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Motivating Job Seekers: A Field Experiment

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Motivating Job Seekers: A Field Experiment*

Abstract

Reduced motivation over the unemployment spell may contribute to declining job-finding rates. We report findings from a low-cost digital intervention that uses motivational emails to enhance job seekers' search effort. Using a randomized controlled trial that included 200,720 newly unemployed job seekers in Sweden, we evaluate both supportive messages aimed at encouraging inner motivation, and controlling messages focused on internal and external pressure. The messages increased compliance with mandatory activity reporting, indicating a clear first-stage behavioral response. However, this behavioral change did not translate in more effective job search as both types of emails caused a moderate but significant reduction in search effort and job finding. The supportive messages reduced the number of job applications and showed weakly negative job-finding effects, particularly among men. The controlling messages slightly backfired for job seekers who report strong autonomous motivation at the onset of unemployment. These findings underscore the challenges of motivating job seekers to actively search for jobs and suggest that low-cost digital interventions, in isolation, are inadequate and may even be counterproductive.

JEL classification

A12, D01, D91, J64, J68

Keywords

autonomous and controlled motivation, job search, supportive and controlling, large-scale RCT, low-cost intervention, field experiment, backfiring, self-determination theory

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1 Introduction

Job-finding rates decline as unemployment duration increases. Two main explanations have been proposed: *selection effects*, whereby job seekers with lower employment prospects remain unemployed longer, and “*true*” *duration dependence*, in which an individual’s probability of finding a job decreases over time. While some recent studies identify selection effects as the primary driver (Ahn & Hamilton, 2020; Mueller & Spinnewijn, 2025), others highlight declining search effort as key (Kroft et al. 2016; Faberman & Kudlyak, 2019; Alvarez et al., 2024; Lalive et al., 2025). For instance, Lalive et al. (2025), using search diary data, document a strong within-individual decline of around 30% in number of job applications submitted during the first year of unemployment. One likely contributing factor is that job seekers lose motivation to continue searching.¹ Specifically, since searching for a job is often a painful process, marked by failed applications and lost social connections, it is challenging to maintain the motivation required to sustain active job search (Krueger & Mueller, 2010, 2011; Faberman & Kudlyak, 2019; Marinescu & Skandalis, 2021; Della Vigna et al., 2022).

Thus, a central public policy question is how to effectively strengthen job seekers’ motivation to achieve sustained search effort. To examine this, we designed a low-cost intervention as part of a large-scale randomized controlled trial (RCT) involving approximately two-thirds of all entrants into the Swedish unemployment insurance (UI) scheme over a period of 20 months (May 2020–December 2021). The intervention consisted of two types of motivational emails, each comprising six messages sent to job seekers over a six-month period. To estimate the effect of the messages, we compared outcomes with those of a control group that received no emails. The two email interventions were designed to address the following questions. Are job seekers more motivated to exert job-search effort if they receive *supportive* emails that foster job seekers’ inner drive to search for jobs? Or are they more motivated by *controlling* emails that emphasize constraints and the threat of sanctions?

These questions relate to the longstanding debate about whether “carrots” or “sticks” are more effective in helping unemployed individuals return to work (e.g., van der Klaauw & van Ours, 2013; Arni et al., 2020). Broadly speaking, “carrots” seek to encourage job search through more supportive means, either by providing explicit monetary rewards (e.g.,

¹ Employers discriminating against the long-term unemployed is another contributing factor. For instance, several correspondence studies have examined differences in callback rates between short- and long-term unemployed individuals using fictitious résumés (e.g., Kroft et al., 2013; Eriksson & Rooth, 2014; Farber et al., 2016).

Woodbury & Spiegelman, 1987; Card & Hyslop, 2005; O’Leary et al., 2005) or through interventions such as advice, information, or job-search assistance that support and encourage job seekers without offering direct financial incentives (e.g., Altmann et al., 2018; Belot et al., 2019; Crépon et al., 2013; Cheung et al., 2025). By contrast, “sticks” refer to measures that constrain individual behavior through monitoring, (the threat of) sanctions, or other behavioral restrictions.

Whether carrots or sticks are more effective remains an open question. While many studies in economics find that monitoring and the threat of sanctions substantially increase transitions into employment (e.g., Klepinger et al., 2002; Black et al., 2003; van den Berg et al., 2004; Abbring et al., 2005; Mc Vicar, 2008, 2010; van der Klaauw and van Ours, 2013; Arni & Schiprowski, 2019; van den Berg & Van der Klaauw, 2019),² a substantial body of research in psychology questions whether highly controlling policies are the most effective means of sustaining job seekers’ motivation (da Motta Veiga & Gabriel, 2016; Deci et al., 2017; Koen et al., 2015; 2016; van der Vaart et al., 2020; Vansteenkiste et al., 2004; 2005).³ In addition, the economics literature provides evidence that a wide range of supportive interventions improve employment outcomes (see, e.g., Card et al., 2018; Arni et al., 2020). However, relatively few studies have directly compared the effectiveness of carrots and sticks. Moreover, in this comparison it is hard to separate the impact of the motivational cue from that of actual program participation.⁴ The present study contributes to this debate by examining the relative effectiveness of carrot-based or stick-based policies.

To operationalize our two email interventions, we rely on Self-Determination Theory (SDT), a well-established psychological framework for understanding human motivation (Deci & Ryan, 1985; 2000; 2008; 2012). SDT distinguishes between autonomous and controlled motivation. Individuals are autonomously motivated when they self-endorse their actions, either (i) because they find them inherently interesting or enjoyable (intrinsic motivation), or (ii) because they find them meaningful, personally relevant, or in line with their personal goals. By contrast, controlled motivation arises when individuals feel pressured to behave in a particular way, either because of external pressure, such as demands or punishments, or

² The paper of Ashenfelter et al. (2005), documenting insignificant effects, is a notable exception.

³ The behavioral economics literature has also recognized that extrinsic incentives may backfire (Fehr & Falk, 1999; Gneezy & Rustichini, 2000a, b). Casar & Meier (2018) review this literature and its relation to Self-Determination Theory, discussed below.

⁴ Arni et al. (2020) is the only study that successfully separates these two effects.

because of internal pressures, such as guilt, shame or feelings of obligation. Accordingly, our supportive messages sought to foster autonomous motivation by supporting three basic psychological needs: autonomy, competence, and relatedness. In contrast, the controlling messages sought to stimulate controlled motivation by increasing perceived internal and external pressures.

This provides two coherent motivational approaches that map naturally onto the broader debate over the effectiveness of “carrot” and “stick” policies. Our *controlling messages* resemble traditional “stick” policies insofar as they increase pressure on job seekers in a manner similar to stricter work-search requirements, albeit without reinforcement of monetary sanctions. Our *supportive messages* are more closely related to carrot policies that emphasize support and assistance than to those that rely on explicit financial rewards. One aim is to address the discouragement often experienced by unemployed individuals. For instance, one of the supportive messages acknowledges the frustration and loss of confidence that can result from repeated job application rejections. We acknowledge that other forms of positive encouragement are also possible, but one advantage of our messages is that they are grounded in explicit and well-established theories of motivation and behavioral change.

In accordance with our registered pre-analysis plan (AEA RCT Registration, trial no. 5502), we use register and survey data to evaluate the impact of these messages on outcomes capturing (i) changes in self-perceived motivation, (ii) behavioral changes (e.g., search effort and search quality), and (iii) labor market results (e.g., job finding and job quality). We also examine heterogeneous effects by initial motivation, gender, local unemployment rate, and labor market attachment.

We initially document that a large share (72%) of job seekers opened our motivational emails, which indicates that the two treatment groups were exposed to the intervention messages. We next show that the messages triggered a notable first-stage behavioral response: job seekers responded to both messages by increasing their monthly mandatory activity reporting, which is a requirement to remain eligible for unemployment benefits. However, this increase in reporting did not translate into more effective job search. Rather, both types of messages appear to have backfired, as they led to a moderate but significant reduction in job finding for two specific groups.

First, male job seekers appear to interpret the soft tone in the supportive messages as a signal that the already controlling environment of the Swedish Public Employment Service (PES) has become less strict, leading to a moderate decline in search effort and job finding. In contrast, women do not exhibit any measurable change in search effort. These novel results

suggest that attempts to stimulate the pursuit of personal goals and autonomous motivation can have unintended negative consequences. The *Relational Incentives Theory* of Gallus et al. (2022) helps us understand this effect. It distinguishes between the *means* of an incentive (what is used as an incentive—here, motivational emails) and its *scheme* (how the incentive is used in the sender-recipient relationship). It further postulates that a scheme-relationship mismatch can weaken engagement (here, job search). In our setting, the autonomy-supportive messages may have been perceived as incongruent with job seekers’ expectations of an otherwise highly controlling PES–client relationship. This creates a scheme-relationship mismatch that may explain the lower search effort and job-finding rates observed following the supportive emails.

A second backfiring effect is that the controlling messages slightly reduce job finding among individuals who, at the onset of unemployment, reported that they were motivated by an inner drive rather than by external constraints. This is consistent with similar crowding-out effects on inner motivation observed in other contexts (Fehr & Falk, 1999; Gneezy & Rustichini, 2000a, b; Huffman & Boganno, 2018; Herz & Zihlmann, 2024). To our knowledge, this is the first study to provide evidence that increased control can undermine motivation during job search.

We make three overall contributions to the literature. First, to our knowledge, no study has examined whether different types of motivation can be induced through low-cost interventions in a job-search context. Second, we employ a large-scale RCT, which strengthens the external validity of our findings (List, 2020). Third, our study contributes to the ongoing debate about the relative effectiveness of supportive or controlling policies by focusing on the role of motivation, independent of external (monetary) incentives and actual program participation effects.

Several recent studies have examined job search from a behavioral economics perspective. Examples include studies on the impact of over-optimism (e.g., Spinnewijn, 2015; Mueller et al., 2021), locus of control (Caliendo et al., 2015), hyperbolic time preferences (e.g., DellaVigna & Paserman, 2005), reference dependent preferences (DellaVigna et al. 2017, 2022), self-regulation (Berger et al., 2022), and information and motivation (Altmann et al.,

2018; Verlaat et al., 2023).^{5,6} Our contribution relates specifically to the last class of studies and lies in the explicit focus on motivational strategies to induce behavioral change.

There is also a growing literature on low-cost digital interventions aimed at changing job seekers' behavior through email or platform-based information and support (e.g., Altmann et al., 2018, Barr & Turner, 2018; Belot et al., 2019; and for surveys Kircher, 2022; Haaland et al., 2023; Le Barbanchon et al., 2024). More specifically, our study relates to the literature examining whether low-cost electronic messages can increase the take-up of employment services (Darling et al. 2017; Briscese et al., 2022; Hopkins & Dorion, 2024; Schimpf et al., 2025) and training programs (Leduc & Tojerow, 2025; Lehner & Schwartz, 2026), to lift the enrolment in postsecondary education and (Barr & Turner, 2018), to correctly navigate rules and regulations (Cairo & Mahlstedt, 2023), and to change application strategies (Behaghel et al., 2024; Belot et al., 2025).⁷ However, most of these studies use emails to address cognitive and behavioral limitations in information processing related to specific well-defined actions, such as participation in training. The underlying idea is that these actions, in turn, should improve job-finding outcomes. Our approach differs in that it aims to promote job search motivation in general without any focus on a specific activity. Moreover, our messages are designed on the basis of a well-established psychological theory of human motivation.

Three studies are more closely linked to our research. Altmann et al. (2018) evaluated an intervention in which treated job seekers received a brochure that not only informed about job-search strategies and consequences of unemployment but also intended to encourage job seekers to actively search for a job by using a supportive tone, notably by acknowledging that frustration caused by repeated application rejections is normal and by highlighting the non-pecuniary benefits of employment. They find modest positive effects on employment. In contrast to our approach, their intervention had a broader (and thus less precise) scope—as they

⁵ Berger et al. (2022) study the role of self-regulation in job search, but, since self-regulation—the ability to set and commit to goals and to regulate effort, emotions, and attention to strive effectively for these goals—constitutes a different behavioral dimension than autonomous and controlled motivation, our paper makes a unique contribution.

⁶ While self-regulation involves setting a goal directly related to the outcome of primary interest, i.e., finding a job, Bjorvatn et al. (2021) show that setting goals indirectly related to job finding, such as sleep, activity and substance use, can also enhance job finding.

⁷ Application strategies are mostly influenced through platform-based recommender systems (E.g., Belot et al., 2019; Altmann et al., 2022; Bächli, et al., 2025; Le Barbanchon et al., 2026; Belot et al., 2026; Bied et al., 2026).

cannot disentangle the roles of information and encouragement. Additionally, their messages were sent by regular mail and dispatched by the research team. The fact that in our experiment the emails were sent by the Swedish PES, reputed to be highly controlling, may explain why our supportive messages did not improve employment.

Cairo & Mahlstedt (2023) study the effects of (1) personalized and (2) generic information about the work requirements imposed on welfare recipients. While their messages are not conceived to promote motivation, their findings are related to our findings. They find that the generic information treatment reduces employment among welfare recipients with a low personal risk of being sanctioned for non-compliance. The information appears to reassure that they fear little risk of sanctions if they reduce their labor supply. This is similar to the reaction we observe to our supportive messages: they also signal a low risk that the PES will sanction in case of reduced search effort.

Verlaet et al. (2023) examine an intervention that exempted welfare recipients from job-search requirements and benefit sanctions and that was, similar to ours, explicitly guided by SDT. Recipients were also told that the explicit aim was to boost autonomy. They find positive effects on employment. A key difference is that their “autonomy” treatment changes the formal requirements by removing the threat of sanctions, whereas our intervention focuses on stimulating motivation without changing any formal requirements. Our RCT also provides evidence at a much larger scale and isolates the effect of autonomy-supportive versus controlling communication.

Our paper also relates to empirical papers based on standard job search theory, which links motivation to extrinsic monetary rewards and monitoring/penalties. The empirical literature in this tradition has mainly focused on various types of control and monitoring. Several studies have shown that enforcing job search effort through sanctions, monitoring and/or job-search requirements increases job finding⁸but negatively affects job quality (e.g., Mc Vicar 2008, 2010; Manning, 2009; Petrongolo, 2009; van den Berg & Vikström 2014; Arni & Schiprowski, 2019). Yet, monitoring always entails a threat of sanctions, leaving open the question of how control functions in their absence. Additionally, these studies are silent on the impact of autonomous motivation and only some of these studies are based on RCTs. While this literature mainly focuses on more controlling policies, some studies have compared different types of policies. Arni et al. (2020) find that both supportive and restrictive policies

⁸ Cf. the references mentioned above.

increase the exit rate from unemployment, but supportive policies have a greater effect. Behnke et al. (2010a, b) find that caseworkers who apply more pressure on unemployed job seekers increase employment rates and job stability for their clients more than caseworkers who are more cooperative. Unlike these earlier studies, we present evidence from a large-scale RCT.

