodically monitored through random visits to further confirm employment and ensure reimbursement claims were legitimate.

The voucher was valid for a maximum of six months within an eleven-month period starting October 3, 2010 and ending August 31, 2011. If a subsidized job ended before the graduate received 6 months of wages, the voucher remained with the graduate, who could use the remaining months on it with a different firm. Graduates could present a formal letter explaining the wage subsidy, and the Jordan NOW pilot was advertised through the Chamber of Commerce, newspapers, and an official government website, and an information helpline in order to further the legitimacy of the voucher and provide more information to firms as needed. Firms were told that "the goal of the program is to increase female community college graduate participation in the labor market", making clear it was a program for women recently graduating from community colleges. As a result, those with vouchers would be unlikely to experience added stigma relative to other female community college graduates.

Due to budget constraints, we could afford to fund a maximum of 450 graduates for the full 6 months each. Thus, we informed the graduates that vouchers would be honored on a first-come-first-



served basis and we would notify them once the cap was reached. In practice, this cap did not bind since only 301 graduates used the vouchers.

### *Soft Skills Training*

Graduates receiving this intervention were invited to free intensive training on interpersonal skills in areas identified by Jordanian employers as essential yet typically missing in recent graduates. The training course was 45 hours over a 9 day period (5 hours per day), with a maximum of 30 participants in each training group. Training took place during September and October 2010. The training was provided by Business Development Center (BDC)<sup>5</sup>, a Jordanian NGO established in 2005 which has widespread local name recognition and a good reputation for skills training, having implemented USAID, UNCTAD, and a wide variety of local training programs. Training took place in 17 sessions offered throughout 6 governorates to maximize access. Training facilities and training content were identical across all 17 sessions. To minimize the effect of social and cultural restrictions on mobility, sessions were held during daylight hours at locally known and trusted institutions such as the Chambers of Industry and local universities where BDC has satellite training facilities.

BDC designed the course which covered effective communication and business writing skills (e.g. making a presentation, writing business reports, different types of correspondence), team-building and team work skills (e.g. characteristics of a successful team, how to work in different roles within a team), time management, positive thinking and how to use this in business situations, excellence in providing customer service, and C.V. and interviewing skills. Sessions were based on active participation and cooperative learning rather than lectures, with games, visual learning experiences, group exercises, and active demonstrations used to teach and illustrate concepts.

The cost of the training was approximately \$150,000, which was based on up to 600 graduates attending – leading to a cost of \$250 per assigned graduate, and given that only 373 attended, an effective cost of \$400 per attendee.

### **3. Experimental Design, Data Collection, and Take-up**

Randomization into treatment and control groups was done via computer. Students were first stratified into 16 strata on the basis of geographic region Amman (Amman, Salt and Zarqa), and outside Amman (Irbid, Ajloun and Karak), whether their Tawjihi examination score at the end of high school was

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<sup>5</sup> <http://www.bdc.org.jo/>

above the sample median or not, whether they indicated at baseline that they planned to work full-time and thought it was likely or somewhat likely that they would have a job within 6 months of graduating, and whether or not she is usually permitted to travel to the market alone (a measure of empowerment). Within each strata, 22.2 percent of the students were allocated to receive the wage voucher only, 22.2 percent allocated to receive the soft skills training only, 22.2 percent allocated to receive both, and 33.3 percent allocated to the control group. This resulted in 300 in each treatment group, and 449 in the control group, for a final experimental sample of 1349.<sup>6</sup> It turned out that two of the individuals assigned to receive a wage voucher (one from the voucher only group, and one from the voucher and training group) were actually males who had incorrectly been recorded as female in the baseline questionnaire. These were dropped from the program, given a final breakdown of 299 in wage voucher only, 300 in training only, 299 receiving training and voucher, and 449 in the control group.

The choice of variables to stratify on was based on two considerations. First, stratifying on the basis of variables which we believe would influence the main outcomes of interest (employment) can improve the power of an experiment to detect a given sized treatment effect (Bruhn and McKenzie, 2009). Since few graduates were employed at baseline, we did not stratify on employment status itself, but instead on key variables we thought might predict employment. We hypothesized that the likelihood of getting a job will be higher for students around Amman than in other areas, for those with higher academic ability (proxied by the Tawhiji score), for those with the desire and confidence to work, and for those with greater empowerment or freedom of movement (proxied by whether they are allowed to travel to the market alone).

Second, stratifying on these variables prevents against chance imbalances in these characteristics, and serves as a means for specifying ex ante our interest in examining the heterogeneity of treatment effects according to these characteristics. A priori, it was difficult to predict in which direction this heterogeneity would act. For example, we expected graduates in Amman to be more likely to find work in the absence of an intervention because the majority of private sector activity is concentrated around the capital and families outside of Amman are more traditional and more reluctant to allow their daughters to work. However, it was unclear whether the interventions would then work better in Amman because there would be fewer other constraints on finding work, or whether they would have less effect there if it is the case that anyone who wants to work should be able to find a job, whereas outside of Amman where it is more challenging to find a job, only those who receive assistance

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<sup>6</sup> Rounding within strata resulted in 449 rather than 450 being allocated to the control group in total.

might be able to find one. Likewise it was unclear whether the interventions would act as a complement or a substitute for higher academic ability, higher desire to work, or higher empowerment.

### **3.1 Baseline Information by Treatment Status**

Table 2 provides summary statistics on the experimental sample by treatment status. As one would expect given computerized randomization, the characteristics look similar across the different treatment groups. At baseline, the graduates express high levels of desire to work, with 93 percent saying they plan to work after they graduate, 91 percent say they would like to work outside the house after they are married, and 82 percent say they think it is very likely or somewhat likely that they will have a job within 6 months of graduating. 59 percent of graduates fall into the category of high likelihood of working full-time category.

One hypothesis for low levels of female employment is that patriarchal norms limit the extent to which women work. We have noted that 91 percent say they would like to work outside the home after marriage, and the baseline also shows that 91 percent think they will be allowed to do so. Most women say that the main reason for obtaining an education is to increase one's earnings (45 percent) or to find a job with better working conditions (32%), with only 3 percent saying the main reason is to improve marriage prospects. However, there is evidence that some women do face mobility restrictions, with only 51 percent saying they are allowed to go to the market alone. Table 2 also shows that upon graduation 81 percent of the graduates say they would prefer a public sector job to a private sector job, which is consistent with evidence from elsewhere in the Middle East.

### **3.2 Data collection**

Data collection consisted of three follow up surveys: the midline in April 2011, the first endline in December 2011, and the second endline in January 2013. This second endline was added to address the possibility that in an environment where it takes graduates some time to find a job, it may take more than one year to see impacts. All rounds collected data on employment outcomes, and the first endline collected information on a range of well-being measures, including mental health, subjective well-being, and empowerment. In the midline, first endline, and second endline surveys, we successfully followed up with 92, 96, and 92 percent of graduates in our sample, respectively. In the first and second endline surveys, we collected a portion of the survey data (3% and 9%, respectively) by proxy through their relatives.

In addition to the survey data, we surveyed graduates' employers and obtained administrative data from the Social Security Corporation of Jordan. In October 2010, we surveyed 368 firms who employed graduates (whether or not they had used the voucher to do so) during the midline survey. This is approximately 100 percent of firms who were employing graduates with a voucher and more than 67 percent of firms who were employing graduates without a voucher. In March 2012, the Social Security Corporation sent us employment data for all graduates who provided their social security numbers in the follow up surveys, which is 95 percent of the sample.

The attrition rates are low in absolute terms and relative to the literature, but do vary slightly with treatment status. Appendix Table A1 shows that the wage voucher group has the lowest attrition (3% midline, 1% 1<sup>st</sup> endline, 4% 2<sup>nd</sup> endline), which is likely due to the additional information gathered through monitoring the voucher usage. The control group experienced the highest attrition (11% midline, 7% 1<sup>st</sup> endline, 11% 2<sup>nd</sup> endline), which is likely because there was no additional contact with the control group outside of the surveys. In contrast to the survey data, there is no difference in attrition rates by treatment status in the social security data. This survey experienced very few problems with outright rejections to answer the survey questions although the proxy responses reflect graduates or their families refusing to allow the graduate respond for herself. The vast majority of attrition comes from disconnected cell phones and the inability to completely track individuals down. In Appendix Table A2, we use bounding approaches to examine the robustness of our results to potential selection bias arising from attrition: our main finding of a large short-term impact of the voucher which dissipates dramatically is found to be robust.

### **3.3 Take-up of the Interventions**

The soft skills training course was completed by 373 of the 599 graduates (62%) assigned to the training only or training and voucher treatments. Only 5 students attended part of the course but did not finish. Qualitative feedback from participants immediately after the course was universally extremely positive, with many participants in particular saying it had given them more confidence and positivity, as well as noting their appreciation for learning practical topics not taught in college. Correlates of take-up are shown in Table 3 and discussed in Appendix 3.

Take-up of the voucher is equivalent to finding a job with an employer that met the voucher requirements and was willing and able to use the voucher. In total 301 of the 598 graduates (50.3%) assigned to receive a voucher used it for at least one month. Usage varied significantly with location;

only 35 percent of those eligible in Amman used it whereas 65 percent of those eligible outside of Amman used it. Three-quarters of those who used the voucher used it for the full 6 months, 9 percent used it for 5 months, 6 percent for 4 months, and only 10 percent for 3 months or fewer. In the midline survey, 85 percent of those employed with a voucher say they earn 150 JD per month, which is the amount of the voucher, 1.9 percent say they were paid less than this, and the highest earnings is 320 JD per month. The most common occupations for those using the vouchers were teachers – most typically in nursery schools (33%), secretaries, clerks or administrative assistants (17%), nurses or medical assistants (10%), data entry workers (9%), and pharmacists (8%).

### 3.4 Correlates of employment in the control group

The remaining columns of Table 3 examine the correlates of being currently employed for the control group at the time of the midline (columns 5 and 6), first endline (columns 7 and 8), and second endline (columns 9 and 10) surveys. The purpose of this analysis is to see whether the correlates of take-up are similar or different to those of working in the absence of an intervention. We see a strong contrast to the wage voucher take-up results in terms of location effect: only 10-11 percent of the control group is employed at the time of the follow up surveys outside of Amman, whereas 28 percent are employed in Amman at the time of the midline, and 38 percent at the time of the endline. Thus, the much lower take-up of wage vouchers inside Amman does not reflect lower employment rates in Amman.

