

An Adaptive Targeted Field Experiment: Job Search Assistance for Refugees in Jordan*

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Abstract

We introduce an adaptive targeted treatment assignment methodology for field experiments. Our Tempered Thompson Algorithm balances the goals of maximizing the precision of treatment effect estimates and maximizing the welfare of experimental participants. A hierarchical Bayesian model allows us to adaptively target treatments. We implement our methodology in Jordan, testing policies to help Syrian refugees and local jobseekers to find work. The immediate employment impacts of a small cash grant, information and psychological support are small, but targeting raises employment by 1 percentage-point (20%). After four months, cash has a sizable effect on employment and earnings of Syrians.

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1 Introduction

Randomized controlled trials (RCTs) have become a widely-used method for policy evaluation (Duflo and Banerjee, 2017). In a conventional RCT, the designer randomly assigns treatments to experimental subjects in order to precisely estimate the effects of all treatments. In many contexts, however, the experimenter is not merely interested in learning whether policies work. Instead, the experimenter wants to maximize the welfare of program participants. To do so, the experimenter only needs to learn which treatment works best. If the experimenter observes treatment outcomes over time, she can use this information in order to adaptively optimize treatment assignment for future experimental participants. The concern for participant welfare is becoming even more salient as the scale and scope of experiments grow, and when experiments are run with vulnerable populations (Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Haushofer and Metcalf, 2020).

Our first contribution is to introduce a methodology for adaptive targeted field experiments that balances the competing goals of precise treatment effect estimation (i.e., minimizing the variance of estimates of effects for *all* treatments) and maximizing the benefits to experimental participants. Our Bayesian algorithm has two key features. First, it is adaptive, i.e., it changes treatment assignment probabilities over time by incorporating information about the successes of treatments of existing experimental participants. Second, it is targeted, i.e., it uses information about the success rates of treatments in every group in order to target treatments for each individual group.

Our second contribution is to implement our methodology in a field experiment. As far as we know, ours is the first implementation of adaptive targeting in a field experiment in development economics. Our field experiment tested three active labour market policies for Syrian refugees and local workers in Jordan. We targeted treatments at 16 different strata of refugees and local workers. We find that our treatments had only a small impact on six-week employment outcomes of jobseekers, but did have larger impacts on longer-run outcomes. In particular, our results suggest that liquidity is an important barrier to labor market access for refugees. We also find that targeting increased employment by one percentage point (or 20 percent). We next describe our methodology, before turning to the context and details of our intervention.

Tempered Thompson Algorithm within a hierarchical Bayesian model The first key feature of our methodology is that our treatment assignment is adaptive. The problem of adaptively assigning treatments in order to maximize outcomes during the experiment is known as a *multi-armed bandit* (MAB) problem; see Bubeck and Cesa-Bianchi (2012) for

a review. MAB problems are often computationally intractable and a large literature in statistics has been devoted to finding tractable and effective heuristics to solve them. But MAB heuristics pose a problem for an experimenter interested in estimating the effects of all treatments: if the experimenter is quickly convinced that a particular treatment is *suboptimal*, she should stop assigning it in the future. As a result, the experimenter might miss out on learning about the effectiveness of good, though suboptimal, policies; furthermore, vanishing assignment shares for some treatments can also result in bias and size distortions for conventional inference (Hadad et al., 2019).

Our Tempered Thompson Algorithm combines the estimation objective of conventional RCTs with the welfare-maximizing objective of bandit algorithms. The designer starts with a prior over the effectiveness of k different treatments; we recommend a diffuse and symmetric default prior. Every period, the designer observes the outcomes of some of the current participants in the experiment. As a result, the designer can estimate the posterior probability \hat{p}_t^{dx} that treatment d is optimal for individuals from stratum x at time t . Then, at time t , the Tempered Thompson Algorithm assigns treatments in the following way, for individuals from stratum x :

With probability γ : assign treatment d to individual i with probability $\frac{1}{k}$;
With probability $(1 - \gamma)$: assign treatment d to individual i with probability \hat{p}_t^{dx} .

The Tempered Thompson Algorithm generalizes two classical treatment assignment protocols. When $\gamma = 1$, our algorithm boils down to a conventional randomized controlled trial. When $\gamma = 0$, our algorithm is the Thompson (1933) algorithm used in many online contexts, including platform revenue management, movie recommendations, and ad placement (Russo et al., 2018). However, when $0 < \gamma < 1$, the Tempered Thompson Algorithm (asymptotically) maximizes welfare of the participants subject to the constraint that every treatment has a probability of assignment at least $\frac{\gamma}{k}$. This allows the designer to target participant welfare while ensuring that she can learn something about the effectiveness of suboptimal treatments. One consequence of the lower bound in the assignment probabilities for each treatment is that we avoid the inferential problems for other adaptive algorithms as described by Hadad et al. (2019). Our main theoretical result (Theorem 1) formally establishes a trade-off between the welfare of participants and the precision of the estimates: as γ increases, the expected variance of treatment effect estimators falls, but the expected outcomes of participants also decrease. We discuss a number of additional considerations that may guide the choice of γ in the main body of the paper.

The second key feature of our methodology is that our adaptive assignment algorithm is targeted. We use a hierarchical Bayesian approach in order to estimate heteroge-

neous treatment effects. Our model allows us to learn the extent of effect heterogeneity across different strata. At each time period t , the treatment effect of each treatment d in each stratum x is estimated as a weighted average of the observed success rate for d in x and the observed success rates for d across all other strata. The weights are determined optimally by the observed amount of heterogeneity across all strata as well as the available sample size in a given stratum. The posterior probability \hat{p}_t^{dx} that treatment d is optimal for stratum x is then calculated from this posterior distribution of treatment effects.

Implementation and Results We implement our methodology in a field experiment designed to help Syrian refugees and local jobseekers in Jordan find wage work. Jordan is a highly relevant context in which to study employment policies for refugees, for at least two reasons. First, employment generation for refugees is a pressing policy concern. Since the start of the Syrian conflict, Jordan received close to 700,000 Syrian refugees — one tenth of its original population (UNHCR, 2020a). Most Syrian refugees live in urban areas, outside camps, and 93% are below the national poverty line (Verme et al., 2015). Second, in 2016, Jordan and the International Community launched the Jordan Compact: in exchange for trade concessions and access to conditional financing, the Government of Jordan agreed to provide 200,000 work permits for refugees, lifting existing legal barriers that prevented them from obtaining work. The Jordan Compact has influenced refugee policy around the world (Betts and Collier, 2017). Legal restrictions to refugee employment are common in both developed and developing countries, and similar compacts are being launched to remove them, for example in Ethiopia. Jordan thus provides an opportune context to understand how to connect refugees to the new employment opportunities that are opening for them. To our knowledge, this is the first field experiment that studies the employment of refugees in a developing country context.

The experimental design and empirical analysis were specified before the start of the experiment in a pre-analysis plan submitted to the AEA registry.¹ The field experiment tests three types of support: a small, unconditional cash transfer (worth about one month of average monthly expenditure); information provision to increase the ability to signal skills to employers; and a behavioral nudge to strengthen job search motivation. These types of support correspond to three barriers – material, informational, and behavioral – that refugees and locals might face in finding and retaining jobs, and are often discussed in the qualitative and non-experimental literature (Schuettler and Caron,

¹ Available at <https://doi.org/10.1257/rct.3870-2.2>.

2020). The program was implemented in Jordan by the International Rescue Committee (IRC) at the height of the Syrian refugee crisis.

In the experiment, we set $\gamma = 0.2$ in the Tempered Thompson Algorithm to ensure that in every period every one of three treatments and the control has at least 0.05 probability of being assigned. We define 16 strata: {Syrian, Jordanian} \times {Female, Male} \times {High school, No high school} \times {Never employed, Ever employed}. Program intake started in mid-February 2019 and ended in December 2019. Overall, we sampled 3,770 individuals, approximately evenly split between Syrians and Jordanians. We track participants' employment outcomes with a 'rapid follow-up' phone interview six weeks after treatment; this survey asks simply about wage employment, and is used to implement our Tempered Thompson Algorithm. We then run detailed follow-up surveys two and four months after treatment. These surveys enable to us measures a broader set of impacts and to study effects over a longer time period.²

Our first finding is that, six weeks after being offered treatment, none of the interventions has significant or meaningful impact on the probability that individuals are in wage employment (the first outcome that we specified in our pre-analysis plan). However, while the control-treatment difference in outcomes is close to zero, we estimate that the average impact of the optimised treatment (i.e. of offering the best possible intervention to each stratum) is about a 1 percentage point increase in employment (a 20 percent gain), suggesting some moderate short-term gains from targeting.

Second, we find that the cash intervention has large and significant impacts on refugee employment and earnings, two and four months after treatment. While employment rates remain stubbornly low in the control group, the cash grant raises job search rates and enables refugees to place more job applications. As a result, four months after treatment, the grant boosts employment by 3.8 percentage points (70 percent) and earnings by 65 percent. These are sizable impacts compared to those documented in the recent literature on active labor market policies (McKenzie, 2017). We also document substantial increases in hourly wages and in the probability of retaining a job between the two and four month interviews, indicating that match quality has also increased. Consistent with the existence of binding liquidity constraints, we find that these impacts are driven by individuals with below-median expenditure at baseline, and that baseline expenditure is significantly associated with job search intensity in the control group.

Third, the information and behavioral nudge interventions also boost job search among refugees and have significant impacts on employment and earnings after two

² We were unable to complete a six month follow-up interview due to the national lockdown in Jordan during the Covid pandemic.

months. However, these impacts are smaller than those of the cash grant and are ultimately short lived. Four months after treatment, we find weaker and insignificant impacts of these interventions.

Fourth, we find essentially no positive effects of treatment on the Jordanian sample.³ The Syrians and Jordanians who take part in our study differ on multiple dimensions at baseline, and their outcomes evolve in different ways over time. In particular, the Jordanians have larger baseline expenditure, search at higher intensity and find jobs faster than the Syrians. This group may thus face weaker or different job search frictions, which are not addressed by our interventions.

These results shed light on the barriers to employment opportunities faced by refugees in a developing-country context. This evidence is particularly relevant for policy, as governments around the world consider expanding legal access to labor markets for refugees. In particular, our results point to the key role played by liquidity constraints, as in classical models of poverty traps (Banerjee and Newman, 1993; Balboni et al., 2020). Our comprehensive findings on these constraints – including the large employment impacts of a small unconditional cash grants, a strong control association between liquidity and job search intensity, and the large heterogeneity of treatment effects with respect to liquidity – gives clean evidence on the impacts of limited liquidity for refugees in a developing country.⁴ At the same time, the job-search impacts of the other two interventions, which do not provide additional liquidity, show that cash is not a binding constraint for all refugees in our sample. Both information and motivation seem to further limit participation in labor markets.

Finally, we evaluate the performance of the Tempered Thompson Algorithm in our setting. We have three central findings. First, the Tempered Thompson Algorithm maximized six-week employment, an outcome that did not respond strongly to treat-

³ We find a large and significant impact of job search from the psychological intervention, but no impacts on employment or earnings. Further, the cash and information interventions do not have significant impacts on any of our pre-specified outcomes.

⁴ There is consistent evidence that interventions that provide much larger cash grants, worth up to one year of income, do not discourage work (Banerjee et al., 2017). These interventions typically aim at fostering entrepreneurship, are often evaluated in rural contexts, and do not measure impacts on job search. Some related evidence comes from interventions that provide transport subsidies to facilitate the migration of the rural poor (Bryan et al., 2014; Meghir et al., 2019). Our findings are unique in that they study the effect of a much smaller grant, they document impacts on job search in an urban labor market, and they focus on a population of refugees. The only comparable intervention design is Banerjee and Sequeira (2020), who find a small unconditional cash grant boosts job search, but not employment, among a different population — young unemployed South Africans. Other urban studies such as Franklin (2018), Abebe et al. (2020) and Abebe et al. (2020) analyse the impacts of conditional transfers or financial application incentives that simultaneously relax individuals’ budget constraint and decrease the ‘price’ of job search relative to other types of consumption. Thus, they do not offer conclusive evidence on the existence of binding liquidity constraints.

ment. As a result, the proportions of individuals assigned to the various treatments did not depart very much from 25 percent and there were no large gains in average employment for our sample that came from adaptive randomization. Second, six-week employment responded to treatment more strongly for some specific subgroups and hence for these groups we see gains from adaptivity. For example, by the end of the trial we assigned 60 percent of the newly sampled Syrian women without tertiary education and work experience to the cash condition. Consistent with this, we find that the optimal targeted policy has a treatment effect on six-week employment that is one percentage point larger than the optimal non-targeted policy (though the confidence sets overlap). This provides suggestive evidence on the benefits of targeting. Third, we simulate what the performance of the Tempered Thompson Algorithm would have been had we targeted two-month employment. We find that the Tempered Thompson Algorithm quickly directs participants towards the optimal interventions and could have doubled the employment gains of a standard RCT. These simulations show that the Tempered Thompson Algorithm can generate substantial gains even with the levels of treatment effects we see in this context, but that the choice of the objective function is quite important. We conclude the paper with a number of specific lessons for the design of future adaptive trials, including the use of surrogate outcomes, continuous (rather than binary) outcomes, choice of sample and wave size, inference, non-stationarity, and alternative adaptive assignment algorithms.

Related literature Our paper spans two distinct literatures. Methodologically, our work is related to experimentation, MAB problems, and targeted treatment assignment. While there is a large theoretical literature on optimal experimentation in MAB problems (e.g., [Gittins \(1979\)](#)), the bedrock of our analysis is the ‘probability matching’ algorithm due to [Thompson \(1933\)](#). Recently, a number of papers have shown for various settings that the Thompson algorithm is asymptotically efficient, among all possible adaptive algorithms, in terms of participant welfare ([Agrawal and Goyal, 2012](#); [Kaufmann et al., 2012](#); [Agrawal and Goyal, 2013](#); [Wager and Xu, 2021](#)). While adaptive experiments are increasingly used in medical trials ([Berry, 2006](#); [FDA et al., 2018](#)) and commonly used in online advertising ([Russo et al., 2018](#)), we contribute to a growing number of papers in economics using these methods ([Kasy and Sautmann, 2020](#); [Kasy and Teytelboym, 2020a,b](#)). There is also a recent literature within economics on targeted treatment assignment both from non-Bayesian (e.g., [Kasy \(2015\)](#); [Kitagawa and Tetenov \(2018\)](#); [Wager and Athey \(2018\)](#); [Chernozhukov et al. \(2018\)](#)) and Bayesian perspectives (e.g., [Dehejia \(2005\)](#); [Chamberlain \(2011\)](#)).

We also contribute to the literature on active labour market policies in developing

and emerging economies. Specifically, ours is the first field experiment on employment of refugees in a development context.⁵ The literature on active labour market policies has generally found that such policies have limited effectiveness (McKenzie, 2017). This includes three novel experiments among educated youth in Jordan: one involving wage subsidy vouchers (Groh et al., 2016a), one involving training in soft skills (Groh et al., 2015, 2016b), and one involving direct matching of job-seekers to firms (Groh et al., 2015). However, in other contexts, recent experiments have identified several effective policy interventions: conditional cash transfers and financial application incentives have been found to increase short-term employment or applicant quality through increasing job search (Franklin, 2018; Abebe et al., 2020; Banerjee and Sequeira, 2020; Abebe et al., 2020), skill-signalling workshops can increase wages through improved assortative matching (Alfonsi et al., 2020; Bassi and Nansamba, 2020; Abebe et al., 2020), and detailed job-search plans have increased employment through more effective job search (Abel et al., 2019). We draw on each of these three recent areas of innovation to design our three treatments. The previous literature tends to focus on young nationals with poor attachment to the labour market (see, for example, Kluge et al. (2019)). Our work is novel in taking insights from those earlier experiments to a population of refugees, for whom constraints may be quite different (Loiacono and Vargas, 2019). In this way, our paper also relates to recent attempts to generalize experimental results across different contexts (see, for example, Meager (2019)).

Roadmap The paper is organized as follows. Section 2 describes the humanitarian and the labour market context in Jordan, our sampling procedure, and the three treatments. Section 3 explains our adaptive treatment assignment algorithm and characterizes its theoretical properties. Section 4 presents our empirical findings, including qualitative evidence from focus group interviews. Section 5 discusses lessons we learnt and recommendations for future adaptive field experiments including the use of surrogate outcomes, continuous (rather than binary) outcomes, choice of sample and wave size, inference, non-stationarity, and alternative adaptive assignment algorithms. Section 6 concludes. Appendix A.1 gives the proof of the main theorem. Appendix A.2 provides details on the Markov Chain Monte Carlo algorithm for the hierarchical Bayesian model, and Appendix A.3 has some additional tables and figures. The Online Appendix contains treatment materials used in the field as well as additional tables and figures.

⁵ Battisti et al. (2019) evaluate a job-matching intervention for recently-arrived refugees in Germany.

2 Context, sampling and treatments

The world is facing the largest refugee crisis since World War II, with over 80 million individuals displaced, about 26 million of whom are refugees ([UNHCR, 2020b](#)). Amidst this crisis, the duration of displacement has increased – with refugees now displaced for 10 years on average ([Devictor and Do, 2017](#)). The unprecedented magnitude and changing nature of displacement has catalyzed a big shift in thinking about how assistance is provided for refugees and internally displaced people.

Over the past decade, the international community has moved away from a model in which refugees are housed in camps – receiving aid in perpetuity – to a model focused on identifying sustainable solutions that integrate refugees and internally-displaced people into local communities and labor markets. In many contexts, this has fueled a change from delivering basic commodities and food items to supporting individuals to gain access to employment. This change in approach is not isolated to any specific location, but is increasingly becoming the dominant model for delivering humanitarian assistance.

A crucial part of integrating displaced individuals into labor markets is providing the support necessary to generate employment opportunities at scale for communities affected by crises. However, there is a dearth of evidence on what works for these groups and in these contexts. In part, this is due to the challenging nature of experimenting in crisis-affected contexts – where security issues and the need to deliver timely services make experimentation difficult. More generally, refugees and internally-displaced people face a unique set of constraints in accessing employment opportunities. They often lack the information, language skills and social networks needed to navigate labor markets effectively. Many have lost assets and have limited savings; this can constrain individuals from accessing the type of childcare, transit or basic needs required to get a job. Trauma, uncertainty and social exclusion may also reduce refugees’ intrinsic motivation to search for an employment opportunity. These micro-level barriers may be compounded at the national level by governments who impose legal restrictions on whether or what types of jobs can be accessed.

2.1 The Syrian refugee crisis

Since 2012, the Syrian crisis has displaced more than 13.1 million people, making it the largest refugee crisis of our time ([UNHCR, 2020a](#)). Approximately seven million are displaced internally within Syria; about another six million fled to neighbouring countries. The Government of Jordan estimates that, since the beginning of the Syrian crisis, nearly

1.3 million refugees have arrived in the country; of these, about 660,000 have registered with UNHCR (UNHCR, 2020a). Eight years into the conflict, Syrian refugees in Jordan face important needs for humanitarian assistance, for basic services, and for economic stability. In 2015, it was estimated that 93% of Syrian refugees in the country lived below the US\$5 per day poverty line (Verme et al., 2015). At the same time, low-skilled Jordanians continue to suffer from pre-existing labor market challenges, including high unemployment, which leaves them also economically vulnerable (IRC, 2017; Government of Jordan, 2019; UNHCR, 2020a).

In an attempt to address some of the issues associated with the protracted displacement, the Government of Jordan and the international community met at the London Conference in 2016 and explored new ways to support countries most affected by the Syrian crisis. For Jordan, a key outcome of the event was the signing of the Jordan Compact – hailed at the time as an innovative approach for host countries and the international community to respond to protracted displacement. Under the Compact, European and international donors pledged a total of US\$2.1 billion in direct grants and US\$1.9 billion in concessional loans to the Government of Jordan (Barbelet et al., 2018). The Compact also granted Jordan trade concessions that relaxed ‘rules of origin’ criteria and opened export markets in Europe. In exchange, the Government of Jordan committed to important policy changes aimed at improving access of Syrian refugees to the labor market. In particular, the government agreed to ease administrative procedures to allow Syrian refugees to apply for work permits in manufacturing, agriculture, and construction – with a goal of providing work permits for up to 200,000 Syrian refugees (IRC, 2017).⁶

2.2 The Jordanian labor market

The labour market in Jordan is characterised by very low employment rates, by international standards. For example, the Employment and Unemployment Survey (EUS) reports, for the last quarter of 2016, an employment rate of 30 percent and overall labor force participation rate of 36 percent.⁷ This very low average masks significant heterogeneity by gender. Among males, labor force participation is close to 59 percent, while among females it only equals 13.5 percent. Fallah et al. (2019) compile EUS figures for a longer period of time, showing that some of these are persistent features of the

⁶ In addition, the Government of Jordan also agreed to: (i) designate and develop five industrial zones, later called the Special Economic Zones (SEZs); (ii) allow Syrian refugees to formalize existing businesses and to set up new businesses; (iii) provide a small number of employment opportunities for Syrians in municipal works.

⁷ The labor force participation rates gives the ratio of economically active individuals (employed or looking for work) over total working-age individuals in the country.

Jordanian labor market.