Another related strand of literature is the psychological literature on SDT. Here, SDT has received extensive empirical support across various research domains, but there is relatively little evidence within the context of job search (Vansteenkiste et al. 2004, 2005; Koen et al. 2015, 2016; da Motta Veiga & Gabriel 2016; van der Vaart et al. 2020). These studies generally find that, compared to controlled motivation, autonomous job-search motivation leads to a better job-search process and improved job seeker well-being. However, there are some inconsistencies in the findings, few studies have examined actual employment outcomes, and the evidence is based on small-scale interventions.

This article is organized as follows. The next section describes the RCT set-up, including the institutional context, randomization design, and intervention. Section 3 outlines the chosen outcome variables, along with the moderators and the motivation measures. Section 4 describes the methods. Section 5 presents and discusses the results, and the final section concludes.

2 The RCT Set-Up

We set up a large-scale RCT in collaboration with the Swedish PES and a leading expert in applied psychology to obtain reliable causal evidence on the impacts of messages that aimed to boost motivation. This provides a unique opportunity to test the effectiveness of using short state-of-the-art messages to activate job seekers.

2.1 Institutional Context

In Sweden, unemployed job seekers aged 20 or older may receive either basic or income-related UI benefits. Eligibility for basic benefits requires registration at a PES office, active job search, availability to work, and a recent work history of at least six months within the past year. The benefit level depends on UI fund membership. Non-members receive a flat-rate basic benefit of SEK 11,220 per month.⁹ Voluntary members of a UI fund (with at least 12 months of documented membership) are entitled to income-related benefits: 80% of previous earnings for

⁹ The average conversion rate in 2020–2021 was 0.097 EUR/SEK. Thus, dividing SEK amounts by ten provides a reasonable approximation of their value in EUR.

the first 200 days of unemployment and 70% for the remaining 100 days, subject to a cap (SEK 26,400 per month during the study period). Benefits are paid for up to 300 days (five days per week). After 300 days, those who remain unemployed transition to the Job and Development Program, which provides welfare benefits for an additional 450 days at 65% of prior earnings, subject to the same minimum and maximum levels as UI.¹⁰

Unemployed job seekers receiving UI benefits in Sweden are subject to monitoring and sanction rules designed to enforce job search effort. Sanctions may be imposed for (1) insufficient job search activity, (2) deliberate delay in re-employment, or (3) self-inflicted unemployment. Violations include missing scheduled meetings, failing to submit required reports, rejecting suitable job offers, or voluntarily leaving employment without valid reason.

To monitor compliance with active job search requirements, job seekers must submit monthly activity reports via a digital form accessible through electronic ID. They select reported activities from a predefined list, including applications for posted vacancies, spontaneous job applications, and job interviews. The report must be electronically signed before it is submitted to the PES. Caseworkers at the PES review the reports to assess whether job seekers have been sufficiently active. If not, they issue a formal notification to the relevant UI fund, which makes the final decision on whether to impose a benefit sanction.

The activity reporting was introduced in 2013, marking a shift to a significantly stricter job-search monitoring-regime. Lombardi (2019) finds that this reform increased job finding rates among men, while women were unaffected. In recent years, the PES has implemented several measures to further strengthen monitoring. First, it uses risk-based algorithms to automatically flag activity reports that are more likely to indicate noncompliance. Second, the reports are reviewed by specialized monitoring staff who focus exclusively on detecting violations and who, hence, do not provide job search support to job seekers. Third, the PES sets specific job search requirements for each job seeker, in the form of a required range for the number of job applications per month, which varies based on the job seeker's profile. All of this indicates that we introduce our intervention into an already rather controlling environment. This is also supported by country comparisons conducted by the OECD. In a comparison from 2022, Sweden ranks as the fifth strictest country out of 38 in terms of the strictness of job-search requirements and monitoring procedures (OECD, 2022). This ranking

¹⁰ Non-UI-members can continue to receive the flat rate during this period.

is based on how frequently job seekers are required to report their job-search activities and how detailed the information they must provide is.

2.2 The Time Period and Randomization Design

The intervention was applied in a field experiment involving two-thirds of all individuals registering as unemployed job seekers at the Swedish PES between May 20, 2020 and December 19, 2021.^{11, 12, 13} For people with multiple unemployment spells, only the first entry of an individual is retained. Messages were sent to this population for up to six months after they entered unemployment, concluding the intervention in June 2022. Among this population, we selected those who are required to search for a job and document their efforts in the monthly activity reports described in Section 2.1, as the intervention is intended to boost the motivation of unemployed individuals who are supposed to actively search for a job.

An important observation related to the COVID-19 crisis, documented by Hensvik *et al.* (2021) and Eliassen (2021), is that in Sweden, after an initial drop, job search effort and the job-finding rates remained at high levels, even surpassing those attained in 2019 in the second half of 2020. This contrasts with the situation in most European countries, which enforced sustained lockdowns and where job search plummeted for an extended period. Thus, external validity is less of a concern, as the experiment was conducted during a relatively normal period.

¹¹ Initially, the experiment started on January 20, 2020, but due to the COVID-19 pandemic, the Swedish PES suspended the requirement for job seekers to report their activities every month. Since the intervention was tied to this reporting, we had to interrupt the experiment until the reporting requirement was reinstated on May 20.

¹² Note that we could not consider the full inflow population. The PES had given the mandate to an internal team to test nudging practices focused on the transition to digital service provision. This team decided to collaborate with two groups of researchers of which we were one. Each research group could use one-third of the population for their intervention, while the remaining third would be shared as control group.

¹³ The timing of the end of the experiment was determined by two main factors: the knowledge that a very large sample size was required to detect statistically significant effects for the low-cost intervention described below, and the willingness of the PES to continue running the experiment. As reported in our pre-analysis plan, we aimed to detect effect sizes of the same order of magnitude as those reported in the study by Altmann *et al.* (2018), who also used a light-touch information intervention to enhance the job finding of unemployed job seekers. Based on extrapolations of historical inflows into unemployment, the power analysis suggested that we should continue our experiment for at least one and a half years, i.e., until the end of 2021, a target that we eventually realized.

Table 1: Summary Statistics and Differences in Background Characteristics by Group:
Research Population

Variable	(1) Contr. Motiv.	(2) Auton. Motiv.	(3) Control group	(4) Diff. (2)–(1)	(5) Diff. (3)–(1)	(6) Diff. (3)–(2)
Age	34.766	34.784	34.740	0.018	-0.026	-0.044
Male	0.532	0.530	0.532	-0.002	-0.000	0.002
Have UI benefits	0.739	0.737	0.736	-0.002	-0.003	-0.000
Health disability	0.029	0.029	0.029	0.000	0.000	-0.000
Matchable	0.905	0.907	0.905	0.002	-0.000	-0.003
Education level						
High school	0.469	0.466	0.469	-0.003	0.000	0.003
College	0.346	0.348	0.346	0.002	-0.000	-0.002
Place of birth						
Nordic country	0.016	0.017	0.016	0.001	0.000	-0.001
Western Europe	0.041	0.041	0.041	-0.000	-0.000	0.000
Outside Western Europe	0.258	0.257	0.254	-0.002	-0.005*	-0.003
Unemployment days						
Year t-1	42.217	42.217	42.134	-0.000	-0.083	-0.083
Year t-2	39.225	39.356	39.071	0.131	-0.154	-0.285
Year t-3	38.515	38.619	37.397	0.105	-1.118**	-1.222**
Year t-4	36.941	37.767	37.218	0.825	0.277	-0.549
# of unemployment spells						
Year t-1	0.181	0.180	0.179	-0.001	-0.002	-0.000
Year t-2	0.286	0.290	0.287	0.003	0.001	-0.003
Year t-3	0.271	0.271	0.267	-0.000	-0.004	-0.004
Year t-4	0.253	0.255	0.252	0.002	-0.001	-0.003
# of programs last 4 years						
Labour market education	0.010	0.010	0.010	0.001	0.000	-0.000
Preparatory education	0.025	0.026	0.024	0.001	-0.001	-0.002
Labour market training	0.016	0.015	0.015	-0.000	-0.000	0.000
Subsidized employment	0.034	0.032	0.031	-0.002	-0.002**	-0.001
Observations	55 857	56 082	88 781	111 939	144 638	144 863

Notes: Significance Codes: ***: 0.01, **: 0.05, *: 0.1. The research population excludes some individuals born in nine East African and Middle Eastern countries as discussed in Appendix 1. As mentioned in the pre-analysis plan (version 3.0), half of the control group could not be used between May 20 and October 31, 2020, which involves 14,912 individuals. To account for this issue, we weighted the retained control individuals in that period by the inverse of their share in the counterfactual control group that would have been used in the absence of this issue; specifically, we weighted them by 1.977454. The reported number of observations is unweighted.

The random assignment of the research population into treatment and control conditions was carried out as follows. Based on their birthday (day of the year), individuals were randomly assigned to the control group with a 50% probability and to each of the treatment conditions with a 25% probability.¹⁴ We excluded immigrants from nine countries in East Africa and the Middle East born on particular dates because missing birth certificates for some individuals

¹⁴ As mentioned in the pre-analysis plan (version 3.0), we were unable to use half of the control group that was randomly assigned to another research team between May 20 and October 31, 2020. Consequently, we reweighted the control group to address this issue, as detailed in the note accompanying the balancing tables below. While this adjustment does not impact the validity of the random assignment, it does reduce statistical power.

originating from these countries may result in a manipulated, and thus, non-random birthday. Details can be found in Appendix 1.

Table 1 presents the balancing tests for the research population. The control variables, taken from the register data, are all measured at the time of registration at the PES prior to the intervention and correspond to those retained by Cheung *et al.* (2025). The table shows that, despite the exceptionally high precision due to the large sample of 200,720 individuals, the control variables are well balanced over the two treatment conditions and the control group.

2.3 The Intervention

To study how to activate job seekers, we contrast motivational messages framed as either “supportive” or “controlling”. There is no strict definition of supportive and controlling interventions, but it usually refers to interventions that provide support and encouragement respectively, focusing on monitoring and constraints. To operationalize these two types of interventions with an explicit focus on motivation, we rely on SDT from psychology about what motivates people to undertake actions (Deci & Ryan, 1985; 2000; 2008; 2012). As mentioned in the introduction, SDT distinguishes between autonomous motivation, which occurs when individuals self-endorse their actions because they find them interesting and/or meaningful, and controlled motivation, which occurs when individuals experience pressure to behave in a certain way. This provides a coherent framework about motivation that can be used to design email messages that focus on support and control, respectively. In the following, we use the terms supportive messages and controlling messages to refer to the interventions designed to elicit autonomous and controlled motivation, respectively.

To this end, we crafted short emails, each approximately 120–160 words in length, with wording and tone tailored to stimulate the desired type of motivation. Each job seeker assigned to a treatment condition received a series of up to six monthly emails during the first six months of the unemployment spell,¹⁵ all of the same type: either stimulating autonomous or controlled motivation. The messages were stopped if a job seeker found a job. We did not send any messages to the control group because it was challenging to design a message with the same content that would not foster motivation. However, we note that all job seekers in the study population – both treated and non-treated – received various other frequent messages from the

¹⁵ The exact timing at which the messages and surveys were sent can be found in Appendix 5 of the pre-analysis plan.

PES, typically in the form of baseline email communications. Therefore, it is unlikely that the emails we sent produced a pure message effect. Caseworkers were unaware of the experiment and of the fact that some of their clients received these messages. They therefore could not have influenced how job seekers responded to the intervention.

For wording and tone, we used SDT and previous research as guidelines. To stimulate autonomous motivation, we framed the messages to support the basic needs for autonomy, competence, and relatedness, which are key drivers of motivation according to this theory (Deci & Ryan, 2000; 2008). To foster controlled motivation, we framed the messages to enhance internal and external pressure, thereby primarily undermining autonomy, and to a lesser extent the needs for competence and relatedness. The need for *autonomy* relates to a sense of volition and the ability to self-direct and manage one's own actions. To stimulate autonomy, we used words such as “can”, “may”, “choose”, and “feel free” in the supportive messages versus words like “must”, “required”, “have to”, and “obliged” in the controlling messages. The supportive messages also explained why particular job search actions might be useful, whereas the controlling messages focused on obligations. *Competence* reflects the desire to feel capable and effective in one's activities. Here, the supportive messages emphasized job search success and expressed confidence in job seekers' ability to attain their job-search goals. Conversely, the controlling messages emphasized avoiding job-search failure and did not refer to job seekers' own goals. *Relatedness* concerns the aspiration to feel connected and involved with others. To this end, the supportive messages expressed support and understanding, and used a more personal tone (e.g., “we”). This includes acknowledgement of the discouragement (e.g., frustration and loss of confidence) often experienced by unemployed individuals. The controlling messages instead focused on sanctions and were more formal and impersonal in tone (e.g., “the PES”).

Since there is no other research that used the wording and tone of messages to trigger different types of motivation in a job search context, we carefully implemented several quality checks to ensure that our goal was attained. The messages were carefully developed, translated, and extensively discussed over multiple rounds involving team members, PES caseworkers, and Swedish labor market experts.¹⁶ This process included detailed discussions of how

¹⁶ The messages were first drafted in English by the Belgian authors and then intensively discussed among all team members to ensure that their wording and tone triggered the intended motivations. They were then translated into Swedish, reviewed by policymakers, PES caseworkers, and Swedish labor market experts, and backtranslated to English to verify that their original meaning and tone were preserved.

caseworkers and others interpreted the messages. Additionally, we validated and refined the messages through a pilot conducted in June 2019 with job seekers who were not part of the experiment, followed by a survey assessing how participants perceived and interpreted the messages. This extensive iterative process should mitigate concerns that the emails were poorly worded or subject to systematic misinterpretation.¹⁷

Our emails were sent by the PES just a few days before the job seekers were required to submit their monthly activity reports. The fact that the messages were sent by the PES rather than by the researchers, and that the email subject lines were directly linked to this mandatory reporting requirement, may have increased the likelihood that job seekers opened and read the messages. Studies that use email interventions to influence job search behavior typically report higher open rates when emails are sent by the PES than when they are sent either by researchers or by providers of non-mandatory employment services.¹⁸ The fact that the PES was the sender of the messages may also have made the messages appear more controlling.

