

# DO WAGE SUBSIDIES PROVIDE A STEPPING-STONE TO EMPLOYMENT FOR RECENT COLLEGE GRADUATES? EVIDENCE FROM A RANDOMIZED EXPERIMENT IN JORDAN

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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. The wage voucher led to a 38 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. The extra job experience gained as a result of the wage subsidy does not provide a stepping-stone to new jobs for these recent graduates, which appears to be due to productivity levels not rising above a binding minimum wage.

## I. Introduction

THE Middle East and North Africa is the region with the highest rates of youth unemployment in the world, with an average rate above 25% (International Labor Organization, 2012). In contrast to most developed countries, unemployment rates are much higher for educated youth than uneducated, with rates particularly high for female educated youth. Youth unemployment in the region has been described as a “jobs shortage” and is currently a major policy issue (Reed, 2011; World Bank, 2013; Sweis, 2014). Jordan shares these general characteristics. Upon graduation from community college, 93% of Jordanian women say they want to work, yet only 25% are working two years later. This enormous gap between expectations and reality highlights the challenge facing young women who want to work.

Firms in the Middle East are often reluctant to hire young women for several reasons. First, many say that youth lack work experience, require on-the-job training, and are a risk because their productivity is difficult to assess. This leads to the catch-22 problem experienced by many youth worldwide: without experience, firms are reluctant to hire them, but without being hired, they cannot get experience. On top of this, employers have doubts about how committed young women are to pursuing careers and whether they are as flexible in working hours and travel as men.

Wage subsidies are a popular policy option to try to help youth overcome these constraints and have a long history of

use by policymakers, particularly with disadvantaged groups (Kaldor, 1936; Layard & Nickell, 1980; Katz, 1998). A subsidy enables firms to take a chance on inexperienced, untested workers, and even if those workers do not remain with the employer who initially hired them, the experience and productivity they gained may act as a stepping-stone to longer-term employment (Bell, Blundell, & Van Reenan, 1999). We test this through a randomized experiment in Jordan with female community college graduates. The Jordan New Opportunities for Women (Jordan NOW) pilot randomly allocated students graduating in 2010 into a group that received a wage subsidy voucher, which could be redeemed by a graduate’s employer for up to six months for an amount equal to the minimum wage, and a control group, which was not eligible for this subsidy.<sup>1</sup> Follow-up surveys then measured employment impacts 6, 14, and 27 months later, covering both the period when the voucher was in effect and the postintervention period.

The wage subsidies led to high short-term gains in employment, with an average treatment impact of 38 percentage points. This increase was particularly large (51.4 percentage points) outside the capital city of Amman. However, by the time of our second follow-up survey, four months after the end of the subsidy period, this treatment effect had dissipated to 1.7 percentage points and is no longer statistically significant. Our longer-term follow-up survey also finds no lasting effect of this initial employment experience on either employment or earnings. Exploring in detail the experience of wage subsidy recipients, we find that they appear to have obtained genuine job experience in jobs associated with their field of study, but that many employers did not find them productive enough to keep on once the subsidy ended. A binding minimum wage appears to prevent employment at wage levels commensurate with productivity.

Wage subsidies have long been used to help disadvantaged groups find jobs in developed countries. Several randomized experiments to measure their impacts in the United States (Burtless, 1985; Dubin & Rivers, 1993) have found disappointing impacts, which the authors attribute to potential stigma effects. Several nonexperimental studies have found some positive impacts (Katz, 1998), although an overview of different wage subsidy evaluations by Betcherman, Olivas, and Dar (2004) concluded that such programs have largely not been effective in developed countries. An experi-

<sup>1</sup> The program also cross-randomized a soft skills training component. Online appendix 1 shows that this component had no direct effect on employment outcomes and no interaction effect with the wage subsidy.

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ment in Canada that gave subsidies for full-time work to welfare recipients (not to their employers) did find positive short-term impacts on employment, but a year and a half after the subsidy had ended, the effect had completely faded (Card & 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 (Almeida et al., 2012). Despite this policy enthusiasm, there is little evidence on the effectiveness of such programs in developing countries. One exception is a randomized experiment by Galasso, Ravallion, and Salvia (2004) in Argentina. They found that wage vouchers given to the unemployed led to a 6 percentage point increase in wage employment 18 months later, although this impact largely occurred in informal and temporary jobs and very few vouchers were redeemed. Early evidence from a recent study in South Africa presents a similar picture, with only 30 out of 2,000 vouchers redeemed (Levinsohn et al., 2013).

In these evaluations, the rather limited effects have in part come from low usage rates of the wage vouchers, preventing studies from seeing whether providing access to subsidized short-term employment can lead to lasting jobs. The literature on whether temporary jobs provide a stepping-stone to long-term employment is largely concentrated in developed countries and has found mixed results. For example, Kvasnicka (2009) in Germany finds no impact of temporary jobs on long-term employment using a matching approach, and Autor and Houseman (2010) used quasi-experimental evidence from Detroit to show temporary jobs do not help, and perhaps hinder, long-term employment there. In contrast, Pallais (2014) experimentally finds an impact of an initial job on subsequent employment in an online market. Since voucher take-up was relatively high in our study, we are able to examine this issue and 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. Our finding that getting a temporary job through a wage subsidy does not help long-term employment is consistent with no stepping-stone effect and in contrast to the view held by many youth that a lack of experience is the main impediment to obtaining a job.

The remainder of this paper is structured as follows. Section II describes the context, our sample, and the details of the intervention; section III the experimental design, data collection, and intervention take-up; section IV the experimental impacts; section V possible explanations for these impacts; and section VI concludes.

## II. Context, Sample, and the Intervention

### A. Labor Market Context

Two striking features of labor markets throughout the Middle East are bulging youth populations and 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 to 29 are the largest demographic group in the country, making up 30% of the total population. In 2011, the unemployment rate for women 20 to 24 years old was 47.6% compared to 23.1% for men of the same age. Only 10.5% of women 20 to 24 years old are employed compared to 49.1% of men of the same age. Women with postsecondary 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 two 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 (International Youth Foundation, 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 overtime, and that women might experience more difficulties interacting with customers in some occupations due to culture (World Bank, 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.

### B. 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 on 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 a 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 as a terminal qualification with skills in a particular field or as a second chance at university admission (Kanaan & Hanania, 2009).

Analysis of labor force survey data from 2007 shows that among 25- to 30-year-olds in Jordan, 67% of women and 72% of men have at most a secondary education. Among

TABLE 1.—MOST COMMON COURSES OF STUDY FOR EXPERIMENTAL SAMPLE

Program Code Level	%	Specialization Level	%
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

the third of women this age with higher levels of education, one-third have a diploma from a community college and two-thirds have a university degree. There are fourteen public community colleges in Jordan, with total enrollment of approximately 11,895 students (7,072 women, 4,823 men) in 2007–2008. These public community colleges have significantly lower tuition than universities and private community colleges. 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 the employment of female graduates.

To conduct this pilot, we chose the eight public community colleges with the largest female enrollment numbers, together comprising over 85% of all female public community college enrollment. 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 refer to central Jordan as “inside Amman” and northern and southern Jordan as “outside Amman.”

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 1,755 female students. In August 2010, the baseline data were merged with administrative data on examination results, which revealed that 1,395 had passed their examinations. We randomly selected 1,350 of these graduates to be our experimental sample.

The typical graduate is 20 to 22 years old, unmarried, and has never worked before. Only 13.8% were married at baseline, and 16.3% have previously worked. Only 7% of these young women’s mothers are currently employed, whereas 57% of their fathers are currently employed. At the

time of the baseline survey, which took place weeks before final examination results were available, only 8% of students had 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.