Employment rates among refugees are much lower than among Jordanians. In early 2017, the Jordan Labor Market Panel Survey (JLMPS) was adapted to include an almost-representative sample of Syrian refugees in Jordan.⁸ According to the JLMPS figures, the employment rate among Syrian refugees stood at 14 percent. Among women refugees, the employment rate was only 2 percent. This employment was often informal and median monthly salaries were below the national minimum wage.⁹

Employment among Syrian refugees is likely to be constrained by both demand and supply side factors. On the labour demand side, firms often report difficulties in processing work permits for Syrians but also fear the consequences of sanctions applied to informal work. Further, refugees face strong competition from both Jordanian nationals and other migrants. This is partly because firms are required to meet a quota of employing at least 15% Jordanians. Moreover, migrant workers (mostly from South Asia) were already employed in large numbers in many of the low-paying jobs that were opened to Syrians as part of the Jordan Compact (Amjad et al., 2017).

On the labor supply side, several search frictions are likely to be present. First, refugees are often credit-constrained due to lost assets, networks, and sources of income (Government of Jordan, 2019). Second, they have little experience in and information on the formal labor market in the host economy, which could drive decisions to work informally or not work at all. Third, they may experience self-control problems when it comes to searching for work, possibly resulting from the traumas of displacement and/or a number of restrictive labor market policies (Shami, 2019). Lastly, job quality in the formal sector is often a barrier to labour supply. Recent evidence shows that both Syrians and Jordanians perceive that formal work, particularly in the manufacturing sector, is often exhausting, exploitative, and potentially risky (Amjad et al., 2017; Razzaz, 2017). Importantly, these barriers are by no means unique to refugees in Jordan, as is consistently highlighted by the non-experimental literature on displacement (Loiacono and Vargas, 2019; Schuettler and Caron, 2020).

⁸ OAMDI, 2018. Labor Market Panel Surveys (LMPS), <http://erf.org.eg/data-portal/>. Version 1.1 of Licensed Data Files; JLMPS 2016. Egypt: Economic Research Forum (ERF).

⁹ 75 percent of employed refugees reported that they did not have a formal work contract. This is most likely an underestimate of the rate of informality, as many refugees may be reluctant to report informal work. In the same questionnaire, 99 percent of refugees reported that their employer was not making social security contributions – a key indicator of formality. In terms of salaries, the median monthly salary was 187 Jordanian Dinars, while the formal minimum wage was 200 Jordanian Dinars.

2.3 Sampling Syrian and Jordanian job-seekers

Our partner IRC enrolled individuals for a program called ‘Project Match’ on a rolling basis over a six-month period between February 10, 2019 and November 30, 2019. The program was active in three cities: the capital Amman, and the northern cities of Irbid, and Mafraq. To be eligible for this study, participants had to be: (i) Syrian refugees or Jordanian nationals with valid government identification, (ii) between 18 and 45 years old (inclusive), and (iii) willing to take up low-skilled formal wage work that pays approximately minimum wage in the immediate future. We verified that the participants met these requirements and further collected information for the research during the intake registration interviews. At the end of the interview, participants were randomized into a treatment group based on the algorithm described in section 3.

Participants were selected using a variety of passive and active recruitment methods. The passive methods entailed IRC employment service officers (ESOs) contacting potential program participants. We refer to this as ‘passive’ selection as it was initiated by the ESO and not by the program participant. In the majority of cases, employment officers learned about potential program participants from referrals given by community leaders, other programs or partner organizations, and other study participants. Additionally, the ESOs conducted door-to-door home visits to neighborhoods that were known to host a high number of refugees. These neighborhoods were identified using UNHCR maps and the experience of ESOs hired to work with Project Match. Further, individuals who had not been contacted by an ESO were also eligible to apply for the program. We refer to this as ‘active’ selection as it was initiated by the program participant. Individuals could enrol by visiting specific community-based organizations (CBOs), visiting the IRC offices, responding to ads posted on social media, or by attending an information session on Project Match at the UNHCR offices.

There were no major differences in the ways that Syrians and Jordanians were sampled. For both Syrians and Jordanians, the largest share of enrolments came from referrals, a passive sampling method. The second largest source of participants for both nationalities was enrolment by the job-seeker at a CBO (an active sampling method). Slightly more Syrians than Jordanians were sampled through home visits conducted by the ESOs. However, overall, low-skilled and more economically vulnerable Jordanians often resided in areas similar to those of refugees and also engaged actively with CBOs to access various forms of welfare. We summarise the frequency of these different sampling methods by nationality in Table A.2 in the Online Appendix. The proportion of participants enrolled through passive versus active methods changed over time, but not dramatically. In particular, in the months of May to July, 2019, more participants

Table 1: **Descriptive statistics**

Sample	All	Syrian	Jordanian
	(1)	(2)	(3)
Female	0.60	0.60	0.60
Age	28.82	29.66	28.15
Household head	0.27	0.38	0.19
Household size	4.88	4.98	4.80
Education (years)	10.24	7.71	12.24
Spent at least 5 years in Jordan	-	0.95	-
Wage employed	0.02	0.02	0.02
Work experience (years)	4.48	4.99	4.10
Monthly HH Expenditure (JOD)	395.83	358.05	425.65
Searched for work	0.46	0.38	0.52
Applications (no.)	1.91	1.41	2.31
Hours search	3.14	2.60	3.58
Money spent (if searching)	8.03	6.90	8.69
Sample size	3770	1663	2107

Note: This table reports descriptive statistics for the Syrian and Jordanian samples at baseline. ‘Monthly HH Expenditure’ is total household expenditure in the last 30 days. This variable is obtained by summing reported expenditure in 9 different categories (including food, housing, utilities, water, transport, health, education, communication, and other). Missing values on specific expenditure items are replaced with the sample average. ‘Searched for work’ is a dummy for whether the person has done any job search in the last 30 days. ‘Applications’ is the number of job applications completed in the last 30 days. ‘Hours search’ is the number of hours of job search in the last seven days. ‘Applications’ and ‘Hours search’ take a value of zero if the respondent is not searching for work. ‘Money spent’ is the amount of money spent on job search in the last seven days, if the respondent is searching for work. ‘Applications’, ‘Hours search’ and ‘Money spent’ are windsorised at the 99th percentile of the nationality-specific distribution.

enrolled in Project Match through active methods. In subsequent months, this was largely reversed. We illustrate these patterns in Figure A.4 of the Online Appendix.

2.4 Key features of the sample

In total, we sampled 1,663 Syrians and 2,107 Jordanians. We report a number of descriptive statistics in Table 1. On several dimensions, the Syrian and Jordanian samples have similar characteristics. For both nationalities, 60 percent of the sample is composed by women, average age is about 29 years, and the average household is composed of about

5 individuals. Also, 2 percent of individuals of both nationalities are in wage employment and the average person has 5 years of work experience. Syrians however tend to be less educated on average (7 years vs 12 years). Our sample, while not intended to be representative as such, is nonetheless broadly similar to the near-representative JLMPS data in terms of respondents' age and education.

Many individuals in our sample live below the poverty line. Average household expenditure among Syrians is about 360 Jordanian Dinars (JOD) per month. This is approximately 72 JOD per person per month, or 2.4 JOD per day, which is well below the national poverty line of 3.5 JOD.¹⁰ Daily expenditure among Jordanians is about 3 JOD – 25 percent higher, but still below the poverty line.

About half of the sample (40 percent of refugees and 50 percent of Jordanians) are actively looking for work at the time of the baseline interview. On average, refugees have placed about 1.4 job applications in the previous month and have spent 3 hours looking for work in the previous seven days. Among those refugees that were actively searching for work, total job search expenditure in the last seven days amounted to 6.9 JOD. Among Jordanians, average expenditure was 8.7 JOD.

We divide this sample in sixteen strata based on four dummy variables: (i) nationality (a dummy for whether the respondent is Jordanian, defined as having a Jordanian national ID); (ii) gender (a dummy for identifying as female), (iii) education (a dummy for having completed high school or more), and (iv) work experience (a dummy for having experience in wage employment). These strata form the basis of our targeting strategy, discussed in the next section. In Figure A.5 of the Online Appendix, we show the distribution of observations across strata. While for most cells we have good sample sizes, we only have a small proportion of people, especially Syrians, who have education beyond high school.

Note that we will *not* report statistics regarding the balance of covariates across treatments. The reason, as discussed further below, is that our treatment assignment algorithm targets assignment based on these covariates. We therefore do not expect that covariates are balanced across treatment arms. The estimates discussed in Section 4 below correct for imbalance across strata by reweighting.

¹⁰ This is a lower bound to daily per-capita expenditure, as in the calculation we assume that all members of the household are adult.

2.5 Treatments

On the basis of these key features, and working closely with local experts at the IRC in Amman, we designed three separate job search interventions.¹¹ Each intervention was designed to represent a distinct form of job search assistance, each having support in the recent empirical literature.¹² These interventions will be denoted by $D \in \{0, 1, 2, 3\}$ where 0 refers to respondents assigned to the control group; the three search interventions respectively provide cash, information, and psychological support. All interventions were delivered at the end of the intake interview or in the following seven days. In addition to these treatments, all respondents received 4 JOD (about US\$5.60 USD at the time of the intervention), and an informational flyer with a very brief discussion of job interviews.

Control group. The control group received the 4 JOD and the basic informational flyer that were offered to everyone upon registration with Project Match.

Treatment 1: A labeled cash transfer. The cash support is a labeled cash transfer (LCT) of a value of 65 JOD (about US\$92 at the time of the intervention). This transfer was intended to support the recipient to pay for the cost of job search – including transport, grooming, time costs and, for at least some study participants, childcare. It was designed based on evidence that small transfers cause large responses in job-search intensity (Herkenhoff et al., 2016; Franklin, 2018; Abebe et al., 2020). As shown in Table 1, the transfer enabled refugees to cover the average cost of job search for a period of about 10 weeks. The transfer was ‘labeled’ in that, at the time of distribution, study participants were offered recommendations on how they should use this cash, i.e., to help with the job search in the above-mentioned ways); however, respondents were also informed that they were free to spend the cash as they thought it was most appropriate (Benhassine et al., 2015). Upon delivery of the intervention, participants received an empty ATM card, which was charged (within an average of seven working days) with a one-time cash payment of 65 JOD. Upon charging of the ATM card, recipients received an SMS notification. They also received an ATM guide pamphlet with a direct hotline number for reporting issues with cash withdrawal from ATMs. To put this intervention in context, it is important to note that this transfer is substantially smaller than the

¹¹ We prototyped and modified the interventions with about 130 respondents before commencing the randomized field experiment.

¹² Some additional respondents were also assigned to one of two separate ‘direct placement’ arms. These arms are not cross-randomized with the treatments that are analyzed in this study and are the focus of a separate paper.

grants studied in the unconditional cash transfer literature. For example, [Haushofer and Shapiro \(2016\)](#) evaluate the impact of a transfer worth about two years of average consumption. In contrast, our transfer amounts to less than one month of average personal refugee expenditure.

Treatment 2: Information. The second intervention provided informational support. Prior evidence suggested that both Syrian refugees and low-skilled Jordanians had little understanding of either the interview process or the legal obligations owed by employers to their workers ([Gordon, 2017](#)). For example, a common myth among Syrian refugees in Jordan is that, by working in a formal job and holding a work permit, the Syrian would lose her or his UNHCR financial assistance package.¹³ Specifically, respondents in this treatment received in-depth information on (i) how to prepare for and interview for a formal job in Jordan (following, in particular, the recent results from [Abebe et al. \(2020\)](#)), and (ii) the legal rights of employees in formal jobs. Information was delivered through face-to-face interaction with a trained Project Match employment service officer, two videos describing the formal jobs and associated labor laws from the eyes of a job-seeker, and two take-home paper tools. The paper tools were designed for low-literacy participants and include cartoons for easy comprehension (see Online Appendix Figures [A.1](#) and [A.2](#)). One of the tools was designed as an interactive myth-busting activity whereby participants are exposed to common myths about formal jobs and worker rights, and then upon scratching the surface of the box below the myth, can see the reality.

Treatment 3: Nudge. The third intervention is psychological support; we refer to this as the ‘nudge’ intervention. We provide a packaged intervention composed of (i) a four-week job-search planning calendar as in [Abel et al. \(2019\)](#) (see Online Appendix Figure [A.3](#)), (ii) an instructional video on how to use the calendar to plan for job search, (iii) a face-to-face demonstration delivered by the ESOs, and finally (iv) reminder SMSs. The instructional video begins with a statement on the potential impacts of job-search planning: ‘Did you know that job-search planning can increase chances of finding work by up to 25%?’ Additionally, if the participants send a text message to IRC with their job-search goals, they receive two reminder SMS, at the beginning of the week and at the end of the week. Through the calendar and the SMSs, participants track the number of jobs they intend to apply for and hours of job search they intend to spend, and can then check whether they met their goals for the week. This goal-setting and goal-reviewing exercise

¹³ The legal reality is that UNHCR financial assistance is not linked to having a work permit; instead, it depends upon a thorough financial needs assessment.

is designed to address self-control problems, which have been highlighted as a key constraint in recent research on job search (DellaVigna and Paserman, 2005; Caliendo et al., 2015; Abel et al., 2019).

2.6 Follow-up surveys and attrition

We measure the impacts of these interventions through three follow-up surveys, all administered by phone. First, we complete a very short follow-up survey six weeks after baseline. This survey is focused exclusively on measuring whether the respondent is currently in wage employment. The purpose of this survey was to provide the key outcome variable to implement the adaptive treatment assignment that we describe in the following section. For logistical reasons, this survey was conducted by a single enumerator, who had not previously interacted with respondents. We refer to this as the ‘rapid follow-up survey’.

We then complete in-depth phone surveys two and four months after the baseline interview. (We had initially planned a six-month follow-up survey, but this was cancelled due to the Covid-19 pandemic and the strict lockdown measures imposed in Jordan.) We use these surveys to document the impacts of the program on a battery of outcomes specified in our pre-analysis plan. These interviews are carried out by the team of ESOs, with each respondent contacted by the same person who had interviewed the respondent at baseline and had enrolled them in the program. We refer to these two surveys as the ‘ESO follow-up surveys’.

Our attrition is low throughout. In the rapid follow-up survey, attrition was 1.2 percent. In the ESO follow-up survey, we suffered attrition of approximately 2% at the two-month mark and 4.5% at the four-month mark (Online Appendix Table A.3). For Syrians, attrition in the nudge intervention is 1 percentage point above control at the two-month follow-up and 3 percentage points higher at the four-month point (Online Appendix Table A.4). For Jordanians, attrition in the information intervention is 1 percentage point above control at the two-month follow-up and 1.5 percentage points higher at the four-month point (Online Appendix Table A.5). Attrition for the cash intervention is very close to that of the control group (and not significantly different), in both follow-up surveys and both for Syrians and for Jordanians.¹⁴

¹⁴ We report p -values in Online Appendix Tables A.3, A.4 and A.5 by using randomisation inference, as described in Section 3.5. Some of the differences in attrition rates are marginally significant. Given the small rate of attrition overall, however, we consider it unlikely that our effect estimates discussed in Section 4 below are affected by differential attrition.

3 Treatment assignment and inference

In this section we describe our treatment assignment algorithm. Our algorithm is a modification of Thompson sampling (Thompson, 1933; Russo et al., 2018). This modification is motivated by the fact that our experiment has two objectives. Our primary objective is to get as many experimental participants into formal employment as possible. Our secondary objective is to test the effectiveness of alternative interventions.

We implement our algorithm using a hierarchical Bayesian model; cf. Gelman et al. (2014). The data-generating process for the binary potential outcomes corresponding to each treatment d and stratum x is governed by a parameter θ^{dx} , which determines the success rate for this treatment and stratum. For a given treatment d , the parameters θ^{dx} are drawn from a prior distribution $Beta(\alpha^d, \beta^d)$. The hyper-parameter (α^d, β^d) determine the average success rate and dispersion of treatment effects across strata, for this treatment d . These hyper-parameters are assumed to in turn be drawn from a diffuse hyper-prior distribution π . The posterior distribution can be interpreted as follows. In every period, the experimenter observes treatment success rates for existing experimental participants for all strata and treatments. This allows her to learn the mean and dispersion of treatment effects across strata. She can then combine the estimate of these hyper-parameters with the observed success rate in a given stratum in order to calculate the posterior distribution of the success parameter θ^{dx} in that stratum. Finally, these posterior distributions can be used to calculate the probabilities \hat{p}_t^{dx} that a given treatment is optimal for a given stratum. These probabilities are then used in the Tempered Thompson Algorithm.

After describing this Bayesian setup, we review Thompson sampling. Thompson sampling is based on the posterior probability that each of the treatments is optimal, conditional on observed covariates. We then introduce our modification, the Tempered Thompson Algorithm, which provides a compromise between Thompson sampling and full (balanced) randomization. In Theorem 1 we characterize how the Tempered Thompson Algorithm trades off our two objectives, helping participants and obtaining precise estimates. The source code for our assignment algorithm is available in a public repository.¹⁵

This section concludes with a discussion of inference. Our first method of inference is Bayesian. Our second method of inference uses p-values based on randomization inference. Randomization inference needs to take into account the adaptive and targeted form of treatment assignment in order to be valid.

¹⁵ At <https://github.com/maxkasy/ThompsonHierarchicalApp>. A corresponding interactive app is available at <https://maxkasy.github.io/home/hierarchicalthompson/>.

We use the following notation. Let t denote the day the participant enrolled in the program and was offered the intervention and let i index individuals within days. Note that we have repeated cross-sections, not a panel, so that individual i on day t is different from individual i on day t' when $t \neq t'$. Let x index strata and d index treatments. Finally, m_t^{dx} denotes the total number of times that treatment d was assigned to individuals in stratum x up to time t , and r_t^{dx} denotes the corresponding total number of successes, that is, individuals for whom $Y_{it} = 1$.

3.1 Hierarchical Bayesian model

We consider a hierarchical Bayesian model with a data generating process, described by Eq. (1), and a prior, described by Eqs. (2) and (3) below. Let θ^{dx} be the average potential outcome for treatment d in stratum x . We assume that

$$Y_{it}^d | (X_{it} = x, \theta^{dx}, \alpha^d, \beta^d) \sim \text{Ber}(\theta^{dx}), \quad (1)$$

$$\theta^{dx} | (\alpha^d, \beta^d) \sim \text{Beta}(\alpha^d, \beta^d), \quad (2)$$

$$(\alpha^d, \beta^d) \sim \pi, \quad (3)$$

where (α^d, β^d) are the hyper-parameters and π is the hyper-prior (cf. [Gelman et al. \(2014, chapter 5\)](#)). Eq. (2) says that for a given treatment d , average potential outcomes θ^{dx} for all strata come from a Beta distribution governed by the hyper-parameters (α^d, β^d) . Eq. (3) states that the hyper-parameters governing the distribution of average potential outcomes of each treatment across strata come from a common hyper-prior distribution π .

We assume that the parameters $(\alpha^d, \beta^d, \theta^{dx})$ are independent across the treatment arms d . We choose a hyper-prior for the hyper-parameters (α^d, β^d) with a common density equal to $(\alpha + \beta)^{-2.5}$, up to a multiplicative constant. In doing so, we follow the recommendation of [Gelman et al. \(2014, p.110\)](#) for picking a “non-informative” hyper-prior.

Intuitively, updating based on this prior works as follows. For each treatment d , we consider the success rates $q_t^{dx} = r_t^{dx} / m_t^{dx}$ across the different strata x . Based on these success rates, we learn the mean and dispersion of θ^{dx} across strata, as reflected in the hyper-parameters (α^d, β^d) . Then we use these as a prior, which together with the cumulative successes r_t^{dx} observed for a given stratum x allows us to form an updated belief about θ^{dx} for that stratum.

Denote by θ, m_t, r_t the vectors of parameters, cumulative trials, and cumulative successes, where each of these is indexed by both d and x , and denote by α, β the

vectors of hyper-parameters indexed by d . We sample from the posterior distribution of (θ, α, β) given $\mathbf{m}_{t-1}, \mathbf{r}_{t-1}$ using the Markov Chain Monte Carlo algorithm described in Algorithm 1 in Appendix A.2.

3.2 Treatment assignment algorithm

Let p_t^{dx} denote the posterior probability that a treatment d is optimal in stratum x , in the sense that it maximizes the probability of employment. That is, define

$$p_t^{dx} = P \left(d = \arg \max_{d'} \theta^{d'x} | \mathbf{m}_t, \mathbf{r}_t \right). \quad (4)$$

Equation (A.1) in the appendix shows how to estimate this probability by an average across Markov Chain Monte Carlo draws, which we denote \hat{p}^{dx} .

Two popular algorithms for assigning treatments in experiments are (i) fully random assignment, with equal probabilities across arms, and (ii) Thompson sampling. Our experiment is based on a combination of these two algorithms.

Fully randomized sampling assigns treatment d with probability $1/k$, where $k = 4$ is the number of different treatments, to units in every stratum. These assignment probabilities maximize power for tests of non-zero treatment effects. Thompson sampling, by contrast, assigns treatment d with probability \hat{p}_t^{dx} to units in stratum x in time period t . Thompson sampling minimizes expected regret (cf. Agrawal and Goyal 2012; Bubeck and Cesa-Bianchi 2012), or equivalently maximizes average outcomes, in the large sample limit. As shown in these papers, it is in particular the case that expected regret only grows at a logarithmic rate with the number of experimental units. Russo and Van Roy (2016) prove worst-case bounds on the performance of Thompson sampling, using information-theoretic arguments.

Our primary goal is to maximize the labor market outcomes of experimental participants, but we also consider the precision of treatment effect estimates to be a secondary objective. Motivated by this combination of objectives, we assign treatment d to units in stratum x with probability

$$(1 - \gamma) \cdot \hat{p}^{dx} + \gamma/k, \quad (5)$$

where γ is the share of observations that are randomized between treatment arms with equal probability. We refer to this procedure as the Tempered Thompson Algorithm.

