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

Stefano Caria,[†] Grant Gordon,[‡] Maximilian Kasy,[§] Simon Quinn,[¶] Soha Shami,[||] Alexander Teytelboym[**]

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Abstract

We introduce a novel methodology for adaptive targeted 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 at different groups. We implement our methodology in a field experiment. We examine the impact of three interventions designed to tackle credit constraints, information frictions and self-control challenges on formal employment outcomes of Syrian refugees and local jobseekers in Jordan. Six weeks after treatment, we find that treatments have had minimal effect on formal employment of refugees or locals. In the next draft of this paper, we will analyze longer-term employment and well-being outcomes and discuss further applications of adaptive targeted field experiments in economic development.

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†Department of Economics, University of Warwick: stefano.caria@warwick.ac.uk.

‡International Rescue Committee: Grant.Gordon@rescue.org.

§Department of Economics, University of Oxford: maximilian.kasy@economics.ox.ac.uk.

¶Department of Economics, University of Oxford: simon.quinn@economics.ox.ac.uk.

||Danish Refugee Council: soha.s.shami@gmail.com. At the time that we ran the experiment described in this paper, Soha worked at the International Rescue Committee.

**Department of Economics, University of Oxford: alexander.teytelboym@economics.ox.ac.uk.

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.

Our first contribution is to introduce a methodology for adaptive targeted experimentation that balances competing goals of precise treatment effect estimation 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. 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 have had minimal impact on six-week employment outcomes of jobseekers. We also find that there have been modest gains from targeting.

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 (Scott, 2010). 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 *sub*optimal, 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.

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. 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 individual from stratum x at time t. Then, at time t, the Tempered Thompson Algorithm assigns treatments in the following way:

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 ensures that the designer is able to target participant welfare while ensuring that they can learn something about the effectiveness of suboptimal treatments. Our main theoretical result (Theorem 1) formally establishes a tradeoff 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 decreases.

The second key feature of our methodology is that our adaptive assignment algorithm is targeted. We implement our algorithm within a hierarchical Bayesian model described by Gelman et al. (2014). The model allows us to learn the extent of effect heterogeneity across different, pre-defined strata. The data-generating process for the binary outcome of a treatment in a particular stratum is governed by a parameter. For a given treatment, these parameters come from a common prior distribution for all strata. The hyper-parameters governing the common prior distribution are assumed to come from a diffuse hyper-prior distribution. In every period, the experimenter observes treatment success rates for existing experimental participants across all strata allowing her to learn the hyper-parameters. She can then combine the estimate of the hyper-parameters with the observed success rate in a given stratum in order to calculate the posterior distribution of the success parameter in that stratum. Finally, these posterior distributions can be used to calculate the probability $\hat{p}_t^{dx}$ that a given treatment is optimal for a given stratum. These probabilities are then used in the Tempered Thompson Algorithm.

Implementation and Results We implement our methodology in a field experiment designed to help Syrian refugees and local jobseekers in Jordan find formal wage work. The field experiment tests three types of support: cash transfer, information provision, and psychological support. These types of support correspond to three barriers—material, informational, and behavioral—that refugees and locals might face in finding and retaining jobs. The program was implemented in Jordan by the International Rescue Committee at the height of the Syrian refugee crisis. Jordan is a 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 in Jordan. In Jordan, an estimated 63% of refugees are unemployed and over 90% of Syrian refugees live below the national poverty line (Verme et al., 2015). The massive influx of unemployed, impoverished refugees into Jordan mirrors the type of displacement shock countries often experience. Second, and in response to the displacement crisis, the international community and Government of Jordan launched the Jordan Compact, the legal framework for refugees to access those jobs. In exchange for preferential access to the European market and access to conditional financing, the Government of Jordan agreed to provide 200,000 work permits for refugees. The Jordan Compact has influenced refugee policy around the world and similar compacts are being launched in other countries, for example Ethiopia. Jordan thus provided an opportune context to understand how to connect refugees to the new employment opportunities that are opening for them. Ours is the first field experiment to study the employment of refugees in a development context.

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} × {Female, Male} × {High school, No high school} × {Never employed, Ever employed}. Six weeks after joining the program and being offered treatment, we find that none of the interventions had significant or meaningful impact on the probability that individuals were in formal wage employment (the primary outcome that we specified in our pre-analysis plan). In the next draft of this paper, we will discuss the possible reasons for these null results in light of the analysis of evidence on longer-term employment and on other socioeconomic outcomes.