The control group is less likely to be employed if they said at baseline that they did not think they would be working full-time after graduation or if they were married at baseline. Students who took administrative and financial courses are less likely to be employed, which is in line with the wage voucher take-up results. Household wealth, tawjih score, and baseline mobility do not predict employment among the control group after conditioning on the other variables in these regressions.

## 4. Results

To evaluate the impact of assignment to one or both of the interventions, we estimate the following equation for graduate  $i$  via OLS:

$$Outcome_i = \alpha + \beta_1 Voucher_i + \beta_2 Training_i + \beta_3 Both_i + \sum_{s=1}^{16} \delta_s d_{i,s} + \varepsilon_i \quad (1)$$

where  $Voucher_i$  is a dummy variable taking the value one if graduate  $i$  was assigned to receive a wage voucher (either alone or with training),  $Training_i$  is a dummy variable taking the value one if graduate  $i$  was assigned to be invited to the training course, and  $Both_i$  is a dummy variable denoting the individual was offered both treatments. Individuals who got both treatments therefore have value one on all three of the  $Voucher$ ,  $Training$ , and  $Both$  variables. The  $d_{i,s}$  are the randomization strata dummies. The coefficient  $\beta_1$  then measures the intention-to-treat (ITT) effect of the wage voucher,  $\beta_2$  the ITT of the training, and  $\beta_3$  whether there is an interaction effect between the two treatments. Since we are explicitly interested in whether the impacts differ in the midline survey - when the vouchers are still in effect – from the endline survey after they have ended, we estimate equation (1) separately for the midline and endline data.

We focus on ITT impacts which give the average effect of being offered training or the voucher, rather than the effect of actually attending training or taking up the voucher. We choose not to estimate the treatment effect on the treated because, especially in the case of the wage voucher, it seems plausible that being offered the treatment may have impacts on employment outcomes even if the treatment is not actually used. Indeed Galasso et al. (2004) find evidence of this in a wage subsidy experiment in Argentina, and they suggest that one main effect of vouchers in their experiment was to encourage workers to exert more effort finding work and to give them more confidence approaching employers, even though actual take-up of the vouchers was low. Similarly, one could imagine that the offer of training or a voucher may affect job search behavior even if the treatments are not taken up.

In addition to average impacts, we examine heterogeneity in treatment response with regard to the four variables used to stratify the randomization. To do this we estimate the following equation for interaction  $Interact_i$ :

$$\begin{aligned}
 Outcome_i = & \alpha + \beta_1 Voucher_i + \beta_2 Training_i + \beta_3 Both_i + \\
 & \gamma_1 Voucher_i * Interact_i + \gamma_2 Training_i * Interact_i + \gamma_3 Both_i * Interact_i \\
 & + \sum_{s=1}^{16} \delta_s d_{i,s} + \varepsilon_i \quad (2)
 \end{aligned}$$

where the strata dummies capture the level effects of the interaction term, and  $Interact_i$  is either a dummy for being in Amman, for not expecting to work full-time at baseline, for having above median tawjihi grade, or for being allowed to travel to the market alone at baseline (denoted “empowered”).

#### 4.1 Average impacts on labor force participation and employment

Table 4 reports the results of estimating equation (1) for different employment outcomes. Panel A reports the results of the midline survey, Panel B, the first endline survey, and Panel C, the second endline survey. We begin in column 1 by looking at the impact on labor force participation, defined as either working or actively looking for work.<sup>7</sup> At the time of the midline, labor force participation was high for everybody, with 77 percent of the control group participating. The treatments had no additional impact on labor force participation at this time. In contrast, the first endline survey revealed that labor force participation had fallen to 48 percent of the control group, reflecting graduates stopping actively looking for work. At that time, the wage voucher treatment had a statistically significant 10 percentage point increase in labor force participation, while the training had an insignificant 5 percentage point increase (although we can also not reject the effect is equal to the voucher effect), and there is no interaction effect. The difference between the control and treatment groups concerning labor force participation was temporary; the second endline survey revealed that labor force participation increased to 58 percent for the control group which is not significantly different from the rates of the treatment groups.

Columns 2 through 4 examine employment. Column 2 looks at whether individuals are currently employed or have worked for cash in the last month. In panel A, we see a large and strongly significant impact of the wage vouchers on this measure at midline. The 39.5 percentage point increase in employment more than triples the employment rate of 17.8 percent in the control group. Of those in the voucher or both treatment groups, 84 percent of those employed at the midline had used the voucher; 20 percent of those who had used the voucher were no longer employed. The large impact does not persist once the voucher period has expired, as evidenced by panel B and panel C, which show that graduates in the wage voucher group were only an insignificant 2.8 and 1.5 percentage points, respectively, more likely to be employed than the control group. For training, the midline and first endline survey revealed a positive but not statistically significant impact on employment while the second endline revealed a slightly negative and still not statistically significant impact on employment. There is no interaction effect between training and the wage voucher.

To improve power further, we can pool together the two endline surveys to obtain an average effect in the second year post-graduation, clustering the standard errors at the individual level (McKenzie,

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<sup>7</sup> Our surveys did not collect information on whether individuals received job offers that they declined, so we can examine job search, and employment, but not whether young women are turning down jobs.

2012). This yields a point estimate of 0.025 with a 95 percent confidence interval of (-0.028, +0.079) for the voucher effect, and a point estimate of 0.024 and 95 percent confidence interval of (-0.029, +0.077) for the training effect. As Hoenig and Heisey (2001) argue, these confidence intervals, rather than post-experiment power calculations, are the appropriate guide for interpreting statistically insignificant results and show the range of possible effect sizes not refuted by our data enables us to rule out large impacts of the treatments.

#### **4.2 Were these real jobs?**

A key question is whether the jobs created by the voucher reflected genuine work opportunities in their field of study, or makeshift less-skilled jobs in which graduates had few chances to practice and enhance their skills. The vast majority of young women hired with the voucher were hired as teachers, nurses and pharmacists, accountants, and secretaries or business administrators.<sup>8</sup> They were therefore employed in sectors consistent with their training. Spot checks by the implementation unit were done before payment of the subsidy, and verified that those hired were actually working in these jobs doing typical tasks for someone in these positions. Despite the baseline preference for public sector work, almost all jobs were in the private sector, with median firm size of ten workers, of which 60 percent on average were women. Only 8 percent of voucher-users said that they were related to the owner of the business that had hired them. The jobs created thus were in the field of study, in relatively small private firms that already had previously hired women. This was very similar to the types of firms and jobs that the control and training group who were employed were working in.

Our survey data also reveal that individuals viewed these as real jobs that produced genuine labor market experience. Those using the vouchers reported working an average of 39.5 hours a week in the jobs that they used vouchers to get; that, on average, only 1.1 hours per week were spent doing tasks that were not related to the occupation or position they had; and that 35.5 hours per week were spent doing “real” work as opposed to just sitting around without much to do. When we asked graduates what that main thing they had learned from this job experience, the most common answers were job-specific technical skills (e.g. accounting skills, teaching skills and nursing skills) (50%); how to deal with people and customers (21%); and general work experience (12%). Only 9.5 percent claimed to have learned nothing in this job, which is comparable to the response of those who did not use a voucher for their first job, for which 10.5 percent said they had learned nothing in the job.

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<sup>8</sup> Consistent with this, we find no significant difference in treatment effect by the three main fields of study (business administration, medical assistance, and education).



### 4.3 Impacts on Other Dimensions of Employment

Columns 3 and 4 look at formal employment, defined by whether individuals report being employed and registered for social security in the survey (column 3), and in the social security administrative data (column 4). We see that almost all of the employment created by the voucher at the time of the midline is not formal employment, with only a 4.5 percentage point increase in formal employment according to survey reports, and no significant increase according to administrative records. There is no significant impact on formal employment in any of the follow up surveys. Outside of the voucher group, formal employment accounts for just over half of all employment - 12.6 and 16.5 percent of the control group were formally employed during the first and second endline surveys, respectively.

Although individuals in the wage voucher group are no more likely to be working after the voucher expired, they have more job experience as result of having had worked. Columns 5 and 6 show they are 19 percentage points more likely to have ever worked than the control group and have accumulated an average of 2.8 months more job experience by the second endline. The total amount paid as wage subsidies was 243900 JD (US\$341,460) to achieve this average gain in experience for 598 individuals. The cost per month of experience gained is thus 147 JD (US\$204).

Finally, we look at the intensity of work and the earnings from it. Column 7 looks at weekly hours worked, coding hours as zero for those not working. Hours worked are significantly higher for the wage voucher group at midline but not significantly different in the next follow-ups. The impact on work income is examined in column 8. The wage voucher group earns 64 JD more per month than the control group at midline, which has the effect of more than tripling the 25 JD per month the control group earns. However, by the endline the difference has fallen to only 6 JD per month and is not statistically significant. To see whether the higher income of the voucher group in the midline is just coming from them being more likely to work or also from them earning higher wages conditional on working, we examine the impact on wages conditional on working in column 9. Since working is an outcome, which is itself affected by the treatment, randomization does not guarantee that the treatments are uncorrelated with other determinants of conditional wages, so this should be considered exploratory and suggestive evidence only. We find that the wage voucher appears to have increased wages conditional on working at the midline by 23 JD, but that there is no significant difference in the endline follow up surveys. Also noticeable is that the mean wage for the control group at the midline is 141 JD per month, which is less than the minimum wage of 150 JD.

We also examined whether any of the interventions lead to changes in the characteristics of the firms in which workers are employed at endline. We see no difference by treatment status in the number of workers or number of female workers at the firm, or in whether the firm is owned by a woman.<sup>9</sup> Nevertheless, conditional on having had a job, graduates assigned to the wage voucher group report statistically higher levels of job satisfaction.