A potential challenge in using emails is capturing the job seekers' attention. To increase the relevance of the messages, each email included practical job-search advice, such as the importance of maintaining a clear and up-to-date CV when applying for jobs. To avoid confounding effects arising from factors other than the type of motivation targeted, the content, job-search tips, and length of the monthly messages (120-160 words) were kept as similar as possible across the two message types. The email subject lines were also identical for both message types in each month. Thus, apart from their focus on either autonomous or controlled motivation, the message types were designed to be as similar as possible.

To further increase engagement, job seekers were addressed by their first names, a common practice in Sweden. In addition, all emails referred to the upcoming reporting

¹⁷ A pilot study tested twelve emails (six supportive, six controlling) on 2,923 unemployed Swedish job seekers. Each participant received only one email tailored to their unemployment duration (one to six months) at the time of the pilot. A follow-up survey (30% response rate) assessed perceived support for autonomy, relatedness, and competence. Based on this feedback, the messages were further revised in consultation with PES employees, with final adaptations made following changes in activity reporting and the COVID-19 outbreak.

¹⁸ Open rates for emails sent by the PES typically range between 54% and 68%: 54% in Leduc & Tojerow (2025), 64% in both Behaghel et al. (2024) and Belot et al. (2025), and 68% in Lehner & Schwartz (2026). However, Cairo & Mahlstedt (2023) are an exception, reporting an open rate of only 37%. In Hopkins & Dorian (2024) and Schimpf et al. (2025), the emails were sent by providers of non-mandatory employment services rather than by the PES. These studies report open rates of 30-35% and 48-49%, respectively. In Darling et al. (2017) and Briscene et al. (2022) the emails were sent by the researchers themselves. They report open rates of 33-48% in the former study and 32% in the latter.

deadline. Whereas the controlling messages emphasized this deadline, the supportive messages explained why activity reporting can be useful. Note that the PES routinely communicates information about activity reporting by email as part of its regular services, further reducing the likelihood that our treatment effects merely reflect the receipt of an additional message.

Before launching the experiment, we assessed potential ethical concerns and obtained approval from the Swedish Ethical Review Authority. Ex ante, both low-cost message interventions had the potential to improve job search motivation, increase job-search intensity, and promote job finding in a cost-effective manner. Moreover, based on the existing literature discussed in the introduction, it was unclear whether the supportive or the controlling intervention would be more effective. Identifying effective ways to boost motivation and to improve employment outcomes is important both for job seekers and for society. We therefore believe that conducting this RCT was ethically justified.

2.4 Examples of the Content of the Messages

Here we provide two examples of the messages. All other messages are reported in Appendix 2. The Swedish versions of the messages are included in the pre-analysis plan that can be downloaded from the AEA RCT registry website.

Email 1 supportive [163 words English; 155 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month]. Thank you for submitting your report.

Your report gives us a better understanding of how your job search is going. It helps us understand your specific situation, so that we can give you the right support on the way to a job or education.

We understand that it is important for you to achieve your personal goals and to find a job or education that suits you. Arbetsförmedlingen believes in your ability and wants to support you in finding the job or the education that you are looking for.

There are several ways to find a fitting and interesting job. Depending on the type of job you are looking for, you can choose to apply for jobs via job listings, by contacting firms directly or by using your own network.

If you have not visited Platsbanken before, you have the opportunity to search for jobs via this link:

[To Platsbanken](#)

Best regards,

Arbetsförmedlingen

Email 1 controlling [155 words English; 156 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month] . Remember to submit your activity report on time.

Arbetsförmedlingen monitors your report to check whether you are active enough in your job search. Being active is a requirement for increasing your chances of quickly finding employment or education. You are expected to accept suitable job offers, so that the time that you remain unemployed is kept as short as possible.

If you have unemployment benefits or receive activity support, the benefit rules require that you are actively seeking and applying for suitable jobs or education. Search for jobs through job listings, using your network and contacting companies directly.

You are obliged to have knowledge of and follow the rules that apply to all job seekers. If you receive benefits and break the rules, you risk a warning or suspension from your benefits.

Visit Platsbanken to look for job openings:

[*To Platsbanken*](#)

Best regards,

Arbetsförmedlingen

2.5 The Expected Impact of the Intervention

Theories from economics and psychology lead to different hypotheses about the impacts of our emails, contributing to why it is relevant to compare the two types of motivation. According to psychology's SDT, triggering of autonomous job search motivation is expected to consistently increase job search effort and, consequently, job finding compared to both controlled motivation and no intervention (i.e., the control condition). This is because the psychological literature suggests that the role of intrinsic (or autonomous) motivation depends on the nature of the task. *“Tasks that are straightforward, highly repetitive, and perhaps even less inherently enjoyable, should be closely linked to extrinsic incentives. (...) tasks that require a great deal of absorption, personal investment, complexity, and overall quality should be less linked to*

incentives, and much more closely linked to intrinsic motivation” (Cerasoli et al. 2014, p. 998-999). While job search may not be *inherently* enjoyable, finding a job is a crucial and meaningful goal for most individuals. Additionally, since job search can be viewed as a complex activity requiring sustained effort over time as well as substantial focus and personal dedication, it is likely that self-regulation and, consequently, autonomous motivation play a key role (Van Hooft et al. 2013). SDT thus predicts that stimulating autonomous motivation should have more favorable effects than controlled motivation (Deci & Ryan 2008; Deci et al. 2017). SDT does not provide a clear prediction on the relative effectiveness of controlled motivation versus no intervention.

In contrast, job search theory (JST) posits that if supportive messages are perceived as *supportive* or *welfare-enhancing* and controlling messages are *welfare-reducing*, controlling messages will be more effective than both supportive messages and no intervention, whereas supportive messages will be less effective than no intervention.¹⁹

3 Data

We collected information on the labor market outcomes using PES register data and three different surveys.²⁰ All three surveys were kept short to encourage higher response rates and to comply with the PES guidelines. An *initial survey* was used to measure initial job search motivation. It was conducted within the first month of unemployment, about a week before the first message was sent. The time lag between this survey and the first message aims to avoid a priming effect. An *intermediate survey* was sent between 3 and 4 months after entry into unemployment, asking about job-search behavior (effort and quality) and motivation. By this time, most respondents in the treated group have already received three of the six intended

¹⁹ In standard economic job search theory (JST) (Ehrenberg and Oaxaca 1976; Mortensen 1977; for a survey, Le Barbanchon et al. 2024), intrinsic (or autonomous) and controlled motivation are considered to matter only indirectly, through their effects on welfare. Consequently, standard economic theory makes predictions that are opposite to those of SDT regarding the relative effectiveness of autonomous and controlled motivation. According to JST, monitoring job search activities incentivizes job finding because individuals seek to avoid the risk of sanctions for non-compliance. In contrast, relaxing control by supporting job seekers' basic needs (autonomy, competence, and relatedness) enhances their welfare, reduces the fear of sanctions, and thus may decrease their motivation to search intensively.

²⁰ The surveys were sent to all job seekers with two exceptions. First, due to practical reasons, survey collection began on August 24, 2020, excluding the first two cohorts from the initial survey. Second, only half of the control group was eligible to receive the surveys.

messages.²¹ Finally, all job seekers who found a job were sent an *exit survey* 2 to 7 weeks after leaving unemployment, with questions focusing on perceived job quality, including job satisfaction, job fit, and intention to stay. Due to restrictions imposed by the PES, the surveys were administered to all job seekers in the two treatment groups but only to a random 61.51% subsample of the control group.²² Among those invited to participate, the response rates were 11.2% in the initial survey, 9.7% in the intermediate and exit surveys. Relative to the full experimental sample, this yields effective response rates of 9.9%, 8.0%, and 5.2%, respectively.

Appendix 3 presents the balancing tables for the samples that responded to the surveys. While some imbalances are observed, they do not raise concerns about selective survey response, as the findings remain robust when adjusting for background characteristics.

3.1 Outcome Variables

Table 2 lists the primary (in bold) and secondary outcome variables, as pre-specified in the pre-analysis plan. We distinguish between three different outcomes: (i) ‘first stage’ outcomes; (ii) job search and job finding outcomes; (iii) measures of job search quality and quality of the first job after unemployment.

The first set of outcomes aims at measuring whether our intervention triggered a first stage behavioral response and whether this response was in line with our intentions. The main purpose here is to measure whether the intervention had any significant bite. First, we measure the proportion of job seekers who opened the emails and click rates for URLs included in the emails. For privacy reasons, we only have this information on the aggregate level and only for two cohorts, those who entered unemployment in months 14 and 16 after the start of the experiment. Second, since our messages were sent each month close to the deadline for job seekers to submit their activity reports, we consider the average monthly share who complied (in months 1–3, 4–6, and 1–6) as *individual* response indicators. We also used the intermediate survey data to check whether our messages triggered any observable change to *controlled and*

²¹ For individuals who exited unemployment before the intermediate survey was sent, the intermediate and exit surveys were merged into a single survey. This was decided in consultation with the PES to avoid these individuals receiving two separate surveys within a short time period.

²² The exit survey was administered only to job seekers who had found employment, which explains its lower effective response rate relative to the other surveys.

autonomous motivation (see Appendix 4 for details), keeping in mind that the power of the survey analyses is lower due to non-responses.

For job search effort and job finding, our two primary outcomes are (i) the average number of job applications²³ per month during months 2 to 7 of unemployment, and (ii) exit from unemployment within 7 months.²⁴ We restrict the number of primary outcomes as to avoid the issue of multiple testing and select them from the administrative data to maximize statistical power. We chose these two outcomes as primary measures, because ultimately, we are interested in whether our motivational emails increase search effort and boost job finding. As secondary outcomes, we consider job search and job finding outcomes measured in other periods, and days in unemployment.

Table 2: Overview of Primary (in bold) and Secondary Outcomes

Outcome category	Register data	Survey data
(i) 'First stage'	- Aggregate email open and URL click rate (selected cohorts)¹ - Monthly share submitting activity reports (months 1-3, 4-6, 1-6).	Motivation in intermediate survey after 3-4 months: ² - Controlled; - Autonomous.
(ii) a. Job search b. Job finding	a. Average number of job applications per month (months 2-4, 5-7, 2-7). b. Leaving unemployment within 7 months (4 and 12 months); - Days unemployed within one year of the unemployment onset.	a. Number of hours searched last week.
(iii) a. Job search quality b. Job quality	a. Average number of spontaneous applications per month (months 2-4, 5-7, 2-7). - Average number of job interviews per month (months 2-4, 5-7, 2-7). b. Employment (as measured by non-unemployment) lasts more than 6 months (3/12 months).	a. Validated measure of job search quality. ³ b. Measure of job quality: ⁴ job satisfaction, perceived fit and stay intention, and the average of these items.

Notes: ¹ In contrast to all other outcomes, this information is not available at the individual level and is pertains only to cohorts 12 and 16, who entered unemployment between April 20-May 19, 2021, and August 20-September 19, 2021, respectively.

² Appendix 4 describes the construction of the validated measures of motivation. The initial search motivation used as moderator (Section 5.2.2) is measured in the initial survey using the same items.

³ Appendix 5 describes how this measures slightly modifies the validated measure of Turban et al. (2009).

⁴ Appendix 6 describes the construction of this measure in more detail.

²³ Job applications include submissions to posted vacancies, excluding spontaneous applications, which are analyzed as a separate outcome category.

²⁴ This outcome, which is also used in other research (see e.g., Cheung et al., 2025), is readily available in the administrative data from the Swedish PES.

The final set of outcomes concerns job-search quality and job quality. *Job-search quality* may be relevant as the supportive treatment could enhance how job seekers search for jobs without affecting the total number of applications submitted. We measure job-search quality in several ways. First, we use data from the monthly activity reports to measure the *average number of spontaneous applications per month* and the *average number of job interviews to which job seekers are invited per month*, as this may reflect both the quality of job search and broader changes in job-finding success. Second, based on survey data we construct a validated psychological measure of job-search quality (Turban et al., 2009). Appendix 5 explains the construction of this measure.

The main measure of *job quality* is an indicator that measures whether the duration of the first job – proxied by the time not in unemployment – exceeds 6 months. For sensitivity, we also consider duration thresholds of 3 and 12 months. We also use data from the exit survey, which asked job seekers about job satisfaction, perceived job fit and their intention to stay.²⁵

3.2 Moderators

Even though the theories provide no clear predictions about moderators of the intervention, we examine the moderating effects of initial job-search motivation, gender, labor market attachment, and the local unemployment rate. The first two moderators are motivated by previous findings. Herz & Zihlman (2024) reported in their working paper (now published) that monitoring could particularly reduce the performance of intrinsically motivated workers in complex tasks. Lombardi (2019) found that males react more strongly to increased monitoring of job search activities. We also explore effect heterogeneity by individual labor market attachment and unemployment rate in the local labor market.²⁶

²⁵ We analyze these three scales separately and also use a composite measure, calculated as the average of the three. See Appendix 6 for more details.

²⁶ In the pre-analysis plan, we also announced that we would consider occupational exposure to Corona as potential moderator. However, this turned out to be less relevant ex post, and we therefore did not include it in the analysis.

4 Methods

We estimate the causal effect of the treatment in a straightforward way by regressing the outcomes of interest (Y_i) on treatment indicators and, possibly, control variables ($\mathbf{X}_i$). We define the treatment indicators as follows: $T_i^c = 1$ if individual i belongs to the random group that receives messages aimed at triggering controlled motivation and $T_i^c = 0$ otherwise; $T_i^a = 1$ in case individual i belongs to the random group that receive messages aimed at triggering autonomous motivation and $T_i^a = 0$ otherwise. The regression model is specified as:

$$Y_i = \alpha + \beta_c T_i^c + \beta_a T_i^a + \mathbf{X}_i \boldsymbol{\gamma} + u_i \quad (1)$$

where β_c and β_a are the parameters of interest, capturing the effect of the supportive respectively the controlling messages versus no intervention. We can also test for differences between the two treatments by comparing β_c and β_a . While Section 5 reports the results without control variables, our sensitivity analysis presented in Appendix 7 demonstrates that including covariates $\mathbf{X}_i$ in the regressions has no impact on our main findings. As recommended by Duflo (2018), we select the control variables in these robustness checks using a double machine learning LASSO method (Belloni *et al.*, 2014; Urminsky *et al.*, 2016; Cilliers *et al.*, 2024). We use conventional standard errors, as using Huber-White robust errors has no measurable impact on the results.