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 "probability matching" algorithm due to (Thompson, 1933). Recently, a number of papers have shown that the Thompson algorithm asymptotically matches the welfare under the optimal dynamic treatment assignment policy (Agrawal and Goyal, 2012; Kaufmann et al., 2012; Agrawal and Goyal, 2013). We contribute to a growing number of papers in economics using adaptive experimental methods (Kasy and Sautmann, 2019; Kasy and Teytelboym, 2020a,b). There is also a recent literature within economics on targeted treatment assignment both from a non-Bayesian (e.g., Kitagawa and Tetenov (2018); Wager and Athey (2018)) and Bayesian perspectives (e.g., Dehejia (2005); Chamberlain (2011); Kasy (2018)).

We also contribute to 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.[1] 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., 2016b, 2015), 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

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

transfers have been found to increase short-term employment through increasing job search (Franklin, 2018; Abebe et al., 2020; Banerjee and Sequeira, 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. Previous literature tends to focus on young nationals with poor attachment to the labour market (see, for example, Kluve 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. 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 sets 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 derives its theoretical properties. Section 4 presents the 6-week follow-ups survey results. Results from the one-month, three-month, and six-month surveys be reported in a subsequent draft of this paper. Section 5 is a conclusion. 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. The Online Appendix contains treatment materials used in the field as well as additional tables and figures.

What is still missing in this preliminary version of the paper The following sections are still under preparation and will be available in the next draft:

- Evidence on shorter- and longer-run outcomes from one-month, three-month, and six-month surveys. It is possible that the null short term results indicate that there are also weak long term effects of the interventions. However, it is also possible that impacts grow with time. Given that we are uncertain on the likely evolution of impacts over time, we think that it is important to wait until this analysis is ready to formulate specific policy recommendations based on our results.
- Policy recommendations and a discussion of the usefulness of thee Tempered Thomp-

son Algorithm for other field experiments.

- Qualitative evidence from focus groups.
- Experts' beliefs elicitation prior to the experiment.

2 Context, sampling and treatments

The world is facing the largest refugee crisis since World War II, with over 70 million individuals displaced, about 25 million of whom are refugees (UNHCR, 2019a). 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 radical 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 IDPs 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 individuals face a unique set of constraints in accessing employment opportunities. They often lack the information, language skills and social networks needed to effectively navigate labor markets. 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, 2020). 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, 2020). Eight years into the conflict, Syrian refugees in Jordan face important needs for humanitarian assistance, for basic services, and for economic stability. Today, it is estimated that 93% of Syrian refugees in the country live below the US$5 per day poverty line. 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, 2020).

In 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 drawing Syrian refugees into the labor market. Among these changes are (IRC, 2017):

1. Easing administrative procedures to allow Syrian refugees to apply for work permits in the sectors open to employing them, namely manufacturing, agriculture, and construction – with a goal of providing work permits for up to 200,000 Syrian refugees;

2. Designating and developing five industrial zones, later called the Special Economic Zones (SEZs), that would be provided with maximum investment and trade incentives under the new investment law;

3. Allowing Syrian refugees to formalize existing businesses and to set up new businesses; and

4. Providing a small percentage of contractual Syrian employment opportunities in municipal works.

The impetus for this breakthrough agreement was that policies that eased access to European markets were expected to lead to higher demand for Jordanian exports, which in turn would create new jobs and boost formal employment for both refugees and Jordanians, mainly in the manufacturing sector and within the SEZs. In short, the Compact aimed to turn 'the Syrian refugee crisis into a development opportunity' (Government of Jordan, 2016).

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.[2] This very low average masks significant heterogeneity by gender. Among males, labor force participation is close to 59 percent, while among females it drops to 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 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. According to the JLMPS figures, the employment rate among Syrian refugees stood at 14 percent. Among women refugees, the employment rate dropped to 2 percent. This employment was often informal and median

[2] 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.

monthly salaries were below the national minimum wage.[3] These figures are broadly consistent with the number of work permits issued under the Jordan compact. Of the targeted 200,000 work permits to be issued to Syrian refugees by 2020, 159,000 had been issued as of the end of 2019 (UNHCR, 2019b). However, this figure includes permits for jobs that have been terminated; it is likely that active permits are a much lower number. For example, according to some estimates, about 40,000 permits were active in May 2017 (out of a refugee population of more than 600,000) (DSP and Columbia, 2020).

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.[4] 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 established and employed in large numbers in many of the low-paying jobs that were opened to Syrians as part of the 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 economic, which could drive decisions to work informally or not work at all. Third, they may experience substantial self-control problems when it comes to searching for work, possibly resulting from the psychological pressures 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

[3] 75 percent of 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 JOD, while the formal minimum wage was 200 JOD.

[4] In particular, Article 12 of the Jordanian Labor Law identifies three violations to employing Syrian refugees: "(i) employing a non-Jordanian without a work permit; (ii) a non-Jordanian working for an employer other than one approved by the Ministry of Labour; and (iii) a non-Jordanian working in a profession other than the one approved by the Ministry of Labour" (Amjad et al., 2017).

exhausting, exploitative, and potentially exposing to risk (Amjad et al., 2017; Razzaz, 2017).