#### **4.4 Heterogeneity in Employment Impact**

Table 5 reports the results of estimating equation (2) to examine how the impact of the different interventions on employment varies with the stratifying variables. Again, panel A shows impacts at midline, panel B, at the first endline, and panel C, at the second endline. Employment status is again defined as being currently employed or having worked for cash in the past month, regardless of formal status.

Column 1 shows how the treatment effect varies with geographic location. Consistent with the much low take-up of the voucher in Amman than outside of Amman, we see that the voucher had much larger impacts on midline employment outside of Amman. Graduates assigned to receive the voucher outside Amman experienced a 50 percentage point increase in the likelihood of being employed at midline, compared to a 25 percentage point increase in Amman. Given the control group had much lower employment rates outside of Amman, this is equivalent to the wage voucher group having six times the employment rate of the control group outside Amman, and double the employment rate in Amman. Moreover, both endline follow-up surveys reveal that the significantly higher employment rate of the voucher group compared to the control group persists over time. In addition, the first endline provides some evidence that the soft skills training also had a positive effect on employment outside Amman. Only 11 (15) percent of the control group are employed outside Amman at the time of the first (2<sup>nd</sup>) endline, with an increase of 8.5 (8.5) percentage points for the wage voucher treatment and 6.1 (5.9) percentage points for the training treatment (and a negative but insignificant interaction effect). In contrast in Amman, the point estimates suggest 4.5 (7.1) percentage point lower employment rates for the voucher treatment group and 4.3 (0) percentage point lower employment rates for the training treatment groups than for the control group, although we cannot reject the null of no treatment effects in Amman.

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<sup>9</sup> Results available upon request.

Figure 1a and 1b use information from the surveys on the start and end dates of employment to construct and plot monthly employment rates by treatment status to graphically illustrate these impacts. We see the voucher only and voucher plus training lines track each other, rising rapidly at first, and then falling as the graduates hit the six-month limits on use of these vouchers. The rise is steeper outside of Amman. The difference in control group behavior inside Amman versus outside is also noticeable – employment rates continue to rise over time inside Amman, but level off much quicker hovering at 11 percent in 2011 and gradually increasing to 15 percent by the end of 2012.

In contrast, columns 2 through 4 of Table 5 show no significant interaction effects with the other stratification variables, with the coefficients generally small in magnitude. Moreover, the sign of the coefficients often varies between the midline and endlines, showing no consistent tendency for the interventions to have differential impacts according to baseline expressed likelihood of working fulltime, *tawjihi* score, or ability to travel to the market alone. This lack of interaction effect with treatment is not because these stratifying variables do not have an impact on employment prospects – the control group means show that employment rates are higher for those with more academic ability, those who face fewer mobility restrictions, and those who at baseline want to work full-time and think they will do so. There is no statistical evidence that the treatment interacts with these determinants of employment.

Table 4 showed a significant impact on labor force participation at the time of the first endline, despite the lack of significant employment effect. When we examine this effect by location, it appears to be coming entirely from outside of Amman – the impact is 17.6 percentage points ( $p < 0.001$ ) outside of Amman, and 0.3 percentage points in Amman ( $p = 0.95$ ). This trend continues into the second endline; however, the impact on labor force participation outside of Amman no longer continues to be statistically significant.

#### **4.5 Impact on Well-being, Empowerment and Attitudes**

Jobs can be more than just a source of income, with employment status associated with improved subjective well-being and increases in female empowerment (World Bank, 2011, 2012). In Table 6, we examine the impacts of the interventions on different measures of well-being, empowerment, and attitudes, which are all measured at the time of the first endline survey.

Column 1 examines the impact on current subjective well-being, measured on the Cantril self-anchoring striving scale (Cantril, 1965), a measure that has been used by Gallup around the world. Respondents are asked to imagine a ladder with 11 rungs, number from 0 at the bottom to 10 at the

top, where the top represents the best possible life for them, and the bottom the worst possible life. Kahneman and Deaton (2010) refer to this as “life evaluation.” The control group mean (s.d.) is 5.0 (2.4) on this scale, and being assigned to the wage voucher intervention results in a significant 0.58 unit increase, while training has an insignificant 0.28 increase. There is a significant and large negative interaction effect for receiving both treatments, so that graduates assigned to both treatments have no better current life evaluation than the control group. One possible rationale for this lack of impact for the dual treatment is that receiving both treatments may increase aspirations of what the best possible life is, relative to just receiving one treatment, leading the individuals in the dual treatment to evaluate their life differently.

Graduates were also asked at endline to assess which step on the Cantril ladder they believe they will be on in five years time, with this forward-looking measure reflecting the degree of optimism they have about their futures. Overall the graduates show a high degree of optimism, with the mean of 8.1 (s.d. of 1.8) for the control group three rungs higher than they assess their current position to be. Column 2 shows that the training intervention leads to additional optimism: graduates assigned to this group think they will be 0.27 steps higher than what the control group thinks. In contrast, there is a negative and insignificant impact of the wage voucher on this measure.

Mental health is a distinct concept of well-being from happiness, and has been shown to have different associations with individual characteristics and with life events (Das et al, 2008). We measure mental health using the Mental Health Inventory (MHI-5) of Veit and Ware (1983). This is a five-item scale with a maximum score of 25 and minimum score of 5, with higher scores indicating better mental health in terms of the experience of psychological well-being and the absence of psychological distress in the past month. While there is no universal cutoff, several studies have used a cutoff of less than 17 as an indicator of major depression (e.g. Urban Institute, 1999; Yamazaki et al., 2005). Twenty percent of the control group has scores below this threshold. Columns 3 and 4 look at the impact on the overall index, and on the binary classification of depression respectively. We find that the training intervention results in a significant 0.58 increase in the MHI-5 and a 4.8 percentage point reduction in the likelihood of having major depression. The wage voucher has no significant impact on either measure. These results are consistent with the ladder of life in the future questions and the qualitative feedback from the training intervention, which suggested that it helped to develop strong positive attitudes.

Columns 5 and 6 of Table 6 examine whether these changes in subjective well-being have also brought about changes in attitudes towards women’s role in home and society and changes in

empowerment, measured by ability to go to different places by themselves.<sup>10</sup> We see that there is no significant impact on attitudes towards the role of women: in the first endline, only 51 percent of female graduates disagree that a girl must obey her brother's opinion even if he is younger than she is, and only 65 percent agree that boys should do as much domestic work as girls. The interventions did not change these attitudes. The last column examines the impact on the number of locations out of 6 that a graduate is allowed to go to by herself. The mean in the control group is 5.2, with those who only get the wage voucher being permitted to go alone to 0.54 fewer locations. Two possible, but speculative, reasons for this reduction are that the additional individuals induced into employment by the wage voucher either have less time available to try to go to other locations (and so she is unsure of whether she is allowed or not), or that traditional households which relax restrictions in one domain (work) compensate by increasing restrictiveness in other domains. Training has a small negative but insignificant impact. The interaction between wage voucher and training is strongly positive though, so that graduates assigned to receive both treatments are permitted to go alone to 0.30 more locations than the control group.

Finally, we note that while at baseline (graduation from community college) only 13.7 percent of the sample was married, this had increased to 31.6 percent by the first endline, 18 months later, and 45.9 percent by the second endline, 31 months later. At the first (second) endline, only 9.6 (13.5) percent of married graduates in our sample were working compared to 29.9 (41.2) percent of those who are engaged and 32.5 (40.5) percent of those who are single. There is a positive, strong correlation between getting married and not working, which makes it of interest to see whether any of the interventions had any impact on the likelihood of being married by the endline. Columns 7 and 8 show that the treatment effects are all small and statistically insignificant, which suggests that the intervention had no impact on marriage.

We examined heterogeneity in these well-being impacts by geographic location. Both the wage voucher ( $p < 0.01$ ) and training interventions ( $p < 0.10$ ) have stronger impacts on current subjective well-being inside of Amman, whereas there is no significant geographic heterogeneity in treatment impacts on position on the life-ladder 5 years from now, on mental health, or on the number of locations the graduate is allowed to go to alone. The wage voucher has significantly more impact on empowered attitudes inside of Amman, although the effect size is relatively modest (0.27) and only significant at the 10 percent level.

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<sup>10</sup> Appendix II defines each variable in more detail.

## 5. Are these impacts unexpected?

Ex post, it is relatively easy for policymakers and academics to view the result of almost any impact evaluation and claim that the results are exactly what they would have expected. In a classic article, Lazarsfeld (1949) sets out six findings from a study of American soldiers (e.g. Southern Soldiers were better able to stand the climate in the hot South Sea Islands than Northern soldiers), and then for each, a probable reader reaction (e.g. of course, Southerners are more accustomed to hot weather). He then reveals that each of these statements was false (e.g. Southerners had no greater ability than Northerners to adjust to a tropical climate). Watts (2011) refers to this as the “myth of common sense”, and means that ex post, either a large positive impact, a zero impact, or a negative impact may all be easily explained away as obvious.

To understand whether this study merely confirms conventional wisdom or generates new and unexpected results, we undertook a novel audience expectations elicitation exercise. During the first four presentations of this research (two in academic departments, and two in international organizations)<sup>11</sup>, no paper was distributed or available in advance. Instead, the audience was presented with 25 slides detailing the motivation, existing evidence, context, design, and implementation of this study. We then distributed paper surveys throughout the audience and asked them to provide their point estimates of the midline and endline ITT impacts for each treatment group: voucher, training, and both combined. We also asked the audience whether they thought the voucher treatment impacts would be larger, the same, or smaller by each of our four stratifying variables. To complement the audience expectations elicitation exercises, we posted a description of the study on the World Bank’s Development Impact blog<sup>12</sup> with a link to an online survey to capture reader expectations.<sup>13</sup> The advantage of seminar presentations is that they offer time to answer any questions, explain the intervention in detail, and get high response rates (very few seminar attendees refused to fill in the one-page expectations sheet). In contrast, online elicitation opens up the process to a wider audience, but

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<sup>11</sup> This comprises presentations in May and June 2012 at the Applied Micro workshop at the University of Virginia; the development seminar at the Paris School of Economics; the applied micro seminar at the World Bank; and a seminar with the labor group of the InterAmerican Development Bank. The World Bank and IADB seminars contained a mix of researchers and development professionals engaged in the implementation of programs. We thank the seminar audiences for their participation in this exercise.