In our experiment, we measure employment outcomes Y_{it} only with a delay, six weeks after the intervention took place for each participant. As a consequence, treatment assignment is conditioned only on the outcomes of participants from six weeks before,

or earlier. We assign participants in the first six weeks randomly to each treatment arm with probability 0.25.

3.3 Large sample properties

We now turn to a formal characterization of the large sample properties of our treatment assignment algorithm. We recapitulate and summarize our assumptions for this characterization in Assumption 1. In the following, we use θ_0 to denote the fixed, true vector of average potential outcomes from which the data are generated. By contrast, we use θ to denote the corresponding random vector which is drawn from the posterior distribution (belief) of the experimenter. The first step in Theorem 1 below, then, is based on the result that the posterior converges to the truth, that is, the distribution of θ concentrates around θ_0 .

Assumption 1 (Setup) Consider a fixed (non-random) $\theta_0 = (\theta_0^{dx})$. Suppose that $d^{*x} = \arg \max_d \theta_0^{dx}$ is unique for all $x \in \{1, \dots, n_x\}$, and denote $\Delta^{dx} = \theta_0^{d^{*x}x} - \theta_0^{dx}$. Assume that $(Y_{it}^1, \dots, Y_{it}^k, X_{it})$ is i.i.d. across both i and t , and that

$$Y_{it}^d | (X_{it} = x, \theta_0) \sim \text{Ber}(\theta_0^{dx}).$$

Assume that $N_t \geq \underline{N}$ for all t and some constant \underline{N} , and that the prior distribution for θ has full support.

Assume that treatment d is assigned to units in stratum x in period t with probability

$$(1 - \gamma) \cdot p_t^{dx} + \gamma/k,$$

where p_t^{dx} equals the posterior probability that treatment d is optimal in stratum x , and $0 < \gamma \leq 1$. Denote q_t^{dx} the cumulative share of observations assigned to treatment d in stratum x across the time periods $1, \dots, t$, and p^x the probability that $X_{it} = x$.

The assumption that d^{*x} is unique holds generically.¹⁶ We are now ready to state the main theorem.

Theorem 1 (Large sample properties of Tempered Thompson Algorithm) Under Assumption 1, the following holds true as t (and thus $M_t = \sum_{t' \leq t} N_{t'}$) goes to ∞ :

1. Consistency: The posterior probability p_t^{dx} that treatment d is optimal in stratum x converges to 1 in probability (conditional on θ_0) for $d = d^{*x}$, and to 0 for all other d .¹⁷

¹⁶ The case where the uniqueness assumption is close to being violated is discussed in Section 3.4 below.

¹⁷ Note that this statement refers to frequentist consistency (given θ_0) of a Bayesian posterior probability (which averages over θ).

2. Converging shares: The cumulative share q_t^{dx} allocated to treatment d in stratum x converges in probability to $\bar{q}^{dx} = (1 - \gamma) + \gamma/k$ for $d = d^{*x}$, and to $\bar{q}^{dx} = \gamma/k$ for all other d .

3. Converging regret: Average in-sample regret,

$$\text{Regret}_t = \frac{1}{M_t} \sum_{i,t} \Delta^{D_{it} X_{it}}$$

converges in probability to

$$\gamma \cdot \frac{1}{k} \sum_{x,d} \Delta^{dx} \cdot p^x.$$

4. Converging estimator: The normalized average outcome for treatment d in stratum x ,

$$\sqrt{M_t} \left(\bar{Y}_t^{dx} - \theta_0^{dx} \right),$$

converges in distribution to

$$N \left(0, \frac{\theta_0^{dx} (1 - \theta_0^{dx})}{\bar{q}^{dx} \cdot p^x} \right).$$

Furthermore, the normalized average outcomes for different treatments and strata converge jointly to a multivariate normal with covariances equal to 0.

The large sample result of Theorem 1 characterizes the trade-offs in choosing γ . The parameter γ allows us to interpolate between non-adaptive, conventional randomization ($\gamma = 1$) and Thompson sampling ($\gamma = 0$). The former is optimal for minimizing the expected variance of all treatment effect estimators. The latter is optimal for minimizing the expected regret (maximizing expected welfare) for the participants in the experiment.

As we increase γ , starting from a value of 0, the expected in-sample regret increases linearly in proportion to γ . On the other hand, the asymptotic variance of conditional average treatment effect estimators, comparing the conditionally optimal treatment to its alternatives, is given by one over the total sample size, times

$$\frac{\theta_0^{d^{*x}x} (1 - \theta_0^{d^{*x}x})}{((1 - \gamma) + \gamma/k) \cdot p^x} + \frac{\theta_0^{dx} (1 - \theta_0^{dx})}{(\gamma/k) \cdot p^x}.$$

This number is decreasing in γ , since higher γ means a more balanced distribution of observations across treatment arms. In our application, we trade off these conflicting objectives by setting the share of observations for which treatment is fully randomized

to $\gamma = 0.2$, which implies that the probability of being assigned to each treatment is bounded below by 0.05. Given that the baseline employment rate is 5% (see Table 2 below), our choice of γ then means that in the large sample limit (i.e., when only 5% of the sample is assigned to some treatment) the standard error of a treatment arm estimate would be bounded by

$$\sqrt{\frac{E[Y|D = d]}{N \cdot (\gamma/k)}} \approx \sqrt{\frac{0.05 \times (1 - 0.05)}{3668 \cdot (0.2/4)}} = 0.016,$$

which is about twice the standard error we would get under a fully randomized design. Given that in practice we do not see a full convergence toward this large sample limit, our standard errors are actually smaller.

More generally, the relative weight that researchers should assign to participant welfare versus estimator precision might depend on a number of contextual factors. One factor is the size of the experimental population relative to the population that will be ultimately reached by interventions informed by the trial. The larger this size, the greater the weight one might wish to put on estimator precision, and the larger the optimal γ . Another factor is whether the treated population is particularly vulnerable and hence requires special protection. If that is the case, ethical considerations suggest a greater weight for participant welfare, and thus a smaller optimal γ .

3.4 Discussion of Theorem 1 and the Tempered Thompson Algorithm

Several observations are worth making about the properties of the Tempered Thompson Algorithm and Theorem 1. First, the theorem implies that the large sample properties of the Tempered Thompson Algorithm do not depend on the prior (as long as the latter has full support): In large samples the data dominate the prior, the posterior is consistent, and thus assignment shares become independent of the prior. Relative to pure Thompson sampling, this happens even faster for the Tempered Thompson Algorithm with $\gamma > 0$.

The flip side of this large-sample robustness to the prior is robustness to the data in the initial periods, for three distinct reasons. First, Bayesian inference optimally combines data and prior, and therefore down-weights outliers among the initial observations. This stabilizes assignment shares in initial periods, and makes them closer to an equal division among treatment arms. Only when evidence has accumulated that some treatment arms are better than others do assignment shares become unequal. Second, relative to Thompson sampling the Tempered Thompson Algorithm additionally shrinks assignment shares toward the balanced assignment. And third, the outcomes in

our setting are bounded, and therefore the influence of any single observation on the posterior is necessarily bounded, as well.

The properties of the Tempered Thompson Algorithm listed in Theorem 1 rely on having sufficient power (enough observations) to be able to distinguish the best treatment with high probability. It therefore presupposes that learning is sufficiently fast. An alternative characterization might consider the worst-case behavior of the Tempered Thompson Algorithm across possible values for the parameter vector θ_0 , for a given T . As it turns out, the worst-case behavior in terms of in-sample regret is driven by intermediate parameter values which are such that the treatment effects Δ^{dx} (relative to the optimal treatment) are of the same order of magnitude as the standard errors for estimates of these treatment effects, that is, of order $1/\sqrt{T}$ (Bubeck and Cesa-Bianchi, 2012; Russo and Van Roy, 2016). If treatment effects are larger, the best treatment is discovered quickly and in-sample regret is low. If treatment effects are smaller, it doesn't matter as much which treatment participants are assigned. The behavior of algorithms such as the Tempered Thompson Algorithm can also be characterized using local-to-zero asymptotics, as in the recent pioneering work by Wager and Xu (2021). A formal characterization of the worst-case and local-to-zero behavior of the Tempered Thompson Algorithm is left for future work.

3.5 Inference

One concern about adaptive experimental designs is that they lead to biased inference (see, e.g., Hadad et al. 2019). Part 4 of Theorem 1 implies, however, that this is not the case for the Tempered Thompson Algorithm in large samples. Sample averages in each treatment arm are consistent, asymptotically unbiased, and normally distributed, so that inference can proceed as if treatment assignment were not adaptive. This is true because assignment probabilities for each arm in each stratum are bounded away from 0 when $\gamma > 0$. In our empirical analysis, we nevertheless consider two methods for inference that do not rely on such asymptotics, but instead are exactly valid in finite samples despite adaptive assignment, as detailed next.

Our first form of inference is Bayesian, based on the hierarchical default prior described in Section 3.1 above. To construct credible sets (i.e., sets that have a given posterior probability of containing the true parameters), we report 0.025 and 0.975 quantiles, based on Markov Chain Monte Carlo draws. We do so for all our estimates listed in the previous section. This yields sets that have a posterior probability of 95% to contain the true parameters, conditional on the data of the experiment.

We would like to emphasize that standard Bayesian inference remains valid in

finite samples for adaptive designs such as ours, since the likelihood function is not affected by adaptivity. In large samples, as long as $\gamma > 0$, and as long as the optimal treatment is unique in each arm, we conjecture that our credible sets also have 95% frequentist coverage probability, i.e., they are confidence sets in the usual sense; cf. [van der Vaart \(2000\)](#), chapter 10. This relies on the fact that the share of observations assigned to each treatment in each stratum is bounded below, asymptotically.¹⁸

Additionally, we provide randomization-based p-values that are valid under the sharp null hypothesis that there are no treatment effects, i.e., under the null that $Y_{it}^d = Y_{it}^{d'}$ for all d, d', i, t . Under this null, we can generate counterfactual data by re-running our assignment algorithm repeatedly, leaving outcomes as they are in our data, but generating new treatment assignments. The distribution of test-statistics over this re-randomization distribution can be used to construct critical values and p-values that are exact in finite samples, under the sharp null.

4 Results

In this section, we discuss the impact of the interventions and the performance of the Tempered Thompson Algorithm. We first present a set of results on wage employment based on the rapid follow-up survey carried out six weeks after baseline. We do this in two different ways. First, we present Bayesian posteriors and credible sets. Second, we report the difference between weighted average employment in each treatment group and in the control group.¹⁹ Here, we use randomization inference to construct a p-value of the sharp null of no treatment effect.²⁰

We then present a broader set of results from the longer surveys we carried out two months and four months after baseline. These surveys capture wage employment,

¹⁸ A formal proof is left for future work, but should follow from item 4 of Theorem 1.

¹⁹ Weighting is necessary as the samples in each experimental group are mechanically unbalanced due to our adaptive randomization procedure. We report *weighted averages* of the form:

$$\beta_j^d = \frac{1}{N} \sum_{it} \frac{\mathbf{1}(D_{it} = d)}{a^{dx}} \cdot W_{it}^j$$

where

$$a^{dx} = \frac{\sum_{it} \mathbf{1}(D_{it} = d, X_{it} = x)}{\sum_{it} \mathbf{1}(X_{it} = x)}.$$

W_{it} is some outcome of interest of individual i sampled on day t , D_{it} is the treatment status of this individual, X_{it} is the stratum, and N is the total number of experimental participants. So a^{dx} is the overall proportion of individuals in stratum x assigned to treatment d by the end of the experiment.

²⁰ We only compute randomization inference p-values for individuals that were contacted during the rapid follow-up survey. For consistency, we thus drop observations that have attrited in the rapid follow-up survey, but not in the two and four month follow-up surveys (39 and 24 observations, respectively).

but also measure job-search, earnings, well-being, social integration and migration intentions. For each outcome, we report weighted averages and randomization inference p-values as explained above.

We present an evaluation of the performance of the Tempered Thompson Algorithm in our setting, including three ‘welfare contrasts’ that quantify the overall impact of our interventions. We also simulate the counterfactual performance of the Tempered Thompson Algorithm with two-month employment as its objective.

Finally, we summarize the quantitative and qualitative evidence for the reasons for ineffective job search among refugees.

4.1 Employment in the rapid follow-up survey

Job-finding rates in the control group are consistently low, especially for Syrians. Six weeks after joining the program, the average control wage-employment rate is 4.9 percent (Table 2). Further, individuals sampled at different points in time tend to have similar six-week employment rates, except for somewhat higher rates for those sampled in the first month of the experiment (Figure A.7 in the Appendix). These averages, however, mask substantial heterogeneity (Table A.9 in the Online Appendix). Employment rates among Jordanians (6.8 percent) are more than twice as large as employment rates among Syrians (2.7 percent). Similarly, the male employment rate (7.7 percent) is more than twice as large as the female employment rate (3.1 percent). Overall, most subgroups have employment rates below 10 percent.²¹ Given that job search at baseline was substantial, this highlights the difficulty of finding work in this labour market.

Our main finding is that, six weeks after the start of the program, none of the interventions increase employment for the average program participant. We report Bayesian posteriors on the impacts of the different treatments and the respective credible sets in Figure A.8 in the Appendix. These posteriors indicate that the impact on employment is always smaller than 1 percentage point. We confirm this result by reporting differences in weighted employment rates in Table 2.

We are unable to find evidence of treatment impacts for specific, pre-specified groups of individuals. In Figure A.8, for example, we show treatment effects after splitting the sample by nationality and do not find evidence of impacts on employment on either Syrians or Jordanians. Posteriors are somewhat larger for Syrians than for Jordanians, but the credible sets always overlap. We report credible sets for all sixteen strata in Table A.1 in the Online Appendix. Further, Table 2 and A.9 report differences

²¹ In Table A.8 in the Online Appendix, we look at the full break-down in sixteen strata, we find that three strata have employment rates above 10 percent. However, in two of these case, the strata have very few observations and so our measure of employment rate is likely to be noisy.

Table 2: Impacts on employment in the rapid follow-up survey

	<i>All</i>	<i>Syrians</i>	<i>Jordanians</i>
Cash	0.006 (0.296)	0.013 (0.123)	-0.001 (0.531)
Information	-0.005 (0.690)	-0.004 (0.626)	-0.006 (0.648)
Nudge	0.003 (0.388)	0.005 (0.348)	0.002 (0.463)
Control mean	0.049	0.027	0.068
Observations	3668	1633	2035

Note: The table reports impacts on wage employment at the time of the rapid follow-up interview (6 weeks after treatment). Next to each treatment effect estimate, we report a randomization inference p-value, obtained using the procedure discussed in Section 3.5. The first panel reports results for the whole sample. The second panel reports results for Syrians. The third panel reports results for Jordanians.

in weighted employment by group which confirm these findings. Employment effects are somewhat larger for Syrians (e.g. employment rates in the cash group are a 1.3 percentage point higher than in the control group), but these effects are not significantly different from zero.

4.2 Estimated impacts in the ESO follow-up surveys

We now turn to explore effects at the two-month and four-month follow-up points, using detailed data from the follow-up surveys conducted by the Employment Service Officers (ESOs).

4.2.1 Impacts on job search

Despite the null impacts on employment after six weeks, we document that all interventions generate marked increases in job search among Syrians.²² As shown in Table 3, the cash transfer raises the proportion of Syrians who look for work two months after baseline by 5.6 percentage points (a 13 percent increase over a control job-search rate of 44 percent) and leads Syrians to place 0.5 more job applications (a 40 percent increase over a control mean of 1.2 applications). Similarly, the information intervention and the nudge intervention raise job search rates by 4.7 percentage points and 3.7 percentage points respectively. Both of these interventions also have significant impacts on job

²² The analysis of job search outcomes in this sub-section was not pre-specified. In the Pre-Analysis Plan we committed to studying the impacts of the interventions on five main longer-term outcomes (which we report in the next subsection). Further, we anticipated that, motivated by our main results on those outcomes, we would run a number of additional exploratory regressions to better understand treatment mechanisms. This subsection presents this exploratory analysis.

Table 3: Job search impacts after 2 months

	Searched for work	Applications	Hours job search
<i>Syrians</i>			
Cash	0.056 (0.077)	0.518 (0.043)	0.794 (0.133)
Information	0.047 (0.123)	0.423 (0.072)	0.056 (0.482)
Nudge	0.037 (0.195)	0.648 (0.016)	0.698 (0.157)
Control mean	0.437	1.19	4.162
Observations	1536	1440	1444
<i>Jordanians</i>			
Cash	0.033 (0.165)	-0.055 (0.553)	-0.672 (0.831)
Information	0.025 (0.255)	-0.501 (0.874)	-0.457 (0.757)
Nudge	0.065 (0.030)	0.458 (0.130)	0.241 (0.350)
Control mean	0.577	2.71	5.792
Observations	1897	1783	1717

Note: This table reports treatment effects on the three variables measuring job search, 2 months after the baseline interview. ‘Job search’ is a dummy for whether the person has done any job search in the last 30 days. ‘Applications’ is the number of job applications completed in the last 30 days. ‘Hours search’ is the number of hours of job search in the last seven days. ‘Applications’ and ‘Hours search’ are windsorised at the 99th percentile of the nationality-specific distribution. The first panel reports impacts for Syrians, and the second panel reports impacts for Jordanians. Next to each treatment effect, we report a randomization inference p-value.

applications: a 35 percent increase for the information intervention and a 55 percent increase for the nudge intervention.

Among Jordanians, the cash and information interventions have smaller and insignificant impacts on job search (Table 3). For example, the cash intervention is associated with a 3.3 percentage point insignificant increase in the job-search rate. However, we find a large and positive impact of the nudge intervention on job search (6.5 percentage points). Importantly, no intervention is associated with a significant impact on job applications among Jordanians and, for both the cash and information intervention, the effect is actually negative. Finally, Table 3 also highlights that Jordanians search much more intensely than Syrian refugees in the absence of the interventions: the control job search rate is 30 percent higher and the control number of job applications is twice as large.

4.2.2 Impacts on labour market outcomes

We find that both the cash and information intervention improve Syrian refugees' labour market outcomes at the two-month and four-month marks.²³ We report the relevant coefficient estimates and randomisation inference p-values in Table 4 and Table 5. Offering cash leads to a significant increase in the employment rate of more than 50 percent (an effect of 5.2 percentage points in the two-month survey and 3.8 percentage points at the four-month survey) and a significant boost in earnings of about 40 percent after two months and of 65 percent after four months.²⁴

We estimate that the information intervention increased employment and earnings by almost the same amount as the cash grant two months after baseline; four months after baseline, this intervention generates a 40 percent increase in employment (i.e., 1.9 percentage points, marginally insignificant) and a 55 percent increase in earnings (again, marginally insignificant). We also find that the nudge intervention has weaker and short-lived effects on the labour market outcomes of refugees. Four months after baseline, we are unable to document any significant impacts of this intervention.

Note that we find different results – in both control group employment rate and in treatment effects – when we compare the six-week rapid follow-up surveys with the two-month ESO follow-up surveys. We attribute this discrepancy to a difference in the interpretation of the employment question in the two surveys. In particular, due both to ethical and confidentiality concerns, we did not ask respondents to specify whether a particular wage job was formal or not. Further, as discussed in section 2.6, ESO surveys were conducted by staff members familiar to the respondents, whereas the rapid-response surveys were conducted by someone whom the respondents had not previously met. It is thus likely that many respondents, and particularly Syrian refugees, had more trust in the ESO enumerators, and hence were more likely to discuss informal work with them. In support of this interpretation, we show in Table A.12 in the Online Appendix that a substantial share of the increase in wage employment documented in the two-month follow-up survey occurs in jobs that pay just below the formal monthly minimum wage: a useful proxy of informality. In section 4.4, we show that these jobs have high hourly earnings, but fewer hours worked per month than formal jobs (and so, overall, lower monthly pay). During our qualitative interviews (section 4.4.3), many

²³ The analysis in this sub-section was pre-specified. We summarise all variable definitions in Table A.11. We report results disaggregated by nationality in Table 4 and 5. We report results for the full sample in Table A.13 and Table A.14 in the Online Appendix.

²⁴ We compute a set of sharpened q -values to control for the fact that we test for impacts on each outcome three times – once per intervention. The q -values for the impacts of the cash intervention on employment after 2 and 4 months are, respectively, 0.051 and 0.081. The q -values for the impacts of the cash intervention on earnings after 2 and 4 months are, respectively, 0.12 and 0.138.

respondents expressed a preference for this type of work.

The magnitude of the effects on refugee employment and earnings that we document is large relative to the estimates reported in the recent literature for other active labor market policies in developing countries (McKenzie, 2017). In proportional terms, both the earning and the employment effects are at the top of the distribution of the estimates reported in the literature. While this is partly driven by the low control employment rate, in absolute terms, the employment effect is still close to the top of the distribution of existing estimates for job search assistance policies (but smaller than the impacts of the most effective wage subsidy and training interventions). If the four-month impacts were sustained for about one year, the increase in earnings would equal the size of the cash grant. In comparison to the existing literature, however, our results speak only to a relatively short impact duration (namely, two-months and four-months after treatment); as explained earlier, the Covid lockdown forced the cancellation of our planned six-month follow-up survey (and, of course, caused massive disruption to the Jordanian labour market more generally).

For refugees, the cash grant and information interventions are also associated with small, insignificant increases in the well-being index and with an insignificant 7 percent drop in the proportion of people that intend to migrate outside of Jordan. While the migration effect is not significant, in absolute terms, the 4 percentage points decrease we estimate is commensurate to the size of the positive employment effects of these interventions. Finally, we do not find any impacts on social integration, consistent with recent evidence suggesting that baseline social integration for Syrian refugees in Jordan is high relative to the experience of Syrian refugees in European countries or in the US (Arababa'h et al., 2019).