2.3 Sampling Syrian and Jordanian job-seekers

Our study sample enrolled in the IRC's 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 (220 JODs per month) in the immediate future. We verified that the participants met these requirements and further collected information on participants for the research during the intake registration interviews. At the end of the interview, participants were then 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 to 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 way Syrians and Jordanians were sampled. For both Syrians and Jordanians, the largest share of enrolments came from referrals, a passive

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
Educaton (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
Sample size	3770	1663	2107

sampling method. The second largest source of participants for both nationalities was enrollment 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 Jordanian 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 B.1 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 enrolled in Project Match through active methods. In subsequent months, this was largely reversed. We illustrate these patterns in Figure B.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 battery 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 much less educated on average (7 years vs 12 years).

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); gender (a dummy for being 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 will form the basis of our targeting strategy, discussed in the next section. In Figure B.5 of the Online Appendix, we show the distribution of observations across strata. While for most cells we have good sample sizes, we tend to have a small proportion of people, especially Syrians, that have some education beyond high school.

An important point to stress is that many individuals in our sample, including the refugees, are actively looking for work; about 40 percent of refugees in the control group are doing so at the time of our one-month follow-up interview. In the next draft of the paper, we will provide a comprehensive description of job search among refugees.

2.5 Treatments

On the basis of these key features, and working closely with local experts at the International Rescue Committee in Amman, we designed three separate job search interventions.[5] Each intervention was designed to represent a distinct form of job search assistance, each having support in recent empirical literature.[6] Search interventions are aimed at facilitating the job search and thereby, increasing job search intensity to improve chances of participants finding work. These interventions will be denoted by $D \in \{0, 1, 2, 3\}$ where 0 refers to respondents assigned to a control group; the three search interventions respectively provide cash, information, and psychological support. In addition to these treatments, all respondents received 4 Jordanian dinars ('JOD': about US$5.60 USD at the time of the intervention)

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

[6] Some respondents were also assigned to one of two separate 'direct placement' arms; this is the focus of a separate paper.

to cover possible costs of transport to a job interview, and an informational flyer covering steps for interview preparation.[7]

Control group: The control group received the 4 JODs and informational flyer that were offered to everyone upon registration with Project Match. Additionally, the received continuous case management conducted by trained employment service officers (ESOs) over the course of six months. During the follow-up calls, ESOs collected information for research purposes and they also responded to job-related concerns whenever possible.

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). 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 there would be no enforcement of whether the cash was actually used in this way (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 receive an SMS notification. They also receive an ATM guide pamphlet with a direct hotline number for reporting issues with cash withdrawal from ATMs.

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.[8]) Specifically, respondents in this treatment

[7] This was done to encourage participants to enrol in Project Match and to partially address potential ethical concerns of randomization by offering a placebo to the control group.

[8] 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.

received 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 (ESO), 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 B.1 and B.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: Psychological support. The third intervention is psychological support (which we refer to as the 'nudge' intervention). We provide a packaged intervention composed of (i) a four-week job-search planning calendar (see Online Appendix Figure B.3), (ii) an instructional video on how to use the calendar to plan for the job search, (iii) a face-to-face demonstration delivered by the ESOs, and finally (iv) reminder SMSs. The instructional video begins with a nudging statement of the potential impact of planning on employment from other contexts, 'Did you know that job search planning can increase chances of finding work by up to 25%?'. Additionally, the reminder SMSs are given once at the beginning of the week and once at the end of the week to help respondents overcome self-control problems related to job search. Through the calendar and the SMSs, participants track the number of jobs and search hours they intend to apply for and spend respectively and then report back on the number of jobs and hours they actually apply for and spend that week. This intervention is motivated by recent evidence indicating substantial self-control problems and intention-behavior gaps in job search (DellaVigna and Paserman, 2005; Caliendo et al., 2015; Abel et al., 2019).

All interventions were delivered at the end of the intake interview or in the following seven days.

2.6 Follow-up surveys and attrition

We measure the impacts of these interventions through four follow-up surveys, all administered over the phone. We complete in-depth surveys one, three and six months after the baseline interview. We use these surveys to document the impacts of the program on a battery of outcomes specified a registered plan.

We also 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. We use the data from this survey to implement the adaptive randomization design which we describe in the following section.

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.

Our algorithm is Bayesian. We first describe the prior distribution we use. In Appendix A.2, we discuss the Markov Chain Monte Carlo method employed to sample from the posterior corresponding to this prior. We use a hierarchical Bayesian model which allows us to learn the degree of effect heterogeneity across demographic strata from the data. Based on this estimated heterogeneity, we can form optimal estimates of effects within each stratum that combine information within and across strata.