### C. *The Wage Subsidy Intervention*

Our pilot program was marketed to participants as the Jordan NOW pilot. Graduates assigned to the wage subsidy treatment were given a voucher that they could present to firms while searching for jobs. The voucher had the graduate’s name on it, was nontransferable, and was worth 150 JD (USD \$210), an amount equivalent to the minimum monthly wage, 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 to 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 they were periodically monitored through random visits.

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 six months’ 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 was used 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.

### III. Experimental Design, Data Collection, and Voucher Usage

Randomization into treatment and control groups was done by computer. Students were informed that this was a pilot program, with insufficient funding to enable program provision to everyone, and that a lottery was being used as a way to ensure everyone had an equal chance of being selected. Students were first stratified into sixteen 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 above the sample median, 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 six months of graduating, and whether they were usually permitted to travel to the market alone (a measure of empowerment). Within each stratum, 44.4% of the students were allocated to receive the wage voucher, for a total voucher treatment group size of 600 and a control group of 749.<sup>2</sup> Two of those randomized to receive a wage voucher were actually men who had incorrectly been recorded as women in the baseline questionnaire, and so were dropped from the program, leaving a treatment group of 598 individuals.

The choice of variables on which to stratify was based on two considerations. First, stratifying on the basis of variables that 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 & McKenzie, 2009). Second, stratifying on these variables prevents against chance imbalances in these characteristics and serves as a means for specifying 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

<sup>2</sup> The wage subsidy intervention was cross-randomized with a second intervention, which provided soft skills training. Online appendix 1 notes that this second intervention had no effect and no complementary effect when added to the wage subsidy treatment. After appropriate weighting, this intervention is orthogonal to the one studied in this paper.

TABLE 2.—COMPARISON OF MEANS OF BASELINE CHARACTERISTICS BY TREATMENT GROUP

	Voucher	Control	<i>p</i> -Value
<i>Stratifying variables</i>			
In Amman, Salt, or Zarwa	0.43	0.44	
Tawjihi score above median	0.55	0.55	
Low desire to work full time	0.41	0.41	
Is allowed to travel to the market alone	0.51	0.51	
<i>Other baseline variables</i>			
Age	21.1	21.2	0.542
Married	0.13	0.14	0.618
Mother currently works	0.08	0.06	0.369
Father currently works	0.58	0.57	0.877
Has previously worked	0.16	0.17	0.626
Has a job set up for after graduation	0.07	0.08	0.636
Has taken specialized English training	0.28	0.28	0.992
Household owns car	0.62	0.65	0.348
Household owns computer	0.73	0.73	0.853
Household has Internet	0.27	0.22	0.058
Prefers government work to private sector	0.81	0.81	0.867
Sample size	598	749	
Joint orthogonality test			0.990

*p*-values are from regressions that control for randomization strata. As a result they are not available for the variables used for stratification.

and families outside Amman are more traditional and more reluctant to allow their daughters to work. However, it was unclear whether the intervention would then work better in Amman because there would be fewer other constraints on finding work, or whether it 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 Amman, where it is more challenging to find a job, only those who receive assistance might be able to find one. It was also unclear whether the wage subsidies would act as a complement or a substitute for higher academic ability, higher desire to work, or higher empowerment.

#### A. 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 treatment and control groups, and a test of joint orthogonality cannot reject the null hypothesis that these characteristics are jointly unrelated to treatment status. At baseline, the graduates express high levels of desire to work, with 93% saying they plan to work after they graduate, 91% saying they would like to work outside the house after they are married, and 82% saying they think it is very likely or somewhat likely that they will have a job within six months of graduating. Fifty-nine percent of graduates fall into the category of a high likelihood of working full time.

#### B. Data Collection

Data collection consisted of three follow-up surveys: the first in April 2011, the second in December 2011, and the third in January 2013. The first follow-up took place while

TABLE 3.—VOUCHER USE STATISTICS

Number of Months Used	Number of Students	Percent of Those Using Voucher	Percent Using Voucher at Only One Employer
1 month	8	2.7	100
2 months	8	2.7	100
3 months	15	5	93.3
4 months	18	6	77.8
5 months	27	9	92.6
6 months	224	74.7	87.5

Data were not available for one student who used a voucher.

the voucher was still in effect. The second follow-up took place four months after the last possible date that the voucher could be used. The third follow-up took place seventeen months after the last date the voucher could have been used and allows for measuring longer-term impacts. For the three follow-ups, we successfully obtained data on 92%, 96%, and 92% of graduates in our sample, respectively. In the second and third follow-up surveys, we collected a portion of the survey data (3% and 9%, respectively) by proxy through their relatives. The attrition rates are low in absolute terms and relative to the literature but do vary slightly with treatment status.<sup>3</sup> Online appendix 2 uses bounding approaches to show the robustness of our results to potential selection bias arising from attrition. All of the survey instruments and data are available in the World Bank's Open Data Library.<sup>4</sup>

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 that employed graduates (whether or not they had used the voucher to do so) during the first follow-up survey. This is approximately 100% of firms that were employing graduates with a voucher and more than 67% of firms 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% of the sample.

### C. Voucher Use

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. This is substantially higher than in other wage subsidy studies where few vouchers were redeemed. Use varied significantly with location; only 35% of those eligible in Amman used it, whereas 65% of those eligible outside Amman used it.

Table 3 provides some basic summary statistics on voucher use. Three-quarters of those who used the voucher

did so for the full six months, 9% used it for five months, 6% for four months, and 10% for three months or fewer. Overall 88.4% of those who used the voucher used it at only one employer, including 87.5% of those who used it for the full six months. Only one student used the voucher at three separate employers (and no one at more than this). The modal use is therefore for six months at the same employer, which comprises 65.4% of all those using the voucher. Online appendix table 5 provides a detailed tabulation of the occupations for which individuals were hired using the vouchers. The most common occupations were teachers—typically in nursery schools or for assisting with disabled students (31%), accountants (11%), secretaries and receptionists (10%), nurses (8%), and pharmacist assistants (7%). Ninety-one percent of the employers of individuals using vouchers were from the private sector. The employers tended to be small and medium enterprises, with a mean size of 66 employees and median of 7 employees. In the first follow-up survey, 85% of those employed with a voucher said they earned 150 JD per month, the amount of the voucher, and 1.9% said they were paid less than this. The highest earnings were 320 JD per month.

## IV. Results

To evaluate the impact of assignment to the voucher treatment, we estimate the following equation for graduate  $i$  via OLS over the three follow-up periods  $t = 1, 2, 3$ :

$$\begin{aligned} Outcome_{i,t} = & \alpha + \beta_1 Voucher_i + \beta_2 Voucher_i \times Time2_t \\ & + \beta_3 Voucher_i \times Time3_t + \theta_2 Time2_t \\ & + \theta_3 Time3_t + \sum_{s=1}^{16} \delta_s d_{i,s} + \varepsilon_{i,t}, \end{aligned} \quad (1)$$

where  $Voucher_i$  is a dummy variable taking the value 1 if graduate  $i$  was assigned to receive a wage voucher;  $Time2$  and  $Time3$  are dummy variables for the second and third follow-up surveys, respectively; and the  $d_{i,s}$  are the randomization strata dummies (Bruhn & McKenzie, 2009). The coefficient  $\beta_1$  then measures the intent-to-treat (ITT) effect of the wage voucher in the first follow-up period, at a point when the voucher was still in use.  $\beta_2$  and  $\beta_3$  enable us to examine whether this effect changed in the postintervention period. The total effects of the treatment in follow-ups 2 and 3 are then given by  $\beta_1 + \beta_2$  and  $\beta_1 + \beta_3$ , respectively, and we tested whether these effects are 0. The standard errors are clustered at the individual level.