For Jordanians, on the other hand, we are unable to find evidence of labour market impacts for any intervention (Table 4 and Table 5). This is particularly surprising for the nudge intervention, which has positive impacts on job search for this population. Further, for the cash intervention, we document an (insignificant) 2.9 percentage point reduction in employment and an (insignificant) 25 percent reduction earnings after four months, but a contemporaneous, significant increase in the well-being index of 0.06 of a standard deviation. This may be consistent with the cash intervention enabling jobseekers to reject offers for undesirable jobs.

4.3 Assessment of performance of the Tempered Thompson Algorithm

We now consider various aspects of the performance of the Tempered Thompson Algorithm in our setting.

Table 4: Treatment effects on main outcomes after 2 months

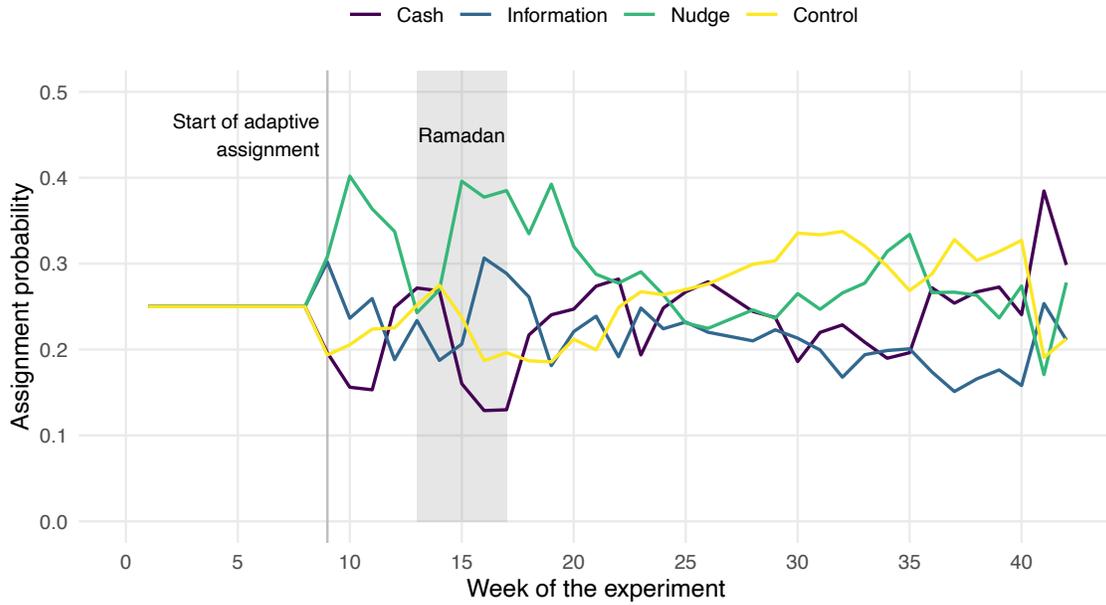
	Employed	Earnings	Well-being	Social integration	Intends to migrate
<i>Syrians</i>					
Cash	0.052 (0.017)	7.204 (0.062)	0.021 (0.333)	-0.009 (0.575)	-0.038 (0.838)
Information	0.047 (0.036)	6.209 (0.092)	0.025 (0.309)	-0.035 (0.728)	-0.041 (0.856)
Nudge	0.035 (0.081)	4.210 (0.185)	0.007 (0.445)	-0.055 (0.817)	-0.031 (0.783)
Control mean	0.091	16.268	0.088	0.011	0.664
Observations	1608	1605	1608	1608	1598
<i>Jordanians</i>					
Cash	-0.007 (0.618)	-1.491 (0.618)	0.101 (0.012)		
Information	-0.006 (0.624)	-2.486 (0.696)	0.019 (0.342)		
Nudge	-0.004 (0.585)	-1.684 (0.631)	0.015 (0.385)		
Control mean	0.128	29.22	0.069		
Observations	1985	1977	1985		

Table 5: Treatment effects on main outcomes after 4 months

	Employed	Earnings	Well-being	Social integration	Intends to migrate
<i>Syrians</i>					
Cash	0.038 (0.027)	6.550 (0.040)	0.043 (0.163)	0.005 (0.472)	-0.044 (0.875)
Information	0.019 (0.148)	4.567 (0.105)	0.003 (0.480)	0.001 (0.470)	-0.046 (0.884)
Nudge	0.003 (0.449)	0.260 (0.484)	0.052 (0.106)	0.005 (0.467)	-0.034 (0.810)
Control mean	0.052	9.76	0.008	-0.005	0.675
Observations	1565	1563	1565	1565	1561
<i>Jordanians</i>					
Cash	-0.025 (0.855)	-7.515 (0.911)	0.068 (0.055)		
Information	-0.009 (0.658)	-4.299 (0.773)	0.041 (0.169)		
Nudge	-0.002 (0.544)	-2.845 (0.709)	0.040 (0.176)		
Control mean	0.144	33.451	0.039		
Observations	1913	1900	1913		

Note: These tables report treatment effects on the five main outcomes specified in the Pre-Analysis Plan, 2 and 4 months after the baseline interview. ‘Employed’ is a dummy for whether the person has a wage-paying job at the time of the interview. ‘Earnings’ is the value earnings from the main job (where individuals who are not in wage employment are assigned a zero). ‘Well-being’ is a weighted index that includes: (i) a measure of expenditure, (ii) a measure of positive affect, and (iii) a measure of life satisfaction. ‘Social integration’ is an index of social integration. ‘Intends to migrate’ is a dummy for whether the respondent intends to migrate to a third country (i.e. this measure does not include return migration). The first panel reports impacts for Syrians, and the second panel reports impacts for Jordanians. Next to each treatment effect estimate, we report a randomization inference p-value.

Figure 1: Assignment probabilities by week



4.3.1 Treatment assignment probabilities over time

Consistently with the results presented in section 4.1, we find that in the last week of the study our algorithm places similar proportions of people in each of the four experimental groups. We show the probability of assignment to the four experimental conditions for each week of the study in Figure 1. By design, individuals are assigned to the different groups in equal proportion up to the sixth week of the study, as we have no information to update the priors up to that point. When learning started, the algorithm initially assigned more weight to the nudge intervention. However, this was slowly reversed after the 20th week of the study.

The algorithm’s departure from equal-proportions randomisation is somewhat more pronounced for specific strata. We show this in Figure A.1 in the Appendix, where we show strata-specific weekly treatment assignment probabilities, and in Table A.10 in the Online Appendix, where we show, for each treatment, the posterior probability that employment rates are highest under that treatment – that is, the posteriors that determine treatment assignment probabilities in our algorithm. While for some strata the assignment probabilities never depart from 25% in a sustained way, in some strata we do observe clear changes. For example, in the last week of the experiment, we assign almost 60% of inexperienced and less educated Jordanian women to the cash intervention. Similarly, for some strata, the probability that the control is optimal drops to a few

percentage points (e.g. inexperienced, less educated female Syrians). However, it should be stressed that, as discussed above, the differences in potential outcomes we estimate are small and hence the impacts of departing from equal-proportions randomization are limited in this context.

4.3.2 Welfare contrasts

Denote by $\hat{\theta}^{dt}$ the posterior expectation of θ^{dt} given all our data, at the end of the experiment. We now present three ‘welfare contrasts’ that quantify the overall impact of our interventions, both against a counterfactual where no treatment is given, and against a counterfactual where treatments are randomized in equal proportion. First, within the experiment, we compare the average potential outcomes for the *actually chosen treatment* assignment to the average that would have obtained under *random assignment*,

$$\Delta_1 = \frac{1}{N} \sum_{i,t} \left(\hat{\theta}^{D_{it}X_{it}} - \frac{1}{4} \sum_d \hat{\theta}^{dX_{it}} \right).$$

This measures how much better we did for our experimental participants, compared to a conventional design with fully random assignment.

Second, we compare the *optimal targeted policy*, and the *optimal non-targeted policy*, to the default of *no intervention* (treatment 0),

$$\Delta_2 = \sum_x \left(\max_d \hat{\theta}^{dx} - \hat{\theta}^{0x} \right) \cdot p^x,$$

$$\Delta_3 = \max_d \sum_x \left(\hat{\theta}^{dx} - \hat{\theta}^{0x} \right) \cdot p^x.$$

The definition of Δ_2 allows the optimized d to depend on x , while the definition of Δ_3 requires the same d to be implemented for all x .

We estimate that overall impacts on employment after six weeks are small; Table 6 reports our corresponding estimates of the three welfare contrasts specified above. We have two key findings. First, if we compare the optimal targeted policy to a counterfactual where no intervention is given (welfare contrast Δ_2), we estimate a gain in employment of 1.7 percentage points (95% credible set: [0.001, 0.034]). Relative to the employment rate in the control groups, this amounts to a 35% increase in employment. The optimal non-targeted policy, on the other hand, delivers a gain in employment of about half of a percentage point (welfare contrast Δ_3), with a credible sets that includes zero (95% credible set: [-0.015, 0.27]). The difference in employment gains between these measures suggests that there may be some modest gains from targeting (though it

Table 6: **Welfare contrasts**

	Estimate	95% Credible set
Δ_1	0.002	(0.000, 0.004)
Δ_2	0.017	(0.001, 0.034)
Δ_3	0.006	(-0.015, 0.027)

Note: The table reports the welfare contrasts defined in Section 4.3.2.

should be noted that the confidence sets overlap). Overall, the percentage point effects are on the lower end of the impacts of ALMPs on employment reported in McKenzie (2017) (which are typically measured over a longer time frame).

In our study, adaptive randomization did not generate any six-weeks employment gains over standard randomization. We show this by reporting welfare contrast Δ_1 , in Table 6, which is very close to zero.

4.3.3 The impact of the Tempered Thompson Algorithm had we targeted two-month employment

We operationalized our Tempered Thompson Algorithm using the rapid, six-week follow-up surveys—where we found no significant impact of any treatment. What if, instead of using employment at the six-week rapid follow-up survey, we had instead targeted the two-month employment rate—where we found large impacts of cash, information and nudge treatments? To answer this question, we run a counter-factual simulation exercise. Specifically, we mimic the arrival rate of respondents (and their strata), but simulate employment outcomes using the observed two-month treatment effects (i.e., posterior means measured at the stratum level); we then simulate alternative treatment assignments based on those simulated employment outcomes.

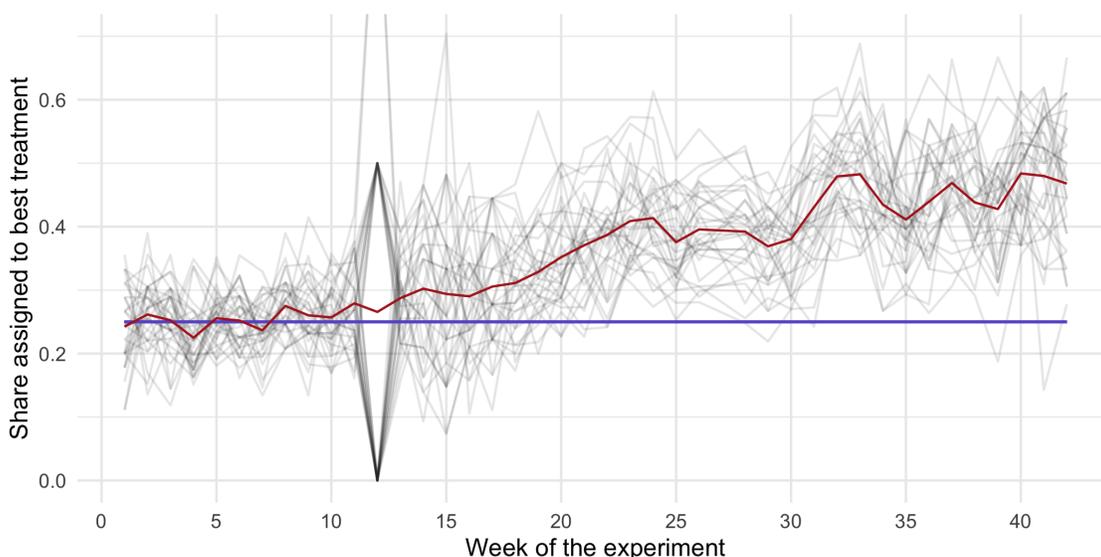
Figure 2 shows the main result from this exercise: it plots the average simulated share assigned, over time, to the best treatment (where ‘best treatment’ is allowed here to vary with the strata).²⁵ The figure confirms that the Tempered Thompson algorithm is quite capable of generating substantial learning in this kind of context, with the kinds of effect sizes that one might observe: indeed, the figure confirms that the limited performance of the algorithm in our experiment is attributable to our decision to target the

²⁵ The volatility in this figure – and in Appendix Figure A.15 – is driven by day-to-day changes in the proportions of different strata; this is reflected in the figures because our simulation replicated the exact arrival patterns of strata in our experiment.

six-week ‘rapid follow-up’ data, rather than the two-month ESO data.

In Appendix Figure A.15, we show the simulated expected employment rates by week. We estimate that the rate of employment in the control group would have been 11.2%, the rate of employment among respondents in a ‘pure RCT’ (with equal weight on each treatment) would be 12.4%, and the rate of employment from the Tempered Thompson algorithm would be 13.4%. That is, had we used the two-month data from the ESO surveys, rather than the six-week rapid follow-up data, Tempered Thompson would have generated approximately double the employment gains of a standard experiment.²⁶

Figure 2: **Simulated probability of being assigned to the best treatment**



This plot shows simulated shares assigned to the best treatment within each stratum from 32 simulations (red), contrasted with the corresponding shares from a pure RCT (blue). The grey lines show each of the 32 simulation trajectories.

4.4 What prevents effective job search among refugees?

4.4.1 Quantitative evidence on liquidity constraints

What have we learned about barriers to job search among refugees? Our ESO follow-up surveys at the two-month and four-month marks show that the cash intervention has the largest impacts on job search and employment for this population. Here, we present evidence suggesting that the cash grant is effective because liquidity constraints are a

²⁶ Appendix Figure A.16 shows the simulated gains by strata; the effect is particularly driven by Syrians with employment experience (both men and women, with and without high-school education), and by Jordanian men with high-school education and work experience.

key labour market barrier for refugees.²⁷ We do this by studying whether proxies for available liquidity are associated with higher control job search intensity and whether treatment effects are heterogeneous with respect to liquidity. Further, we report impacts on additional measures of job quality. Liquidity-constrained individuals would forgo desirable employment opportunities due to the inability to pay for search and application costs; if these constraints are binding, the marginal jobs obtained by cash beneficiaries would have similar or better quality as the control jobs.

First, among control refugees, we find a strong association between job-search intensity and expenditure at baseline (a proxy of liquidity). We plot this relationship non-parametrically in Figure A.10 in the Online Appendix; both the probability of searching for work and the number of job applications increase with expenditure, especially for individuals with expenditure below the median. These associations are sizeable. Using a linear regression, we find that a one standard deviation increase in expenditure at baseline is associated with a 0.6 standard deviation increase in the number of job applications sent (and a 0.08 standard deviation increase in the probability of job search). In contrast, among Jordanians, this relationship is much weaker: an increase in expenditure by one standard deviation is associated with a 0.3 standard deviation increase in the number of job applications and a 0.03 standard deviation increase in the probability of job search (also see Figure A.11). This is consistent with several recent quantitative reports from international organisations working with refugees, which report that cash-constraints are a first-order concern both for Syrian refugees in Jordan (Abu Hamad et al., 2017), and for Syrian refugees in Lebanon (Lehmann and Masterson, 2014; Chaaban et al., 2020); see Schuettler and Caron (2020) for a review. It is also consistent with our qualitative interviews – described shortly – in which we found that financial constraints are a key barrier, and that the cash grant was seen as facilitating job search.²⁸

Second, we show that the impacts of the cash intervention are driven by refugees who have expenditure below the median. We show this in Figures A.12, A.13 and A.14: impacts on job search and employment are concentrated among the poorest respondents (while impacts on job applications are more evenly distributed). In contrast, the information and nudge intervention have (i) generally weaker impacts on job search among low-expenditure refugees, and (ii) employment impacts for low expenditure refugees that are about half of those of the cash intervention. Additional evidence in support of credit constraints comes from refugees' reports on how they spent the cash: 26 percent

²⁷ The analysis in this section was not pre-registered, but is part of our exploration of treatment mechanisms.

²⁸ Hagen-Zanker et al. (2018) also report on qualitative interviews with Syrian refugees in Jordan, and also emphasize the primary importance of financial constraints.

of recipients in the low-expenditure group report that they mostly spend the money on job search. Among above-median expenditure recipients, this proportion drops to 18 percent.²⁹

Third, we find that the cash intervention boosts job retention and, after four months, hourly wages; we show this in Table 7. The grant doubles the probability of having retained a job between the two- and the four-month interviews – from 3.3 percent to 6.2 percent. Further, mean hourly wages among employed cash beneficiaries are .63 of a standard deviation higher than in the control group. The other two interventions, on the other hand, are associated with much smaller increases in retention and hourly wages (for example, job retention among information recipients is 5.2 percent, and the impact on hourly wages is about .04 of a standard deviation). These impacts indicate that, consistently with the prediction of a model where job search is constrained by limited liquidity, the cash intervention enables jobseekers to find jobs that have higher match quality – and hence are more stable and better paid.

Table 7: **Retention and wages for Syrians**

	Job retention month 4	Hourly wage month 2	Hourly wage month 4
Cash	0.030 (0.036)	0.037 (0.393)	0.308 (0.055)
Information	0.017 (0.139)	-0.131 (0.835)	-0.087 (0.673)
Nudge	0.011 (0.270)	-0.124 (0.812)	-0.010 (0.527)
Control mean	0.034	1.377	1.197
Observations	1565	193	94

Note: This table reports treatment effects on the three additional outcomes. ‘Job retention month 4’ is a dummy capturing whether, at the time of the 4 month follow-up, the respondent holds the same job they held at the time of the 2 month follow-up. ‘Hourly wage’ is a variable obtained by dividing monthly wage earnings by hours worked. Next to each treatment effect estimate, we report a randomization inference p-value.

4.4.2 Eliciting expert forecasts

At the time of launching our interventions, we conducted an incentivized elicitation exercise with IRC staff. We ran this online, surveying 16 staff based in Amman, and four senior staff based in New York. For all respondents, we began by providing descriptive

²⁹ Among Jordanians, 32 percent of respondents report to have spent the cash mostly on job search. However, this proportion does not vary by baseline expenditure. Given the null impacts on job search and the higher control job-search intensity, it is likely that cash given to Jordanians has mostly financed infra-marginal job search.

quantitative information on the background of our sample, a brief description of each intervention, and information on the employment rate for a similar sample in 2018 (namely, a rate of 2.5%). We then asked a series of questions about each respondent's prediction for the rate of employment after six weeks; this was directly incentivized.³⁰ We illustrate forecast employment rates in the Online Appendix (Figures A.17 and A.18).

We have three main findings. First, relative to our estimated treatment effects, local staff were very optimistic in their forecasts: at the median, they predicted employment rates of 20%, 10% and 9% for the cash, information and nudge interventions respectively (the median forecast for the control group was 2.25%). Senior staff had more accurate forecasts: medians of 7%, 5% and 4% (against a median prediction in the control group of 3%). Second, the dispersion in forecasts was very large, indicating substantial uncertainty about treatment impacts. Third, interestingly, both local and senior staff correctly anticipated that the cash intervention would be most effective.

4.4.3 Qualitative fieldwork

Five months after the trial began, we conducted structured qualitative interviews in the form of focus group discussions with participants who have received one of the three search interventions. The purpose of the interviews was to build a deeper understanding of the job search process and the mechanisms by which some interventions may have worked for different groups of participants. Participants were divided into six single-gender groups, each group focusing on one of the three interventions (cash, informational, or psychological support). We found the following results.

First, consistently with our experimental findings, respondents identified financial constraints as a key barrier to economic opportunity. The cash grant was by far the most popular intervention, irrespective of the demographics of the respondents. Among those who received the grant, some indicated that they used the cash directly to cover transportation costs when searching for work. Several others, in particular, Syrian females, reported using the cash to cover immediate basic needs such as medical bills for themselves or the family. Given participants' highly vulnerable economic situations, the cash was seen as necessary step before searching for work. One Syrian female cash recipient reported that she used the cash for medical care, which allowed her to then begin searching for work and eventually to find a job in a factory. These qualitative findings match our quantitative results.

³⁰ Specifically, we told respondents that we would randomly draw one of their employment forecasts; if this forecast was within 1 percentage point of the correct answer, we would provide a lottery ticket having 50 tickets and a prize of US\$200.

Second, participants reported mixed views on the informational and nudge interventions. The information intervention was only reported to be useful or accessible by Jordanians who had higher education levels than the typical participant in our study. At the same time, for some of those who understood the content, the information intervention was a source of frustration given that employers frequently violated labor laws. As such, the information on labor laws was not deemed to represent *de facto* labor rights.

The nudge intervention was reported to be useful by the Syrian women we talked to, but for reasons quite different from our original theory of change. Rather than working as a commitment device, these respondents reported that the intervention motivated them by making them feel ‘like someone cared about them’, and by making job search top of mind. One Syrian woman indicated that following the last SMS message, she continued to use her tool and shared it with her female neighbours. However, others (mostly Jordanian males) reported a lack of interest or desire to receive SMS reminders on job search intentions and achievements. For this group of participants, the intervention was perceived not to be useful given they were already searching for work.