One potential concern is dynamic selection as some of our outcome variables, such as job-search intensity, are measured conditional on being unemployed. Thus, if the treatment affects the likelihood of exiting unemployment, it alters the composition of treated individuals in (un)employment relative to the untreated, and this may create selection problems. However, our results are robust to the adjusting for control variables (Appendix 7). Moreover, the impacts of our light-touch email intervention are only moderately sized, so the resulting compositional changes are marginal. As a result, any dynamic selection bias should be less of a concern.

For the moderating analysis, we run the regressions separately for binary moderators. For non-binary moderators, we include the moderator of interest (e.g., initial motivation) both without interaction and in interaction with the two treatment indicators.²⁷

²⁷ Since the estimates with and without controls are nearly identical, we do not include the controls in the moderating analysis presented in Section 3.3.

5 Results & Discussion

5.1 Main Effects

This section reports effects of the two email interventions on (i) the ‘first stage’ outcomes, measured by aggregate email open and URL click rates, the share submitting activity reports, and survey scales of controlled and autonomous motivation; (ii) job search and job finding outcomes measured in the register data; (iii) job-search quality and job quality indicators measured in the register data and surveys.

Note that the number of observations differ largely depending on the data source: outcomes measured from survey data are based on a much smaller number of observations than those from register data, and therefore, may be measured much less precisely.

5.1.1 ‘First stage’: Open Rates, and Impact on Activity Reporting and Motivation

As mentioned in Section 3.1, we possess aggregate-level data on the email open rate and click-through rates for two inflow cohorts. Overall, the open rate is 72%: 11,102 of the 15,436 messages sent to these cohorts were opened. This suggests that the majority of the job seekers have seen the messages. This open rate exceeds all those reported in the studies using email interventions to influence job search behavior cited in Section 2.3. It is 4 to 8 percentage points higher than the open rates reported in the three studies with the highest rates among those in which the PES sent the messages, a setting that already tends to generate higher open rates than emails sent by the researchers or providers of non-mandatory employment services. This may reflect the fact that the messages were sent only a few days before job seekers were required to submit their monthly activity reports.

The open rate of the first email cannot be influenced by its content and is therefore balanced across the two conditions: 74.9% in the controlled and 75.0% in the supportive condition, with a p-value of 0.949. However, once the first message has been opened, the treatments may lead to different subsequent open rates. Indeed, we observe an open rate of 72.1% for individuals in the controlling condition, compared to 70.0% in the supportive condition (p-value < 1%). Similarly, conditional on opening, 10.9% of individuals in the

controlling condition click on the information-URLs in the emails compared to only 8.9% in the supportive condition.²⁸ Thus, the messages triggered behavioral reactions.

In principle, the treatment effect reflects the combined effect of receiving motivational messages *and* accessing the tips provided on the webpages linked via the embedded URL's. However, given the low conditional click-through rates, it is unlikely that the tips are the primary driver of the effects reported below. Moreover, the directions of the observed treatment effects do not align with the expected impact of such tips.

Table 3: Effects on the Monthly Share Submitting Activity Reports and on Survey Scales of Motivation

	Monthly share submitting activity reports				Motivation: intermediate survey	
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)	Controlled motivation (5)	Autonomous motivation (6)
Treatment						
Controlling $\hat{\beta}_c$	0.0231*** (0.003)	0.0113*** (0.002)	0.0151*** (0.002)	0.0075*** (0.002)	0.0283 (0.025)	0.0058 (0.015)
Supportive $\hat{\beta}_a$	0.0204*** (0.003)	0.0076*** (0.002)	0.0091*** (0.002)	0.0060** (0.002)	0.0112 (0.025)	-0.0056 (0.016)
Observations	200 720	200 720	200 720	200 720	16 093	16 093
Mean control gr.	0.589	0.456	0.534	0.377	3.187	4.190
Sup. vs Contr. = $\hat{\beta}_a - \hat{\beta}_c$	-0.0027 (0.003)	-0.0037 (0.002)	-0.0060** (0.002)	-0.0014 (0.003)	-0.0171 (0.025)	-0.0114 (0.016)
Observations	111 939	111 939	111 939	111 939	10 726	10 726
Mean controlling tr.	0.612	0.467	0.550	0.385	3.215	4.195

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 report show effects on the monthly share of submitted activity reports over various intervals. Columns 5–6 on motivational scales, based on survey data described in detail in Appendix 4. Effects relative to the mean of the control group and relative to the controlling treatment. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Next, Table 3 shows that both treatments significantly impact activity reporting, with p-values less than 1%. The fraction of submitted reports during the first six months increased by 1.1 percentage points (pp) for emails triggering controlled motivation and by 0.8 pp for the autonomous motivation emails.²⁹ These changes correspond to proportional increases relative

²⁸ We include the clicks in the first message in these statistics, as individuals' clicking behavior may have been influenced by its content. The p-value for the difference in the conditional click rates is also below 1%.

²⁹ These are intention-to-treat (ITT) effects. Although we cannot merge the email open data with individual responses, we can infer from the aggregate open rate that the local average treatment effects (LATE) are approximately 1.39 times larger than the reported ITT: based on an open rate of 72%, $1.39=1/0.72$. Throughout the paper, we report only the ITT estimates.

to the baseline by 2.5% and 1.7%, respectively. Although the effects are small, as might be expected from an email intervention, they nonetheless demonstrate a clear behavioral response. We also see that the effects on reporting are more pronounced in the beginning of the intervention than later on. The intervention's impact on the first activity reporting is about two to three times larger than for reports 4–6. However, it is difficult to assign a structural interpretation to this time dependence, as it may reflect dynamic selection: those who respond more strongly early may also exit unemployment more quickly.

While the effect is larger in the controlling than in the supportive condition, the difference is statistically significant only during the first three months. Given the emphasis on compliance with formal requirements in the controlling condition, a stronger effect on reporting was to be expected.

The right panel of Table 3 reveals that the impacts of the intervention on the two motivational scales measured from the intermediate survey are statistically not different from zero. These imprecise estimates may be attributed to the reduced statistical power of the survey data or the difficulty of measuring these different motivational concepts. However, since – as shown below – the messages did influence job search behavior and outcomes, albeit in the opposite direction than anticipated, this suggests that the absence of measured motivational are due to a measurement problem rather than a lack of impact.

5.1.2 Effects on Job Search and Job Finding

Table 4 reports effects on job-search effort and job finding. The two primary outcomes—the number of job applications between months 2 and 7, and the job finding rate within the first 7 months—are shown in bold. We conclude that the controlling treatment does not affect any of these outcomes. The zero impact of the controlling treatment is confirmed by the statistically insignificant (and small) estimates for the secondary outcomes as well.

We see a slightly different pattern for the supportive treatment. The number of job applications decreases by 0.09 (p-value < 10%), corresponding to 1.1% reduction relative to baseline. This is reinforced by a more statistically significant (p-value < 5%) decrease by 0.13 (1.5% relative to baseline) in the corresponding short-run outcome measuring the number of

job applications between months 2 and 4.³⁰ Because of the zero effects of the controlling treatment, the negative effects of the supportive treatment relative to the control group are also reflected in significant differences between the two treatments.

Table 4: Effects on Number of Job Applications and Job Finding

	Job applications		Job finding			
	Report 2–4	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Controlling $\hat{\beta}_c$	-0.0081 (0.055)	0.0083 (0.049)	0.0005 (0.003)	0.0009 (0.003)	-0.0013 (0.002)	0.1170 (0.684)
Supportive $\hat{\beta}_a$	-0.1283** (0.055)	-0.0938* (0.049)	-0.0030 (0.003)	-0.0003 (0.003)	-0.0043** (0.002)	0.5368 (0.683)
Observations	125 357	131 518	200 720	200 720	200 720	200 720
Mean control gr.	8.734	8.286	0.462	0.631	0.817	184.2
Auto. vs Contr. = $\hat{\beta}_a - \hat{\beta}_c$	-0.1202** (0.059)	-0.1021* (0.053)	-0.0034 (0.003)	-0.0012 (0.003)	-0.0030 (0.002)	0.4198 (0.749)
Observations	70 363	73 796	111 939	111 939	111 939	111 939
Mean controlling tr.	8.726	8.294	0.462	0.632	0.815	184.3

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–2 show the effects on the average number of job applications per month in months 2–4 and 2–7; Columns 3–5 on job finding within 4, 7, and 12 months; and Column 6 on the number of days unemployed within one year of unemployment onset. Effects relative to the mean of the control group and relative to the controlling treatment. Standard errors in parentheses. ***, 0.01, **, 0.05, *, 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

For job finding we see no significant effect of the supportive treatment on the primary outcome (job finding within 7 months) in column 4. However, we observe a slight decline in the job finding rate relative to the control group within the first 4 and 12 months, with the latter being significant at the 5% level: –0.4 pp –0.5% relative to baseline. Below, we show that these effects are driven by men, for whom we find significant effects on job finding.

5.1.3 Effects on Job-Search Quality and Job Quality

We find no systematic effects on job-search quality nor job quality for either of the two treatments (Tables A8.1 and A8.2 in Appendix 8). Relative to the control group, only the controlling treatment condition has a statistically significant effect on one of the seven

³⁰ To save space, we omit the effects on job applications between months 5 and 7. These effects have the same sign as those between months 2 and 4 but they are not significantly different from zero. Results are available upon request.

measures of job-search quality and one of the seven measures of job quality. It has a positive effect on the intermediate survey measure of job-search quality and a negative effect on the exit survey measure of job satisfaction. Relative to the supportive treatment, only the former effect remains significant at the 10% level. However, since the messages designed to induce controlled motivation had no significant impact on any of the primary outcomes, these effects are likely due to sampling noise and should not be interpreted substantively.

5.1.4 Discussion

How can we understand these findings? A first observation is that a high share (72%) opened the emails. This is important, as a high open rate is a necessary condition for the intervention to generate behavioral responses.

Second, messages stimulating *controlled* motivation had no impact on search effort or on job finding. These zero effects are precise and cannot, therefore, be attributed to low statistical power. Nevertheless, we do observe a highly significant increase in activity reporting, confirming that the controlling messages did have a meaningful behavioral impact. One possible interpretation is that these messages may have primarily acted as a reminder of the reporting requirements rather than encouraging genuine changes to search effort. This would explain why we find no impact on the reported number of job applications or on job finding, despite observing a positive effect on activity reporting. The zero effect on formal applications also suggests that there seems to be no shift from informal applications to formal applications, as previously observed by van den Berg & van der Klaauw (2006).

Third, the modest declines in search effort and job finding of supportive emails were unexpected. *Relational Incentives Theory* (Gallus et al., 2022) offers a useful interpretative framework. The theory distinguishes between the means of an incentive—what is used as the incentive (here, motivational emails)—and its scheme, that is, how the incentive is used within the sender–recipient relationship. A premise is that incentives are more likely to be effective when their scheme is *congruent* with the prevailing relational structure of the persons involved. When the scheme is *incongruent*, it may backfire. This is relevant in our context with a strongly monitoring-oriented institutional environment at baseline. Autonomy-supportive messages—using language of choice, understanding, and support—may therefore have been perceived as a poor fit with job seekers’ expectations about the PES–client relationship. Such a scheme–relationship mismatch can weaken engagement with the focal activity (i.e., active job search.).

This interpretation is consistent with the finding that the supportive emails appear to have led to a small but significant decline in job finding.

Fourth, Cairo & Mahlstedt (2023) find that providing generic information about work requirements can reduce employment among welfare recipients who face a low risk of being sanctioned for non-compliance. Similar to the supportive messages in our experiment, such information may be incongruent with recipients' relationship with the welfare agency, as it may signal to low-risk recipients that the incentive scheme is largely irrelevant for their behavior because they already comply with the agency's requirements. The backfiring in Cairo & Mahlstedt (2023) is confined to the low-risk population. Although, we cannot identify the subgroup for whom the supportive messages are perceived as most incongruent with the PES's existing controlling relationship with job seekers, the next subsection shows that the negative effect is substantially stronger among men than among women.

Fifth, one alternative interpretation of the moderate negative employment effects of the supportive messages is that they encouraged job seekers to "take their time" to find a better match. However, the fact that we do not find improvements in job quality (neither using administrative or survey data) speaks against this interpretation of our results.

Table 5: Moderating Effects of Gender on the Monthly Share Submitting Activity Reports

	Monthly share submitting activity reports			
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)
Treatment				
Controlling x Male	0.0264*** (0.004)	0.0098*** (0.003)	0.0143*** (0.003)	0.0052 (0.003)
Supportive x Male	0.0253*** (0.004)	0.0101*** (0.003)	0.0116*** (0.003)	0.0086*** (0.003)
Controlling x Female	0.0193*** (0.004)	0.0131*** (0.003)	0.0161*** (0.003)	0.0101*** (0.004)
Supportive x Female	0.0145*** (0.004)	0.0045 (0.003)	0.0060* (0.003)	0.0030 (0.003)
Observations	200 720	200 720	200 720	200 720
Mean control group (male)	0.552	0.431	0.507	0.356
Mean control group (female)	0.632	0.479	0.567	0.391

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 show effects on the monthly share submitting activity reports over various time periods. Effects in deviation from the mean shares of activity reporting by females and males in the control group reported in the two last lines. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

5.2 Moderators³¹

We now discuss the moderating impact of gender, initial motivation, labor market attachment and unemployment rate in the local labor market.

5.2.1 Gender

We have reported some moderate negative effects of the supportive messages on job-search effort and job finding. Here, we demonstrate that these effects are driven by significant effects among men. Table 5 shows that the controlling treatment significantly increased activity reporting for both men and women. Notably, the response to the controlling treatment is equally strong irrespective of gender – suggesting that the increased ambition to comply with the reporting requirements is general. In contrast, we find that the supportive treatment consistently has a stronger effect on activity reporting for men than for women throughout the unemployment spell. For the first report, the effect on reporting is significantly higher for men than for women, with a difference of +1.1 pp (p-value < 5%). This finding already suggests a stronger behavioral response of supportive messages among men than among women.