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.[9]

This section concludes with a discussion of inference. Our primary method of inference is Bayesian. We also discuss p-values based on randomization inference, as a secondary method. The latter needs to take into account the adaptive and targeted form of treatment assignment in order to be valid.

We use the following notation. Let t denote the day of 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, individual 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

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

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

$$(\alpha^d, \beta^d) \sim \pi, \tag{3}$$

where (α^d, β^d) are the hyper-parameters and π is the hyper-prior (see 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. 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 distributon π.

We assume that parameters $(\alpha^d, \beta^d, \boldsymbol{\theta}^{d\cdot})$ are independent across the treatment arms d. We

[9] At `https://github.com/maxkasy/ThompsonHierarchicalApp`. A corresponding interactive app is available at `https://maxkasy.github.io/home/hierarchicalthompson/`.

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 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 $\boldsymbol{\theta}, \boldsymbol{m}_t, \boldsymbol{r}_t$ 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 hyper-parameters indexed by d. We sample from the posterior distribution of $(\boldsymbol{\theta}, \boldsymbol{\alpha}, \boldsymbol{\beta})$ given $\boldsymbol{m}_{t-1}, \boldsymbol{r}_{t-1}$ 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

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

Equation (A.1) in the appendix shows how to estimate this probability by as 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. This 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 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

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

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

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 $\boldsymbol{\theta}_0$ to denote the fixed, true vector of average potential outcomes from which the data are generated. By contrast, we use $\boldsymbol{\theta}$ 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 $\boldsymbol{\theta}$ concentrates around $\boldsymbol{\theta}_0$.

Assumption 1 (Setup) *Consider a fixed (non-random)* $\boldsymbol{\theta}_0 = (\theta_0^{dx})$. *Suppose that* $d^{*x} = \arg\max_d \theta_0^{dx}$ *is unique for all* $x \in \{1, \ldots, n_x\}$, *and denote* $\Delta^{dx} = \max_d \theta_0^{dx} - \theta_0^{dx}$. *Assume that* $(Y_{it}^1, \ldots, Y_{it}^k, X_{it})$ *is i.i.d. across both* i *and* t, *and that*

$$Y_{it}^d | (X_{it} = x, \boldsymbol{\theta}_0) \sim Ber(\theta_0^{dx}).$$

Assume that $N_t \geq \underline{N}$ *for all* t *and some constant* $\underline{N}$, *and that the prior distribution for* $\boldsymbol{\theta}$ *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* $1, \ldots, t$, *and* p^x *the probability that* $X_{it} = x$.

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* $\boldsymbol{\theta}$*) for* $d = d^{*x}$, *and to* 0 *for all other* d.[10]

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,
   $$Regret_t = \frac{1}{M_t} \sum_{i,t} \Delta^{D_{it} X_{it}}$$
   converges in probability to
   $$\gamma \cdot \frac{1}{k} \sum_{x,d} \Delta^{dx} \cdot p^x.$$

4. *Converging estimator:*

[10] Note that this statement refers to frequentist consistency (given $\boldsymbol{\theta}$) of a Bayesian posterior probability (which averages over $\boldsymbol{\theta}$).

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

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

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 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 wich 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.

3.4 Inference

Our primary 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, in contrast to standard frequentist inference, remains valid for adaptive designs such as ours, since the likelihood function is not affected by adaptivity. In large samples, as long as $\gamma > 0$, 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 holds because the share of observations assigned to each treatment in each stratum is bounded below, asymptotically.

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 $\theta^{dx} = \theta^{d'x}$ for all d, d', x. 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 results from the six-weeks follow-up surveys. These surveys focused on employment status and provided the data for the Thompson algorithm. We also carried out more extensive follow-up interviews with program participants one month, three months and six months after joining the program. We will report results based on these follow-up surveys in a future draft of the paper.[11] We report six-week impacts 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

[11] We will use these data to carry out a comprehensive assessment of the effects of the interventions and the mechanisms behind them. In particular, we are interested to document impacts on five key outcomes — wage employment, earnings, psychological well-being, social integration and migration intentions — and also on a set of mediators related to job search.

control group.[12] Here, we use randomization inference to construct a p-value of the sharp null of no treatment effect. We conclude this section by presenting three 'welfare contrasts' that quantify the overall impact of our interventions, as detailed in Section 4.3 below.

4.1 Employment and treatment impacts on employment after six weeks

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-weeks employment rates, except for somewhat higher rates for those sampled in first month of the experiment. We show this in Figure 1 where we plot employment rate against week of sampling. These averages, however, mask substantial heterogeneity (Table 3). 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.[13] 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 2. 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

[12] 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:

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

where

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

W_{it} is the six weeks employment status 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.