We focus on ITT impacts that give the average effect of being offered the voucher rather than the effect of actually using the voucher. We chose not to estimate the treatment effect on the treated because 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

<sup>3</sup> For example, the attrition rate by the last survey in Galasso et al. (2004) is 22.5%, and in Levinsohn et al. (2013) it is 39%.

<sup>4</sup> <http://microdata.worldbank.org/index.php/catalog/1549>.

more confidence approaching employers, even though actual take-up of the vouchers was low.

A. Impacts of the Wage Subsidy on Employment

Table 4 reports the results of estimating equation (1) for different employment outcomes. (Appendix A provides more details on how each outcome is constructed.) We begin in column 1 by looking at the impact on labor force participation, defined as either working or actively looking for work. At the time of the first follow-up survey, labor force participation was high for everybody, with 77% of the control group participating. The voucher treatment raised labor force participation by 5 percentage points, which is statistically significant at the 5% level. Labor force participation then fell for the control group by the second follow-up survey, reflecting graduates' stopping looking for work actively. The voucher treatment interaction was positive but not significant in the second follow-up period, so that we can again reject the null of no treatment effect on labor force participation. However, the interaction was negative in the third follow-up period, and the bottom of the table shows that we cannot reject the null of no treatment effect on labor force participation by the last survey.

Column 2 then reports on our main outcome of employment, defined as whether individuals are currently employed or have worked for cash in the last month. Recall that the first follow-up took place in April 2011, approximately eight months after graduation. Only 19.1% of the control group was working at this point. We see an extremely large and strongly significant impact of the wage vouchers on employment, with the 38.4 percentage point treatment effect tripling the employment rate in the control group. However, this impact did not persist once the voucher period expired. There is a large and significant negative interaction effect of 36.7 percentage points with the second follow-up period and 36.9 percentage points with the third follow-up period. As a result, the overall effect of the treatment on employment at follow-up 2 and at follow-up 3 is only 1.7 and 1.5 percentage points, respectively, and in neither case, can we reject the null hypothesis of no effect.

Columns 3 to 5 look at formal employment. Column 3 shows that the vouchers resulted in a 20 percentage point increase in the likelihood of having a job with a formal employment contract in the first follow-up, with this effect then disappearing in the second and third follow-ups. In contrast, columns 4 and 5 show no significant impact even in the short run of the voucher treatment on being registered as employed and registered for social security. Recall that the majority of those who used the voucher did so with the same employer for more than three months. By law, they therefore should have been registered with social security, but firms did not do so. We also saw in the control group that more than half of those who were employed were not registered for social security.

TABLE 4.—TREATMENT IMPACTS ON EMPLOYMENT

	Labor Force Participation (1)	Employed (2)	Formal Contract (3)	Employed and Registered for Social Security (Survey) (4)	Employed and Registered for Social Security (Admin data) (5)	Hours Worked Last Week (6)	Work Income (not conditional on working) (7)	Work Income (conditional on working) (8)	Ever Employed (9)	Months Employed since Graduation (10)
Assigned to Voucher	0.054** (0.023)	0.384*** (0.026)	0.202*** (0.022)	0.018 (0.017)	0.000 (0.015)	13.318*** (0.988)	62.351*** (4.339)	20.402*** (5.568)	0.316*** (0.027)	1.459*** (0.146)
Assigned to Voucher × Follow-up 2 (December 2011)	0.017 (0.031)	-0.367*** (0.029)	-0.205*** (0.025)	-0.033* (0.020)	-0.021 (0.015)	-12.519*** (1.184)	-56.991*** (4.931)	-6.081 (7.511)	-0.022 (0.019)	0.946*** (0.246)
Assigned to Voucher × Follow-up 3 (January 2013)	-0.050 (0.033)	-0.369*** (0.032)	-0.177*** (0.027)	-0.031 (0.025)	N.A. N.A.	-12.405*** (1.268)	-54.335*** (6.405)	-5.810 (9.992)	-0.118*** (0.026)	0.694 (0.462)
Follow-up 2 (December 2011)	-0.265*** (0.023)	0.026 (0.017)	0.040*** (0.013)	0.056*** (0.013)	0.075*** (0.011)	2.459*** (0.729)	10.525*** (2.825)	23.372*** (5.491)	0.124*** (0.014)	1.789*** (0.162)
Follow-up 3 (January 2013)	-0.202*** (0.022)	0.075*** (0.020)	0.071*** (0.014)	0.093*** (0.017)	N.A. N.A.	2.944*** (0.785)	22.826*** (3.827)	64.881*** (7.134)	0.233*** (0.018)	4.704*** (0.318)
Control Mean in first follow-up	0.772	0.191	0.076	0.082	0.071	7.045	27.315	144.102	0.273	0.873
Sample size	3,759	3,759	3,720	3,603	2,564	3,721	3,721	1,049	3,713	3,604
p-value: No effect at follow-up 2	0.010	0.465	0.873	0.422	0.272	0.456	0.236	0.044	0.000	0.000
p-value: No effect at follow-up 3	0.883	0.572	0.254	0.566	N.A.	0.397	0.175	0.132	0.000	0.000
p-value: Equality of follow-up 2 and 3 effects	0.041	0.917	0.217	0.901	N.A.	0.919	0.613	0.977	0.000	0.541

Huber-White standard errors in parentheses, clustered at the student level. Significant at \*10%, \*\*5%, \*\*\*1%. All regressions also control for stratification dummies. Outcome of Ever Employed not available in first follow-up survey but derived from recall data in second follow-up. N.A. denotes administrative data on social security registration not available for third follow-up period.

Columns 6 through 8 look at the intensity of employment and the earnings from this employment. Column 6 looks at weekly hours worked, coded as 0 for those not working. Treatment results in an average of 13 hours more work per week in the first follow-up period, but then we cannot reject the null of no effect in either of the subsequent follow-up periods. The wage voucher group earned 62 JD more per month than the control group in the first follow-up, with the effect of more than tripling the 27 JD per month earned by the control group. However, again, the strong negative interaction effects between treatment and the two subsequent rounds makes the overall treatment effects statistically insignificant in both postintervention survey rounds.

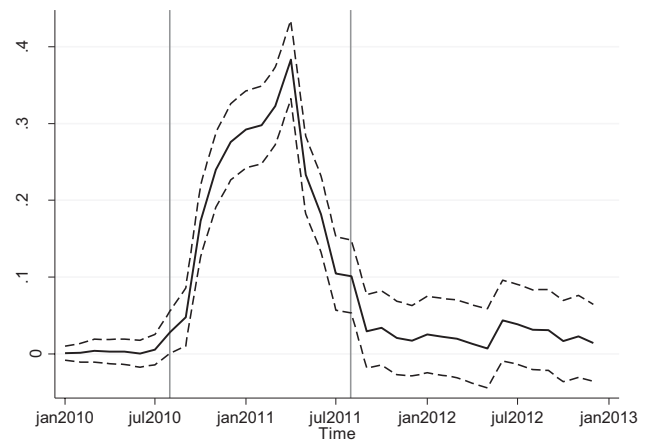
To see whether the higher income of the treatment group in the first follow-up stems from their being more likely to work or also from their earning more conditional on working, we examined the impact on wages conditional on working in column 8. Since this is conditioning on an outcome (employment), which is itself affected by treatment, randomization does not guarantee that the treatment is uncorrelated with other determinants of wages, and this analysis should be considered exploratory only. We find the wage voucher to have increased wages conditional on working in the first follow-up by 20 JD. Since one would expect any selection bias to be negative (individuals who would not have found work otherwise are working because of the voucher), this should be considered a lower bound of the short-term voucher effect on wages earned. Note that the control mean is 144 JD per month, which is slightly less than the minimum wage of 150 JD. By the third follow-up, we cannot reject that there is no long-term effect on wages conditional on working.