Table 6 shows that the negative effect of messages stimulating autonomous motivation primarily reduces the number of job applications per month among men. The negative effect for men appears in all the three different time intervals that we consider, and it is significant (10%-level) during months 5 to 7 and during months 2 to 7 of unemployment. We also find negative effects on job finding among men. For them the job-finding rate within the first 4 months decreases by 0.8 pp (a 1.8% decline relative to baseline), and the number of days in unemployment within the first year increases by 1.6 days (a 0.8% increase relative to baseline).

Although effect sizes are moderate, these gender differences are interesting in light of previous evidence. The literature on the effectiveness of active labor market policies consistently finds larger impacts for women (Bergemann & van den Berg 2008; Card et al. 2018), but this has been attributed to selection effects related to fertility decisions (Lechner & Wiehler, 2011; Vooren et al., 2019). However, our findings align with those of Lombardi (2019), who found that, within the same institutional environment, only men responded to tighter monitoring and threats of sanctions by finding jobs faster. Related evidence from

³¹ We only report the moderating effects on the activity reporting shares, the job search and job finding indicators. The moderating effects on other outcomes are available from the authors upon request.

German social assistance also points to stronger behavioral responses among male welfare recipients under stricter monitoring regimes (van den Berg et al., 2024). Based on the Relational Incentives Theory Gallus et al. (2022), we interpreted the negative effect of the supportive emails as a result of a mismatch between supportive messages in a controlling environment. Thus, the more negative effects for men suggest that men react more strongly to this perceived scheme–relationship mismatch.

Table 6: Moderating Effects of Gender on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling x Male	0.0090 (0.076)	-0.0430 (0.079)	0.0099 (0.068)	0.0001 (0.004)	0.0033 (0.004)	-0.0015 (0.003)	-0.0773 (0.938)
Supportive x Male	-0.1243 (0.076)	-0.1378* (0.079)	-0.1157* (0.069)	-0.0081** (0.004)	-0.0023 (0.004)	-0.0038 (0.003)	1.5674* (0.938)
Controlling x Female	-0.0261 (0.078)	0.0402 (0.081)	0.0067 (0.070)	0.0009 (0.004)	-0.0019 (0.004)	-0.0010 (0.003)	0.3367 (1.000)
Supportive x Female	- 0.1326* (0.078)	-0.0052 (0.082)	-0.0708 (0.070)	0.0028 (0.004)	0.0019 (0.004)	-0.0049 (0.003)	-0.6109 (0.997)
Observations	125 357	85 769	131 518	200 720	200 720	200 720	200 720
Mean contr. gr (male)	8.205	7.265	7.772	0.463	0.637	0.828	183.3
Mean contr. gr (female)	8.199	7.063	7.750	0.466	0.636	0.821	181.2

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. Effects in deviation from the means in the control group for males and females reported in the two last lines. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

5.2.2 Initial Motivation

Within the behavioral economics literature much attention has been given to the prediction that extrinsic incentives (i.e., controlled motivation) may backfire by crowding out intrinsic motivation (Fehr & Falk 1999; Gneezy & Rustichini 2000a, b; Huffman & Boganno 2018). While not all forms of extrinsic motivation constitute controlled motivation in SDT (Ryan &

Deci 2000),³² this crowding-out effect aligns with SDT’s prediction that controlled motivation can undermine autonomous motivation.

Table 7: Moderating Effects of Initial Autonomous Motivation on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment Controlling x Initial auton. motivation	0.0042 (0.201)	-0.2531 (0.194)	-0.0742 (0.180)	-0.0165 (0.010)	-0.0190* (0.010)	-0.0113 (0.008)	4.2825* (2.566)
Observations	14 855	10 201	15 297	19 806	19 806	19 806	19 806
Mean control group	9.501	7.752	8.870	0.428	0.614	0.806	188.6

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. Effects in deviation from the controlling treatment with initial motivation set to zero. The complete table including the interaction effect between the supportive treatment and initial autonomous motivation is reported in Table A9.2 in Appendix 9. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

Consistent with this, Herz & Zihlmann (2024) found in a field experiment with Amazon Mechanical Turk workers that introducing greater control can reduce performance driven primarily by intrinsic (“non-pecuniary”) motivation. This negative effect was observed only for challenging tasks, not for simple ones, supporting the psychological research suggesting that intrinsic motivation is more crucial for complex tasks than for simple ones (Cerasoli et al. 2014). Given that job search is generally considered a complex task (Van Hoof et al. 2013), reinforcing controlled motivation could reduce job-search effort and hinder job finding of individuals who were initially (i.e., at the start of unemployment) autonomously motivated.

We find some evidence in line with these expectations. Table 7 shows that the interaction effects between the controlling messages and initial autonomous motivation are negative for the number of job applications between months 2 and 7, and 5 and 7, though not statistically significant. They are also negative for job finding rates measured after 4, 7, and 12 months,

³² When people identify with the desired outcomes and internalize the regulation of their behavior, it becomes self-determined, and external incentives can even enhance autonomous motivation (Cerasoli et al. 2014).

with statistical significance at the 10% level after 7 months. Conversely, the effect is positive (p-value < 0.10) for the number of days in unemployment during the first year.

While this evidence is not significant across all outcomes, the fact that the effects on the other job finding outcomes share the same sign and that these findings align with prior research in different contexts, points in the direction of negative effects of the controlling treatment for initially autonomously motivated job seekers.³³ This is another interesting way in which our messages appear to have backfired.

By contrast, as expected, no significant effects are found for the interaction between controlling messages and initial controlled motivation, nor between messages stimulating autonomous motivation and either type of initial motivation.

5.2.3 Other moderators

In Section 3.2, we hypothesized that motivational effects would be more pronounced for job seekers with strong labor market attachment or residing in areas with low local unemployment rates, but we find no systematic evidence of such moderating effects (Tables A9.5-A9.8 in Appendix 9). Residing in a municipality with unemployment rate below the median significantly *reduces* the share submitting activity reports in all months by about 1 pp (p-value < 5%) in the supportive treatment condition, but has no significant impact in the controlling treatment. However, we do not observe any statistically significant moderating effect of the local unemployment rate on other outcomes, except for a small increase in the 4-month job-finding rate for the supportive treatment in low-unemployment municipalities. This is the only result that is significant and directionally consistent with the hypothesis. Strong labor market attachment does not moderate any outcome for either treatment, except for a reduction in the share submitting the first activity report by 1.5 pp (p-value < 1%) in the controlling treatment.

6 Conclusion

One of the greatest challenges in job search is maintaining motivation, as repeated failure can lead to significant frustration. To address this challenge, this research presents a low-cost

³³ The full estimation results for the interaction effects of initial autonomous and controlled motivation with the two treatments on the activity reporting shares, the job search and job finding indicators are presented in Table A.9.1-9.4 in Appendix 9. There are no additional significant effects to report.

digital intervention with motivational emails aimed at enhancing and sustaining motivation and search effort among job seekers. Unique in its approach, the intervention evaluates a “supportive” and a “controlling” approach to stimulate motivation, by sending short, theory-based email messages to unemployed job seekers. Specifically, the emails were operationalized using Self-Determination Theory (SDT) in psychology (Deci & Ryan 1985; 2000; 2008; 2012), focusing on what is referred to as autonomous and controlled motivation – the two main drivers of motivation according to SDT. The intervention was implemented on an unprecedented scale in a randomized controlled trial (RCT) involving approximately 200,000 individuals entering unemployment in Sweden.

Most of the job seekers opened the motivational emails, and the intervention increased compliance with job-search reporting requirements, likely in part because they served as reminders to submit activity reports. Interestingly, we find evidence of two ways in which motivational messages can backfire. First, the controlling messages designed to stimulate controlled motivation backfire for job seekers who, at the onset unemployment, report being motivated by an inner drive rather than by constraints. Similar patterns have been reported in both economics and psychology for complex tasks (Herz & Zihlmann, 2024; Cerasoli et al., 2014). In contrast, for other job seekers, enhancing controlled motivation has no measurable behavioral effects, possibly because job seekers already operate in a highly controlling environment.

The supportive messages designed to stimulate autonomous motivation also backfire. While the negative effects, especially on job finding, are modest in magnitude, they are consistently opposite to the intervention’s objective and driven by men. One interpretation is that the messages encouraged job seekers to “take their time” to find a better match. However, we do not find improvements in job quality, suggesting that the reduction in search effort is not offset by higher-quality matches. Relational Incentives Theory (Gallus et al., 2022) offers a more plausible interpretation. The autonomy-supportive messages may have been perceived as incongruent with job seekers’ expectations about the PES–client relationship in a setting dominated by rather strict monitoring. Such incongruence can disrupt relational motives, undermining the coordinating relationship and reducing search effort.

Taken together, these findings demonstrate that enhancing motivation to search for jobs through a low-cost digital messaging intervention is challenging and may even be counterproductive. They also highlight the importance of context: supportive messages are more likely to succeed when the message scheme is congruent with a more supportive institutional environment. An important question for future research is therefore whether light-

touch messages can effectively stimulate motivation when delivered in a more supportive institutional relationship, or whether such messages need to be complemented by more substantive changes in service delivery, such as counselor interaction or other forms of in-person support.

These questions resonate with broader findings from the literature on low-cost digital interventions. While this literature documented modest positive effects for specific populations, achieving substantial impacts has generally proved difficult (DellaVigna & Linos, 2022), especially when digital interventions are not combined with in-person assistance (Gallego et al., 2025). Our findings add that congruence between digital communication and the broader institutional environment also determine whether low-cost digital interventions succeed or fail.

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Online Appendix

Appendix 1: Trimming and Selection Criteria on the Research Population

As mentioned in the main text, for some migrants originating from particular countries from East Africa and the Middle East the exact birth dates are unknown due to lacking birth certificates. For these individuals, the officially registered day of birth may not be random in a year, as it is either assigned by an administrator or chosen by the individuals themselves. Typically, the registered birthdays in these instances fall on the first day of the month, a round number, or a religious holiday. As a result, the assignment based on day of birth is not truly random for individuals originating from such countries. Ignoring this issue leads to an unbalanced distribution of the research population across treatment conditions.

To address this issue, we implemented the following procedure. We hypothesized that the problem primarily affects migrants from specific countries where birth date registration is uncommon. We calculated the standard deviation of the frequency observed on each day of birth by country of origin within our research population. The rationale is that countries where specific birth days are commonly chosen would show a significantly higher standard deviation compared to countries where this is much less common.

When ranking countries by this standard deviation, we identified two notable discontinuities at the top of the distribution. For nine countries from East Africa and the Middle East, the standard deviations exceeded this second discontinuity. We decided not to exclude all individuals but rather to exclude all six-day blocks³⁴ in which the standardized difference in the share of births within the year between individuals born in any of these nine countries and those born in Sweden exceeded 0.2 on any given day within the block. We chose 0.2 as the threshold, as Rosenbaum and Rubin (1985) consider this the point above which a standardized difference indicates significant imbalance. Even though higher thresholds (0.3 or 0.4) resulted in balanced samples across treatment conditions, we opted for a conservative approach by selecting the analysis sample based on the 0.2 threshold. This exclusion affected 36,570 individuals, or approximately 15% of the research population. This restored the balancing property. In a sensitivity analysis, we used the 0.3 threshold and found that the results were

³⁴ To implement the randomization, the calendar year was divided into a sequence of six-day blocks. The treatment condition was determined by the specific day within the block on which an individual's birthday fell.

very robust. Further details of this trimming procedure can be obtained from the authors upon simple request.

Appendix 2: The Messages Aimed to Trigger Motivation

- *Email 1 supportive [163 words English; 155 Swedish]:*

Hi [#name]

The activity reporting opens on the first of [#month]. Thank you for submitting your report.

Your report gives us a better understanding of how your job search is going. It helps us understand your specific situation, so that we can give you the right support on the way to a job or education.

We understand that it is important for you to achieve your personal goals and to find a job or education that suits you. Arbetsförmedlingen believes in your ability and wants to support you in finding the job or the education that you are looking for.

There are several ways to find a fitting and interesting job. Depending on the type of job you are looking for, you can choose to apply for jobs via job listings, by contacting firms directly or by using your own network.

If you have not visited Platsbanken before, you have the opportunity to search for jobs via this link:

[To Platsbanken](#)

Best regards,

Arbetsförmedlingen

- *Email 1 controlling [155 words English; 156 Swedish]:*

Hi [#name]

The activity reporting opens on the first of [#month] . Remember to submit your activity report on time.

Arbetsförmedlingen monitors your report to check whether you are active enough in your job search. Being active is a requirement for increasing your chances of quickly finding employment or education. You are expected to accept suitable job offers, so that the time that you remain unemployed is kept as short as possible.

If you have unemployment benefits or receive activity support, the benefit rules require that you are actively seeking and applying for suitable jobs or education. Search for jobs through job listings, using your network and contacting companies directly.

*You are obliged to have knowledge of and follow the rules that apply to all job seekers. If you receive benefits and break the rules, you risk a warning or suspension from your benefits.
Visit Platsbanken to look for job openings:*

[To Platsbanken](#)

Best regards,

Arbetsförmedlingen

- Email 2 supportive [129 words English; 128 Swedish]:

Hi [#name]

Thank you for letting us know how your job search is going through your activity reports. The next submission period opens on [month].

Arbetsförmedlingen understands that it is important for you to find a job that fits and feels right for you. We believe in your ability to accomplish your goals. If you want support and inspiration in your job search, you may find tips on our website. If you have questions about your job search or other questions, do not hesitate to mention these in your contact with us.

Having an interesting CV may be key to getting the job you are looking for. Our website might help you to write a CV that suits the jobs you are interested in:

[Write cv](#)

Best regards,

Arbetsförmedlingen

- Email 2 controlling [129 words English; 123 Swedish]:³⁵

Hi [#name]

Starting from the first of [#month] you can submit your activity report. Your report is essential as it allows us to monitor your job search. Remember that each report is screened by Arbetsförmedlingen. Therefore, do not provide incorrect information.

According to the rules, you are obliged to actively search for jobs or education. You are expected to continue to be active, because it is important that you find a job as soon as possible. If you fail to meet the requirements for active job search, you may receive a warning or suspension from the unemployment insurance or activity support benefits.