[13] In Table B.4 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: Weighted mean differences in employment, with randomisation inference p-values

Treatment	Success rate	Δ	P-value
Cash		0.006	0.296
Information		-0.005	0.690
Nudge		0.003	0.388
Control	0.049		

Note: The table reports results for wage employment at the time of the six weeks 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 3.4.

Figure 1: Employment rate by week of sampling

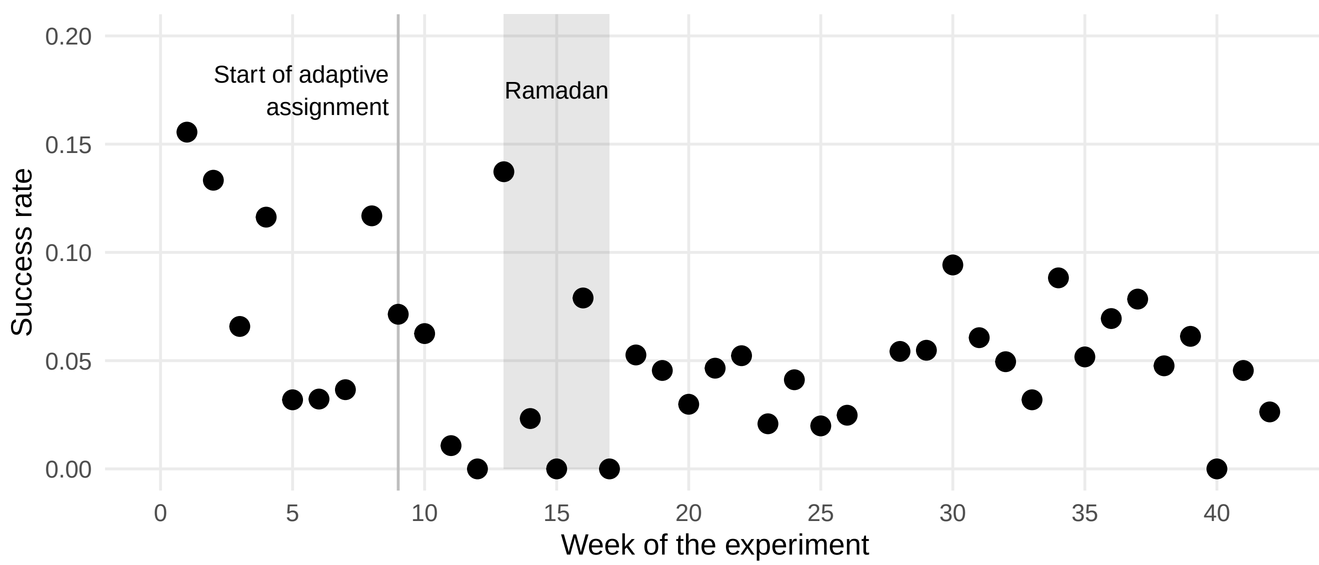


Figure 2.

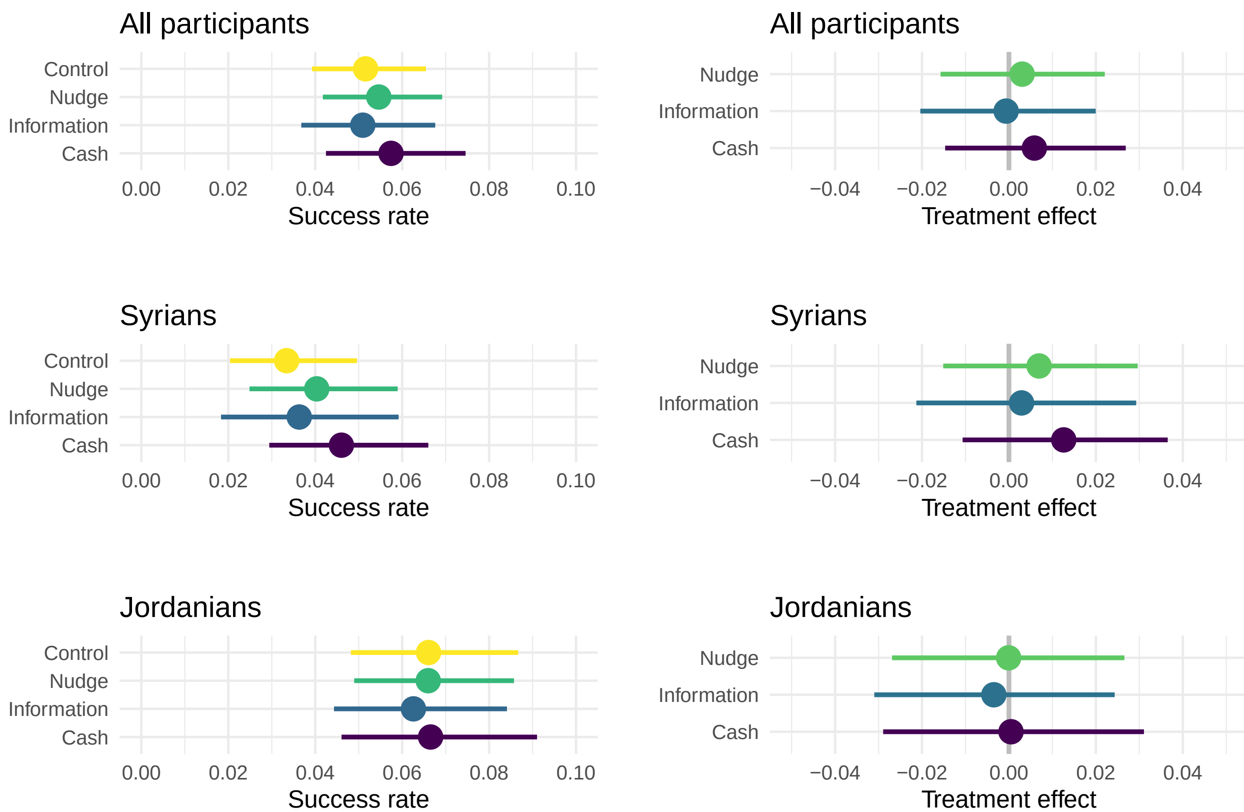
We are equally unable to find evidence of treatment impacts for specific, pre-specified groups of individuals. In Figure 2, for example, we show treatment effects after splitting the sample by nationality and do not find evidence of impacts on employment on either Syrians or Jordanians. Posteriors are somewhat larger for Syrians than for Jordanians, but