Finally, the last two columns of table 4 examine the treatment effects on job experience. This is measured at both the extensive margin (ever employed) and intensive margin (number of months employed). We see that the voucher treatment results in a 31 percentage point increase in the likelihood of ever having been employed since graduation in the first follow-up, with this falling to a 20 percentage point increase in the third follow-up round. The voucher treatment group accumulated 1.4 more months of job experience on average by the first follow-up and 2.2 months by the final follow-up. The treatment therefore had a lasting effect on job experience. The total amount paid as wage subsidies was 243,900 JD (US\$341,460) to achieve this average gain in experience for 598 individuals. The cost per month of experience gained was thus 185 JD per month of experience gained.

#### B. Employment Dynamics and Pooling to Increase Power

To improve power further, we can pool together the second and third surveys to obtain an average effect in the postintervention period (McKenzie, 2012). Online appendix 4 shows that doing this reduces the standard errors for the follow-up estimates slightly but does not change the main

FIGURE 1.—TREATMENT EFFECT ON EMPLOYMENT, BY MONTH



Month-by-month treatment effects on employment, with 95% confidence intervals indicated by the dashed lines. The two vertical lines indicate the period between which the vouchers were announced and the last possible date for voucher use.

results. The average employment effect in the postintervention period has a point estimate of 0.016 with a 95% confidence interval of  $(-0.025, +0.057)$ .

The follow-up surveys asked for job histories, including for each job the month the job started and the month it ended. This enables us to explore the employment dynamics at a monthly frequency. We begin by estimating the following regression,

$$\text{Employment}_{i,t} = \alpha + \sum_{q=1}^{36} \beta_q w_{q,t} \text{Voucher}_i + \sum_{q=1}^{36} \lambda_q w_{q,t} + \sum_{s=1}^{16} \delta_s d_{i,s} + \varepsilon_{i,t}, \quad (2)$$

where the effect of the voucher is allowed to differ by month;  $w_{q,t}$  are month dummies that take the value 1 when  $q = t$  and 0 otherwise;  $d_{i,s}$  are randomization strata dummies, and the standard errors are again clustered at the student level.

Figure 1 plots the estimated voucher effects by month, along with 95% confidence intervals. We see the voucher treatment and control groups are similar before treatment; a treatment effect emerges once the voucher is launched, peaks around the time of the first follow-up survey, is reduced in the last couple of months of the possible period for using vouchers (when many recipients would have already completed their six months of use), and then is small, positive, and no longer significant in any of the post-intervention periods.

We then pool together the sixteen months of postintervention data from September 2011 to December 2013 to estimate the average effect on employment over this period. This allows estimation on 18,567 observations, with standard errors clustered at the student level and with month and stratification dummies included. The pooled treatment effect of 0.024 is not statistically significant, with a 95% confidence interval of  $(-0.016, +0.064)$ . The gains from pooling multiple months are relatively small in this case, since the autocorrelation in monthly employment is high

TABLE 5.—IMPACT ON EMPLOYMENT TRANSITIONS BETWEEN SURVEY ROUNDS

	Stayed Employed	Gained a Job	Exited a Job	Stayed Unemployed	Stayed Employed	
					Same Employer	Switched Employed
Assigned to Voucher × Follow-up 2	0.084*** (0.019)	-0.060*** (0.014)	0.293*** (0.022)	-0.262*** (0.027)	0.049*** (0.016)	0.035*** (0.012)
Assigned to Voucher × Follow-up 3	0.027 (0.019)	-0.002 (0.017)	-0.005 (0.014)	0.018 (0.027)	0.028* (0.016)	-0.002 (0.012)
Second Follow-up	-0.027** (0.011)	-0.012 (0.017)	-0.006 (0.014)	0.037** (0.016)	-0.014 (0.010)	-0.013 (0.010)
Control mean in second follow-up	0.100	0.099	0.067	0.623	0.067	0.033
Control mean in third follow-up	0.126	0.112	0.075	0.585	0.080	0.045
Sample size	2,694	2,694	2,694	2,694	2,694	2,694

Huber-White standard errors in parentheses, clustered at the student level. All regressions also control for randomization strata. Significant at \*10%, \*\*5%, \*\*\*1%.

(McKenzie, 2012). As Hoenig and Heisey (2001) argue, confidence intervals, rather than postexperiment power calculations, are the appropriate guide for interpreting statistically insignificant results and show the range of possible effect sizes not refuted by our data, which enables us to rule out large long-term impacts of the voucher treatment.

### C. Churn and Dynamics

One potential explanation for the short-term impact of the wage subsidies is that they accelerated the process of finding a job, with the control group then subsequently catching up to the treatment group's employment over time as graduates continued to look for jobs. An alternative is that the reduced impact comes from individuals employed with the voucher losing their jobs after the voucher expired. Table 4 shows the control group experienced only modest increases in employment over time. To examine this more systematically, we use our three rounds of follow-up surveys to examine the employment dynamics between the first and second follow-up rounds and between the second and third follow-up rounds.

We then estimate treatment effects on several outcomes that measure transitions. An individual is defined as staying employed in the second follow-up if she was employed in both the first follow-up and the second follow-up and as staying employed in the third follow-up if she was employed in the second and third follow-ups regardless of the first follow-up status. We then similarly define staying unemployed, while gaining a job and exiting a job are defined as having changed employment status between one round and the next. For example, an individual who was employed in follow-ups 1 and 3 but not in follow-up 2 will be defined as having exited a job in follow-up 2 and having gained a job in follow-up 3, while an individual who is employed in follow-ups 2 and 3 but not in follow-up 1 is coded as having gained a job in follow-up 2, and having stayed employed in follow-up 3.

Table 5 provides the results. Only 10% of the control group was employed in both the first and second follow-up rounds, whereas 62% was unemployed in both rounds.

Those assigned to the wage subsidy treatment were 8.4 percentage points more likely to be employed in both rounds and 26.2 percentage points less likely to be unemployed in both rounds. This increase in the likelihood of being employed in both rounds comes from both an increase in the likelihood of staying with the same employer as well as an increase in the likelihood of being employed with different employers in the two rounds. However, we also see that while 9.9% of the control group gained a job between the first and second rounds, this was 6 percentage points lower for the treatment group, and the treatment group was 29.3 percentage points more likely to have exited a job. As a result, it appears that only 17% of the drop in treatment effect can be attributed to the control group catching up, whereas the majority is due to the treatment group exiting the jobs they had. In contrast, the employment dynamics are very similar for the treatment and control groups between the second and third follow-up rounds. The main action is therefore occurring immediately after the end of the voucher use. Online appendix 6 shows that the treatment impact on the rate of job exit between the first and second follow-up rounds is much higher outside Amman than inside, undoing the initially much higher voucher use rates.