Having a clear CV is crucial for your chances of getting a job. Our website describes how you write a good CV:

[Write a cv](#)

Best regards,

Arbetsförmedlingen

³⁵ Note that this translation differs slightly from the one provided in the pre-analysis plan, as it seemed to make the message less controlling than in the Swedish version.

- Email 3 supportive [152 words English; 147 Swedish]:

Hi [#name]

Writing your activity report helps you reflect on everything that you have done so far on the way to a job or education. The activity reporting opens on the first of [month]

We understand that you already have done a lot to try to achieve your goals. Reflecting on what you have done so far can help you understand what you are doing well and how you can develop.

For instance, being invited to a job interview means that your job application was convincing. If you have not been invited for an interview, it is important not to take it personally. A missed invitation is a natural part of job search. Remember that you always have a new chance if you apply for other jobs!

There are many good ways to prepare yourself for a job interview. Feel free to read our tips at:

[Before an interview](#)

Best regards,

Arbetsförmedlingen

- Email 3 controlling [147 words English; 142 Swedish]:

Hi [#name]

Arbetsförmedlingen monitors your job search and it is therefore important that you continue submitting your activity reports. The activity reporting opens on the first of [month].

The rules demand that you actively search for suitable jobs or education. You are expected to continually evaluate how you can improve your job search. For instance, it is important to get a sense of why some job applications were unsuccessful and figure out what you may need to change to increase your chances of getting a job.

Writing an informative CV and a thought through application letter may be key for the chance of getting a job interview. You must therefore spend time on going through your application documents. To receive a job offer, you should also prepare yourself thoroughly before all job interviews. Our website describes how you prepare for an interview:

[Before an interview](#)

Best regards,

Arbetsförmedlingen

- Email 4 supportive [143 words English; 131 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month]. Your reports help us understand how your job search is going.

We at Arbetsförmedlingen want to encourage you to continue working towards your personal goals. There are many things that you can do to avoid the stress that sometimes arises when trying to find a job. For example, it may help to preserve the structure of everyday life, engage in activities that provide you with meaning, or seek support from people around you.

We realize that you might experience some obstacles that hinder your job search during the current crisis. You are welcome to talk about it when you have contact with us at Arbetsförmedlingen. We want to help you find solutions and discover new opportunities.

Our website may perhaps inspire you in your job search:

[Find the job](#)

Best regards,

Arbetsförmedlingen

- Email 4 controlling [126 words English; 123 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month]. Each month, Arbetsförmedlingen monitors your activity report. Do not give false or misleading information.

To receive unemployment insurance benefits or activity support, you need to meet certain requirements. For instance, you must be actively searching for jobs and be prepared to take an offered job, even under the current crisis.

As a job seeker, it is important that you maintain a structured time schedule, engage in meaningful activities, and spend time with people close to you. If there are obstacles that hinder your job search, it is important that you report them in your contact with Arbetsförmedlingen.

Read the guidelines at the PES website on how you may proceed:

[Find the job](#)

Best regards,

Arbetsförmedlingen

- *Email 5 supportive [149 words English; 133 Swedish]:*

Hi [#name]

The activity reporting opens on the first of [month]. Your reports help us understand how your job search is going.

Arbetsförmedlingen wants to support you in your job search. We believe in your capacity to accomplish your goals. At the same time, we realize that searching for jobs can be frustrating during these times. Maybe you can find interesting jobs within occupations you have not considered yet.

Talking to friends, family or other persons about any setbacks you may experience may be a good way to boost your confidence and help you to progress.

Many employers often directly ask people they know when they are recruiting. Discussing with others may therefore help you when searching for a job. Your own network is bigger than you can imagine, and it may be worth a lot! More inspiration can be found here:

[Use your own network](#)

Best regards,

Arbetsförmedlingen

- *Email 5 controlling [140 words English; 134 Swedish]:*

Hi [#name]

The activity reporting opens on the first of [month]. Remember to submit your activity report on time.

It is important that you avoid ending up as long-term unemployed. Some employers hesitate to employ people who have been unemployed for a longer time. Being unemployed for a long time may therefore decrease your chances of finding a job. Therefore, it is important that you continue searching for jobs or suitable education.

Remember that you are expected to actively search for jobs. You may also have to broaden your job search and try new occupations. Sometimes this is the only way to get a job.

To find a job, you should also use your network (family, friends, others). This is often crucial, as several jobs are never advertised by employers. Read more here:

[Use your own network](#)

Best regards,

Arbetsförmedlingen

- Email 6 supportive [135 words English; 124 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month]. Your previous reports have helped us gain an understanding of how your job search has gone the last six months.

We want to encourage you to keep on developing your job search. Reflecting on your goals and activities to reach them can help you take the next step in your job search.

Perhaps you may try other ways of finding job openings. Many job seekers are helped by contacting potential employers directly or by turning to temporary employment agencies. If it fits your situation, you may also consider further expanding your job search to other occupations and locations. It may open new opportunities.

You may also choose to revise your application letters. You may find inspiration on our website:

[Write application letters](#)

Best regards,

Arbetsförmedlingen

- Email 6 controlling [132 words English; 129 Swedish]:

Hi [#name]

The activity reporting opens on the first of [#month].

You have now been registered at Arbetsförmedlingen for about six months. During this period, Arbetsförmedlingen has checked your reports and monitored how it is going for you. Actively searching for a job is a requirement for receiving unemployment insurance benefits or activity support.

It is important that you reflect on how you can improve the way you search for jobs. You may have to go through job advertisements, contact potential employers directly or turn to temporary employment agencies. You may also have to expand your job search further, both in terms of occupations and where in the country you can find jobs.

Spend time on revising your application letters. You find more information on our website:

[Write application letters](#)

Best regards,

Arbetsförmedlingen

Appendix 3: Balancing Tables for the Surveys

Table A3.1 presents the balancing test for the sample of respondents to the initial survey. This sample is much smaller (19,806 individuals) than the full research population because (i) less than half of the control group was eligible for this survey, and (ii) the response rate to this survey was relatively low, i.e., 12.9%. The table reveals that this sample is somewhat less balanced across the treatment conditions, with some imbalances, notably in the unemployment history. However, these imbalances stem from the high correlation among these variables and do not indicate selective non-response, as confirmed by the robustness analysis, which includes the conditioning variables retained in the LASSO regressions (see Appendix 7). Note that the constructed measures for initial controlled and autonomous motivation are generally well balanced. There is a slight imbalance for initial controlled motivation between the controlling treatment condition and the control group (p -value $< 10\%$). However, as our main interest is in initial autonomous motivation as moderator (see Sections 3.2 and 5.2.2), this is not problematic. The balancing tables for the intermediate and exit survey are reported in Tables A3.2 and A3.3. While they show some imbalances, they are not a cause for concern, as findings remain robust after including conditioning variables

Table A3.1: Summary Statistics and Differences in Background Characteristics by Group:
Initial Survey-Sample

Variable	(1) Contr. Motiv.	(2) Auton. Motiv.	(3) Control group	(4) Diff. (2)–(1)	(5) Diff. (3)–(1)	(6) Diff. (3)–(2)
Age	39.800	39.587	39.226	-0.213	-0.574**	-0.361
Male	0.488	0.491	0.481	0.003	-0.007	-0.010
Unemployment benefits	0.796	0.792	0.790	-0.005	-0.006	-0.002
Health disability	0.026	0.024	0.022	-0.002	-0.004	-0.002
Matchable	0.938	0.935	0.937	-0.003	-0.001	0.002
Education level						
High school	0.412	0.408	0.409	-0.004	-0.003	0.001
College	0.440	0.444	0.451	0.004	0.011	0.007
Place of birth						
Nordic country	0.019	0.023	0.021	0.004	0.002	-0.001
Western Europe	0.051	0.050	0.050	-0.001	-0.001	0.000
Outside Western Europe	0.275	0.281	0.274	0.006	-0.001	-0.008
Unemployment days						
Year t-1	40.538	38.927	39.722	-1.611**	-0.816	0.795
Year t-2	36.702	35.223	34.209	-1.479	-2.492	-1.013
Year t-3	32.840	32.889	30.494	0.049	-2.346	-2.395*
Year t-4	33.479	33.788	30.181	0.309	-3.298**	-3.607**
# of unemployment spells						
Year t-1	0.159	0.150	0.156	-0.010	-0.004	0.006
Year t-2	0.252	0.257	0.244	0.004	-0.009	-0.013
Year t-3	0.236	0.236	0.221	0.000	-0.014*	-0.015*
Year t-4	0.223	0.221	0.208	-0.003	-0.015*	-0.013
# of programs last 4 years						
Labour market education	0.008	0.010	0.008	0.001	-0.001	-0.002
Preparatory education	0.021	0.026	0.018	0.005	-0.002	-0.007**
Labour market training	0.012	0.015	0.012	0.003	0.001	-0.002
Subsidized employment	0.035	0.028	0.028	-0.007*	-0.007*	0.000
Initial motivation						
Autonomous	4.203	4.216	4.215	0.013	0.012	-0.001
Controlled	3.144	3.127	3.100	-0.017	-0.044*	-0.026
Observations	6 478	6 660	6 680	13 138	13 146	13 328

Notes: Significance Codes: ***, 0.01, **, 0.05, *, 0.1. The research population excludes some individuals born in nine East African and Middle Eastern countries as discussed in Appendix 1. In this table, only the respondents to the initial survey are retained. As mentioned in the pre-analysis plan (version 3.0), half of the control group could not be used between May 20 and October 31, 2020, which involves 14,912 individuals. To account for this issue, we weighted the retained control individuals in that period by the inverse of their share in the counterfactual control group that would have been used in the absence of this issue; specifically we weighted them by 1.977454. The reported number of observations are unweighted.

Table A3.2: Summary Statistics and Differences in Background Characteristics by Group:
Intermediate Survey-Sample

Variable	(1) Contr. Motiv.	(2) Auton. Motiv.	(3) Control group	(4) Diff. (2)–(1)	(5) Diff. (3)–(1)	(6) Diff. (3)–(2)
Age	39.683	39.483	39.258	-0.200	-0.425	-0.224
Male	0.509	0.511	0.513	0.002	0.004	0.002
Unemployment benefits	0.837	0.832	0.845	-0.006	0.007	0.013*
Health disability	0.028	0.031	0.031	0.003	0.003	0.000
Matchable	0.945	0.942	0.938	-0.003	-0.006	-0.004
Education level						
High school	0.417	0.403	0.410	-0.014	-0.007	0.007
College	0.414	0.432	0.432	0.019*	0.018*	-0.000
Place of birth						
Nordic country	0.021	0.021	0.020	0.001	-0.001	-0.002
Western Europe	0.049	0.042	0.049	-0.007*	-0.000	0.007*
Outside Western Europe	0.279	0.286	0.273	0.007	-0.006	-0.013
Unemployment days						
Year t-1	43.194	43.675	43.010	0.481	-0.185	-0.666
Year t-2	42.396	39.858	39.250	-2.538	-3.146*	-0.608
Year t-3	43.320	40.263	37.707	-3.057*	5.614***	-2.557
Year t-4	40.960	41.890	39.649	0.930	-1.311	-2.241
# of unemployment spells						
Year t-1	0.183	0.175	0.170	-0.008	-0.013*	-0.005
Year t-2	0.300	0.287	0.272	-0.013	0.028***	-0.014
Year t-3	0.293	0.277	0.263	-0.016	0.030***	-0.014
Year t-4	0.281	0.265	0.265	-0.015	-0.016	-0.001
# of programs last 4 years						
Labour market education	0.009	0.009	0.013	-0.000	0.003	0.003
Preparatory education	0.022	0.030	0.020	0.007*	-0.002	0.010***
Labour market training	0.015	0.016	0.017	0.001	0.001	0.001
Subsidized employment	0.044	0.035	0.043	-0.009*	-0.001	0.007
Observations	5 411	5 315	5 367	10 726	10 778	10 682

Notes: Significance Codes: ***: 0.01, **: 0.05, *: 0.1. The research population excludes some individuals born in nine East African and Middle Eastern countries as discussed in Appendix 1. In this table, only the respondents to the initial survey are retained. As mentioned in the pre-analysis plan (version 3.0), half of the control group could not be used between May 20 and October 31, 2020, which involves 14,912 individuals. To account for this issue, we weighted the retained control individuals in that period by the inverse of their share in the counterfactual control group that would have been used in the absence of this issue; specifically we weighted them by 1.977454. The reported number of observations are unweighted.

Table A3.3: Summary Statistics and Differences in Background Characteristics by Group:
Exit Survey-Sample

Variable	(1) Contr. Motiv.	(2) Auton. Motiv.	(3) Control group	(4) Diff. (2)–(1)	(5) Diff. (3)–(1)	(6) Diff. (3)–(2)
Age	36.643	36.475	36.438	-0.169	-0.206	-0.037
Male	0.459	0.477	0.474	0.018	0.015	-0.003
Unemployment benefits	0.888	0.884	0.892	-0.004	0.004	0.008
Health disability	0.005	0.005	0.007	0.000	0.002	0.002
Matchable	0.987	0.982	0.985	-0.005*	-0.003	0.002
Education level						
High school	0.440	0.459	0.458	0.019	0.018	-0.001
College	0.495	0.475	0.483	-0.019	-0.012	0.007
Place of birth						
Nordic country	0.019	0.018	0.019	-0.001	-0.000	0.001
Western Europe	0.045	0.042	0.043	-0.003	-0.002	0.001
Outside Western Europe	0.170	0.171	0.163	0.001	-0.007	-0.008
Unemployment days						
Year t-1	40.096	40.811	39.218	0.715	-0.878	-1.593
Year t-2	28.745	31.655	26.253	2.910	-2.492	5.402***
Year t-3	27.364	28.498	24.916	1.134	-2.448	-3.582**
Year t-4	26.506	28.359	25.372	1.853	-1.134	-2.987*
# of unemployment spells						
Year t-1	0.170	0.169	0.154	-0.001	-0.016*	-0.015
Year t-2	0.252	0.274	0.238	0.022*	-0.014	0.037***
Year t-3	0.233	0.237	0.216	0.005	-0.017	-0.021*
Year t-4	0.230	0.226	0.205	-0.004	-0.025**	-0.020*
# of programs last 4 years						
Labour market education	0.010	0.010	0.012	0.000	0.003	0.003
Preparatory education	0.015	0.018	0.012	0.003	-0.002	-0.006
Labour market training	0.010	0.010	0.011	0.000	0.001	0.001
Subsidized employment	0.030	0.029	0.020	-0.000	-0.010**	-0.010**
Observations	3 567	3 542	3 405	7 109	6 972	6 947

Notes: Significance Codes: ***: 0.01, **: 0.05, *: 0.1. The research population excludes some individuals born in nine East African and Middle Eastern countries as discussed in Appendix 1. In this table, only the respondents to the initial survey are retained. As mentioned in the pre-analysis plan (version 3.0), half of the control group could not be used between May 20 and October 31, 2020, which involves 14,912 individuals. To account for this issue, we weighted the retained control individuals in that period by the inverse of their share in the counterfactual control group that would have been used in the absence of this issue; specifically we weighted them by 1.977454. The reported number of observations are unweighted.