Table 3: Weighted mean differences in employment by stratum, 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 six weeks 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 3.4.

Figure 2: Credible sets for average potential outcomes,
and for average treatment effects relative to the control treatment



the credible sets always overlap. We report credible sets for all sixteen strata in Table 4. Further, Table 3 reports differences 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 (p=.123).

There are multiple factors that may explain these null results. First, the demand for refugee labour may be constrained by the bureaucratic hurdles involved in registering for work permits. Further, despite the promise of the Jordan Compact, few firms took advantage of the preferential trade agreement and increased exports to the European market, which in turn meant that the job opportunities for refugees that this agreement generated were fewer than expected. Second, the fact that this intervention focused on placing Syrian refugees and Jordanians into formal employment opportunities may either have constrained the potential impact of the intervention or obscured its effects on informal income-generating activities. The fact that refugees may avoid formal opportunities that require their status to be made legible, and reported to, the state, may have deterred them from accessing these opportunities. Third, the impacts of these interventions may not be visible in the six weeks time frame that we report in the current draft. We will investigate impacts on a longer-term employment and on other socioeconomic outcome in a future draft of the paper.

4.2 Performance of Tempered Thompson Algorithm

Consistently with the results presented above, 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 3. 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 psychological intervention. However, this was slowly reversed after the 20th week of the study.

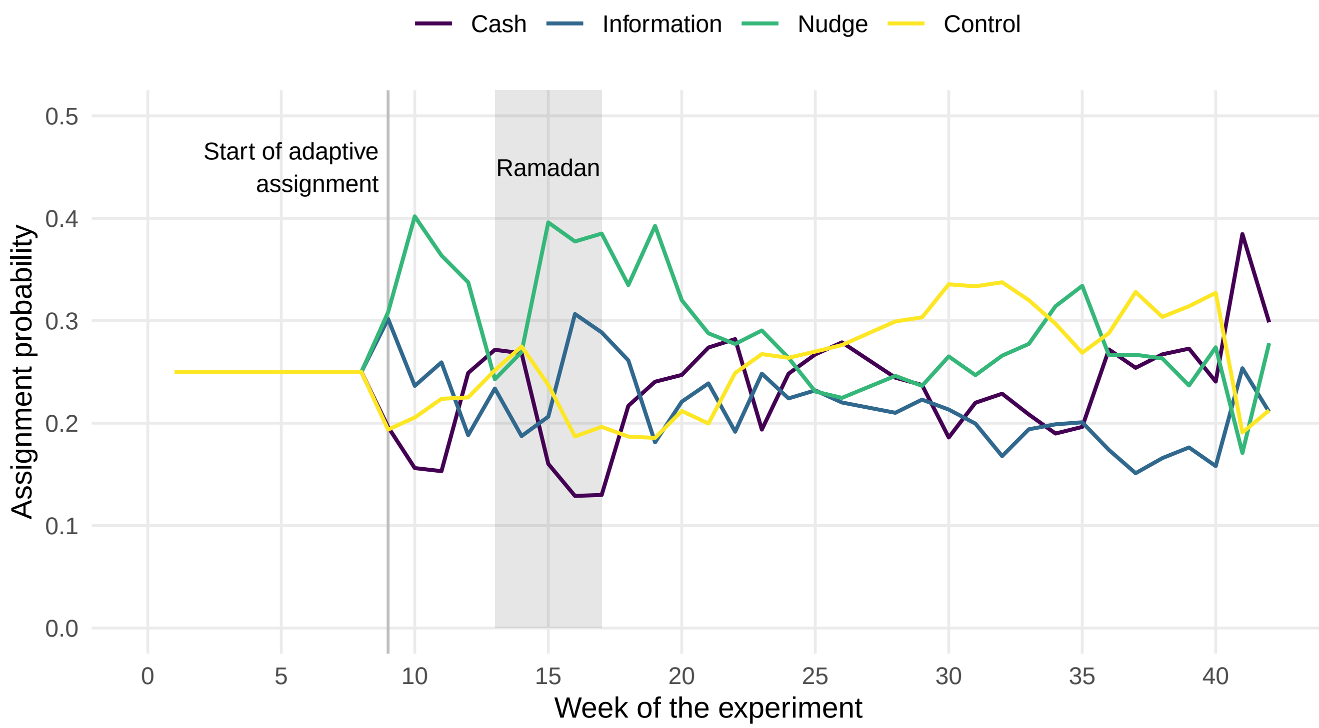
The algorithm's departure from equal-proportions randomisation is somewhat more pronounced for specific strata. We show this in Figure 4, where show strata-specific weekly treatment assignment probabilities, and in Table 5, where we show, for each treatment, the posterior probability that employment 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, uneducated 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 randomizaton are limited in this context.