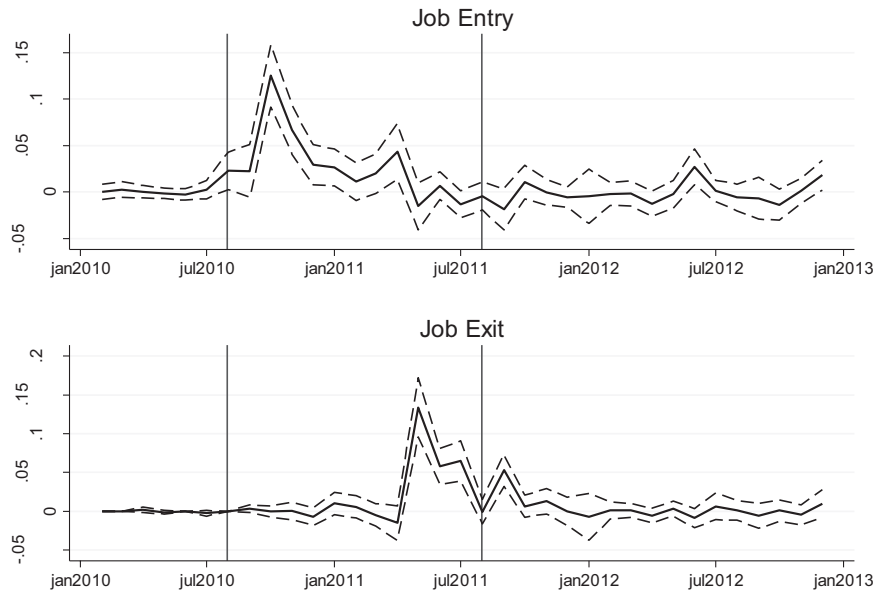
Figure 2 complements this analysis with a plot of the month-by-month treatment effects for job entry and job exit. We see positive treatment effects on job entry in the first few months after the launch of the vouchers and then significant treatment effects on job exit in the last few months of the voucher period.

This population is therefore experiencing high churn in the labor market. By the time of the third follow-up, 50% of the control group had worked at some point since graduation, even though only 26% were currently employed. The mean (median) time in the first job for the control group was 9.6 months (6 months).<sup>5</sup> The mean (median) of 8.4 months (6 months) in the first job for those in the treatment group who got their job using the wage subsidy was thus

<sup>5</sup> This is a censored mean, since 14% of the control group was still employed in their first job at the time of this survey.



FIGURE 2.—TREATMENT EFFECTS ON EMPLOYMENT DYNAMICS, BY MONTH



Month-by-month treatment effects on job entry and job exit, with 95% confidence intervals indicated by the dashed lines. The two vertical lines indicate the period between when the vouchers were announced and the last possible date for voucher use.

providing similar amounts of job experience to the first job of the control group.

#### D. Heterogeneity of Treatment Effects

Online appendix 5 shows that the employment experience of the control group varies considerably with certain baseline characteristics. Individuals in the control group were more likely to be employed in each of the three follow-up rounds if they lived in Amman, had a high desire to work full time, were not married at baseline, and majored in the program of medical assistance. We can then examine whether the wage subsidy treatment had more of an effect for individuals who had a better chance of finding work anyway or for those who were less likely to find work in the absence of any intervention.

To do this, we pool together follow-up rounds 2 and 3 to increase power and ease in interpretation of coefficients, and estimate the following equation:

$$\begin{aligned}
 Outcome_{i,t} = & \alpha + \beta_1 Voucher_i + \beta_2 Voucher_i \\
 & \times Time2or3_t + \gamma_1 Voucher_i \times X_i \\
 & + \gamma_2 Voucher_i \times X_i \times Time2or3_t \\
 & + \theta_2 Time2_t + \theta_3 Time3_t + \delta_2 Time2_t \times X_i \\
 & + \delta_3 Time3_t \times X_i \sum_{s=1}^{16} \delta_s d_{i,s} \\
 & + \mu X_i + \varepsilon_{i,t},
 \end{aligned} \quad (3)$$

where  $X$  is the baseline characteristic of interest for heterogeneity testing and  $Time2or3$  indicates the interaction with being in the second or third follow-up round. We examine

heterogeneity with respect to the four variables used to stratify the randomization (and which can therefore be considered prespecified dimensions of heterogeneity to examine), as well as with respect to baseline marital status, and to whether the major studied was medical assistance, since these latter two variables are strong predictors of employment for the control group.

Table 6 presents the results. The first column examines how the treatment effect varied with geographic location. We see that the wage subsidy had a much larger short-term effect (51.4 percentage points) outside Amman than it did in Amman ( $51.4 - 29.7 = 21.7$  percentage points). The foot of the table shows that mean employment for the control group in this first follow-up period was only 10.8% versus 30% inside Amman. The treatment effect outside Amman thus represents a 500% increase in the likelihood of being employed eight months after graduation versus a 72% increase in Amman. The postintervention interaction is negative 44.8 percentage points for Amman, showing that most of this gain in employment disappeared after the voucher period, but the remaining effect of 6.6 percentage points is statistically significant ( $p = 0.010$ ). In contrast, we cannot reject that the treatment effect was 0 in Amman in the postintervention period ( $p = 0.166$ ). We can reject that the effect in Amman was the same as the effect outside Amman in the postintervention period ( $p = 0.008$ ).

In contrast, we see no significant interaction effects with the other randomization stratification variables or with baseline marital status or majoring in medical administration. The foot of the table shows that we cannot reject the null of no treatment effect for any of these subgroups in the postintervention period. This is despite some of these vari-

TABLE 6.—HETEROGENEITY IN TREATMENT EFFECTS  
DEPENDENT VARIABLE: EMPLOYED

	Interaction Is between treatment and:					
	Amman	High Aptitude	Low Desire to Work	Allowed to Travel	Married	Medical Graduate
Assigned to Voucher	0.514*** (0.032)	0.407*** (0.039)	0.372*** (0.034)	0.351*** (0.038)	0.381*** (0.028)	0.391*** (0.030)
Assigned to Voucher × Follow-up 2 or 3	-0.448*** (0.034)	-0.382*** (0.040)	-0.377*** (0.037)	-0.342*** (0.039)	-0.369*** (0.030)	-0.376*** (0.031)
Assigned to Voucher × Interaction	-0.297*** (0.053)	-0.042 (0.052)	0.029 (0.053)	0.064 (0.052)	0.001 (0.071)	-0.034 (0.063)
Assigned to Voucher × Follow-up 2 or 3 × Interaction	0.184*** (0.056)	0.026 (0.055)	0.022 (0.055)	-0.051 (0.055)	0.016 (0.070)	0.038 (0.066)
Follow-up 2	0.019 (0.018)	0.024 (0.022)	0.031 (0.023)	0.014 (0.023)	0.028 (0.019)	0.023 (0.018)
Follow-up 3	0.075*** (0.020)	0.074*** (0.027)	0.084*** (0.025)	0.071*** (0.027)	0.076*** (0.021)	0.030 (0.020)
Follow-up 2 × Interaction	0.016 (0.035)	0.004 (0.033)	-0.012 (0.033)	0.023 (0.033)	-0.019 (0.031)	0.014 (0.042)
Follow-up 3 × Interaction	-0.002 (0.039)	0.002 (0.037)	-0.021 (0.037)	0.008 (0.037)	-0.014 (0.039)	0.184*** (0.045)
Control mean in first follow-up when interaction equals 0	0.108	0.166	0.226	0.182	0.213	0.165
Control mean in first follow-up when interaction equals 1	0.300	0.212	0.140	0.199	0.061	0.279
Sample size	3,759	3,759	3,759	3,759	3,759	3,759
<i>p</i> -value: no effect when interaction = 0 at follow-ups 2 and 3	0.010	0.405	0.852	0.753	0.613	0.488
<i>p</i> -value: no effect for interaction = 1 at follow-ups 2 and 3	0.166	0.771	0.138	0.461	0.509	0.701
<i>p</i> -value: equality of effect by interaction at follow-ups 2 and 3	0.008	0.699	0.220	0.749	0.725	0.949

Each column shows results from interacting treatment effect with the variable listed at the top of the column. Huber-White standard errors in parentheses, clustered at the student level. Significant at \*10%, \*\*5%, \*\*\*1%. All regressions also control for randomization strata dummies, while the last two columns also control for the baseline level of the interacting variable (which is collinear with the strata dummies for the first four columns).

ables being significant predictors of employment for the control group.