Finally, participants in all focus groups stressed that formal jobs are often poorly paid and offer bad working conditions. Women reported concerns about working in the formal manufacturing sector, where they would be required to mix with men and undergo long hours away from their children. In fact, many women indicated a strong interest in starting home-based businesses like a kitchen, beauty salon, accessory shop, or nursery which would offer them both a ‘safer’ and more flexible work environment. Men also perceived formal work in factories to be undesirable, but for different reasons. They mostly complained about low pay: wages in manufacturing firms are considered insufficient to support a family. Per diem informal work, mainly in services like plumbing, carpentry, and painting, was seen as more profitable. Additionally, factory work was seen as being potentially exploitative and men shared stories about wages being withheld or delayed by employers for arbitrary reasons. These observations suggest that constraints to labor market participation among both refugees and Jordanians are by no means restricted to the search frictions we have focused on in this study.

5 Lessons and recommendations for future adaptive field experiments

5.1 Short term employment as a surrogate

In our application, we designed the algorithm to maximize the short-term employment outcome of participants. As we discussed in Section 4.3.3, we would have been more

successful had we been able to target longer-term employment.

We might therefore ask whether using short-term employment as a proxy-outcome is systematically misleading—or simply less informative—about long-term welfare. This is the question discussed in the literature on “surrogate outcomes”(see, e.g., [Athey et al. \(2019\)](#) for a recent review). Suppose that we are interested in the long-term outcome \tilde{Y} , and that short term employment Y satisfies the so-called “surrogacy condition”,

$$\tilde{Y} \perp D|X, Y.$$

This condition requires that the long-term outcome should be independent of the treatment assignment conditional on observable characteristics X and short-run outcomes. Then, by the law of iterated expectations, we get that

$$E[\tilde{Y}|D, X] = E[[\tilde{Y}|D, X, Y]|D, X] = E[[\tilde{Y}|X, Y]|D, X].$$

An immediate implication of this result is that an algorithm maximizing the average of Y will also maximize the average of \tilde{Y} if (i) the surrogacy condition holds, and (ii) $[\tilde{Y}|X, Y]$ is increasing in Y for all X .

Ex post, we can empirically check the surrogacy condition in our data, by regressing \tilde{Y} on D, Y, X . The surrogacy condition is supported if we do not find a predictive effect of D on \tilde{Y} given Y and X . We test the surrogacy condition for 4-month (long-term) employment outcome \tilde{Y} using either 6-week or 2-month (short-term) employment outcomes Y as surrogates. The results are shown in Table 8. For either subgroup (Syrians and Jordanians) and either short-run surrogate outcome Y (6-week employment and 2-month employment) we do not reject the null hypothesis of the surrogacy condition; cf. the p -values for the Wald test in Table 8. That said, for 6-week employment it does appear that cash is marginally predictive of 4-month employment, suggesting a possible violation of the surrogacy condition. Overall, these findings suggest that using short-term employment as a proxy-outcome is not systematically misleading, though it is not very informative either.

5.2 Non-binary outcomes

In our discussion and empirical application we have focused on binary outcomes. This is motivated by the nature of our application, where the outcome of interest is employment status at the follow-up survey. Nothing prevents us, however, from considering non-binary outcomes for the Tempered Thompson Algorithm. For normally distributed

Table 8: Test of the surrogacy condition

Dependent variable: 4-month employment				
Cash	0.037 (0.017)	0.026 (0.016)	-0.026 (0.021)	-0.026 (0.020)
Information	0.024 (0.019)	0.007 (0.018)	-0.000 (0.022)	0.000 (0.021)
Nudge	-0.001 (0.014)	-0.007 (0.014)	-0.002 (0.020)	0.002 (0.019)
6-week employment	0.358 (0.073)	0.257 (0.034)		
2-month employment			0.497 (0.044)	0.452 (0.035)
Wald test (p)	0.081	0.245	0.575	0.527
Sample	Syrian	Jordanian	Syrian	Jordanian

Note: Robust standard errors in parentheses. All regressions are re-weighted using strata-specific weights. The second row from the bottom reports the p -value from a Wald test of the joint null hypothesis that the coefficients on all the treatment dummies are equal to zero.

outcomes, one could – for instance – consider the following hierarchical Bayesian model:

$$\begin{aligned}
 Y_{it}^d | (X_{it} = x, \theta^{dx}, \mu^d, \tau^{2d}, \sigma^2) &\sim N(\theta^{dx}, \sigma^2), \\
 \theta^{dx} | (\mu^d, \tau^{2d}, \sigma^2) &\sim N(\mu^d, \tau^{2d}), \\
 (\mu^d, \tau^{2d}) | \sigma^2 &\sim \pi_1, \\
 \sigma^2 &\sim \pi_2.
 \end{aligned}$$

As before, such a model leads to an optimal combination of within and between stratum information, leading to posterior estimates of θ^{dx} which can be used in the Tempered Thompson Algorithm. More complicated outcome models – for example, allowing for mixture distributions, truncation, and so on – can be similarly defined and implemented: the Tempered Thompson Algorithm can be applied to any hierarchical Bayesian model that generates a posterior distribution of the mean of the potential outcomes Y_{it}^d , conditional on covariates.

5.3 Choosing the sample size

In conventional experiments, the sample size is often chosen based on power calculations. For a given effect size, the sample size is set such that the probability of rejecting

the null hypothesis of no effect equals or exceeds a conventional value (e.g., 80%) for a test using a conventional significance level (e.g., 5%).

In our context, this approach does not apply directly, as our objective is not simply to satisfy a particular conventional power constraint. Instead, it would be more natural to choose the sample size according to cost-benefit calculations. Even better, one might use a data-dependent stopping rule to maximize welfare.

In our setting, increasing the sample size (number of waves and/or number of participants per wave) impacts (i) the welfare of participants and potential participants, (ii) the precision of estimates (and hence the power of statistical tests), and (iii) the cost of implementing the experiment. Let us consider each of these in turn.

For small sample sizes, the (Tempered) Thompson Algorithm assigns roughly equal shares of observations to the different treatment arms. Correspondingly, the welfare of initial participants equals the average of the θ^{dx} across treatment arms d . As the algorithm learns the (conditionally) optimal treatment d^* , the welfare of additional participants increases, and then converges to the constrained optimum, as described by Part 3 of Theorem 1. Therefore, as a function of sample size, total participant welfare shows increasing returns to scale initially, and eventually becomes linear.

The same argument characterizes the total cost of the experiment. Per participant costs initially equal the average of costs across treatment arms, and then converge to

$$(1 - \gamma) \times (\text{cost of the best treatment}) + \gamma \times (\text{average cost across all treatments}).$$

Estimator precision and power, on the other hand, exhibit decreasing returns to scale. This is for two reasons. First, and this is true generally, the variance for each treatment arm is inversely proportional to the sample size for that treatment arm. Second, and this is specific to our algorithm, sample sizes for different treatment arms become more unbalanced over time, reducing the marginal decrease of variances for treatment effect estimation.

The following (myopic) rule provides a good rule-of-thumb for choosing when to stop the experiment, once we are in the regime of Theorem 1 (where we can, to a first order, ignore the dynamic returns of learning): In each period, calculate the expected return to and cost of an additional wave. We can do so based on the assignment shares $(1 - \gamma)\hat{p}_t^{dx} + \gamma/k$ implied by the algorithm, as well as the expected participant welfare θ^{dx} for each stratum and treatment arm as calculated by the hierarchical Bayesian model. The net return is given by the sum of the weighted average of expected participant welfare net of costs (averaged across treatment arms with weights given by the assignment shares) and the expected decrease of treatment effect estimator variance based on the

expected increase of sample size for each arm. Continue the experiment if and only if this expected net return exceeds 0.

5.4 Choosing the wave size

There is a separate question that arises conditional on the total sample size: *How many waves should there be?* (Equivalently, *how large should each wave be?*) Theory provides guidance. In terms of large sample behavior, it turns out that the wave size does not matter.

Intuitively, larger waves reduce the short-run possibility of adaptation. A design with smaller waves can always replicate a design with larger waves by ignoring the most recent information. But the delay in learning induced by larger wave sizes becomes relatively negligible as the sample size increases. That said, in smaller samples, choosing smaller waves (given total sample size) leads to slightly better performance, because of the possibility to adapt assignment shares more quickly.

On the other hand, practical constraints might restrict the frequency of waves, based on the time it takes for outcomes to be realized and measured, or based on institutional considerations (e.g., school children arriving in cohorts). Furthermore, there might be limits on the total run-time of the experiment. As a matter of practice, then, we recommend to choose waves as small as possible, subject to such practical constraints.

5.5 Alternative adaptive assignment algorithms

The purpose of Tempered Thompson Algorithm proposed in this paper is to trade off participant welfare and estimator precision. There are, of course, many alternative algorithms which have been discussed in the multi-armed bandit literature (see, e.g., [Slivkins \(2019\)](#) for a review).

A particularly simple alternative would be to first run a traditional RCT, with equal assignment shares across treatment arms. After a certain number of observations, one could then switch to the “greedy” choice of assigning the best-performing treatment to all future participants. This is called the “Explore-First” algorithm. This procedure can, in large samples, achieve similar welfare as the Tempered Thompson Algorithm, since there is a limited need for exploration, and thus for balancing of exploration and exploitation.

For smaller samples, the Explore-first algorithm is dominated by procedures based on Thompson sampling (such as the Tempered Thompson Algorithm; see also the discussion in Section 1.2 and the remainder of Chapter 1 in [Slivkins \(2019\)](#)).

5.6 Inference and the winner’s curse

We have discussed inference for various objects in Section 3.5. In particular, we considered both Bayesian inference and randomization inference for individual treatments, and for the expected welfare gains due to the Tempered Thompson Algorithm. Inference on expected welfare gains is related to the problem of “inference on winners” (Andrews et al., 2019) although it is orthogonal to the algorithm used for treatment assignment. Remarkably, neither Bayesian inference nor randomization inference need to explicitly correct for the selection issue of focusing on winners. For Bayesian inference, this holds because the posterior already conditions on the full data. The choice of “winner” is a function of the data. Conditioning on the identity of the winner, then, does not carry any further information which would need to be reflected in the posterior.

Randomization inference compares a statistic to the distribution of this same statistic over permutations (re-assignments) of treatment, according to the assignment algorithm used. No matter which statistic is used, this results in a test which controls size (the probability of rejection under the null) for the null hypothesis that all potential outcomes are the same. In particular, this is true for statistics such as the largest mean outcome across treatments. For any re-assignment of treatment, the largest mean might of course correspond to another treatment than in the actual data.

In this paper, we have not considered Neyman-style inference which needs to account for selection; this is akin to multiple testing corrections (Andrews et al., 2019). Implementing such inference remains an interesting avenue for future work.

5.7 Non-stationarity

In our hierarchical Bayesian model, we have assumed that the data-generating process is stationary, i.e, the distribution of potential outcomes, conditional on covariates, is constant over time. This assumption might be problematic in our setting. Potential outcome distributions change over time. However, this does not mean that our approach is flawed. In the multi-armed bandit literature, non-stationary settings are considered using the framework of “adversarial” bandits, cf. Cesa-Bianchi and Lugosi (2006). This branch of the literature provides worst-case performance guarantees for any sequence of potential outcomes, rather than just for sequences of potential outcomes generated by i.i.d. draws from a fixed distribution, as in our Theorem 1. Regret guarantees can be provided even for this adversarial setting, where regret is defined relative to the comparison point of any *fixed* assignment policy, mapping covariates into a distribution over treatments.

Remarkably, bandit algorithms (such as Thompson sampling) are able to achieve

worst-case average regret (relative to any fixed treatment or targeted assignment policy) on the order of $1/\sqrt{T}$ (up to logarithmic terms). This is no worse than the performance for the worst-case stochastic setting, where potential outcomes are i.i.d. draws. Put differently, algorithms such as the Tempered Thompson Algorithm perform well (in terms of participant welfare) if there exists any fixed assignment policy that performs well—no matter how the outcomes are generated (see, e.g., Chapter 3 of [Bubeck and Cesa-Bianchi 2012](#), and Chapter 6 of [Slivkins 2019](#)).

It is also instructive to consider the sequences of potential outcomes leading to worst-case behavior. [Lewi et al. \(2020\)](#) provide a detailed characterization for Thompson sampling for the bit-prediction setting. They show that for this setting the worst-case form of non-stationarity *delays learning for as long as possible*. Holding constant average potential outcomes across time, the worst-case sequence is obtained by shuffling outcomes across time such that in initial periods outcomes are balanced, and posteriors converge towards a precise belief that the true effect equals zero. Then, in the latter part of the time window, maximum mean differences imply that maximum regret is incurred due to the algorithm essentially sticking to the assignment probabilities of a static RCT, which are derived from the beliefs learned during the beginning of the time window.

6 Conclusion

Randomized controlled trials have come under criticism from an ethical perspective. For example, [Deaton \(2020, p. 21\)](#) points out that “It is particularly worrying if the research addresses questions in economics that appear to have no potential benefit for the subjects.” Relatedly, implementation partners might be reluctant to engage in continued experimentation if they believe that they already know which intervention works best. Adaptive experimentation can help mitigate both ethical criticisms of RCTs and the reluctance of implementation partners to engage in experimentation, by setting welfare maximization (or regret minimization) as the main objective of an experiment. Indeed, any optimal adaptive experimental design has the property that participant welfare cannot be increased in the long-run by using any other (experimental or non-experimental) treatment allocation procedure.

In this paper, we have reported the results of an implementation of adaptive targeted treatment allocation in a field experiment. Our Tempered Thompson Algorithm strikes a balance between maximizing participant welfare and providing precise estimates of treatment effects. Our implementation context was novel: We looked at the effects of active labor market policies on Syrian refugees and local job-seekers in Jordan. Our treatments did not have a significant effect on refugee employment after six weeks,

but the cash grant had a substantial impact on longer-term employment outcomes.

Our results show that adaptive targeted experiments can be straightforwardly deployed in the field and can be used to draw scientific and policy conclusions. Moreover, our methodology creates many possibilities for further applications. The Tempered Thompson Algorithm is a powerful tool for any setting in which subjects arrive over time and their outcomes are observed within a short time-frame. In addition to employment programs, our methodology may be applied in many other development contexts, including drug and vaccination programs, agricultural technology adoption programs, climate change mitigation interventions, and emergency relief programs. Further work on adaptive experiments can tailor experimental designs to specific applications.

One of the most important considerations in future adaptive experiments will be defining the objective Y_t .³¹ The choice of the objective can change the performance of the algorithm affecting short-run and long-run participant outcomes. In our case, the choice of target outcome, i.e., formal employment six weeks after the intervention, was mainly driven by the organizational objectives of our implementation partner and their donors. Our results in Section 4.3.3 suggest that, had we used a broader definition of employment, our adaptive algorithm would have quickly found the optimal intervention. More generally, in our context, as in many others (such as education and health), policymakers do not only care about short-run outcomes that adaptive treatment policies might typically target. But adapting treatment allocation based on long-term outcomes can often make the field experiment too long and costly. Instead of only measuring and targeting long-term outcomes, the designer might therefore wish to find a set of short-run proxies, i.e., ‘statistical surrogates’, for long-term welfare (Athey et al., 2019); an adaptive targeted field experiment would therefore be designed in order to target these statistical surrogates.

³¹ The challenge of aligning decisions based on machine learning (e.g., of robots and other artificial intelligence systems) with broader societal interests is by no means unique to adaptive experiments (Taylor et al., 2016; Russell, 2019).

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Appendix

A.1 Proofs

A.1.1 Preliminaries

Our characterization of the large sample properties of our Tempered Thompson Algorithm relies on the following two useful results from the literature. The first is a law of large numbers for adaptive sequences, which can be found as Lemma 5 in [Russo \(2016\)](#). The second is a sufficient condition for consistency of Bayesian posteriors, known as Schwartz's theorem, which can be found as Theorem 6.16 in [Ghosal and Van der Vaart \(2017\)](#).

Lemma 1 (LLN for adaptive sequences) *Let $\{Y_n\}$ be an i.i.d sequence of real-valued random variables with finite variance and let $\{W_n\}$ be a sequence of binary random variables. Suppose each sequence is adapted to the filtration $\{\mathcal{H}_n\}$, and define $Z_n = P(W_n = 1 | \mathcal{H}_{n-1})$. If, conditioned on \mathcal{H}_{n-1} , each Y_n is independent of W_n , then with probability 1,*

$$\lim_{n \rightarrow \infty} \sum_{l=1}^n Z_l = \infty \Rightarrow \lim_{n \rightarrow \infty} \frac{\sum_{l=1}^n W_l Y_l}{\sum_{l=1}^n Z_l} = E[Y_1].$$

Theorem 2 (Schwartz) *If $p_0 \in KL(\Pi)$ and for every neighborhood \mathcal{U} of p_0 there exist tests φ_n such that $P_0^n \varphi_n \rightarrow 0$ and $\sup_{p \in \mathcal{U}^c} P^n(1 - \varphi_n) \rightarrow 0$, then the posterior distribution $\Pi(\cdot | X, \dots, X)$ in the model $X, \dots, X | p \sim^{iid} p$ and $p \sim \Pi$ is strongly consistent at p_0 .*

In the statement of this theorem, Π is the prior distribution, $KL(\Pi)$ is its Kullback-Leibler support.

We furthermore leverage the following result from [Melfi and Page \(2000\)](#). This is a re-statement in our notation of their Corollary 2.1 (item 1 in the following theorem) and their Theorem 3.2, specialized to Bernoulli outcomes (item 2 in the following theorem).

Theorem 3 (Adaptive central limit theorem) *Suppose that Assumption 1 holds, except that we allow for general adaptive assignment algorithms determining D_{it} .*

1. *Suppose that D_{it} is a measurable function of X_{it} , the history $(D_{is}, Y_{is})_{s < t}$, as well as an independent randomization device. Let I_s^{dx} be the index pair (i, t) of the s -th time for which $D_{it} = d$ and $X_{it} = x$. Then the sequence $(Y_{I_1^{dx}}, Y_{I_2^{dx}}, \dots)$ is i.i.d. $\text{Ber}(\theta_0^{dx})$. These sequences are furthermore independent across different values of d, x .*

2. Suppose additionally that the cumulative share q_t^{dx} allocated to treatment d in stratum x converges in probability to \bar{q}^{dx} for all d, x , and the share of units in stratum x converges in probability to p^x . Then

$$\left(\sqrt{M_t \cdot \bar{q}^{dx} \cdot p^x} \frac{\bar{Y}_t^{dx} - \theta_0^{dx}}{\sqrt{\theta_0^{dx}(1 - \theta_0^{dx})}}, \sqrt{M_t \cdot \bar{q}^{d'x'} \cdot p^{x'}} \frac{\bar{Y}_t^{d'x'} - \theta_0^{d'x'}}{\sqrt{\theta_0^{d'x'}(1 - \theta_0^{d'x'})}} \right) \rightarrow^d (Z_{dx}, Z_{d'x'})$$

as $t \rightarrow \infty$, where $Z_{dx}, Z_{d'x'}$ are independent normal variables, for any pair $(d, x) \neq (d', x')$.

A.1.2 Proof of Theorem 1

Let $W_{it} = \mathbf{1}(D_{it} = d, X_{it} = x)$, and

$$Z_{it} = E_t[W_{it}] = \left((1 - \gamma) \cdot p_t^{dx} + \gamma/k \right) \cdot p^x,$$

where E_t denotes the conditional expectation given observations up to wave $t - 1$, and conditional on θ . We can rewrite the sample average as

$$\bar{Y}_t^{dx} = \frac{\sum_{i,t' \leq t} W_{it'} Y_{it'}}{\sum_{i,t' \leq t} Z_{it'}} \cdot \frac{\sum_{i,t' \leq t} Z_{it'}}{\sum_{i,t' \leq t} W_{it'}}.$$

We have by construction that $Z_{it} \geq p^x \cdot \gamma/k$, and since $N_t \geq \underline{N}$, it follows that $\sum_{i,t' \leq t} Z_{it'} \rightarrow \infty$ as $t \rightarrow \infty$. Applying Lemma 1 to the first fraction, and a standard law of large numbers to the inverse of the second fraction, we get that

$$\bar{Y}_t^{dx} \rightarrow \theta_0^{dx}$$

in probability as $t \rightarrow \infty$.

1. Given the assumed uniqueness of d^{*x} , there exists an ϵ -neighborhood of θ_0 such that d^{*x} is constant for all x in this neighborhood. The claim follows if we can show that the posterior probability of such an ϵ -neighborhood goes to 1 in probability as $t \rightarrow \infty$.

Given our assumption that the prior for θ has full support, this condition follows from Schwartz's theorem (Theorem 2), if we can show existence of a consistent test for the hypothesis that $\theta = \theta_0$ against the alternative that $\|\theta - \theta_0\| > \epsilon$.

In our setting such a test can be constructed by setting

$$\varphi_t = \mathbf{1} (\|\bar{\mathbf{Y}} - \boldsymbol{\theta}_0\| > \epsilon/2).$$

The required consistency follows by convergence in probability of $\bar{\mathbf{Y}}$.

2. By construction of our algorithm, treatment d is assigned with probability $(1 - \gamma) \cdot p_t^{dx} + \gamma/k$ to units in stratum x in period t . It follows from item 1 that this probability converges to \bar{q}^{dx} as $t \rightarrow \infty$.

Since N_t is bounded below, the same holds for the cumulative share \bar{q}_t^{dx} .