Table 4: 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 six weeks follow-up interview.

Figure 3: Assignment probabilities by week



4.3 Welfare contrasts

We conclude by presenting 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*,

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

This estimate 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

Figure 4: Assignment probabilities by stratum and by week

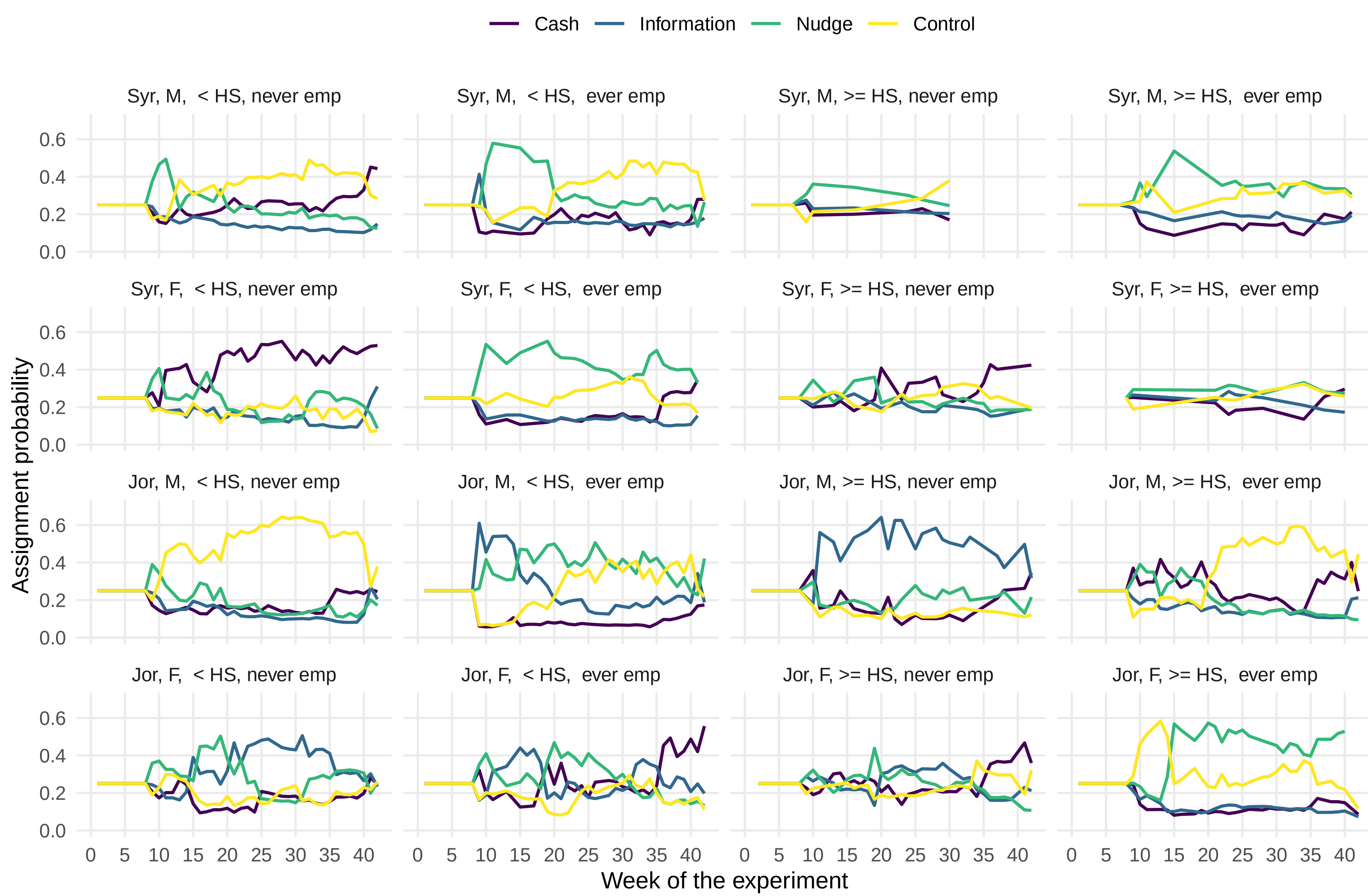


Table 5: Probability treatment is optimal, by stratum

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

default of *no intervention* (treatment 0),

$$\Delta_2 = \sum_x \left(\max_d \hat{E}\left[\theta^{dx}\right] - \hat{E}\left[\theta^{0x}\right] \right) p^x,$$
$$\Delta_3 = \max_d \sum_x \left(\hat{E}\left[\theta^{dx}\right] - \hat{E}\left[\theta^{0x}\right] \right) p^x.$$

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 six-weeks employment are small; Table 6 reports our

Table 6: Welfare contrasts

	Estimate	95% Credible set
Δ_1	.002	(0.000,0.004)
Δ_2	.017	(0.001,0.034)
Δ_3	.006	(-0.015,0.027)

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. 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).

Second, we document that, in our study, adaptive randomization does not generate any six-weeks employment gains over standard randomization. We show this by reporting welfare contract Δ_1, in Table 6, which is very close to zero.

5 Conclusion

Our methodology proved to be straightforward to implement in the field and 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 the employment program that we study, many settings in development fall into our setting, including drug and vaccination programs, agricultural technology adoption programs, emergency relief programs, and so on.

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Appendix

A.1 Proofs

A.1.1 Preliminaries

Our characterization of the large sample properties of our γ-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 $\{\mathscr{H}_n\}$, and define $Z_n = P(W_n = 1|\mathscr{H}_{n-1})$. If, conditioned on $\mathscr{H}_{n-1}$, each Y_n is independent of W_n, then with probability 1,*

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

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

A.1.2 Proof of Theorem 1

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

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