Note that we have examined heterogeneity with respect to six variables here. However, the strength of the Amman interaction effect is sufficient that it remains significant even after correcting for multiple hypothesis testing. For example, applying a Bonferroni correction, the uncorrected *p*-value of 0.008 for testing for equality of postintervention treatment effects when the interaction is 0 versus 1 becomes a corrected value of 0.048, which is still significant at conventional levels.

### V. Understanding the Initial Large Treatment Effect and Subsequent Decline

Multiple competing theories could potentially explain the low employment rates of female community college graduates. These theories offer different predictions for the impact of wage subsidies in the short term and for what the likely long-term effect could be, and we can use the evidence from this experiment to help distinguish among the theories.

A first potential explanation for low employment rates is that we are in a standard market-clearing labor market, but high reservation wages cause labor supply to be low at prevailing wages. High reservation wages may arise because family support enables young graduates to live at home and receive support from their parents, while they wait for the chance of a job in the public sector. The result is that labor demand and labor supply intersect at a relatively low level of total employment and relatively high wage, with all

unemployment voluntary. In this model, a wage subsidy increases labor demand while it is in existence by increasing the effective marginal product of labor (the sum of the return to labor plus the subsidy). This should result in an increase in wages, which will then induce more graduates to work, increasing employment while the subsidy is in effect. But once the subsidy is removed, if reservation wages remain unchanged, wages and employment will fall back to their initial levels unless the intervention changes labor productivity.

A second potential explanation for low employment rates is offered by search and matching theory (Mortensen & Pissarides, 1994). Here, high unemployment occurs as a result of search frictions that prevent firms with vacancies from connecting with qualified job candidates. Matching may be particularly difficult for young female workers who have no prior job experience and face employer concerns about their level of commitment to work. In this model, the wage subsidy acts to subsidize the matching process by making it less costly for firms to take a chance on inexperienced workers. This should result in increased wages and employment while the subsidy is in effect. Then when the subsidy ends, even in the absence of a productivity effect, employment and wages could still remain higher than their initial levels if the employment experiences enables current employers to find some good matches or enables workers to more credibly signal their ability to future employers through a job reference.

A third competing explanation is that high unemployment arises due to the presence of a binding minimum wage

that lies above the perceived marginal product of labor for many workers. In this model, the wage subsidy increases the demand for labor, causing a movement along an inelastic supply curve of workers at the level of the minimum wage. This should result in no change in the wage paid to graduates and large increases in employment. However, once the subsidy ends, employment will fall back to its initial level unless the initial job experience increases labor productivity.

#### A. Understanding the Initial Large Treatment Effect

Most of the response to the wage subsidy occurred on the employment rather than wage margin. Our short-term treatment effects are equivalent to a 200% increase in the employment rate of the control group and a 14% increase in the wage earned conditional on working. We find that 85% of the graduates employed with the voucher at the time of the first follow-up survey were hired at a wage of exactly 150 JD per month, the prevailing minimum wage, and the minimum amount permitted by the program.<sup>6</sup> These results seem most consistent with the predominant cause of unemployment being a binding minimum wage. In order for reservation wages to explain this pattern, we would need the supply curve for labor to be highly elastic at wage rates around 150 JD per month. Our second follow-up survey elicited reservation wages for the unemployed, and while there is some clumping at 150 JD, wages would need to have risen to 200 JD per month to get the control group supply of labor up to the level of employment observed in the treated group during the wage subsidy period. Similarly, the predominance of new contracting at exactly 150 JD does not seem consistent with a search and matching model.

Even with binding minimum wages, there are several mechanisms through which a short-term subsidy could have long-term effects on employment and, as a result, through which initial job experience can provide a stepping-stone to subsequent employment. First, it may increase human capital by on-the-job learning, increasing subsequent labor productivity and therefore increasing the number of workers whose marginal product exceeds the minimum wage. Second, it may increase the ability of individuals to credibly signal their human capital to future employers by providing a job reference. This could reduce inefficiencies arising from the perceived marginal product of labor being lower than the actual marginal product for some young workers. Third, if employers are unable to identify *ex ante* who is a good match and who is a bad match for their firm, labor demand may be based on expected productivity rather than actual productivity; subsidizing learning may therefore mean that those found to be high quality may stay in the

job. To attempt to understand the lack of long-term impact, we therefore examine the extent to which these processes took place.

#### B. Is the Initial Increase in Employment Genuine and in Jobs That Could Plausibly Build Human Capital?

A first possible reason for seeing no long-term effect could be that the jobs for which wage subsidies were used did not increase human capital or did not provide job experience that was a credible signal to other employers. The most extreme form of this would be if the students never actually worked in the jobs, colluding with friends or family members to set up fake positions for which they shared the subsidy. A less extreme form would be if employment took place in makeshift, less-skilled jobs unrelated to graduates' field of study, and offering few opportunities to practice and enhance their skills.

Spot checks by the implementation unit were done before payment of the subsidy and verified that those hired were actually working in the jobs they were hired for doing: typical tasks for someone in these positions. Table 7 compares the characteristics of the jobs individuals obtained using the wage subsidy vouchers to those of the other jobs found by graduates in our sample by the end of the voucher eligibility period (August 2011). As noted in online appendix 3, the vast majority of young women hired with the vouchers were hired in positions and sectors consistent with their training, with the main occupations being teachers, nurses, pharmacists, accountants, and secretaries or business administrators. Compared to the jobs found by the control group, those who used wage subsidies were relatively more likely to take accounting jobs and relatively less likely to take positions as nurses and pharmacists.

Only 8% of those using the wage subsidy voucher reported in our surveys that they were related to the owner of the business, which is lower (but not significantly so) than those finding a job without a voucher. When asked how they found the job, the most common way was contacting an employer directly, with a lower percentage relying on family and friends than in the control group (33% versus 44%). Only 9% say they relied on *wasta*, or connections, to get the job.

Despite the baseline preference for public sector work, most jobs are found with small enterprises in the private sector, which is also true of the control group. These jobs were just as likely to come with a formal written contract as jobs by the control group, but much less likely to be registered for social security. Those using the voucher reported working an average of 7.1 hours per day, for 23.4 days a month. We asked what they were doing during that time, and on average they report spending only one hour per week doing tasks unrelated to the occupation or position they had and about 6.3 hours per day doing "real" work as opposed to just sitting around without much to do. When

<sup>6</sup> Jordan subsequently raised the minimum wage to 190 JD per month beginning February 1, 2012, which was thus in effect at the time of our third follow-up survey.

TABLE 7.—HOW DO CHARACTERISTICS OF VOUCHER JOBS DIFFER FROM OTHER JOBS?