Appendix 4: Procedure for the Construction of the Motivation Scales

Our measure of controlled and autonomous motivation is based on Da Motta Veiga & Gabriel (2016) who adapted Moran et al.'s (2012) theory-based motivation scales to a job search context. Similar to da Motta Veiga and Gabriel's (2016) revisions, we adapted some of the wording to our particular research context (i.e., unemployed job seekers in Sweden versus U.S. job seeking students), in close consultation with experts at the PES. The adjustment and the predetermined procedure to select the items for the measures of initial and intermediate job motivation is the same and described in Section 3.4 (p. 20) and Appendix 2 (p. 38) of the pre-analysis plan. Here we provide a summary of the results of this procedure. Detailed output can be obtained from the authors upon request.

Participants were asked to indicate how strongly they agree with ten statements about why they are searching for a job on a 5-point scale ranging from 1 (*completely disagree*) to 5 (*completely agree*). Initially, we intended to measure controlled job search motivation with 6 items and autonomous job search motivation with 4 items. However, based on the reliability and factor analyses described below, in the final measure only two items were retained for controlled job search motivation (c-d), and three items for autonomous job search motivation (g-i).

Controlled job search motivation
a. Because other people want me to find a job
b. Because I need to start getting paid
c. Because I would feel guilty if I did not search for a job
d. Because I would feel ashamed if I did not find a job
e. Because Arbetsförmedlingen demands it
f. Because I am afraid of having my benefits cut if I do not search for a job
Autonomous job search motivation
g. Because my job search is important to me
h. Because finding a job is important to me
i. Because it is interesting to search for fitting jobs
j. Because job seeking is fun

A4.1 Results of the procedure for the initial survey:

In the initial survey, the total number of observations (N) is 20,658. After conducting a listwise deletion due to missing values, the valid sample size is reduced to 16,665 for controlled job search motivation, with the six items yielding a Cronbach's alpha (α) of 0.681. For the four autonomous job search motivation items, α is 0.710 based on a valid sample size of 18,455.

The α 's cannot be further improved by deleting any item. Whereas these reliability estimates reflect satisfactory internal consistency of the items, we also conducted a confirmatory factor analysis (CFA) to check whether our expected two-factor model fitted the data. In this model, each item is specified to load only on the motivation factor it was intended to measure (controlled or autonomous), and not on the other one. Initial CFA results suggested that a two-factor model with these 10 items loaded on their respective factor showed a very poor fit: $\chi^2(34) = 17751.509$, $p < .001$, $CFI = 0.578$, $SRMR = 0.118$. The acceptable fit indices are CFI close to 0.95 or higher, and $SRMR$ close to 0.08 or lower (Hu & Bentler, 1998). Consequently, items with lowest factor loadings were successively removed from this two-factor model until the desired thresholds are reached. The final two-factor model with acceptable fit emerged with factor 1 (i.e., controlled job search motivation) with 2 items: (c-d) and factor 2 (i.e., autonomous job search motivation) with 3 items (g-i): $\chi^2(4) = 415.180$, $p < .001$, $CFI = 0.979$, $SRMR = 0.023$. This final two-factor model with 5 items fitted the data significantly better than the original two-factor model with all items included, $\Delta\chi^2(30) = 17336$, $p < .001$. Cronbach's alpha for the final controlled job search motivation (2 items) is 0.717 and for final autonomous job search motivation (3 items) 0.667.

A4.2 Results of the procedure for the intermediate survey

The intermediate survey has total sample size (N) of 16,630. After deleting missing values on all variables, the valid sample size is reduced to 13,886 for controlled job search motivation, yielding a Cronbach's alpha (α) of 0.657. For autonomous job search motivation, the α is 0.680 based on valid sample size of 15,040. The α 's cannot be further improved by deleting any item in these variables. However, similar to the initial survey CFA results, the two-factor model with these 10 items being loaded on their intended factor showed a poor fit: $\chi^2(34) = 11351.279$, $p < .001$, $CFI = 0.619$, $SRMR = 0.128$. Therefore, the final two-factor model suggested in the initial survey results was also tested here with factor 1 (i.e., controlled job search motivation) with 2 items (c-d) and factor 2 (i.e., autonomous job search motivation) with 3 items (g-i).³⁶ This model fitted the data really well, $\chi^2(4) = 193.284$, $p < .001$, $CFI =$

³⁶ We used the final two-factor model (controlled job search motivation with c and d and autonomous job search motivation with g, h, and i) from the initial survey results to test the fit of our intermediate survey data. This approach was chosen because in the intermediate survey data, the final solution emerging after successively removing one item at a time with the lowest factor loading, ultimately resulted in the occurrence of a Heywood case. Specifically, a Heywood case arose in our analysis when factor 1 i.e., controlled job search motivation is left at the end of this successive deletion process with only the 2 PES items i.e., e and f. In this case, the variance

0.987, $SRMR = 0.018$, and significantly better than the original one with all 10 items, $\Delta\chi^2(30) = 11158$, $p < .001$. Cronbach's alpha for the final controlled job search motivation (2 items) is 0.684 and for final autonomous job search motivation (3 items) 0.658.

of e becomes negative. A Heywood case in (confirmatory) factor analysis occurs when a solution produces improper or inadmissible results such as negative variance estimates or variance estimates exceeding one (Farooq, 2022). This can occur due to several reasons such as model misspecification, small sample sizes, multicollinearity, poorly measured variables, missing data, or the presence of outliers in the dataset among other reasons. Fixing the negative variance to zero or a specific value is the most widely used solution to fix Heywood cases. However, there needs to be strong theoretical reasons behind that (Kline, 2016). In view of this, we opted to go with the solution suggested by the initial survey, rather than the model with a Heywood case as suggested by the intermediate survey data, as it is advised to avoid interpreting/reporting a model with a Heywood case. Also note that the Heywood case also appears in the initial survey results if we measure factor 1 i.e., controlled job search motivation with only the 2 PES items.

Appendix 5: Procedure for the Construction of the Job Search Quality Scale

To assess the quality of people's job search process we used a measure of metacognitive activities during job search developed by Turban et al.(2009). This involves self-regulation of job search activities such as setting goals, developing plans, and monitoring and analyzing progress toward goal accomplishment. In close consultation with the PES, this measure was slightly modified to take into account the specific local context, the intended target group, and the PES guidelines. The adjustment and the predetermined procedure to select the items is described in Section 3.4 (p. 21) and Appendix 3 (p. 44) of the pre-analysis plan. Here we provide a summary of the results of this procedure. Detailed output can be obtained from the authors upon request.

Participants were asked to indicate how strongly they agree with five statements (see below items a to e) about how they approached their job search or up till they obtained a job, on a scale from 1 (*Completely disagree*) to 5 (*Completely agree*). The Cronbach's alpha (α) for job search quality is 0.820 based on a valid sample size of 14,784, suggesting good internal consistency of the scale items.

Job search quality
a. I set personal goals to guide my job search activities
b. I plan when and how I search for jobs
c. I reflect on what progress I have made in my job search
d. I think about how I may improve my job search
e. I think about how best to present myself in my job applications

Appendix 6: Procedure for the Construction of the Job Quality Scale

Job quality is measured using both three separate scales (job satisfaction, perceived fit, and stay intention) and a composite score which is just the mean of the three mentioned scales. Participants were asked to indicate how strongly they agree with a number of statements about the job they obtained. All items were rated on a 5-point scale ranging from 1 (*Completely disagree*) to 5 (*Completely agree*). Job satisfaction was assessed with the one-item scale proposed by Cammann et al. (1983): “I am satisfied with my job”. The satisfaction and fit measures are based on Wanberg et al. (2002), and the stay intention measure is based on Colarelli (1984). The reader can find this description in Section 3.4 (p. 22) and in Appendix 4 (p. 51) of the pre-analysis plan.

Appendix 7: Sensitivity of Main Findings when Conditioning Variables Based on LASSO Regressions are Included

Here we report a sensitivity analysis for our main findings in which the conditioning variables are selected according the double machine learning LASSO method of Belloni et al. (2014). As described in Section 3.5 of the pre-analysis plan (p.23), we include quadratic terms of non-binary variables and first-order interaction terms in the set of potential variables. We only report the estimates of the main treatment effects in the tables below. Full estimation results can be obtained from the authors upon request.

Table A7.1: Effects on the Probability to Report and on Survey Scales of Motivation Based on LASSO Regressions (Table 3 in the main text without conditioning variables)

	Monthly share submitting activity reports				Motivation: intermediate survey	
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)	Controlled motivation (5)	Autonomous motivation (6)
Treatment						
Controlling $\hat{\beta}_c$	0.0221*** (0.002)	0.0103*** (0.002)	0.0142*** (0.002)	0.0065*** (0.002)	0.0238 (0.025)	0.0025 (0.015)
Supportive $\hat{\beta}_a$	0.0189*** (0.002)	0.0063*** (0.002)	0.0078*** (0.002)	0.0049** (0.002)	0.0082 (0.025)	-0.0069 (0.015)
Observations	200 720	200 720	200 720	200 720	16 093	16 093
Mean control gr.	0.589	0.456	0.534	0.377	3.187	4.190

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) with background characteristics using the LASSO procedure as described above. Columns 1–4 report show effects on the monthly share of submitted activity reports over various intervals. Columns 5–6 on motivational scales, based on survey data described in detail in Appendix 4. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A7.2: Effects on Number of Job Applications and Job Finding Based on LASSO Regressions (Table 4 in the main text without conditioning variables)

	Job applications		Job finding			
	Report 2–4	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment						
Controlling $\hat{\beta}_c$	-0.0064 (0.054)	0.0067 (0.048)	0.0007 (0.003)	0.0014 (0.002)	-0.0010 (0.002)	-0.0895 (0.640)
Supportive $\hat{\beta}_a$	-0.1248** (0.054)	-0.0959** (0.048)	-0.0025 (0.003)	0.0003 (0.002)	-0.0040** (0.002)	0.4187 (0.639)
Observations	125 357	131 518	200 720	200 720	200 720	200 720
Mean control gr.	8.734	8.286	0.462	0.631	0817	184.2

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) with background characteristics using the LASSO procedure as described above. Columns 1–2 show the effects on the average number of job applications per month in months 2–4 and 2–7; Columns 3–5 on job finding within 4, 7, and 12 months; and Column 6 on the number of days unemployed within one year of unemployment onset. Standard errors in parentheses. ***, 0.01, **, 0.05, *, 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

Table A7.3: Effects on Job Search Quality Based on LASSO Regressions (Table A8.1 in Appendix 8 without conditioning variables)

	Average # of spontaneous job applications			Job search quality (survey)	Average # of interviews		
	Report 2-4	Report 5-7	Report 2-7		Report 2-4	Report 5-7	Report 2-7
	(1)	(2)	(3)		(5)	(6)	(7)
Treatment							
Controlling $\hat{\beta}_c$	-0.0030 (0.011)	0.0008 (0.011)	-0.0016 (0.009)	0.0301** (0.014)	-0.0081 (0.007)	0.0047 (0.006)	-0.0027 (0.006)
Supportive $\hat{\beta}_a$	0.0105 (0.011)	0.0058 (0.011)	0.0111 (0.009)	0.0024 (0.014)	-0.0098 (0.007)	0.0015 (0.006)	-0.0043 (0.006)
Observations	125 357	85 769	131 518	14 784	125 357	85 769	131 518
Mean control gr.	0.619	0.533	0.582	4.093	0.525	0.372	0.506

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) with background characteristics using the LASSO procedure as described above. Columns 1–3 show the effects on the average number of spontaneous job applications per month during months 2–4, 5–7, and 2–7, as reported in the activity reports; Column 4 on job search quality as measured in the intermediate and exit surveys using the measure described in Appendix 5; and Columns 5–7 on the average number of interviews during months 2–4, 5–7, and 2–7, as reported in the activity reports. Standard errors in parentheses. ***, 0.01, **, 0.05, *, 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A8.2: Effects on Job Quality Based on LASSO Regressions (Table A8.2 in Appendix 8 without condition variables)

	Non-unemployment duration			Survey based job quality measures			
	> 3 months	> 6 months	> 12 months	Job satisfactio n	Perceived fit	Stay intention	Composite score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling $\hat{\beta}_c$	0.0000 (0.003)	-0.0012 (0.002)	-0.0010 (0.001)	-0.0482** (0.023)	-0.0278 (0.024)	-0.0093 (0.029)	-0.0284 (0.022)
Supportive $\hat{\beta}_a$	0.0009 (0.003)	0.0023 (0.002)	0.0004 (0.001)	-0.0184 (0.023)	-0.0104 (0.024)	0.0164 (0.029)	-0.0042 (0.022)
Observations	127 148	127 148	127 148	10 514	10 514	10 514	10 514
Mean control gr.	0.251	0.154	0.0434	4.382	4.092	4.053	4.175

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) with background characteristics using the LASSO procedure as described above. Columns 1–3 show the effects on employment duration as measured by the first period of non-unemployment exceeding 3, 6, and 12 months (where only exits occurring within the first 7 months of unemployment are considered); Column 4–7 on the exit survey based measures job quality the construction of which is reported in Appendix 6: on job satisfaction, perceived fit, job stay intention, and on a composite score that is the average of the preceding three measures. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Appendix 8: Findings of the Effects on Job Search Quality and Job Quality