<sup>12</sup> <http://blogs.worldbank.org/impac evaluations/are-our-blog-readers-better-predictors-of-impact-results-than-seminar-audiences-evaluating-programs>

<sup>13</sup> To our knowledge this is the first paper to systematically collect audience expectations for an impact evaluation. Dean Karlan and Annie Duflo concurrently collected qualitative expectations during an online poll at the Stanford Social Innovation Review

[http://www.ssireview.org/articles/entry/can\\_management\\_consulting\\_help\\_small\\_firms\\_grow](http://www.ssireview.org/articles/entry/can_management_consulting_help_small_firms_grow).

offers less of a chance to explain the intervention, and has low and self-selected response rates.<sup>14</sup> Finally, a simplified version of the expectations was also elicited from nine Jordanian policymakers in a presentation of the results made in Jordan, who were just asked expectations in five bin ranges (<0, 0, 0 to 5%, 5% to 10%, more than 10%).

Figure 2 provides histograms of the resulting expectations, while Tables 7a and 7b summarize the resulting expectations. We see that only 4 percent of the 136 respondents gave an expected value of the voucher impact at midline that lies within the 95 confidence interval of the treatment effect, with the median expected impact of 8 percentage points less than one-quarter of the actual impact of 39.5 percentage points. In contrast, the modal and median response of a 5-percentage point impact in the first endline is close to the 3 percent estimated impact although there is still substantial heterogeneity in expectations with a long right tail to the distribution. The median expected impact of training was 5 percentage points in both the midline and first endline, which was close to the estimated ITTs, but the mean expected impacts were 9-10 percentage points, which suggests that many respondents overestimated the impact expecting the soft skills training to have more sizeable impacts. Audiences believed the combination of the voucher and training was likely to have larger impact than either treatment alone, but also dramatically underestimated the training impact at midline. None of the respondents had all six of their expectations lie within the 95 percent confidence intervals of the actual impacts. Furthermore, there was considerable heterogeneity among respondents in their relative rankings of the interventions: at midline (endline) 59 (47) percent thought the voucher would have a larger impact than the soft skills training, 11 (14) percent that the impact would be the same and 30 (39) percent that the voucher would have less impact than the soft skills training. The policymakers' responses also show considerable heterogeneity in responses, and the majority of policymakers underestimated the short-term impact of the voucher.

These results therefore show that i) the midline impact of the voucher is larger than most people would expect; ii) there is considerable uncertainty as to what the impacts of such a program are likely to be; iii) that the impacts of the soft skills training are less than expected on average; and iv) the lack of complementarity between the two interventions was a surprise to the average respondent. There was also considerable heterogeneity in beliefs about how the voucher treatment would vary with randomization stratifying variables: 48 percent thought the effect would be stronger in Amman versus

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<sup>14</sup> The Development Impact blog post had 900 page views, along with approximately 2000 RSS and Email subscribers (of which only some would have actually read the post). From this we received 48 responses, a click-through rate consistent with click-through from blog post links to academic papers (McKenzie and Özler, 2013).

36 percent outside Amman (the rest thought the impact would be the same in both places; 42 percent thought the impact would be higher for high academic ability, versus 41 percent for low academic ability; 56 percent thought the impact would be higher for the more mobile, versus 20 percent for the less mobile; and 62 percent thought the impact would be higher for those more interested in work at baseline, versus 17 percent for those less interested in work.<sup>15</sup>

## **6. Discussion**

The above analysis shows that the wage voucher treatment led to a large (and unexpected) increase in employment during the voucher's eligibility period but mostly in jobs not registered with social security. After the vouchers expired, the increase in employment in the voucher group compared to the control group mostly dissipated, except for outside Amman, in contrast to what many people would expect. In this section we explore possible mechanisms that could lead to these results.

### **6.1. Are temporary impacts due to the wage voucher groups losing jobs or the other groups gaining them?**

One potential explanation for the short-term impact of the wage subsidies is that they accelerated the process of finding a job, with the training and control groups catching up to the voucher group's employment rate over time as the graduates find jobs. An alternative explanation is that the reduced impact stems from individuals employed with the voucher losing their jobs after the vouchers expired. To distinguish between these explanations, we explore the employment dynamics in Table 8 by looking at the 2 x 2 transition matrices for employment and unemployment between the midline and first endline.

The results show that part of the reduction in impact is due to the control and training groups being more likely to transition into employment, but the majority is due to the voucher and voucher and training groups being much more likely to transition out of employment. For the full sample, 12 percent of the control group and 13 percent of the training group found jobs between the midline and endline surveys, compared to only 5 percent of the voucher and voucher and training groups. However, 37-38 percent of the training and training and voucher groups lost their jobs between midline and endline,

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<sup>15</sup> In the interests of space we do not discuss in detail here differences among the different audiences. The UVA and IADB audiences underestimated more the midline impact of the voucher, while the Paris School of Economics audience overestimated more both the midline and endline impacts of the soft-skills training and the endline impact of the voucher. Blog respondents (of whom only 20 percent were World Bank staff) had similar average responses to the World Bank seminar audience.



compared to only 7-8 percent of the control and training only groups. Thus, voucher group graduates losing their jobs accounts for approximately 80 percent of the closing of the gap in employment rates between voucher and the other groups while catch-up of those newly getting jobs accounts for approximately 20 percent of the gap. This basic pattern holds both inside and outside Amman.

The first endline survey directly asked graduates who had been employed with vouchers but were no longer in these jobs what the main reason for stopping work was. The most common reason for a job exit as claimed by 70 percent of job exits from wage-subsidized jobs was that the voucher ended and the job was temporary in nature. Only 2 percent of exits were for other employer-related reasons such as the employer firing them or the employer going bankrupt, while the remaining 28 percent were because the graduates decided to quit. Those who quit either were not satisfied with some aspect of the job, including travel and hours or had to take care of responsibilities with their families. In a minority (4%) of cases, they quit because of salary disputes, with firm owners wanting to lower their wages once the subsidy ended. Among those who remained in the job, there are only 5 cases where the midline wage was 150 JD and the endline wage less than this (mean of 98 JD), but more cases where the midline wage was above 150 JD but had been lowered (e.g. from 220 JD at midline to 150 JD at endline).

When the same question as to why a job ended was asked of firms that had hired graduates with the vouchers but no longer employed this worker as of November 2011, firm owners said that in 51 percent of the cases it was because the worker was unaffordable without the subsidy, 10 percent because they had fired the worker, 30 percent because the worker had quit either to get another job or to get married, and 7 percent for other reasons. Firms were relatively more likely to say the reason was the graduate quitting in Amman than outside of Amman (42% vs. 30%), and less likely to say the reason was that the worker was unaffordable without the subsidy in Amman. In only 36 percent of the cases, firm owners say they would have hired the worker who later lost their job without the wage voucher, and the main reasons they did hire these workers were to train and test them risk free (40%) and to have an extra employee at low cost (32%).

Taken together, this evidence suggests that the main reason that the impact of the voucher was mostly temporary was that the hard cash of the wage subsidy induced firms to take a chance on hiring workers they wouldn't have otherwise hired, but these workers then either proved not to be productive enough to earn the wages they would need to be paid, or ex post, these workers decided that the characteristics of the job were not a good match for them.

There are two other reasons why the vouchers may not have longer-term effects. First, one potential goal of the voucher was to induce employers who had not hired women before to give them a chance, thereby overcoming prejudice and giving the graduates a chance to prove they could be productive. However, almost all of the voucher recipients were employed in typically female-dominated occupations (such as nurse, teacher, or clerk), so there was no new information about the productivity of women generated for most firms. Secondly, schools and hospitals may find it harder to generate additional profits out of new workers than other sectors of the economy (e.g. because they have less control over pricing), so even productive workers may be difficult for these firms to finance.

#### **6.4 Is this a gender or a youth effect?**

Our initial survey of students at these eight community colleges also surveyed the males. However, there were only 427 male students, of which 345 passed their examinations. This small sample coupled with budget limitations led us to restrict the experiment to females. A consequence is that we do not know whether the impacts of the treatments reflect attributes of youth or are a feature of the gender of these youth. However, we followed up with the male graduates in our surveys, and we find much higher employment rates for the male than female graduates of these same community colleges. At midline, 54 percent of male graduates inside Amman and 51 percent outside Amman were employed, which rises to 75 (83) percent inside Amman and 61 (65) percent outside Amman by the first (2<sup>nd</sup>) endline. In the first endline after controlling for faculty of study and specific college major, men in Amman (outside Amman) are 30 (45) percentage points more likely than women in Amman to be employed. Clearly, female youth have a particularly hard time finding jobs relative to their male peers.

#### **6.5 Are labor laws partly responsible for the temporary nature of the job?**

Article 35 of Jordan's labor law specifies a 3-month probationary period, during which an employer can terminate a worker without notification or termination remuneration. After this period, employers are required to give one month's notification, and remunerate workers one month per year of service on a pro-rated basis upon termination. The length of the wage voucher period was set at 6 months with this 3-month rule in mind, the idea being that the six-month subsidy may induce them to hire graduates beyond the three-month promotion period and thereby bring graduates into the formal employment system. We see that in practice, this does not occur; most of the added employment was not registered for social security, and only 5 percent of vouchers were used for three months. The threat of being subject to labor regulations in the future may still deter firms from keeping youth employed for long

periods, but this was not something that came out in the firm surveys. Thus, it seems that these labor laws were not the main constraint to firms keeping the voucher hires employed because they were able to avoid these regulations.

In contrast, 84 percent of the graduates employed with the voucher at the midline were hired at a wage of exactly 150 JD per month – which was the minimum permitted by the program. Coupled with the evidence above that a prime reason that graduates were let go by their firms once the subsidy period ended, this suggests that minimum wages that are higher than marginal products may be one important regulatory reason that the impacts were temporary and that youth unemployment rates are so high. The main incentive to avoid registering workers in social security is likely to be avoiding social security taxes (18.75% of wages) and payroll taxes (7% of wages), which together add 25 percent to the cost of employing a worker.