	Group Assigned to Voucher Treatments						Control Group	p-Value Voucher Group versus Control	p-Value Used versus Not Used	p-Value Used versus Control
	All Assigned to Voucher	Used Voucher to Get Job				Didn't Use Voucher				
		All Who Used Voucher	Stayed in Job	Left Job	p-value : Stayed versus Left					
<i>Type of job</i>										
Teacher	0.29	0.32	0.44	0.31	0.123	0.20	0.024	0.28	0.732	0.284
Accountant	0.25	0.27	0.18	0.29	0.173	0.17	0.043	0.17	0.019	0.004
Nurse	0.07	0.06	0.03	0.06	0.451	0.12	0.056	0.11	0.167	0.049
Administrative	0.15	0.13	0.09	0.13	0.477	0.21	0.042	0.16	0.644	0.251
Pharmacist	0.04	0.05	0.06	0.05	0.727	0.02	0.276	0.08	0.028	0.095
<i>Type of employer</i>										
Private sector	0.89	0.91	0.94	0.91	0.496	0.84	0.061	0.89	0.944	0.469
Number of employees	31	29	56	25	0.118	40	0.457	49	0.142	0.098
<i>How they found job</i>										
Related to someone	0.09	0.08	0.03	0.09	0.228	0.11	0.493	0.11	0.510	0.393
Used Wasta	0.09	0.09	0.03	0.10	0.269	0.11	0.608	0.06	0.208	0.296
Contacted employer	0.39	0.40	0.26	0.42	0.093	0.38	0.809	0.35	0.296	0.287
Found through family/friends	0.34	0.33	0.26	0.34	0.399	0.37	0.440	0.44	0.011	0.008
Replied to job ad	0.09	0.09	0.12	0.09	0.613	0.09	0.800	0.13	0.125	0.180
<i>Job terms</i>										
Monthly income	159	160	168	159	0.167	158	0.679	145	0.004	0.004
Hours per day	7.24	7.11	7.25	7.09	0.496	7.73	0.001	7.41	0.259	0.061
Days per month	23.4	23.4	23.4	23.3	0.859	23.6	0.521	23.7	0.436	0.344
Formal contract	0.50	0.50	0.59	0.49	0.278	0.50	0.954	0.42	0.131	0.140
Registered SSC	0.19	0.13	0.22	0.12	0.122	0.42	0.000	0.43	0.000	0.000
Hours per week unrelated work	1.19	1.02	1.10	1.01	0.894	1.71	0.173	1.17	0.953	0.695
Hours per day real work	6.43	6.31	5.83	6.38	0.371	6.79	0.220	6.89	0.084	0.046
Sample size	371	277	34	243		94		228		

Data for sample who had found jobs by end of the voucher period (August 2011). Left job versus stayed in job defined as of the second follow-up survey (December 2011).

we asked graduates what the 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% claimed to have learned nothing in the job, which is comparable to the response of those who did not use a voucher for their first job, for which 10.5% said they had learned nothing on the job. When asked in the second follow-up survey, 94.9% of those who used the wage subsidy voucher said they thought the job experience would provide a long-lasting effect for them.

Taken together, this evidence suggests that the additional job experience attained by those using the wage subsidy vouchers was genuine job experience in occupations related to their field of study and in the types of companies similar to those that individuals without a voucher worked in. Note that this does not necessarily imply that these jobs were of the same average productivity as jobs that would have been obtained without the voucher. Productivity could be lower if either the workers are inherently of lower productivity or if employers face diminishing returns to adding more of the same types of jobs. For example, adding a second pharmacist or second accountant may increase firm output by less than adding the first worker did, even if these workers were of equal ability. The next section looks at how those

using the voucher compare to those obtaining jobs without vouchers.

### C. How Do Those Who Used the Voucher Differ, and How Much Did the Voucher Screen Workers?

We divide the sample of individuals who had found a job by the end of the voucher period into four groups: the control group, those in the treatment group who found a job without using the voucher, those in the treatment group who used the voucher for a job and who were still in that job by the second follow-up (four months after the end of the voucher eligibility period), and those who used the voucher but were no longer working for the employer who had redeemed this voucher by the second follow-up. Table 7 then compares the characteristics of jobs among these four groups, while table 8 compares the baseline characteristics of the individuals.

This categorization allows us to examine the extent to which the voucher led to job experience for the types of individuals who would not have otherwise found jobs and the extent to which employers subsequently were selective in which workers they kept when the voucher period ended. Comparing all those who used vouchers to get their job to those who did not, we see that those who used the wage subsidies were much less likely to be in Amman, more likely to

TABLE 8.—HOW DO CHARACTERISTICS OF INDIVIDUALS TAKING VOUCHER JOBS DIFFER FROM OTHER JOBS?

	Group Assigned to Voucher Treatments						Control Group	p-Value Voucher Group versus Control	p-Value Used Voucher versus Control	
	All Assigned to Voucher	Used Voucher to Get Job			p-Value : Stayed versus Left	Didn't Use Voucher				p-Value Used versus Not Used
		All Who Used Voucher	Stayed in Job	Left Job						
Amman, Salt, or Zarqa	0.42	0.34	0.53	0.31	0.011	0.67	0.000	0.64	0.000	0.000
Tawjihi score above median	0.56	0.54	0.65	0.52	0.174	0.64	0.090	0.61	0.265	0.105
Low desire to work full time	0.37	0.38	0.38	0.38	0.966	0.34	0.504	0.29	0.034	0.026
Is allowed to travel to market alone	0.54	0.56	0.35	0.58	0.011	0.50	0.348	0.56	0.717	0.981
Age	21.38	21.36	21.32	21.37	0.930	21.41	0.888	21.00	0.115	0.134
Married	0.10	0.13	0.12	0.13	0.871	0.03	0.009	0.05	0.019	0.002
Mother currently works	0.08	0.09	0.09	0.09	0.972	0.05	0.298	0.08	0.973	0.756
Father currently works	0.59	0.57	0.62	0.56	0.525	0.66	0.115	0.60	0.798	0.441
Has previously worked	0.18	0.17	0.24	0.16	0.248	0.20	0.428	0.21	0.230	0.163
Had a job set up after graduation	0.08	0.07	0.00	0.08	0.092	0.10	0.391	0.09	0.472	0.331
Has taken specialized English training	0.28	0.28	0.24	0.28	0.551	0.29	0.782	0.30	0.710	0.658
Household owns a car	0.61	0.61	0.65	0.60	0.634	0.63	0.778	0.64	0.553	0.513
Household owns a computer	0.72	0.71	0.71	0.71	0.925	0.74	0.552	0.77	0.186	0.148
Household has Internet	0.26	0.24	0.24	0.24	0.905	0.31	0.197	0.24	0.569	0.924
Prefers government to private sector work	0.83	0.83	0.88	0.82	0.357	0.83	0.935	0.83	0.861	0.847
Sample Size	371	277	34	243		94		228		

Data for sample who had found jobs by end of the voucher period (August 2011). Left job versus stayed in job defined as of the second follow-up survey (December 2011).

be married, and had a lower baseline desire to work full time than those finding jobs in the control group or in the treatment group without vouchers. They are also somewhat less likely to have a tawjihi (high school exam) score above average. This suggests that the voucher in part led to job experience for individuals who were living in areas with fewer other job opportunities available, who were less attached to the labor force, and possibly of lower productivity.