3. By definition,

$$\text{Regret}_t = \sum_{x,d} \Delta^{dx} \bar{q}_t^{dx} \bar{p}_t^x,$$

where \bar{p}_t^x is the share of observations in stratum x up to period t . The claim follows from item 2, and the law of large numbers for \bar{p}_t^x , once we note that $\Delta^{dx} = 0$ for $d = d^{*x}$.

4. This is an immediate consequence of Corollary 2.1 and Theorem 3.2 in [Melfi and Page \(2000\)](#), as re-stated in our Theorem 3 above. The necessary condition of their Corollary 2.1 follows immediately from the fact that our Tempered Thompson Algorithm is adapted to the filtration induced by the history of data. The necessary conditions of their Theorem 3.2 (converging shares) are verified by our item 2 above.

■

A.2 Markov Chain Monte Carlo

Denote by $\boldsymbol{\theta}, \mathbf{m}_t, \mathbf{r}_t$ the vectors of parameters, cumulative trials, and cumulative successes, where each of these is indexed by both d and x , and denote by $\boldsymbol{\alpha}, \boldsymbol{\beta}$ the vectors of hyperparameters indexed by d . Let ρ index replication draws, with ρ ranging from 1 to $B + R$. We sample from the posterior distribution of $(\boldsymbol{\theta}, \boldsymbol{\alpha}, \boldsymbol{\beta})$ given $\mathbf{m}_{t-1}, \mathbf{r}_{t-1}$ using the Markov Chain Monte Carlo algorithm described in Algorithm 1. Markov Chain Monte Carlo methods are reviewed in [Gelman et al. \(2014\)](#), chapter 11.

Algorithm 1 converges to a stationary distribution that equals the joint posterior of $\boldsymbol{\alpha}, \boldsymbol{\beta}$ and $\boldsymbol{\theta}$ given $\mathbf{m}_t, \mathbf{r}_t$. In particular, we have that the posterior probability that a treatment d is optimal given x , in the sense that it maximizes the probability of employment,

Algorithm 1 Markov Chain Monte Carlo for the hierarchical Bayes model

Require: The cumulated assignment frequencies m^{dx} and success numbers r^{dx} .

Starting values α_0, β_0 , length of the burn in period B , and number of draws R .

- 1: **for** $\rho = 1$ **to** $B + R$ **do**
- 2: Gibbs step:
Given $\alpha_{\rho-1}$ and $\beta_{\rho-1}$, for all d, x
draw θ^{dx} from the $Beta(\alpha_{\rho}^d + r^{dx}, \beta_{\rho}^d + m^{dx} - r^{dx})$ distribution.
- 3: Metropolis step 1:
Given $\beta_{\rho-1}$ and θ_{ρ} , draw α_{ρ}^d
by sampling from a normal proposal distribution (truncated below).
Accept this draw if an independent uniform draw is less than the ratio of the posterior for the new draw, relative to the posterior for $\alpha_{\rho-1}^d$.
Otherwise set $\alpha_{\rho}^d = \alpha_{\rho-1}^d$.
- 4: Metropolis step 2:
Similarly for $\beta_{\rho-1}$ given θ_{ρ} and $\alpha_{\rho-1}$.
- 5: **end for**
- 6: Throw away all draws from the burn-in period $\rho = 1, \dots, B$.
- 7: **return** For all x and d , the estimated probabilities

$$\hat{p}^{dx} = \frac{1}{R} \sum_{\rho=B+1}^{B+R} \mathbf{1} \left(d = \arg \max_{d'} \theta_{\rho}^{d'x} \right). \quad (\text{A.1})$$

is given by

$$p_t^{dx} = P \left(d = \arg \max_{d'} \theta^{d'x} | \mathbf{m}_t, \mathbf{r}_t \right) = \text{plim}_{R \rightarrow \infty} \frac{1}{R} \sum_{\rho=1}^R \mathbf{1} \left(d = \arg \max_{d'} \theta_{\rho}^{d'x} \right). \quad (\text{A.2})$$

In our implementation of this algorithm, we use a warm-up period of $B = 1,000$, and then draw $R = 10,000$ replications; averaging over these gives our estimated posterior distribution. These values are generously chosen relative to standard recommendations (cf. [Gelman et al. \(2014\)](#) chapter 11), making convergence likely. In our simulations these values yield stable posterior probabilities.

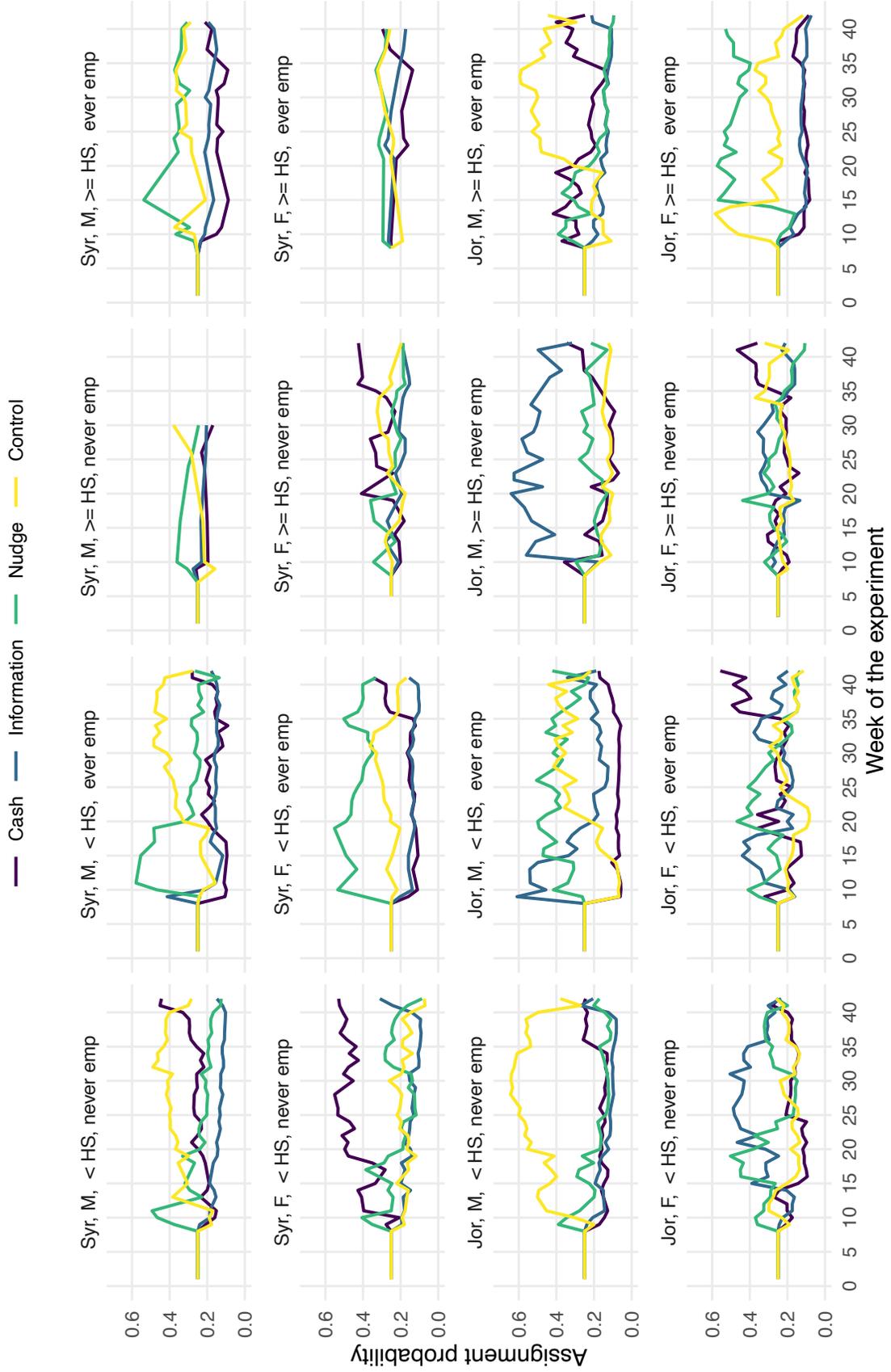
A.3 Additional Table and Figures

Table A.1: 95% credible sets for average potential outcomes

stratum	Cash	Information	Nudge	Control
Syr, M, < HS, never emp	(0.010, 0.110)	(0.000, 0.080)	(0.010, 0.090)	(0.010, 0.100)
Syr, M, < HS, ever emp	(0.030, 0.120)	(0.010, 0.090)	(0.030, 0.100)	(0.030, 0.100)
Syr, M, >= HS, never emp	(0.020, 0.260)	(0.000, 0.170)	(0.010, 0.140)	(0.020, 0.240)
Syr, M, >= HS, ever emp	(0.010, 0.170)	(0.000, 0.170)	(0.020, 0.150)	(0.010, 0.180)
Syr, F, < HS, never emp	(0.010, 0.050)	(0.010, 0.060)	(0.000, 0.050)	(0.000, 0.030)
Syr, F, < HS, ever emp	(0.010, 0.080)	(0.010, 0.110)	(0.020, 0.080)	(0.010, 0.070)
Syr, F, >= HS, never emp	(0.020, 0.190)	(0.000, 0.150)	(0.020, 0.150)	(0.000, 0.140)
Syr, F, >= HS, ever emp	(0.010, 0.180)	(0.000, 0.160)	(0.010, 0.130)	(0.000, 0.160)
Jor, M, < HS, never emp	(0.010, 0.110)	(0.010, 0.090)	(0.020, 0.120)	(0.030, 0.120)
Jor, M, < HS, ever emp	(0.030, 0.170)	(0.040, 0.150)	(0.050, 0.140)	(0.060, 0.160)
Jor, M, >= HS, never emp	(0.040, 0.230)	(0.040, 0.220)	(0.020, 0.150)	(0.000, 0.140)
Jor, M, >= HS, ever emp	(0.030, 0.150)	(0.020, 0.160)	(0.010, 0.110)	(0.040, 0.150)
Jor, F, < HS, never emp	(0.010, 0.070)	(0.020, 0.080)	(0.030, 0.090)	(0.010, 0.080)
Jor, F, < HS, ever emp	(0.060, 0.190)	(0.050, 0.170)	(0.020, 0.100)	(0.030, 0.130)
Jor, F, >= HS, never emp	(0.030, 0.150)	(0.010, 0.100)	(0.010, 0.080)	(0.020, 0.130)
Jor, F, >= HS, ever emp	(0.000, 0.110)	(0.000, 0.100)	(0.060, 0.180)	(0.020, 0.110)

Note: The table reports results for wage employment at the time of the rapid follow-up interview.

Figure A.1: Assignment probabilities by stratum and by week



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An Adaptive Targeted Field Experiment: Job Search Assistance for Refugees in Jordan*

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ONLINE APPENDIX

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*See <https://maxkasy.github.io/home/files/papers/RefugeesWork.pdf> for the latest version of the paper and this supplement.

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A.3 Treatment materials

Figure A.1: Treatment paper tool: Interview preparation skills

مشروع تشييك

دليل إعداد المقابلات



هذا الدليل قابل للتطبيق على المقابلات بتر اجرائها لأي وظيفة، وليس فقط للوظائف الصناعية.
- تلقى هذا الدليل لا يعني أن مشروع تشييك قد وجد وظيفة لك، يتم استخدامه لأغراض تعليمية فحسب.
- التحضير للمقابلات مهمل للغاية، بحيث تبت الأبحاث الاقتصادية أن التحضير للمقابلات يساهم في زيادة تتراوح بين 5 و 15٪ في معدلات التوظيف.

١. الحصول على معلومات عن المقابلة



أخيراً، يجب على الباحث الاستفسار حول الوظيفة نفسها، راتبها وظروفها، بما في ذلك ساعات العمل وإذا كانت الوظيفة تتطلب الاختلاط مع جنسيات أخرى غير عربية أو الاختلاط بين الجنسين.



لم يجب على الباحث المعرفة أكثر عن الشركة أو المؤسسة التي تقدم لها، بما في ذلك اسم الشركة أو المؤسسة، وصفها، وقطاع أنشطتها.



أول شيء يجب أن يعرفه الباحث هو مكان المقابلة، تاريخ ووقت المقابلة، مدة المقابلة وأسم ومنتصب الشخص الذي سيجري المقابلة.



يجب على الباحث عن عمل الاتصال بمسؤول خدمة التوظيف أو مدير الموارد البشرية في الشركة لمعرفة التفاصيل المتعلقة بالمقابلة قبل يوم أو يومين على الأقل من موعد المقابلة.

٢. تحضير محتوى المقابلة



إظهار إهتمام الباحث في الشركة والوظيفة التي تقدم لها، يجب إعداد قائمة أسئلة التي يمكن في طرحها على الشخص الذي يجري المقابلة.



على الباحث إعداد قائمة من نقاط ضعفه الرئيسية، من المهر عن الخوف من التحدث بصدق عن نقاط الضعف، ولكن الاستعداد لمناقشة الطرق المحتملة لمعالجتها.



ومن الضروري أيضاً أن يعد الباحث عرضاً يقدّم الأسباب التي ستفتح الشركة لتوظيفه، يمكن أن يتضمن العرض معلومات عن الخبرة الحالية والسابقة، نقاط القوة والأهداف المهنية.



من المهر مراجعة الأسئلة التي يتم طرحها عادة في المقابلات وتحضير أجوبة مناسبة لهذه الأسئلة يمكن التطور على هذه الأسئلة من خلال البحث على جوجل أو سؤال مسؤولي خدمة التوظيف.

٣. الاستعداد للمقابلة وإجراء المقابلة



عند السؤال عن التجربة السابقة، من الأفضل عدم التقليل من المهارات التي اكتسبها الباحث، حتى من العمل غير الرسمي.



أثناء المقابلة، يجب الباحث أن يحاول الإسترخاء، الإنشامة، والبقاء مهذبا من المهر الاستماع إلى الأسئلة بعناية قبل الإجابة، ويذكر الشخص الذي يجري المقابلة في النهاية.



يجب على الباحث أن يتأكد من أنه مهتم وأن يرتدي ملابس لائقة وبدون الكثير من الإكسسوار. لم عليه ترتيب النقل للذهاب إلى المقابلة مسبقاً، والتخطيط للوصول قبل الموعد المحدد للمقابلة.



من اللازم طباعة وإحضار أي مستندات رئيسية للمقابلة، مثل السيرة الذاتية، طلب التوظيف، وأي معلومات أو وثائق من الوظائف السابقة.

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Figure A.2: Treatment paper tool: Legal rights



مشروع تشبيك ما نعرفه عن قوانين العمل

<p>١. لا يحق للسوري الأشتراك في الضمان الاجتماعي</p>	<p>٢. يتضرر السوري الغير مسجل في المفوضية</p>	<p>٣. لا توجد فوائد للتسجيل في المفوضية</p>
<p>يحق لكافة العاملين بأجر في الأردن الأشتراك في الضمان الاجتماعي، والاستفادة من خدماته</p>	<p>لا يوجد اي ضرر على غير المسجلين هم فقط لا يتمتعون بامتيازات المفوضية في حال حاجتهم</p>	<p>يوفر التسجيل في المفوضية حماية من التسفير التعسفي أو الجبري</p>
<p>٤. لا يحق لصاحب العمل خصم مبلغ الضمان الاجتماعي من راتب العامل الشهر</p>	<p>٥. يتم الغاء مساعدات المفوضية في حال حصول السوري المسجل على تصريح عمل</p>	<p>٦. يحق لصاحب العمل تشغيل العامل ساعات طويلة و متعبة</p>
<p>يحق لصاحب العمل خصم مبلغ الضمان الاجتماعي من راتب العامل الشهري، حتى لو كان راتب العامل الشهري هو ٢٢٠ دينار - أقل راتب قانونيا</p>	<p>وجود تصريح عمل لا يعني عدم حصول السوري المسجل على مساعدات، يتم دراسة حالته وعلى اساسها تقدم المساعدات والامتيازات</p>	<p>لا يجوز تشغيل العامل أكثر من ثماني ساعات يوميا الا في الحالات المنصوص عليها في القانون ولا يحسب منها الوقت المخصص لتناول الطعام والراحة</p>
<p>٧. لا يحق للعامل طلب أيام اجازة بأجر كامل</p>	<p>٨. تعويض ساعات العمل الاضافية بأيام اجازة اضافية</p>	<p>٩. يحق لصاحب العمل فصل أو توجيه أي إنذار للعامل اذا توجه بشكوى اتجاه صاحب العمل</p>
<p>لكل عامل الحق في اجازة مرضية مدتها اربعة عشر يوما بأجر كامل بناء على تقرير من الطبيب المعتمد من قبل المؤسسة، علما بأنه لا يجوز احتساب الاجازة المرضية من الاجازات السنوية</p>	<p>على صاحب العمل تعويض ساعات العمل الاضافية بأجر اضافي الا في حال موافقة الموظف بتعويضها بأيام اجازة اضافية</p>	<p>يجوز أن يتوجه العامل بتوجيه أي شكوى اتجاه صاحب العمل و لا يحق لصاحب العمل فصله أو توجيه أي إنذار له لقيامه بذلك</p>

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Figure A.3: Treatment paper tool: Job search planning



مشروع تشبيك مخطط البحث عن عمل

١ مخطط الأسبوع الاول

تم تصميم هذه الأداة لمساعدتك في التخطيط للبحث عن وظيفة. اكتب خططك الشخصية
 لأهدافك الأسبوعية للبحث عن الوظائف. يرجى أن تكون واقعيًا أثناء تحديد أهدافك لهذا الأسبوع
 أرسل أهدافك في بداية الأسبوع و نتائجك في نهاية الأسبوع على الرقم التالي: ٠٧٧٠٤٦٣٦٠١

عدد طلبات العمل التي أخطط التقديم عليها

(اكتب العدد على الخط)

عدد الساعات التي اخطط قضاءها للبحث عن عمل

(اكتب العدد على الخط)

هل تم تحقيق الأهداف؟

إذا لم يتم تحقيق الأهداف،
اكتب عدد طلبات العمل التي تم التقديم عليها و عدد الساعات التي قضيتها للبحث عن عمل

(اكتب العدد على الخط)

(اكتب العدد على الخط)

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A.4 Additional tables

Table A.2: Sampling methods by nationality

Sampling method	Jordanian		Syrian	
	Number	Percentage	Number	Percentage
Referral	662	31%	577	35%
Community-based Organization	753	36%	360	22%
Home visit	167	8%	420	25%
Social media	101	5%	29	2%
UNHCR visit	3	0%	95	6%
IRC office visit	405	19%	178	11%
Other	16	1%	4	0%
Total	2107		1663	

Table A.3: **Attrition: full sample**

	Rapid-response	ESO (2 months)	ESO (4 months)
Cash	-0.001	-0.003 (0.637)	0.006 (0.258)
Information	0.005	0.009 (0.069)	0.016 (0.072)
Nudge	0.011	0.004 (0.212)	0.013 (0.118)
Control mean	0.012	0.020	0.048
Observations	3770	3770	3770

Note: This table reports the regressions of attrition on treatment for the full sample. Next to each treatment effect estimate, we report a randomization inference p-value. We only compute randomization inference p-values for observations that did not attrite in the rapid follow-up survey. This implies that we do not report p-values for impacts on attrition in the rapid follow-up survey. Further, for consistency, the coefficients in columns (2) and (3) are calculated for the restricted sample for which we calculate p-values. The control mean value of attrition reported in the last row always refers to the unrestricted sample.

Table A.4: **Attrition: Syrian sample**

	Rapid-response	ESO (2 months)	ESO (4 months)
Cash	0.007	-0.004 (0.671)	0.007 (0.303)
Information	0.013	0.006 (0.203)	0.018 (0.110)
Nudge	0.009	0.010 (0.076)	0.031 (0.016)
Control mean	0.005	0.010	0.026
Observations	1663	1663	1663

Note: This table reports the regressions of attrition on treatment for the Syrian refugee sample. Next to each treatment effect estimate, we report a randomization inference p-value. We only compute randomization inference p-values for observations that did not attrite in the rapid follow-up survey. This implies that we do not report p-values for impacts on attrition in the rapid follow-up survey. Further, for consistency, the coefficients in columns (2) and (3) are calculated for the restricted sample for which we calculate p-values. The control mean value of attrition reported in the last row always refers to the unrestricted sample.

Table A.5: **Attrition: Jordan sample**

	Rapid-response	ESO (2 months)	ESO (4 months)
Cash	-0.005	-0.002 (0.579)	0.006 (0.343)
Information	-0.001	0.011 (0.106)	0.015 (0.183)
Nudge	0.012	-0.001 (0.523)	-0.002 (0.568)
Control mean	0.017	0.028	0.065
Observations	2107	2107	2107

Note: This table reports the regressions of attrition on treatment for the Jordanian sample. Next to each treatment effect estimate, we report a randomization inference p-value. We only compute randomization inference p-values for observations that did not attrite in the rapid follow-up survey. This implies that we do not report p-values for impacts on attrition in the rapid follow-up survey. Further, for consistency, the coefficients in columns (2) and (3) are calculated for the restricted sample for which we calculate p-values. The control mean value of attrition reported in the last row always refers to the unrestricted sample.