where E_t denotes the conditional expectation given observations up to wave $t-1$, and conditional on $\boldsymbol{\theta}$. 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 $\sum_{i,t' \leq t} Z_{it'} \to \infty$ as $t \to \infty$. Applying Lemma 1 to the first fraction, and a standard law of large numbers to the inverse of the second fraction, we get that

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

in probability as $t \to \infty$.

1. Given the assumed uniqueness of d^{*x}, there exists an ϵ-neighborhood of $\boldsymbol{\theta}_0$ 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 $t \to \infty$.

   Given our assumption that the prior for $\boldsymbol{\theta}$ 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 $\boldsymbol{\theta} = \boldsymbol{\theta}_0$ against the alternative that $\|\boldsymbol{\theta} - \boldsymbol{\theta}_0\| > \epsilon$.

   In our setting such a test can be constructed by setting

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

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

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 $t \to \infty$.

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

3. By definition,

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

   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), where the necessary conditions of their Theorem 3.2 are verified by our item 2.

□

A.2 Markov Chain Monte Carlo

Denote by $\boldsymbol{\theta}, \boldsymbol{m}_t, \boldsymbol{r}_t$ 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 hyper-parameters 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 $\boldsymbol{m}_{t-1}, \boldsymbol{r}_{t-1}$ 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 $\boldsymbol{m}_t, \boldsymbol{r}_t$. 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, is given by

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

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.

Algorithm 1 Markov Chain Monte Carlo for the hierarchical Bayes model

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Require: The cumulated assignment frequencies m^{dx} and success numbers r^{dx}.
Starting values $\boldsymbol{\alpha}_0, \boldsymbol{\beta}_0$, length of the burn in period B, and number of draws R.

1: **for** $\rho = 1$ **to** $B + R$ **do**

2: Gibbs step:
Given $\boldsymbol{\alpha}_{\rho-1}$ and $\boldsymbol{\beta}_{\rho-1}$, 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 $\boldsymbol{\beta}_{\rho-1}$ and $\boldsymbol{\theta}_\rho$, 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 $\boldsymbol{\beta}_{\rho-1}$ given $\boldsymbol{\theta}_\rho$ and $\boldsymbol{\alpha}_{\rho-1}$.

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

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