#### **6.4. Did the interventions just change who got the jobs, or actually create new jobs?**

A common concern with many active labor market policy experiments is the possibility of spillovers or general equilibrium effects. In this experiment, there are two elements of this concern. The first is a concern about interference among the experimental sample. In particular, the concern would be that the voucher, and perhaps training, groups gained jobs at the expense of the control group, so that there is no net increase in employment, just a reallocation within sample of who gets the jobs. If this were the case, while our experiment would still give an internally valid estimate of the impact of giving vouchers to some youth and not others, one could not extrapolate from this to estimate the impact of offering this program to all community college graduates. A second, related, concern is whether the jobs gained by the community college graduates are coming at the expense of other workers outside of the experimental sample who would otherwise have been hired. For example, the vouchers may induce firms to hire youth instead of an unemployed older worker. If this were the case, the experiment would still show that these policies help a disadvantaged group obtain jobs, but not whether there are costs to others in society of doing so.

In a large country with segregated labor markets one could experimentally address this issue by randomizing the intensity of the treatments, as was done by Crepón et al. (2013) in France. This was not a possibility in a small country like Jordan, and so we use a mixture of evidence to assess how important these spillovers are likely to have been.

We note first that the fact that most of the effect was temporary, and came from firms saying they hired the wage voucher workers when they wouldn't have hired them otherwise suggests that most of the short-lived effect is additional (temporary) hires, rather than firms substituting hires they would have made anyway. Second, we do not see firms who let go of the wage voucher workers subsequently hiring a control group or training group worker to replace them: there are only 12 firms in our firm survey that hired graduates from both the voucher and from either the control or training only groups (almost all hospitals hiring nurses), and all of these were cases of concurrent hires rather than terminating a voucher student and replacing them with one of the other group students.

However, our intervention worked with approximately 80 percent of females graduating from public community colleges in Jordan in 2010, and thus if there are a limited number of jobs that these graduates are competing for, it seems plausible that they are likely to have been competing for some of the same jobs, resulting in displacement effects. Indeed, when the graduates were asked in the endline survey whether they think the voucher prevented women without vouchers from getting jobs because employers would only hire workers with vouchers, 12 percent of the control group in Amman, and 24 percent of the control group outside Amman agreed.

Further evidence on this displacement comes from looking at the employment rates in other recent years. Table 9 uses the 2007 to 2012 Jordanian Employment and Unemployment Surveys<sup>16</sup> to report the employment rates and labor force participation rates of intermediate diploma students (the group community college students fall into), and compares these to our endline employment rates. We see that the employment rate in central Jordan (inside Amman) for our control group is slightly higher than that of community college graduates in recent years, whereas that northern and southern Jordan (outside Amman) is lower.<sup>17</sup> Taking the difference between the two locations, we see the employment gap between Amman and outside Amman for the control group is more than double in sample for both 2011 and 2012 than it was since 2008. Coupled with the reports from graduates, we view this as compelling evidence of a displacement effect – graduates outside Amman in the control group appear to have not been hired at the same rates that recent years would suggest or at the rate that one would

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<sup>16</sup> We thank the Hashemite Kingdom of Jordan Department of Statistics for supplying us with these summary statistics. It should be noted that EUS employment reports are published 1 year after data collection, so the 2007 EUS reports employment for 2006.

<sup>17</sup> Note that although the control mean is higher than the Central Jordan employment rate in previous years, the confidence intervals overlap. We focus on the Central Jordan vs. Northern and Southern Jordan difference.

predict given the employment rate of the control group in Amman while graduates outside Amman in the voucher group were hired at similar rates of recent years.

This displacement also appears to be taking place in terms of labor force participation outside of Amman. Labor force participation for intermediate diploma students outside of Amman over the 2007 to 2012 period averaged 52.5 percent and was never more than 5 percentage points off except for outside of Amman in 2011. In contrast, although labor force participation was 72.9 percent for the control group outside Amman (and 82.9 percent in Amman) at the time of the midline survey, the labor force participation rate fell to 60.9 percent and 38.7 percent inside and outside Amman, respectively, by the first endline survey. By the second endline survey, the rate changed to 66.5 percent and 50.6 percent inside and outside of Amman, respectively. Of the control group individuals outside Amman who stopped looking for work at the first endline, 45 percent say it was because they are pessimistic about the chance of finding a job or believe no job is available in the area, 30 percent say they have given up because of marriage, pregnancy, or looking after other family members, and 12 percent because they are waiting on a response from a government agency they have applied to.

The main occupations for graduates employed outside Amman at the first and second endline surveys are teacher (23%, 15%), nurse (19%, 30%), pharmacist (16%, 15%), and clerk (11%, 18%), respectively. The labor market outside Amman in most of these occupations is likely to be relatively thin, with a limited number of openings for new graduates each year. It does therefore seem reasonable that graduates may have been competing for some of the same jobs, and that, in addition to the temporary additional hires firms made using the vouchers, they chose voucher or training graduates over the control group for positions they were planning to hire anyway. As a result, the treatment effects seen in the endline outside of Amman are likely to reflect displacement rather than added employment.

## **7. Conclusions**

Wage subsidies and soft skills training are two popular types of policies that governments are turning to around the world as part of their efforts to deal with high youth unemployment. Our experimental analysis shows these policies do not appear to have had large impacts on generating sustained employment for young, relatively educated women in Jordan. Short-term wage subsidies generated large and significant increases in employment while the subsidies were in effect, but most of these jobs disappeared when the subsidies expired. High minimum wages may be one reason, with firms saying that graduates were not productive enough to be affordable without subsidies. Since our

intervention ended, the minimum wage has been raised even higher, suggesting young women will continue to struggle to find paid employment.

Using an audience expectations elicitation exercise we show that there is considerable heterogeneity in beliefs about the likely effectiveness of such programs, and substantial underestimation of the short-term impacts of the voucher program. This both serves as an illustration of an approach that could be used in other studies to reveal the extent to which impact evaluations confirm or contrast with existing priors, as well as serving to show that the results generated here are different from what many people would expect.

The wage subsidy intervention did succeed in getting graduates to have work experience they otherwise would not have had, while the soft skills training intervention resulted in improvements in positive thinking and mental health. The fact that we do not see employment impacts 16 months or 28 months after graduation shows the tremendous challenge in creating employment for this demographic. Interventions to address supply-side constraints that constrain firms from creating more jobs, especially jobs for young women, may instead be needed to address the problem of persistent low employment for women throughout most of the Middle East.

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**Table 1: Most Common Courses of Study for Experimental Sample**

Program Code Level	%	Specialization Level	%
The Administrative and Financial Program	43	Nursing	13
Program of Medical Assistance	24	Accounting	12
Educational Program	10	Electronic administration	12
Performing Arts Program	7	Management information systems	10
Social Action Program	6	Other -- Educational Programs	9
Information Management and Libraries Program	6	Pharmacy	5
Engineering Program	2	Interior Design & Graphic	5
Science Program of Sharia and Islamic Civilization	2	Special education	5
Hotels Program	1	Information technology	4
Agriculture Program	0	Accounting Information Systems	4

**Table 2. Comparison of Means of Baseline Characteristics by Treatment Group**

	Voucher Only	Training Only	Voucher & Training	Control Group
<i>Stratifying Variables</i>				
In Amman, Salt, or Zarqa	0.43	0.44	0.43	0.44
Tawjihi score above median	0.55	0.55	0.55	0.55
Low desire to work full time	0.41	0.41	0.41	0.41
Is allowed to travel to the market alone	0.51	0.51	0.51	0.51
<i>Other Baseline Variables</i>				
Age	21.2	21.1	21.1	21.3
Married	0.14	0.16	0.12	0.13
Mother Currently Works	0.07	0.06	0.08	0.06
Father Currently Works	0.59	0.61	0.57	0.53
Has Previously Worked	0.15	0.18	0.16	0.16
Has a Job Set Up for After Graduation	0.05	0.08	0.10	0.08
Has Taken Specialized English Training	0.31	0.26	0.26	0.30
Household Owns Car	0.62	0.66	0.62	0.64
Household Owns Computer	0.72	0.75	0.74	0.70
Household Has Internet	0.28	0.18	0.26	0.26
Prefers Government Work to Private Sector	0.82	0.81	0.79	0.81
Sample Size	299	300	299	449

Note: The only statistically significant difference across groups is internet access which is significant at the 10% level.

**Table 3. Take-Up Regressions, and Correlates of Employment in the Control Group**

	Training Take-up		Voucher Take-up		Midline Employment in Control Group		First Endline Employment in Control Group		Second Endline Employment in Control Group	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>Stratifying Variables</i>										
Amman, Salt, or Zarqa	0.025 (0.040)	-0.024 (0.042)	-0.174*** (0.041)	-0.159*** (0.043)	0.171*** (0.040)	0.167*** (0.041)	0.270*** (0.042)	0.244*** (0.043)	0.218*** (0.045)	0.184*** (0.048)
Tawjihi score above median	-0.086** (0.040)	-0.078** (0.039)	0.001 (0.041)	-0.005 (0.041)	0.041 (0.037)	0.014 (0.037)	0.003 (0.039)	-0.027 (0.040)	0.003 (0.043)	-0.028 (0.043)
No desire to work full time	-0.102** (0.041)	-0.100** (0.041)	-0.051 (0.042)	-0.038 (0.041)	-0.073** (0.037)	-0.051 (0.036)	-0.082** (0.038)	-0.068** (0.038)	-0.084** (0.042)	-0.070* (0.042)
Is allowed to travel to the market alone	-0.031 (0.040)	-0.023 (0.039)	0.069* (0.041)	0.071* (0.041)	0.012 (0.037)	0.021 (0.037)	-0.005 (0.039)	-0.010 (0.039)	0.033 (0.042)	0.033 (0.043)
Dual Treatment Group	0.058 (0.039)	0.050 (0.039)	0.007 (0.040)	0.004 (0.040)						
<i>Other Variables according to Baseline Status</i>										
Married at Baseline		-0.197*** (0.060)		-0.087 (0.061)		-0.101*** (0.036)		-0.127*** (0.036)		-0.129*** (0.045)
Wealth Index		0.004 (0.011)		0.013 (0.012)		-0.013 (0.011)		0.011 (0.011)		0.018 (0.012)
Number of brothers		0.021** (0.010)		0.016 (0.012)		-0.006 (0.010)		-0.008 (0.009)		-0.005 (0.011)
Number of sisters		-0.007 (0.009)		-0.009 (0.009)		-0.006 (0.008)		0.005 (0.008)		-0.006 (0.010)
Has E-mail		0.113** (0.054)		-0.013 (0.058)		0.077 (0.071)		0.103 (0.074)		0.061 (0.080)
In Admin/Finance Program		0.114*** (0.040)		-0.113*** (0.042)		-0.115*** (0.036)		-0.104** (0.038)		-0.129*** (0.041)
Mean Employment of Control Group					0.18	0.18	0.23	0.23	0.25	0.25
Sample Size	599	599	598	598	398	398	419	419	401	401

Note: Huber-White standard errors in parentheses. \*, \*\*, and \*\*\* indicate significance at the 10, 5 and 1% levels respectively.