One-quarter of the treatment group who had found a job by the end of the voucher period did not use the voucher. Why did firms not take advantage of this subsidy? The modal reason, given by 46% of respondents, was that firms did not want the extra paperwork involved, while another 10% said the employer was ineligible because the employer was informal. This is consistent with main reason vouchers were not used in Galasso et al. (2004) in Argentina: somewhat informal firms were concerned that participating in the program might focus too much government attention on them. Five percent noted the employer was ineligible because it was a government organization, and the main remaining reason was that the worker decided not to mention the voucher to the firm, in part due to imperfect understanding of how the voucher worked.

Among those who used the vouchers, only 14% were still employed in the same job four months after the voucher eligibility period ended. We therefore have a relatively small sample of stayers to compare to those who left the jobs they had used subsidies to get. With this caveat in mind, we see from table 7 that the job characteristics are reasonably similar for those jobs that voucher users kept versus those that they left. In contrast, we see from table 8 more differences in the characteristics of the individuals between these two groups. The most significant differences

are in location, with 53% of those who stayed on the job being located in Amman, versus 31% of those who left the job; and in empowerment, with 35% of those who remained in their job allowed to travel to the market alone versus 58% of those who left the job. Those who left the job are also more likely to have already had a job arranged at the time of graduation. We do not see large differences in characteristics that might be difficult for employers to observe ex ante, but could plausibly determine the success of the job match, such as the individual's desire for full-time work and attitude toward private sector versus public sector work.

We view this evidence as suggesting that those who used the vouchers were not doing different types of jobs but had characteristics that may have made them lower productivity in the jobs they had. This lower productivity may therefore have made them unaffordable at the minimum wage. We examine this next by looking at why those with subsidies left their jobs.

#### D. Why Did the Wage Subsidy Recipients Leave Their Jobs at the End of the Subsidy?

At the time of the first follow-up survey, 57.2% of those who had used the wage subsidy to get a job said that they expected the job to continue once the voucher ended. But as we have seen, only 14% were still employed in the same job four months after the voucher period ended. The second follow-up survey asked those who had been employed with wage subsidies but had left these jobs what the main reason for leaving was. The most common reason, given by 68.7%, was that the job had ended because the voucher had ended; 19.4% quit because they didn't like the job, 5.6% quit for family reasons, and 3.2% quit because they had

TABLE 9.—WHY DID EMPLOYERS HIRE GRADUATES AND WHY DID THEY LET THEM GO?

	All Employers Using Vouchers	Employers Who Retained Voucher Workers	Employers Who Did Not Retain Worker	<i>p</i> -Value: Retained versus Not	Did Not Retain In Amman	Did Not Retain In Outside Amman	<i>p</i> -Value: In Amman versus Outside
Was looking to hire a new worker at the time	0.62	0.70	0.59	0.149	0.62	0.58	0.575
Would have hired the worker without a voucher	0.40	0.48	0.38	0.173	0.45	0.33	0.101
Would have hired the worker had voucher been 50JD	0.70	0.72	0.70	0.726	0.67	0.72	0.498
Worker was an addition to the firm, rather than replacement	0.79	0.69	0.82	0.030	0.79	0.84	0.348
<i>Reason employee no longer works for them</i>							
Employee quit	0.27		0.35		0.42	0.30	0.103
Employee fired	0.07		0.09		0.11	0.07	0.457
Employee unaffordable without subsidy	0.37		0.48		0.36	0.55	0.016
Sample size	229	54	175		66	109	

Source: Firm survey.

found a better job. Voluntary quits were much higher in Amman, where only 51% said the reason was the voucher had ended, compared to outside Amman, where 77% said this was the reason for the job ending.

Table 9 reports responses from the survey of firms that had hired voucher workers, which took place in November 2011. Only 62% of firms using vouchers said that they were looking to hire a new worker at the time they hired the student with the wage subsidy voucher. The main reasons given for hiring a worker when they were not looking to hire were to train and test out a new employee risk free (46%) and that at the subsidized wage, they felt their business could benefit (29%). Only 40% of firms said they would have hired the student without the subsidy, although 70% claimed they would have hired the worker if the subsidy had been only one-third of its actual level (50 JD instead of 150 JD). Firms were mostly (79% of the time) using the voucher to add to the firm's employment rather than replacing an employee who had recently left.

When asked why the wage subsidy recipient was no longer working for them, firm owners said that in 48% of the cases, the worker was unaffordable without the subsidy. This was stronger outside Amman than inside Amman. Thirty-five percent of the time it was because the worker quit due to finding another job, getting married, or giving birth, with this more common outside Amman.

This evidence suggests that the wage subsidy induced firms to take a chance on hiring some workers they would not 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 the workers decided that the characteristics of the job were not a good match for them.

### E. Discussion

We have shown that wage subsidy voucher recipients appear to have received genuine job experience in occupations related to their field of study. The voucher appears to have given some of this additional experience to graduates

who were less attached to the labor market, less able, or working in locations with fewer outside options than other graduates gaining jobs during this time. Most of the decline in treatment effect after the end of the subsidy comes from these individuals losing their jobs rather than the control group catching up. The firms that hired them were not always looking to hire and in many cases would not have hired them without the subsidy. The main reason they appear to have been let go was that the hiring firms did not find them productive enough to hire at existing wages, while minimum wages limit the extent to which wages fall to a level that reflects productivity.

Even if all wage subsidy recipients lost their jobs at the end of the subsidy period, there could still be a stepping-stone effect if the experience gained helps them find employment more easily with other employers. We do not see this occurring despite the typical length of employment in a wage subsidy job being similar to the typical time spent in a first job for those not using subsidies. This is again consistent with the idea that the constraint to employment may not be lack of experience so much as productivity levels that are lower than minimum wages, even after the job experience.

The only significant lasting effect comes outside Amman, where the treatment group has 6.6 percentage points higher employment than the control group in the last two follow-up periods. There are several likely reasons for this larger effect outside Amman. First, since wages are higher in Amman than outside Amman and there are fewer options outside Amman, the wage subsidy was potentially more valuable to both students and employers outside Amman, resulting in larger initial take-up. It is also possible that the wage subsidy resulted in some displacement of the control group outside Amman. We are unable to provide strong evidence on this, but in appendix 8, we offer some suggestive evidence that any displacement was higher outside Amman.

As with any other experiment, the results here are valid for the context in which they were tested. This raises the question of whether Jordan is an outlier in terms of its youth unemployment rates or minimum wage. Available data sug-

gest that this is not the case and that Jordan is indeed fairly representative of countries in the Middle East and North Africa. It is ranked seven out of thirteen countries in the region for female labor force participation and has female youth unemployment rates similar to those in Tunisia, Egypt, and the Palestinian Territories. The minimum wage is also not abnormally high for the region or even globally: the minimum wage of US\$212 per month compares to monthly minimum wages of \$120 in Egypt, \$203 to \$244 in Algeria, and \$333 in Lebanon; at 56% of per capita GDP, it is at a similar relative level to the minimum wages in Turkey and New Zealand. As such, the results may also generalize to helping explain highly educated female youth unemployment in other countries in the region.

## VI. Conclusion

Wage subsidies are a common policy tool that governments around the world use as part of their efforts to deal with high youth unemployment. Our experimental analysis shows that 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. The experience provided by this subsidized job did not result in any higher levels of employment in the postsubsidy period. These findings are consistent with several studies in developed countries that have cast doubt on the idea that temporary jobs provide a stepping-stone to long-term employment (Card & Hyslop, 2005; Kvasnicka, 2009; Autor & Houseman, 2010). The minimum wage 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 that young women will continue to struggle to find paid employment.

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