Table A.6: Observations by stratum and treatment

	Cash	Information	Nudge	Control
Syr, M, < HS, never emp	51	35	61	58
Syr, M, < HS, ever emp	86	75	102	152
Syr, M, >= HS, never emp	3	3	3	4
Syr, M, >= HS, ever emp	4	2	11	12
Syr, F, < HS, never emp	244	111	151	156
Syr, F, < HS, ever emp	61	32	89	89
Syr, F, >= HS, never emp	10	5	10	9
Syr, F, >= HS, ever emp	3	3	5	5
Jor, M, < HS, never emp	47	44	44	106
Jor, M, < HS, ever emp	40	90	120	110
Jor, M, >= HS, never emp	18	23	12	9
Jor, M, >= HS, ever emp	47	23	27	65
Jor, F, < HS, never emp	101	193	153	117
Jor, F, < HS, ever emp	65	68	78	54
Jor, F, >= HS, never emp	58	52	60	48
Jor, F, >= HS, ever emp	22	23	87	60

Table A.7: Successes by stratum and treatment

	Cash	Information	Nudge	Control
Syr, M, < HS, never emp	2	0	2	2
Syr, M, < HS, ever emp	6	3	6	9
Syr, M, >= HS, never emp	1	0	0	1
Syr, M, >= HS, ever emp	0	0	1	1
Syr, F, < HS, never emp	6	2	1	0
Syr, F, < HS, ever emp	1	1	3	2
Syr, F, >= HS, never emp	1	0	1	0
Syr, F, >= HS, ever emp	0	0	0	0
Jor, M, < HS, never emp	2	1	3	8
Jor, M, < HS, ever emp	4	9	13	13
Jor, M, >= HS, never emp	3	4	1	0
Jor, M, >= HS, ever emp	4	2	1	6
Jor, F, < HS, never emp	2	9	8	4
Jor, F, < HS, ever emp	9	8	4	4
Jor, F, >= HS, never emp	5	2	1	3
Jor, F, >= HS, ever emp	0	0	13	3

Note: The table reports results for wage employment at the time of the rapid follow-up interview.

Table A.8: **Success rates by stratum and treatment**

	Cash	Information	Nudge	Control
Syr, M, < HS, never emp	0.04	0.00	0.03	0.03
Syr, M, < HS, ever emp	0.07	0.04	0.06	0.06
Syr, M, >= HS, never emp	0.33	0.00	0.00	0.25
Syr, M, >= HS, ever emp	0.00	0.00	0.09	0.08
Syr, F, < HS, never emp	0.02	0.02	0.01	0.00
Syr, F, < HS, ever emp	0.02	0.03	0.03	0.02
Syr, F, >= HS, never emp	0.10	0.00	0.10	0.00
Syr, F, >= HS, ever emp	0.00	0.00	0.00	0.00
Jor, M, < HS, never emp	0.04	0.02	0.07	0.08
Jor, M, < HS, ever emp	0.10	0.10	0.11	0.12
Jor, M, >= HS, never emp	0.17	0.17	0.08	0.00
Jor, M, >= HS, ever emp	0.09	0.09	0.04	0.09
Jor, F, < HS, never emp	0.02	0.05	0.05	0.03
Jor, F, < HS, ever emp	0.14	0.12	0.05	0.07
Jor, F, >= HS, never emp	0.09	0.04	0.02	0.06
Jor, F, >= HS, ever emp	0.00	0.00	0.15	0.05

Note: The table reports results for wage employment at the time of the rapid follow-up interview.

Table A.9: Weighted mean differences in employment by covariates, with randomisation inference p -values

Subgroup	Treatment	Success rate	Δ	P-value
Female	Cash		0.010	0.211
Female	Information		0.005	0.342
Female	Nudge		0.011	0.201
Female	Control	0.031		
Male	Cash		-0.001	0.501
Male	Information		-0.020	0.857
Male	Nudge		-0.009	0.676
Male	Control	0.077		
Jordanian	Cash		-0.001	0.531
Jordanian	Information		-0.006	0.648
Jordanian	Nudge		0.002	0.463
Jordanian	Control	0.068		
Syrian	Cash		0.013	0.123
Syrian	Information		-0.004	0.626
Syrian	Nudge		0.005	0.348
Syrian	Control	0.027		
No high school	Cash		0.005	0.329
No high school	Information		-0.002	0.574
No high school	Nudge		0.002	0.428
No high school	Control	0.046		
High school	Cash		0.009	0.387
High school	Information		-0.015	0.723
High school	Nudge		0.007	0.405
High school	Control	0.061		
Never employed	Cash		0.011	0.206
Never employed	Information		-0.001	0.514
Never employed	Nudge		0.004	0.402
Never employed	Control	0.031		
Ever employed	Cash		0.000	0.501
Ever employed	Information		-0.010	0.730
Ever employed	Nudge		0.003	0.445
Ever employed	Control	0.071		

Note: The table reports results for wage employment at the time of the rapid follow-up interview. Δ is the difference between weighted mean employment in a given treatment group and in the control group. p -values obtained with the randomization inference procedure discussed in Section 3.5.

Table A.10: Probability treatment is optimal, by stratum

Stratum	Cash	Information	Nudge	Control
Syr, M, < HS, never emp	0.38	0.09	0.29	0.24
Syr, M, < HS, ever emp	0.44	0.10	0.23	0.23
Syr, M, >= HS, never emp	0.42	0.12	0.12	0.34
Syr, M, >= HS, ever emp	0.24	0.20	0.26	0.30
Syr, F, < HS, never emp	0.45	0.33	0.19	0.03
Syr, F, < HS, ever emp	0.19	0.35	0.33	0.13
Syr, F, >= HS, never emp	0.41	0.16	0.29	0.14
Syr, F, >= HS, ever emp	0.32	0.23	0.23	0.22
Jor, M, < HS, never emp	0.18	0.06	0.29	0.46
Jor, M, < HS, ever emp	0.20	0.18	0.20	0.41
Jor, M, >= HS, never emp	0.41	0.45	0.09	0.05
Jor, M, >= HS, ever emp	0.31	0.24	0.08	0.36
Jor, F, < HS, never emp	0.08	0.29	0.48	0.15
Jor, F, < HS, ever emp	0.58	0.32	0.02	0.09
Jor, F, >= HS, never emp	0.58	0.10	0.04	0.27
Jor, F, >= HS, ever emp	0.04	0.02	0.89	0.05

Table A.11: **Main outcomes**

Outcome	Definition
Wage employment	A dummy for whether the respondent currently has a wage-paying job.
Earnings	Earnings from main job (0 if not in wage employment).
Well-being	An index that comprises (i) monthly expenditure, (ii) life satisfaction (0-10 scale), (iii) an indicator of negative affect (feeling anxious on previous day on a 0-10 scale), (iv) an indicator of positive affect (feeling happy on previous day on a 0-10 scale).
Social integration	An index of seven social integration questions (each question asks the respondent to report on a scale from 1 to 5 how much he or she agrees with a given statement, for example, 'I feel connected to Jordan').
Intends to migrate	A dummy for whether the respondent intends to migrate to a different country in next 12 months (this does not include return migration to Syria).

Note: All indices are constructed using the method outlined in [Anderson \(2008\)](#).

Table A.12: Treatment effects on employment for Syrians, after 2 months

	Employed below 200 JOD	Employed above 200 JOD
Cash	0.033 (0.042)	0.019 (0.117)
Information	0.048 (0.007)	-0.002 (0.537)
Nudge	0.027 (0.077)	0.006 (0.354)
Control mean	0.043	0.048
Observations	1623	1608

Table A.13: **Treatment effects after 2 months, full sample**

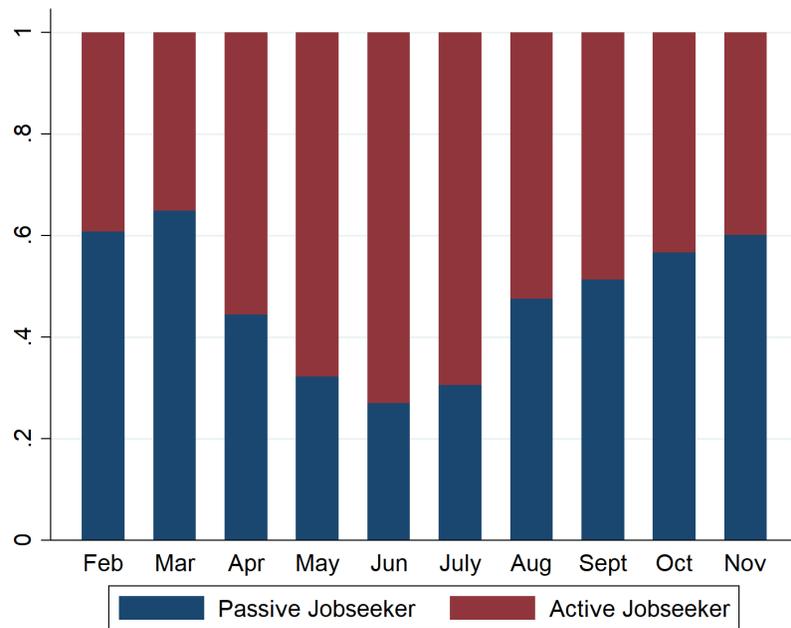
	Employed	Earnings	Well-being
Cash	0.020 (0.107)	2.405 (0.236)	0.065 (0.029)
Information	0.017 (0.121)	1.410 (0.341)	0.022 (0.259)
Nudge	0.013 (0.202)	0.957 (0.401)	0.012 (0.374)
Control mean	0.111	23.416	0.077
Observations	3593	3582	3593

Table A.14: **Treatment effects after 4 months, full sample**

	Employed	Earnings	Well-being
Cash	0.003 (0.420)	-1.167 (0.626)	0.057 (0.036)
Information	0.003 (0.404)	-0.298 (0.537)	0.024 (0.217)
Nudge	0.000 (0.521)	-1.444 (0.680)	0.046 (0.075)
Control mean	0.103	22.758	0.025
Observations	3478	3463	3478

A.5 Additional figures

Figure A.4: Active and passive sampling



Note: This Figure reports the proportion of jobseekers selected through active and passive sampling.

Figure A.5: Distribution of observations across strata

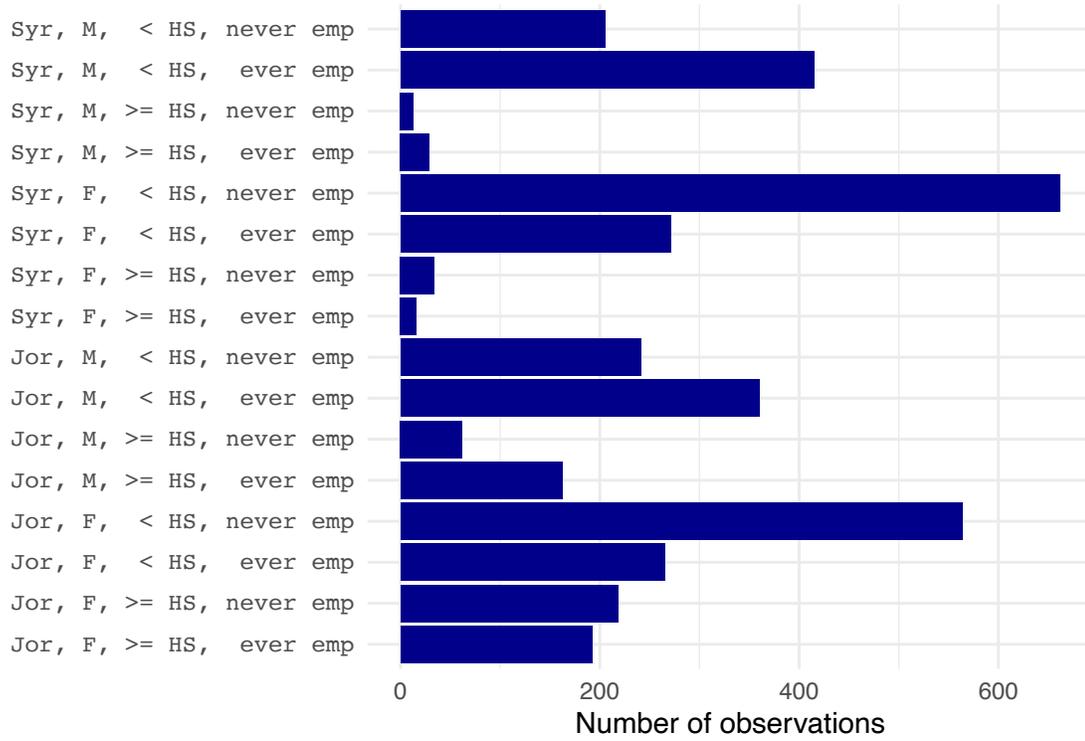


Figure A.6: Observations by week

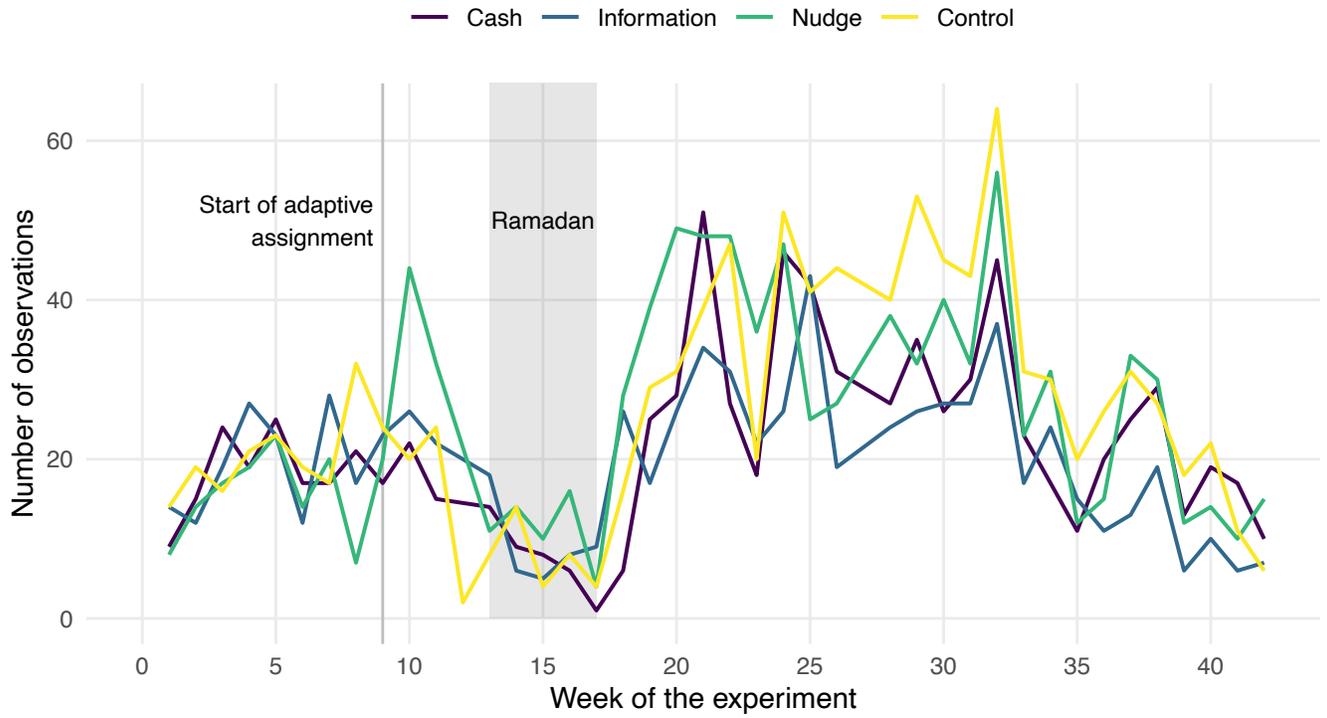


Figure A.7: Employment rate by week of sampling

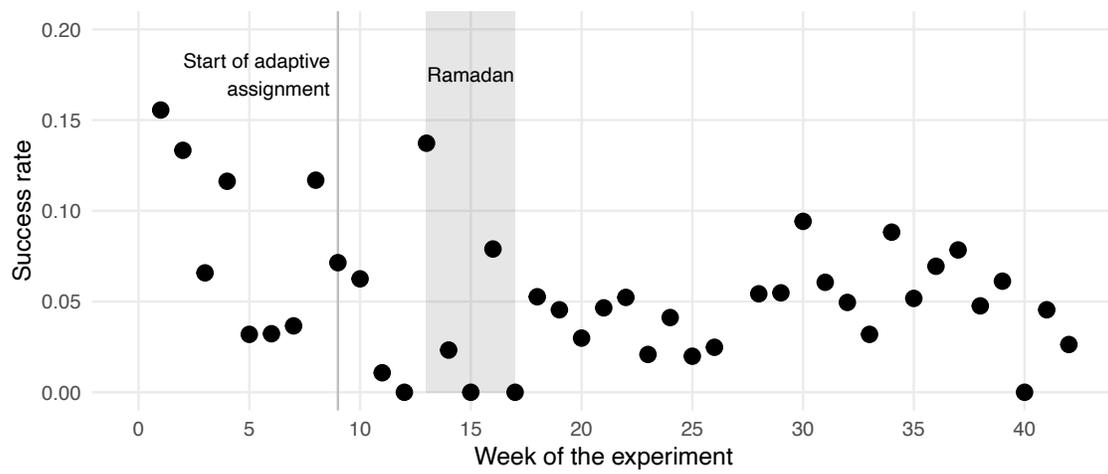


Figure A.8: Credible sets for average potential outcomes, and for average treatment effects relative to the control treatment