Even numbered regressions include dummy variable for missing baseline variables

**Table 4: Impacts on Different Dimensions of Employment**

	Labor Force Participation	Employed	Employed and registered for Social Security (Survey)	Employed and registered for Social Security (Admin data)	Ever Employed	Months Employed Since Graduation	Hours Worked Last Week	Work Income (not conditional on working)	Work Income (conditional on working)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Midline Results (April 2011)</i>									
Assigned to Voucher	0.028 (0.031)	0.395*** (0.035)	0.045** (0.022)	0.005 (0.020)	0.286*** (0.036)	1.186*** (0.461)	13.417*** (1.301)	64.499*** (5.784)	23.728*** (8.176)
Assigned to Training	-0.002 (0.033)	0.031 (0.031)	0.034 (0.022)	-0.002 (0.019)	-0.001 (0.034)	-0.000 (0.479)	1.094 (1.172)	5.689 (4.828)	5.585 (10.350)
Assigned to Both	0.055 (0.046)	-0.022 (0.052)	-0.055 (0.034)	-0.010 (0.029)	0.063 (0.054)	0.075 (0.534)	-0.156 (1.963)	-4.208 (8.600)	-6.625 (11.138)
Sample Size	1,237	1,237	1,237	1,282	1,249	1,237	1,237	1,237	448
Control Mean	0.77	0.18	0.07	0.07	0.27	1.2	6.6	24.9	142
<i>Panel B: 1st Endline Results (December 2011)</i>									
Assigned to Voucher	0.100*** (0.037)	0.028 (0.032)	0.016 (0.026)	-0.015 (0.026)	0.271*** (0.036)	2.456*** (0.393)	1.534 (1.426)	6.422 (6.140)	7.560 (9.438)
Assigned to Training	0.054 (0.037)	0.015 (0.032)	0.039 (0.027)	-0.005 (0.026)	0.036 (0.037)	0.115 (0.368)	1.535 (1.448)	1.065 (5.817)	-10.652 (9.954)
Assigned to Both	-0.044 (0.055)	-0.025 (0.048)	-0.066* (0.039)	-0.012 (0.038)	0.037 (0.054)	-0.106 (0.575)	-1.463 (2.201)	-3.207 (9.133)	7.107 (14.410)
Sample Size	1,287	1,287	1,249	1,282	1,250	1,249	1,249	1,249	312
Control Mean	0.48	0.23	0.13	0.15	0.39	2.6	9.6	40.3	172
<i>Panel C: 2nd Endline Results (January 2013)</i>									
Assigned to Voucher	-0.012 (0.038)	0.014 (0.033)	-0.010 (0.030)		0.214*** (0.035)	2.827*** (0.671)	0.679 (1.404)	6.516 (7.491)	4.793 (13.511)
Assigned to Training	-0.017 (0.038)	0.033 (0.034)	0.019 (0.030)		0.040 (0.037)	0.499 (0.665)	1.625 (1.436)	9.007 (7.573)	-2.547 (13.181)
Assigned to Both	0.031 (0.057)	0.000 (0.051)	-0.004 (0.045)		0.022 (0.052)	-1.370 (0.975)	0.311 (2.198)	3.074 (11.843)	19.070 (19.674)
Sample Size	1,236	1,236	1,117		1,255	1,118	1,206	1,236	310
Control Mean	0.58	0.25	0.17		0.51	5.3	9.6	46.5	212

Note: Huber-White standard errors in parentheses. \*, \*\*, and \*\*\* indicate significance at the 10, 5 and 1% levels respectively.

All regressions also control for stratification dummies. Outcome of (5) and (6) not available in midline survey but derived from recall data in 1st endline.

**Table 5. Heterogeneity of Employment Impact by Randomization Stratification Variables**

Interaction	Amman	High Aptitude	Low Desire to work full-time	Allowed to Travel to Market Alone
<b>Dependent Variable: Employed</b>	(1)	(2)	(3)	(4)
<i>Panel A: Midline Results (April 2011)</i>				
Assigned to Voucher	0.504*** (0.043)	0.426*** (0.050)	0.380*** (0.046)	0.339*** (0.049)
Assigned to Training	0.019 (0.033)	0.061 (0.043)	0.022 (0.042)	0.049 (0.044)
Assigned to Both	0.021 (0.064)	-0.041 (0.076)	-0.010 (0.069)	0.025 (0.075)
Voucher*Interaction	-0.254*** (0.071)	-0.058 (0.069)	0.036 (0.070)	0.111 (0.069)
Training*Interaction	0.021 (0.064)	-0.056 (0.062)	0.023 (0.061)	-0.033 (0.062)
Both*Interaction	-0.087 (0.105)	0.035 (0.104)	-0.029 (0.105)	-0.094 (0.104)
Sample Size	1,237	1,237	1,237	1,237
Control Mean when Interaction=0	0.100	0.142	0.217	0.163
Control Mean when Interaction =1	0.284	0.209	0.123	0.193
<i>Panel B: 1st Endline Results (December 2011)</i>				
Assigned to Voucher	0.085** (0.037)	0.013 (0.044)	-0.007 (0.043)	0.031 (0.045)
Assigned to Training	0.061* (0.035)	0.015 (0.045)	0.012 (0.044)	-0.026 (0.042)
Assigned to Both	-0.059 (0.057)	0.022 (0.068)	-0.019 (0.064)	-0.005 (0.066)
Voucher*Interaction	-0.130* (0.066)	0.028 (0.063)	0.088 (0.064)	-0.005 (0.064)
Training*Interaction	-0.105 (0.066)	0.001 (0.063)	0.009 (0.062)	0.080 (0.063)
Both*Interaction	0.079 (0.099)	-0.085 (0.096)	-0.014 (0.096)	-0.039 (0.096)
Sample Size	1,287	1,287	1,287	1,287
Control Mean when Interaction=0	0.106	0.205	0.275	0.223
Control Mean when Interaction =1	0.386	0.249	0.163	0.235
<i>Panel B: 2nd Endline Results (January 2013)</i>				
Assigned to Voucher	0.085** (0.041)	0.016 (0.046)	0.016 (0.046)	0.018 (0.046)
Assigned to Training	0.059 (0.040)	0.027 (0.047)	0.029 (0.047)	0.077 (0.048)
Assigned to Both	-0.016 (0.064)	0.029 (0.073)	-0.034 (0.069)	-0.072 (0.071)
Voucher*Interaction	-0.158** (0.067)	-0.004 (0.066)	-0.005 (0.065)	-0.009 (0.066)
Training*Interaction	-0.056 (0.070)	0.011 (0.068)	0.010 (0.067)	-0.086 (0.068)
Both*Interaction	0.038 (0.104)	-0.052 (0.102)	0.084 (0.102)	0.142 (0.102)
Sample Size	1,236	1,236	1,236	1,236
Control Mean when Interaction=0	0.151	0.232	0.300	0.228
Control Mean when Interaction =1	0.379	0.273	0.189	0.279

Note: Huber-White standard errors in parentheses. \*, \*\*, and \*\*\* indicate significance at the 10, 5 and 1% levels respectively. All regressions also control for stratification dummies.

**Table 6: Impacts on Wellbeing, Empowerment, Attitudes, and Marriage**

	Life Ladder (1)	Life Ladder Future (2)	MHI 5 (3)	Severely Poor MH (4)	Mobility Index (5)	Empowerment Index (6)	Married at 1st Endline (7)	Married at 2nd Endline (8)
Assigned to Voucher	0.577*** (0.180)	-0.200 (0.159)	-0.177 (0.276)	0.017 (0.031)	-0.543*** (0.142)	-0.060 (0.068)	0.026 (0.036)	0.009 (0.038)
Assigned to Training	0.283 (0.185)	0.266** (0.131)	0.581** (0.281)	-0.048* (0.029)	-0.092 (0.125)	-0.017 (0.072)	-0.003 (0.036)	-0.039 (0.039)
Assigned to Both	-1.006*** (0.265)	-0.072 (0.210)	-0.555 (0.415)	0.030 (0.045)	0.927*** (0.189)	0.083 (0.102)	-0.032 (0.053)	-0.000 (0.057)
Sample Size	1,249	1,249	1,249	1,249	1,249	1,249	1,249	1,236
Control Mean	4.970	8.128	19.266	0.197	5.192	4.847	0.313	0.489

Note: Huber-White standard errors in parentheses. \*, \*\*, and \*\*\* indicate significance at the 10, 5 and 1% levels respectively. All regressions also control for stratification dummies.

Severely Poor MH indicates MHI-5 index is below 17.