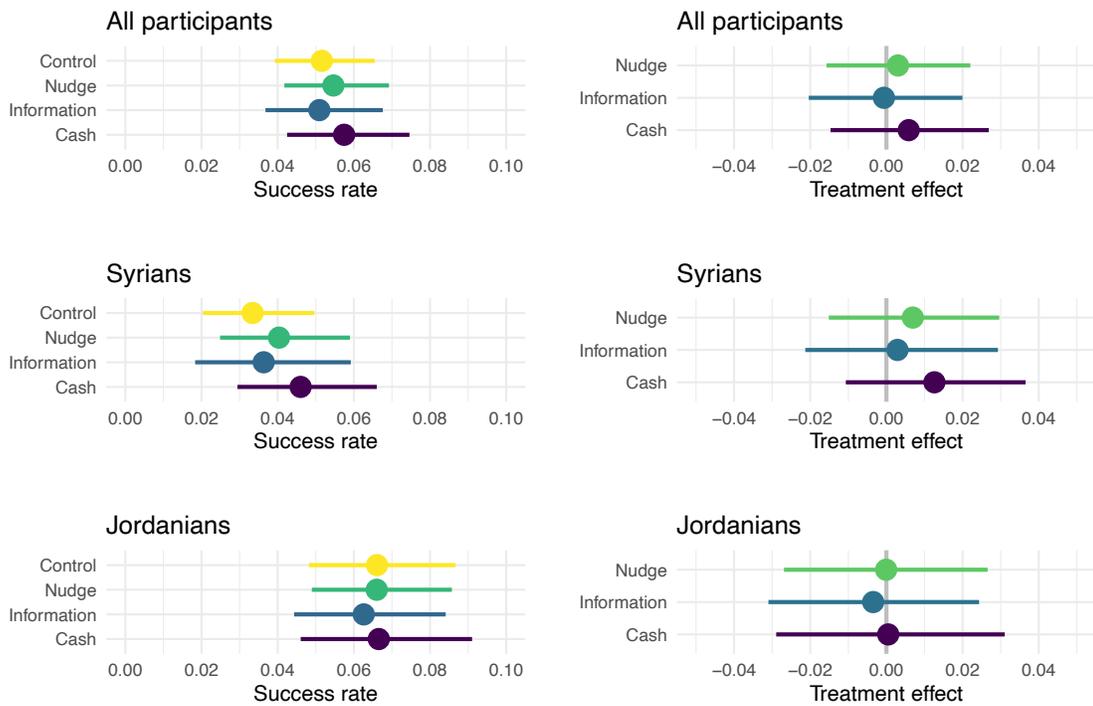


Figure A.9: 95% Credible sets for average potential outcomes across strata

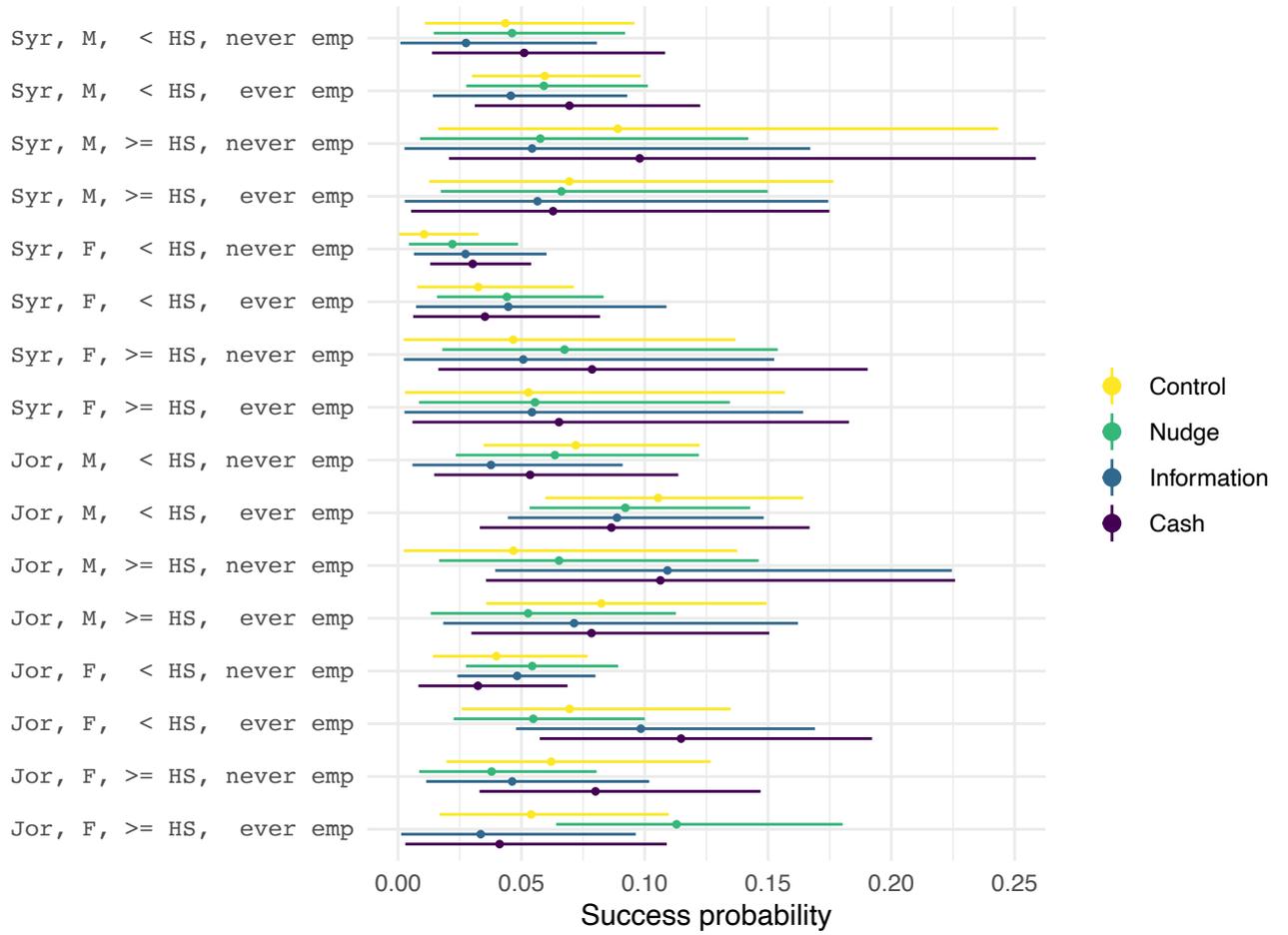
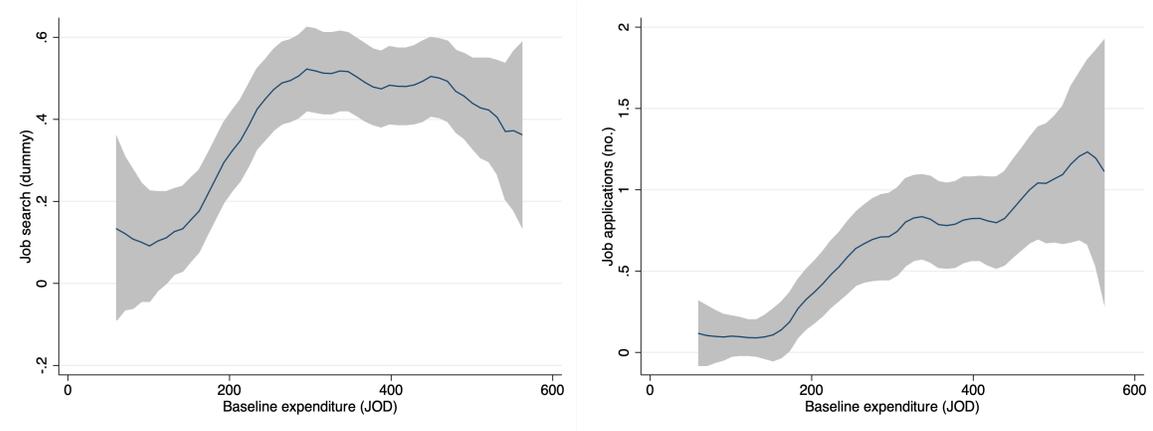


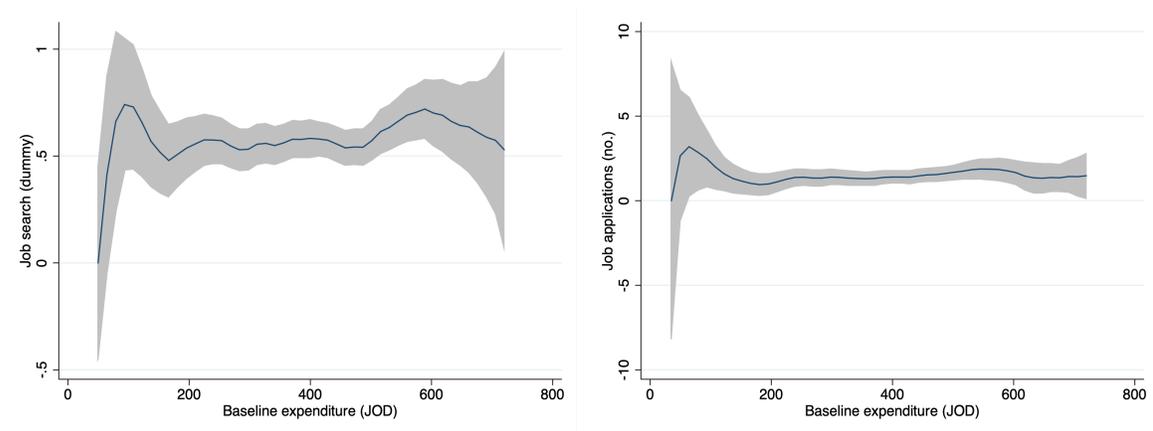
Figure A.10: Job search and baseline expenditure (control Syrians)



(a) Job search (dummy)

(b) Job applications (no.)

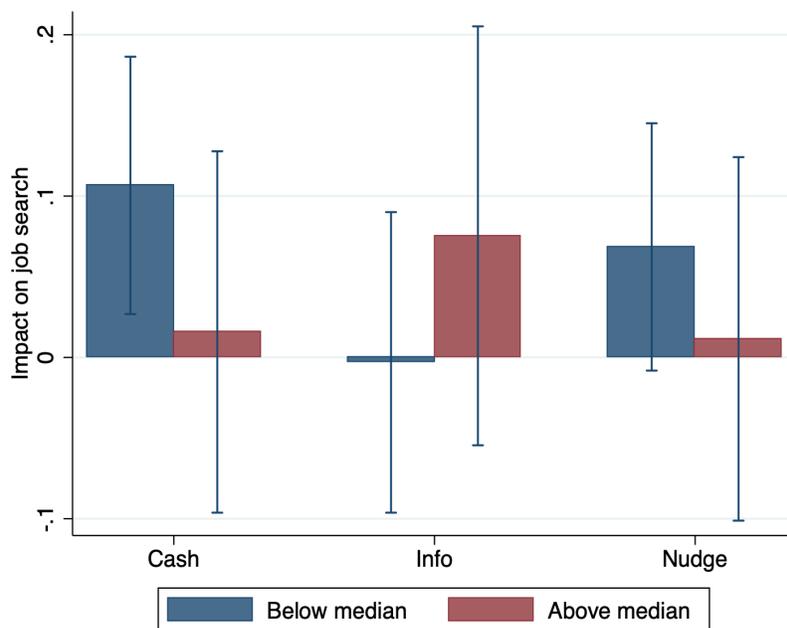
Figure A.11: Job search and baseline expenditure (control Jordanians)



(a) Job search (dummy)

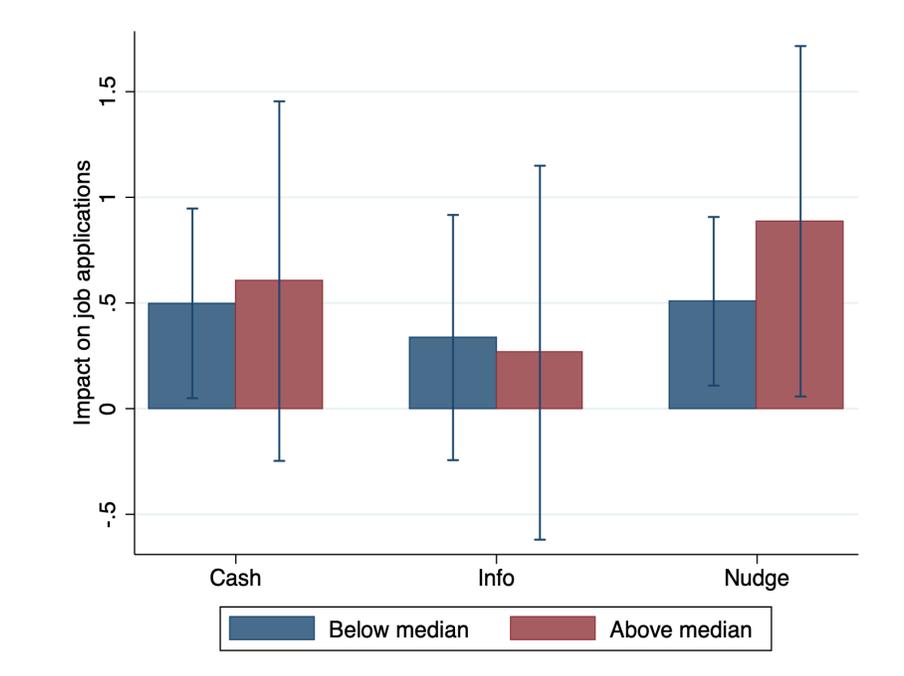
(b) Job applications (no.)

Figure A.12: Treatment effects on job search by baseline expenditure (Syrians)



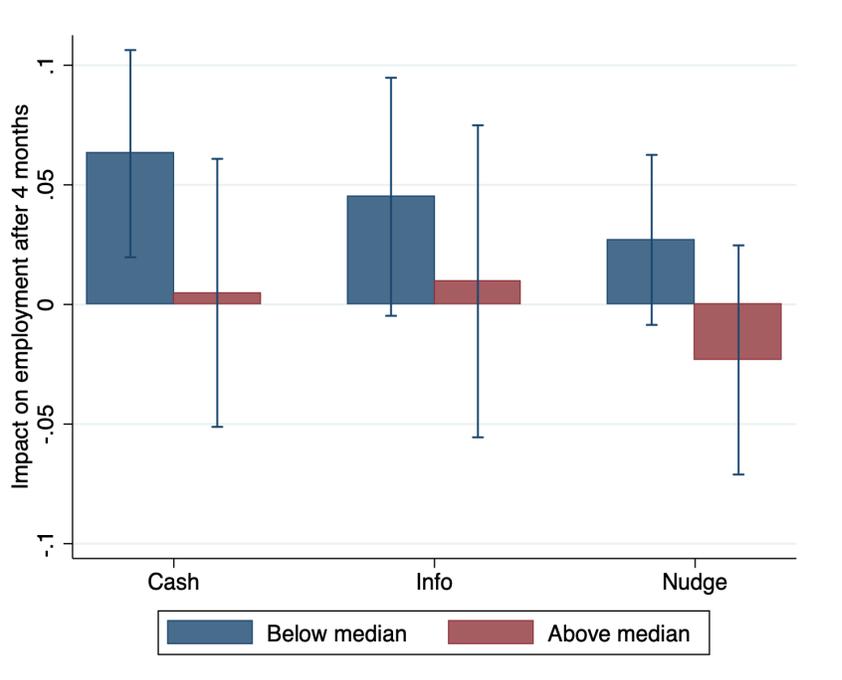
Note: This figure shows treatment effects and 90% confidence intervals by baseline expenditure.

Figure A.13: Treatment effects on applications by baseline expenditure (Syrians)



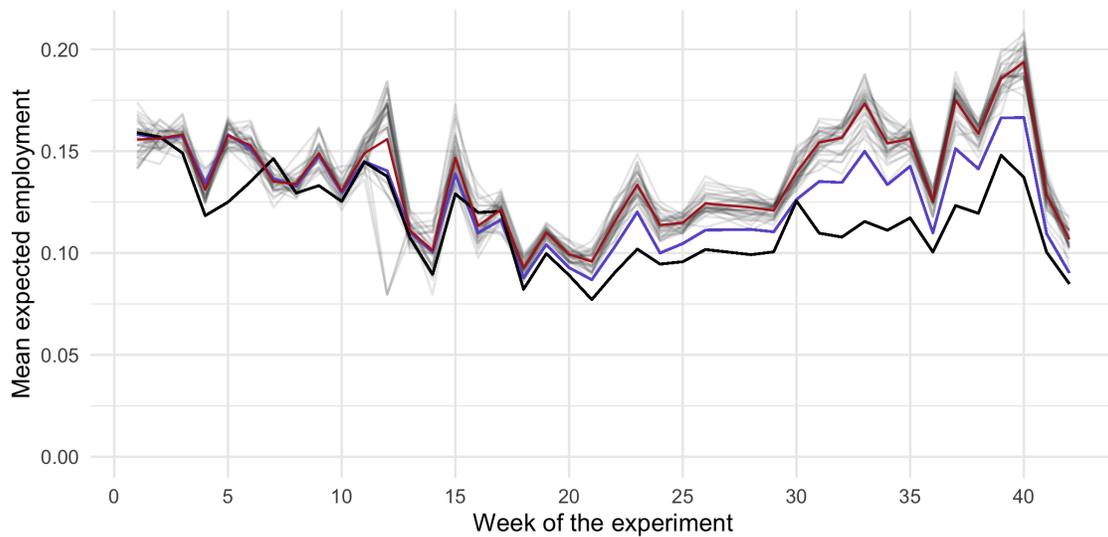
Note: This figure shows treatment effects and 90% confidence intervals by baseline expenditure.

Figure A.14: Treatment effects on 4-month employment by baseline expenditure (Syrians)



Note: This figure shows treatment effects and 90% confidence intervals by baseline expenditure.

Figure A.15: Simulated expected employment rate by week



This plot shows simulated expected employment from 32 simulations (red), contrasted with expected employment from a pure RCT (blue), and the counterfactual where everybody receives the control treatment (black). The grey lines show each of the 32 simulation trajectories.

Figure A.16: Simulated outcomes by group

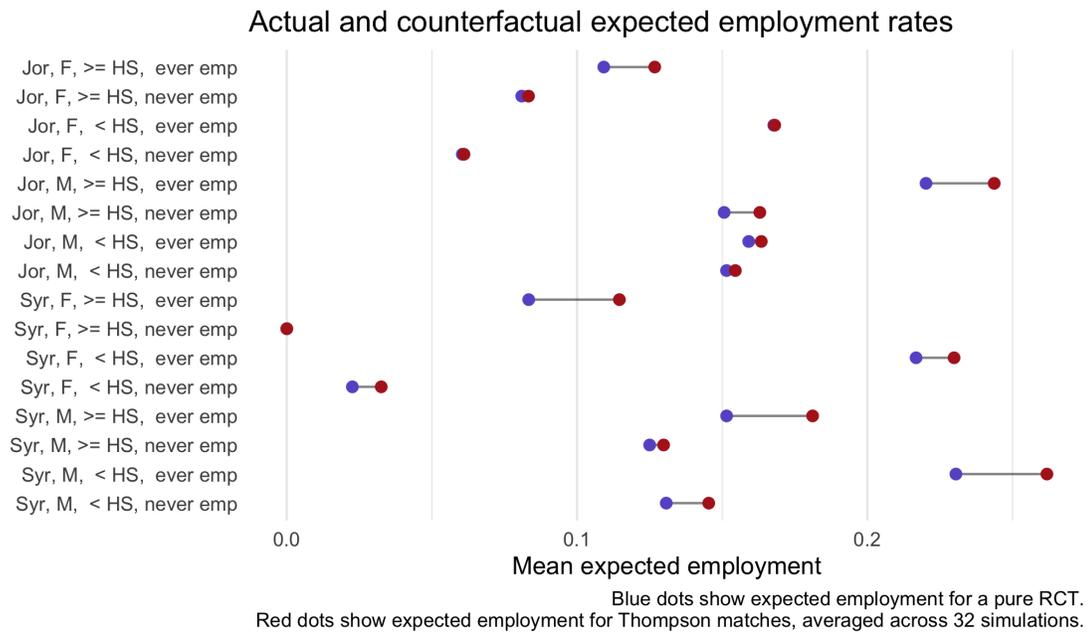
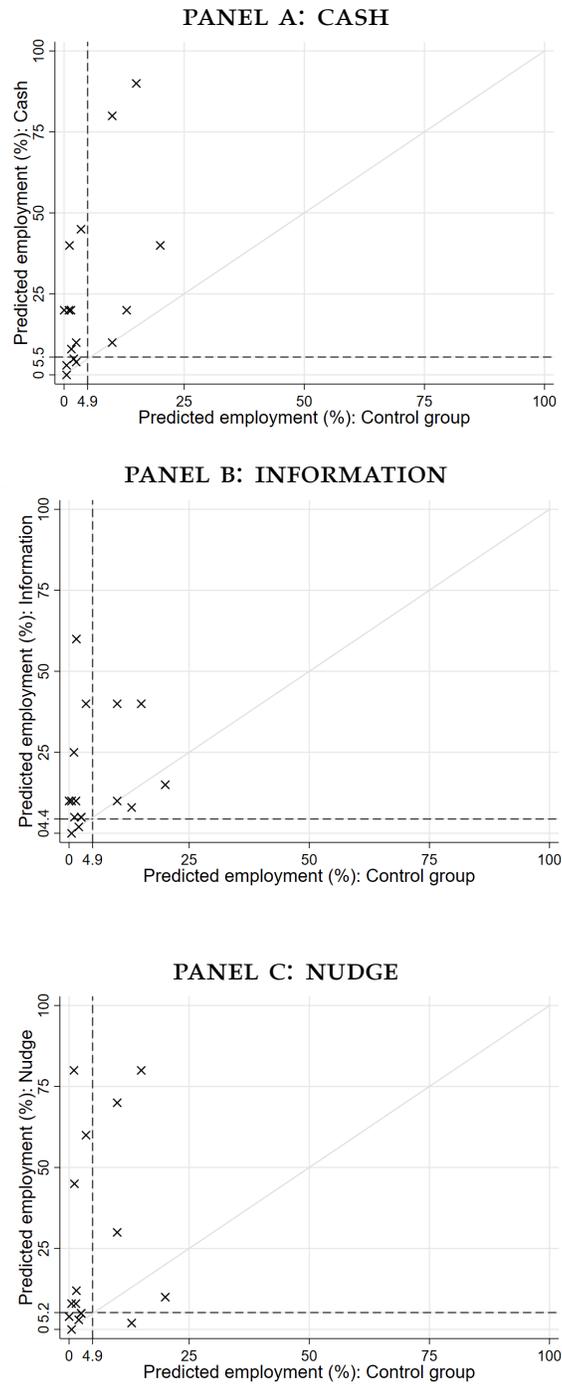
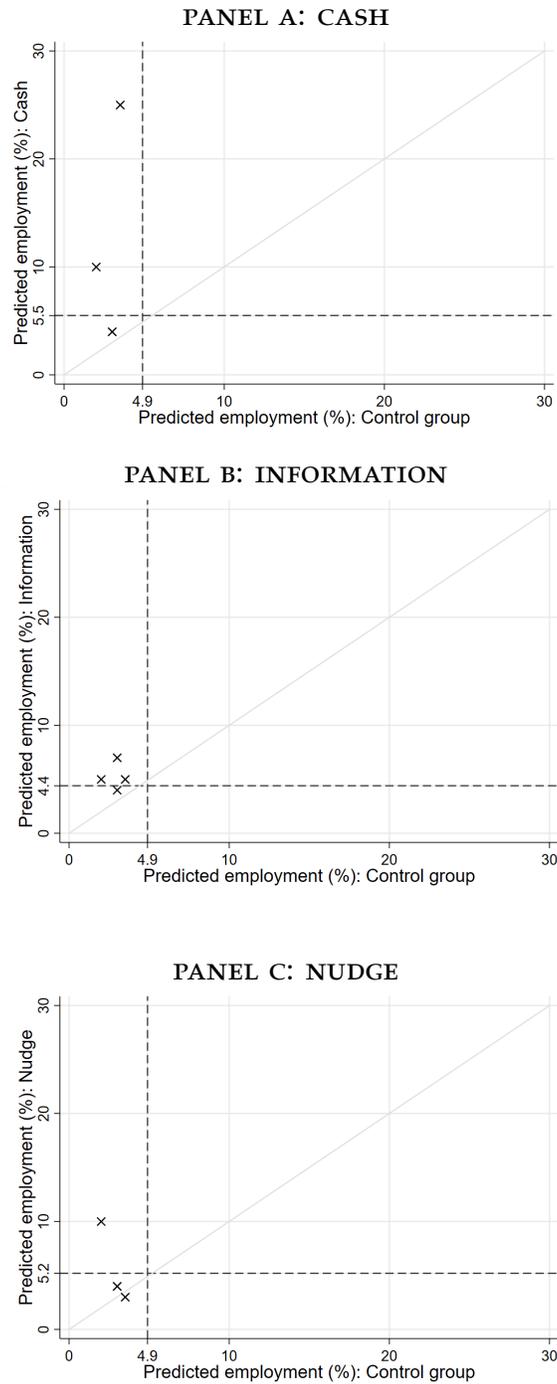


Figure A.17: Forecast employment outcomes: Local staff



Note: These scatterplots show IRC employees' incentivized forecasts of six-week employment rates under each of the three treatment arms; for each plot, we graph against the incentivized forecast of the six-week rate for the control group. On each plot, we superimpose the weighted average employment rates from Table A.9.

Figure A.18: Forecast employment outcomes: Head-office staff



Note: These scatterplots show IRC employees' incentivized forecasts of six-week employment rates under each of the three treatment arms; for each plot, we graph against the incentivized forecast of the six-week rate for the control group. On each plot, we superimpose the weighted average employment rates from Table A.9.

References

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. *Journal of the American statistical Association* 103(484), 1481–1495.