**Table 7a: Expected Impact of the Interventions from seminar participants and online respondents**

	N	Actual Treatment Effect	$\mu_{\text{expected}}$	$\sigma_{\text{expected}}$	median <sub>expected</sub>	% of expectations within 95% C.I.
<b>Expected Impact at Midline</b>						
Voucher only	137	40	12	12	8	4
Training only	139	3	9	10	5	63
Voucher and Training	135	40	17	14	11	9
<b>Expected Impact at 1st Endline</b>						
Voucher only	137	3	10	12	5	60
Training only	139	2	10	11	5	63
Voucher and Training	135	2	16	15	10	38

**Table 7b: Expected Impact of Jordanian Policymakers (N=9)**

	0% or less	0 to 5%	5 to 10%	10% or more
<b>Expected Impact at Midline</b>				
Voucher	22%	22%	11%	44%
Training	33%	22%	11%	33%
<b>Expected Impact at 1st Endline</b>				
Voucher	22%	22%	33%	22%
Training	33%	11%	44%	11%

**Table 8: Employment Transitions (Percentage Transitioning from one state to another between survey rounds)**

	Voucher Group		Training Group		Both Treatments		Control Group	
	Endline unemp.	Endline employ.	Endline unemp.	Endline employ.	Endline unemp.	Endline employ.	Endline unemp.	Endline employ.
<i>Panel A: Entire Sample</i>								
Midline unemployed	37.5	5.2	66.1	13.3	37.1	4.7	70.4	12.2
Midline employed	36.8	20.5	8.1	12.6	37.8	20.5	6.6	10.9
Sample Size	288		278		271		395	
<i>Panel B: Amman, Salt and Zarqa</i>								
Midline unemployed	36.8	9.6	50.8	17.5	41.9	7.3	52.1	20.4
Midline employed	28.0	25.6	13.3	18.3	25.8	25.0	7.8	19.8
Sample Size	125		120		124		167	
<i>Panel C: Outside Greater Amman</i>								
Midline unemployed	38.0	1.8	78.2	9.9	33.1	2.6	83.8	6.1
Midline employed	43.6	16.6	4.0	8.0	47.4	16.9	5.7	4.4
Sample Size	163		151		154		228	

**Table 9: Employment rates of 20-25 year old Female Community College Graduates**

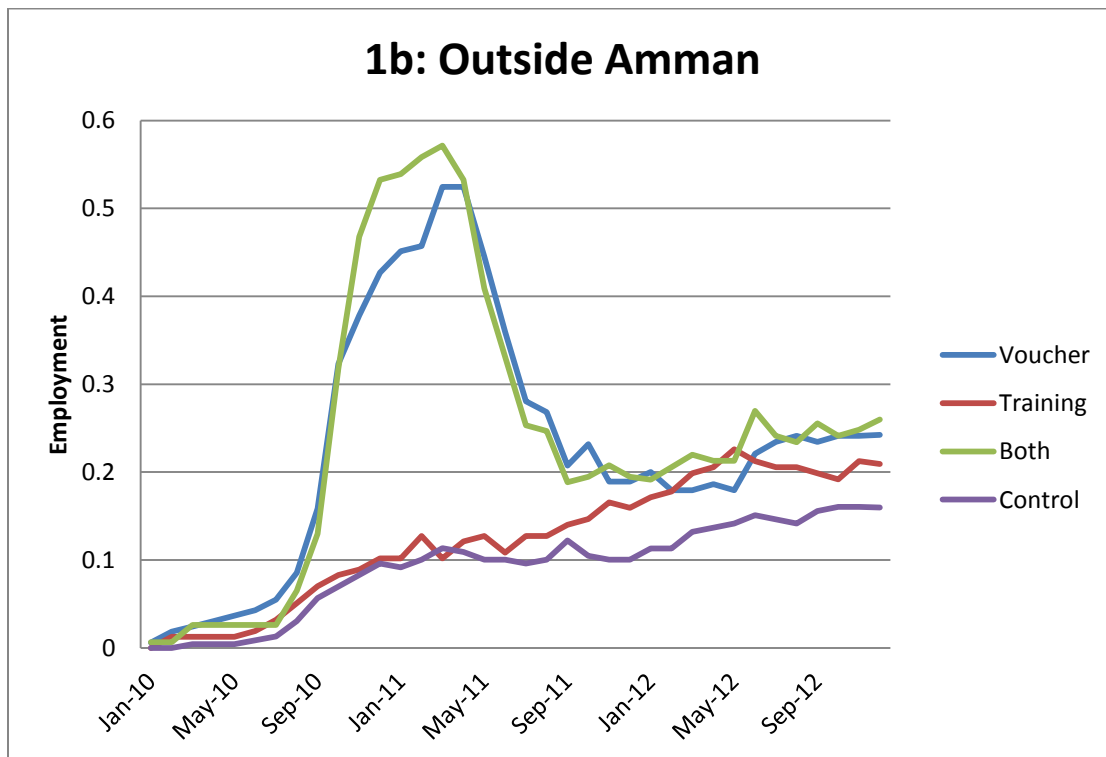
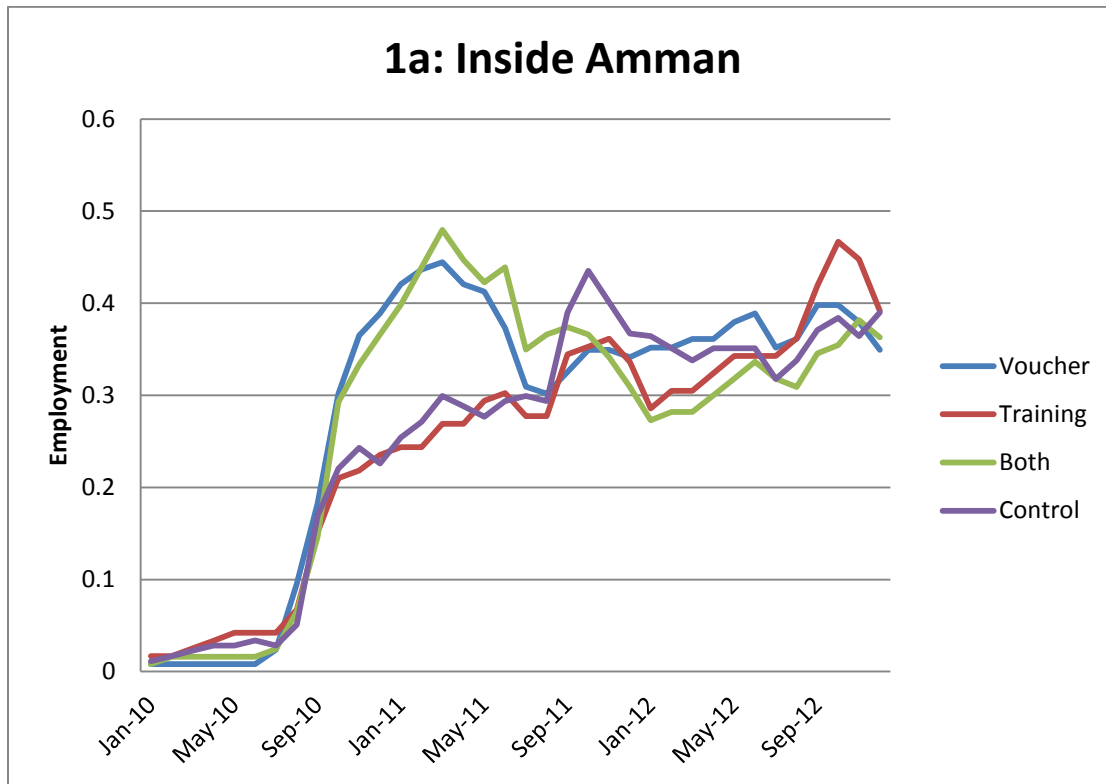
	Official Statistics						Control Group		Voucher Group		Training Only	
	2006	2007	2008	2009	2010	2011	2011	2012	2011	2012	2011	2012
Central Jordan	31	32	31	29	26	28	40	36	34	32	36	39
Northern and Southern Jordan	19	19	20	18	22	23	11	14	19	26	16	19
<i>Difference</i>	13	13	11	11	4	5	29	22	15	6	20	20

Source: 2007 to 2012 Jordan Employment and Unemployment Surveys for 20-25 year old females with intermediate diplomas

Survey standard error is approximately 2 percentage points on sample means

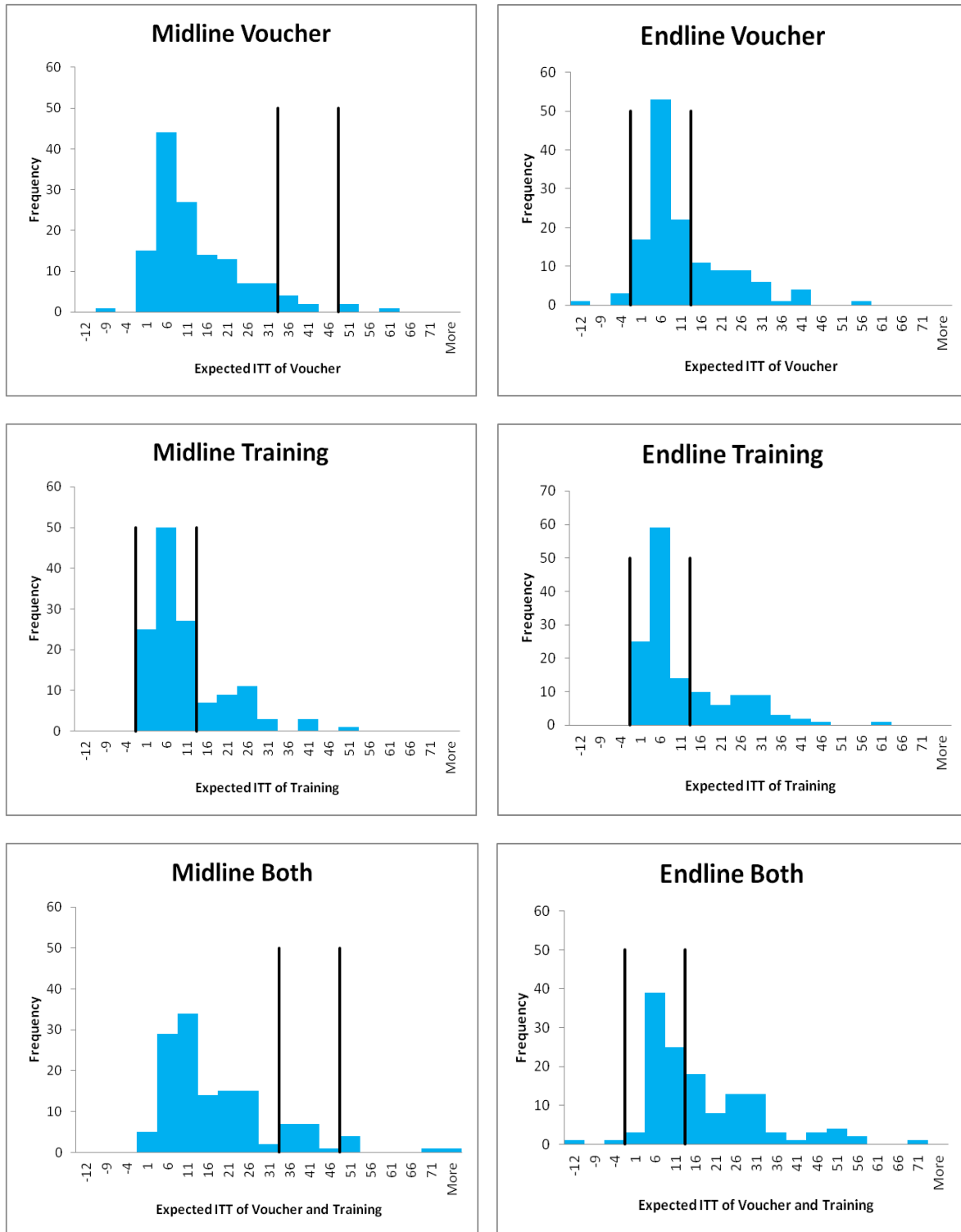
Voucher group includes both voucher only and voucher plus training group

Figure 1: Employment rates over time by location and treatment status





**Figure 2: Histograms of Distribution of Audience Expectations of ITT Impacts on Employment**



Vertical lines show 95 percent confidence interval for estimated treatment effect. Histograms show distribution of audience expectations.

## Appendix I: Robustness of Results to Attrition

Table A1 shows attrition rates by treatment status and round. Attrition levels are low, but do vary slightly with treatment status, being lowest for the wage voucher group. To assess the sensitivity of our employment results to survey attrition, Table A2 provides conservative upper and lower bounds on our point estimates. To construct the lower bound, we assume that all missing control group and training only group individuals were employed, whereas all missing voucher and voucher and training individuals were unemployed. The upper bound reverses this. Our main findings of a large and strongly significant impact of the voucher on employment at midline, and a small and insignificant effect at the first endline continue to hold at either bound. The higher attrition rate at the second endline means wider bounds in this case.

**Table A1. Attrition by Treatment Status**

	Midline	1st Endline	2nd Endline	Social Security
Voucher	0.03	0.01	0.04	0.04
Training	0.09	0.05	0.09	0.04
Both	0.07	0.04	0.08	0.03
Control	0.11	0.07	0.11	0.07
Total	0.08	0.04	0.07	0.05

**Table A2: Upper and Lower Bounds on Voucher Effect due to attrition**

	Midline Employment			1st Endline Employment			2nd Endline Employment		
	Point Estimate (1)	Lower Bound (2)	Upper Bound (3)	Point Estimate (4)	Lower Bound (5)	Upper Bound (6)	Point Estimate	Lower Bound	Upper Bound
Assigned to Voucher	0.394*** (0.035)	0.283*** (0.036)	0.429*** (0.033)	0.0279 (0.032)	-0.0265 (0.032)	0.0504 (0.031)	0.0134 (0.033)	-0.0768** (0.033)	0.0669** -0.0323
Assigned to Training	0.029 (0.031)	0.010 (0.033)	0.030 (0.028)	0.014 (0.032)	(0.002) (0.033)	0.019 (0.030)	0.032 (0.034)	0.016 (0.035)	0.0334 -0.0314
Assigned to Both	-0.017 (0.052)	-0.021 (0.053)	-0.004 (0.049)	-0.023 (0.048)	-0.014 (0.048)	-0.002 (0.047)	0.002 (0.051)	0.005 (0.050)	0.0343 -0.0497
Sample Size	1237	1347	1347	1287	1347	1347	1,236	1,347	1,347

Note: Huber-White standard errors in parentheses. \*, \*\*, and \*\*\* indicate significance at the 10, 5 and 1% levels respectively. All regressions also control for stratification dummies.

## Appendix II: Variable Definition

- *Amman* – a dummy indicating the respondent attended community college in Amman, Salt, or Zarqa, which are all located in Central Jordan
- *Employed* – a dummy indicating the respondent worked for pay in the last month or is currently employed full or part-time.
- *Employed and Registered for Social Security (Administrative)* – a dummy indicating the respondent is formally recorded in the Social Security Corporation’s administrative database
- *Employed and Registered for Social Security (Survey)* – a dummy indicating the respondent reports being formally registered with the Social Security Corporation of Jordan
- *Empowerment Index* – a proxy for female empowerment on a scale from zero to six with a higher score indicating greater empowerment. The index is computed as the number of pro-female responses: 1. Disagree, “A thirty year old woman who has a good job but is not yet married is to be pitied” 2. Agree, “Women should occupy leadership positions in society” 3. Agree, “Women should be allowed to work outside of home” 4. Disagree, “Educating boys is more important than educating girls” 5. Agree, “Boys should do as much domestic work as girls” 6. Disagree, “A girl must obey her brother’s opinion even if he’s younger than her”
- *Ever Employed (Endline)* – a dummy indicating the respondent reports having at least one paid job since she graduated from community college
- *Ever Employed (Midline)* – a dummy indicating the respondent reports being currently employed or having worked for pay within the last three months
- *High Aptitude* – a dummy indicating an above median score on the Tawjihhi test for our sample
- *Hours Worked Last Week* – the number of hours that the respondent reports working in the previous week
- *Labor Force Participation* – a dummy indicating the respondent is *Employed* or has looked for a job in the last month
- *Life Ladder* – a measure of how the respondent feels about her life at the moment measured by the eleven point Cantril self-anchoring striving scale with higher scores indicating positivity
- *Life Ladder Future* – how the respondent feels about how her life will be in five years measured by the eleven point Cantril self-anchoring striving scale with higher scores indicating positivity
- *Low Desire to Work Full Time* – a dummy indicating either the respondent doesn’t plan to graduate, is pessimistic about finding a job after graduation, or expects to work only part-time if she found a job

- *MHI 5* – a measure of psychological well-being and the presence of psychological distress in the past month measured by the Mental Health Inventory (MHI-5) of Veit and Ware (1983) with higher scores indicating better mental health. The five questions are:

1. *During the past month, how much of the time were you a happy person?*

*(1=All of the time, 2=Most of the Time, 3=Some of the time, 4=A little of the time, 5=None of the time)*

2. *How much of the time, during the past month, have you felt calm and peaceful?*

*(1=All of the time, 2=Most of the Time, 3=Some of the time, 4=A little of the time, 5=None of the time)*

3. *How much of the time, during the past month, have you been a very nervous person?*

*(1=All of the time, 2=Most of the Time, 3=Some of the time, 4=A little of the time, 5=None of the time)*

4. *How much of the time, during the past month, have you felt down-hearted and blue?*

*(1=All of the time, 2=Most of the Time, 3=Some of the time, 4=A little of the time, 5=None of the time)*

5. *How much of the time, during the past month, did you feel so down in the dumps that nothing could cheer you up? (1=Always, 2=Very often, 3=Sometimes, 4=Almost Never, 5=Never)*

Questions 1 and 2 are reverse-scored, so that answer one receives score 5, answer two score 4, and so on. Questions 3 through 5 are scored as they appear. This gives a maximum MHI-5 score of 25, and a minimum of 5, with higher scores representing better mental health.

- *Mobility Index* – a proxy for mobility on a scale from zero to six with a higher score indicating more places a girl can travel by herself
- *Months Employed Since Graduation (Midline)* –months that the respondent was employed in her current job
- *Months Employed Since Graduation (1<sup>st</sup> Endline and 2<sup>nd</sup> Endline)* –months that the respondent was employed in her current or most recent job plus the months employed in the her first job
- *Severely Poor Mental Health* – a dummy indicating the respondent reports an *MHI5* score of less than 17, which indicates severe depression according to several studies (e.g. Urban Institute, 1999; Yamazaki et al., 2005)
- *Work Income* – the monthly salary reported by the respondent
- *Wealth Index* – a proxy for wealth with a higher index indicating greater wealth created by a principal component analysis on household assets

### **Appendix III: Correlates of Treatment Take-up**

The first two columns of Table 3 examine the correlates of training attendance among those invited to training. Column 1 first examines how the four stratifying variables relate to take-up: we see no significant relationship with geographic location or with ability to travel to the market unaccompanied, suggesting that the choice of trusted locations in many governorates was successful in reducing geographical and mobility constraints. Attendance is 9.9 percentage points less likely for those who do not believe they are likely to be working full-time after graduation, and 8.6 percentage points less likely for those with tawjih scores above the median, perhaps reflecting that more academically skilled individuals felt they had less need for such training. There was no difference in training take-up among those who also were assigned the wage voucher (which was known to graduates at the time of deciding whether to attend training).

Column 2 of Table 3 then adds several other characteristics of the graduate and their household. We see participation in training is 18.4 percentage points less likely for those graduates who were already married at baseline, even conditional on their expectations of working, empowerment and household wealth levels. Attendance is significantly higher for those taking administrative or financial courses, perhaps because they expect to be in more of a position to need business writing skills than graduates going into nursing or teaching, and higher for those who have email at home.

Columns 3 and 4 of Table 3 examine the correlates of voucher use. We see significantly lower use of the voucher in greater Amman than outside of it, even conditioning on the other stratification variables, and a weakly significant positive effect of having fewer mobility restrictions. Voucher use is not significantly different for those who also were assigned to the training treatment. Being married at baseline has a negative, but not statistically significant impact on using the voucher, while having more female siblings has a significant negative effect of similar magnitude to the relationship to training take-up. In contrast to the impact on training take-up, graduates of administrative or financial courses are less likely to use the voucher than graduates of other specializations.