Table A8.1: Effects on Job Search Quality

	Average # of spontaneous job applications			Job search quality (survey) (4)	Average # of interviews		
	Report 2-4 (1)	Report 5-7 (2)	Report 2-7 (3)		Report 2-4 (5)	Report 5-7 (6)	Report 2-7 (7)
Treatment							
Controlling $\hat{\beta}_c$	-0.0032 (0.011)	0.0014 (0.012)	-0.0014 (0.009)	0.0291** (0.014)	-0.0089 (0.007)	0.0038 (0.007)	-0.0032 (0.007)
Supportive $\hat{\beta}_a$	0.0101 (0.011)	0.0053 (0.012)	0.0111 (0.009)	0.0038 (0.014)	-0.0092 (0.007)	0.0015 (0.007)	-0.0033 (0.007)
Observations	125 357	85 769	131 518	14 784	125 357	85 769	131 518
Mean control gr.	0.619	0.533	0.582	4.093	0.525	0.372	0.506
Auto. vs Contr.				-			
$= \hat{\beta}_a - \hat{\beta}_c$	0.0133 (0.012)	0.0039 (0.013)	0.0125 (0.010)	0.0253* (0.014)	-0.0003 (0.008)	-0.0023 (0.007)	-0.0000 (0.007)
Observations	70 363	48 537	73 796	9 850	70 363	48 537	73 796
Mean controlling tr.	0.616	0.534	0.580	4.122	0.517	0.375	0.503

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show the effects on the average number of spontaneous job applications per month during months 2-4, 5-7, and 2-7, as reported in the activity reports; Column 4 on job search quality as measured in the intermediate and exit surveys using the measure described in Appendix 5; and Columns 5-7 on the average number of interviews during months 2-4, 5-7, and 2-7, as reported in the activity reports. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A8.2: Effects on Job Quality

	Non-unemployment duration			Survey based job quality measures			
	> 3 months	> 6 months	> 12 months	Job satisfactio n	Perceived fit	Stay intention	Composite score
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling $\hat{\beta}_c$	0.0004 (0.003)	-0.0011 (0.002)	-0.0010 (0.001)	-0.0482** (0.023)	-0.0278 (0.024)	-0.0093 (0.029)	-0.0284 (0.022)
Supportive $\hat{\beta}_a$	0.0011 (0.003)	0.0025 (0.002)	0.0004 (0.001)	-0.0184 (0.023)	-0.0104 (0.024)	0.0164 (0.029)	-0.0042 (0.022)
Observations	127 148	127 148	127 148	10 514	10 514	10 514	10 514
Mean control gr.	0.251	0.154	0.0434	4.382	4.092	4.053	4.175
Auto. vs Contr.							
$= \hat{\beta}_a - \hat{\beta}_c$	0.0007 (0.003)	0.0036 (0.003)	0.0014 (0.002)	0.0297 (0.024)	0.0174 (0.025)	0.0257 (0.029)	0.0243 (0.022)
Observations	70 635	70 635	70 635	7 109	7 109	7 109	7 109
Mean controlling tr.	0.251	0.153	0.0424	4.333	4.064	4.044	4.147

Notes: Regressions estimates for the analyses sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show the effects on employment duration as measured by the first period of non-unemployment exceeding 3, 6, and 12 months (where only exits occurring within the first 7 months of unemployment are considered); Column 4-7 on the exit survey based measures job quality the construction of which is reported in Appendix 6: on job satisfaction, perceived fit, job stay intention, and on a composite score that is the average of the preceding three measures. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Appendix 9: Interaction Effects of Initial Motivation (Tables A9.1-A9.4) and of Other Moderators (Tables A9.5-A9.)

Table A9.1: Moderating Effects of Initial Autonomous Motivation on the Monthly Share Submitting Activity Reports

	Monthly share submitting activity reports			
	1st report	Reports	Reports	Reports
	(1–6)	(1–3)	(1–3)	(4–6)
	(1)	(2)	(3)	(4)
Treatment				
Controlling tr.	-0.0712* (0.037)	-0.0222 (0.033)	0.0158 (0.033)	-0.0601 (0.040)
Supportive tr.	-0.0566 (0.037)	-0.0214 (0.033)	0.0110 (0.033)	-0.0538 (0.040)
Initial autonomous	0.0001 (0.006)	0.0028 (0.005)	0.0110** (0.005)	-0.0053 (0.006)
Controlling tr. x initial autonomous	0.0250*** (0.009)	0.0104 (0.008)	0.0031 (0.008)	0.0177* (0.009)
Supportive tr. x initial autonomous	0.0218** (0.009)	0.0090 (0.008)	0.0027 (0.008)	0.0152 (0.009)
Observations	19 806	19 806	19 806	19 806
Mean control group	0.756	0.566	0.670	0.461

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 show effects on the monthly share submitting activity reports over various time periods. Interaction effects of controlling and supportive treatment with initial autonomous motivation. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A9.2: Moderating Effects of Initial Autonomous Motivation on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling tr.	-0.7146 (0.866)	0.5992 (0.831)	-0.2850 (0.774)	0.0619 (0.044)	0.0764* (0.043)	0.0529 (0.035)	-16.5245 (11.007)
Supportive tr.	0.2382 (0.869)	0.7374 (0.832)	0.4270 (0.777)	0.0174 (0.044)	0.0429 (0.043)	0.0082 (0.035)	-6.3988 (11.039)
Initial autonomous	0.8354*** (0.137)	0.8216*** (0.129)	0.7881*** (0.122)	0.0105 (0.007)	0.0155** (0.007)	0.0105* (0.005)	-3.0812* (1.712)
Controlling tr. x initial autonomous	0.0042 (0.201)	-0.2531 (0.194)	-0.0742 (0.180)	-0.0165 (0.010)	-0.0190* (0.010)	-0.0113 (0.008)	4.2825* (2.566)
Supportive tr. x initial autonomous	-0.2072 (0.202)	-0.2887 (0.194)	-0.2315 (0.180)	-0.0066 (0.010)	-0.0107 (0.010)	-0.0022 (0.008)	2.0303 (2.570)
Observations	14 855	10 201	15 297	19 806	19 806	19 806	19 806
Mean control group	9.501	7.752	8.870	0.428	0.614	0.806	188.6

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. Interaction effects of controlling and supportive treatment with initial autonomous motivation. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

Table A9.3: Moderating Effects of Initial Controlled Motivation on the Monthly Share Submitting Activity Reports

	Monthly share submitting activity reports			
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)
Treatment				
Controlling tr.	0.0189 (0.018)	0.0214 (0.016)	0.0313* (0.016)	0.0115 (0.020)
Supportive tr.	0.0485*** (0.018)	0.0154 (0.016)	0.0363** (0.016)	-0.0055 (0.019)
Initial controlled	0.0105*** (0.004)	0.0022 (0.003)	0.0075** (0.003)	-0.0030 (0.004)
Controlling tr. x initial controlled	0.0047 (0.005)	0.0000 (0.005)	-0.0010 (0.005)	0.0010 (0.006)
Supportive tr. x initial controlled	-0.0043 (0.005)	0.0003 (0.005)	-0.0045 (0.005)	0.0051 (0.006)
Observations	19 806	19 806	19 806	19 806
Mean control group	0.756	0.566	0.670	0.461

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 show effects on the monthly share submitting activity reports over various time periods. Interaction effects of controlling and supportive treatment with initial controlled motivation. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A9.4: Moderating Effects of Initial Controlled Motivation on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling tr.	-0.4965 (0.419)	-0.1930 (0.406)	-0.3038 (0.375)	0.0015 (0.022)	0.0127 (0.021)	-0.0128 (0.017)	0.6445 (5.431)
Supportive tr.	-0.3417 (0.416)	0.0703 (0.403)	-0.1875 (0.373)	0.0011 (0.021)	0.0270 (0.021)	0.0086 (0.017)	-2.8892 (5.367)
Initial controlled	0.4049*** (0.082)	0.3112*** (0.080)	0.3936*** (0.073)	0.0044 (0.004)	0.0071* (0.004)	0.0026 (0.003)	-1.6980 (1.053)
Controlling tr. x initial controlled	-0.0721 (0.123)	-0.0871 (0.120)	-0.1001 (0.110)	-0.0029 (0.006)	-0.0053 (0.006)	0.0057 (0.005)	0.2990 (1.598)
Supportive tr. x initial controlled	-0.0977 (0.122)	-0.1719 (0.119)	-0.1180 (0.110)	-0.0038 (0.006)	-0.0094 (0.006)	-0.0030 (0.005)	1.6281 (1.583)
Observations	14 855	10 201	15 297	19 806	19 806	19 806	19 806
Mean control group	9.501	7.752	8.870	0.428	0.614	0.806	188.6

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. Interaction effects of controlling and supportive treatment with initial controlled motivation. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

Table A9.5: Moderating Effects of the Local Unemployment Rate on the Monthly Share Submitting Activity Reports

	Monthly share submitting activity reports			
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)
Treatment				
Controlling tr.	0.0230*** (0.005)	0.0102*** (0.004)	0.0136*** (0.004)	0.0067 (0.004)
Supportive tr.	0.0125*** (0.005)	0.0002 (0.004)	0.0012 (0.004)	-0.0008 (0.004)
High local unemployment rate	-0.0032 (0.003)	0.0134*** (0.003)	0.0049* (0.003)	0.0220*** (0.003)
Controlling tr. x high local unemployment rate	-0.0000 (0.006)	0.0016 (0.005)	0.0022 (0.005)	0.0010 (0.005)
Supportive tr. x high local unemployment rate	0.0113** (0.006)	0.0105** (0.005)	0.0113** (0.005)	0.0097* (0.005)
Observations	200 548	200 548	200 548	200 548
Mean control group	0.589	0.453	0.535	0.372

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 show effects on the monthly share submitting activity reports over various time periods. High local unemployment rate is defined as above the median. Interaction effects of controlling and supportive treatment with indicator of high local unemployment rate. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A9.6: Moderating Effects of the Local Unemployment Rate on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling tr.	-0.0409 (0.098)	-0.0644 (0.104)	-0.0490 (0.088)	0.0057 (0.005)	0.0045 (0.005)	-0.0029 (0.004)	-0.9650 (1.219)
Supportive tr.	-0.0971 (0.099)	-0.0885 (0.104)	-0.0730 (0.089)	0.0040 (0.005)	0.0035 (0.005)	-0.0041 (0.004)	-0.2119 (1.223)
High local unemployment rate	0.5901*** (0.069)	0.5654*** (0.072)	0.5449*** (0.062)	- (0.003)	- (0.003)	- (0.003)	12.6925*** (0.854)
Controlling tr. x high local unemployment rate	0.0448 (0.118)	0.0894 (0.124)	0.0824 (0.106)	-0.0077 (0.006)	-0.0055 (0.006)	0.0022 (0.005)	1.6491 (1.473)
Supportive tr. x high local unemployment rate	-0.0507 (0.119)	0.0192 (0.125)	-0.0337 (0.107)	-0.0099* (0.006)	-0.0052 (0.006)	-0.0001 (0.005)	1.0155 (1.474)
Observations	125 255	85 705	131 410	200 548	200 548	200 548	200 548
Mean control group	8.202	7.166	7.761	0.465	0.637	0.825	182.3

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. High local unemployment rate is defined as above the median. Interaction effects of controlling and supportive treatment with indicator of high local unemployment rate. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***, **: 0.01, *: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

Table A9.7: Moderating Effects of the Labor Market Attachment on the Monthly Share Submitting Activity Reports

	Monthly share submitting activity reports			
	1st report (1)	Reports (1–6) (2)	Reports (1–3) (3)	Reports (4–6) (4)
Treatment				
Controlling tr.	0.0302*** (0.004)	0.0128*** (0.003)	0.0185*** (0.003)	0.0070** (0.003)
Supportive tr.	0.0195*** (0.004)	0.0069** (0.003)	0.0090*** (0.003)	0.0049 (0.003)
Strong attachment	0.0648*** (0.003)	0.0533*** (0.002)	0.0596*** (0.003)	0.0471*** (0.003)
Controlling tr. x strong attachment	-0.0150*** (0.005)	-0.0033 (0.004)	-0.0072 (0.004)	0.0006 (0.005)
Supportive tr. x strong attachment	0.0018 (0.005)	0.0012 (0.004)	0.0003 (0.004)	0.0022 (0.005)
Observations	200 720	200 720	200 720	200 720
Mean control group	0.589	0.453	0.535	0.372

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–4 show effects on the monthly share submitting activity reports over various time periods. Strong labor market attachment is defined as having experienced less than the median number of days in unemployment during the last four years prior to registration. Interaction effects of controlling and supportive treatment with indicator of strong labor market attachment. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***: 0.01, **: 0.05, *: 0.1 denote significance levels at the 1%, 5%, and 10%, respectively.

Table A9.8: Moderating Effects of the Labor Market Attachment on Job Search and Job Finding

	Job applications			Job finding			
	Report 2–4	Report 5–7	Report 2–7	4 months	7 months	12 months	Days unemployed 12 months
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment							
Controlling tr.	-0.0649 (0.078)	-0.0049 (0.083)	-0.0348 (0.070)	0.0010 (0.004)	0.0003 (0.004)	-0.0009 (0.003)	-0.1947 (0.957)
Supportive tr.	-0.1373* (0.078)	-0.0753 (0.083)	-0.0956 (0.070)	-0.0028 (0.004)	0.0027 (0.004)	-0.0016 (0.003)	-0.1660 (0.954)
Strong attachment	0.3262*** (0.064)	0.2846*** (0.066)	0.2653*** (0.057)	0.0706*** (0.003)	0.0538*** (0.003)	0.0338*** (0.002)	22.2693*** (0.790)
Controlling tr. x strong attachment	0.1102 (0.109)	0.0035 (0.114)	0.0836 (0.098)	-0.0005 (0.005)	0.0016 (0.005)	-0.0007 (0.004)	0.4876 (1.363)
Supportive tr. x strong attachment	0.0173 (0.109)	0.0047 (0.114)	0.0030 (0.098)	-0.0002 (0.005)	-0.0061 (0.005)	-0.0055 (0.004)	1.4177 (1.361)
Observations	125 357	85 769	131 518	200 720	200 720	200 720	200 720
Mean control group	8.202	7.166	7.761	0.465	0.637	0.825	182.3

Notes: Regression estimates of treatment effects for the analysis sample described in Section 2.2, using the model in Equation (1) without background characteristics. Columns 1–3 show effects on the average number of job applications per month in months 2–4, 5–7, and 2–7; Columns 4–6 on job finding within 4, 7 and 12 months; and Column 7 on the number of days unemployed within one year of unemployment onset. Strong labor market attachment is defined as having experienced less than the median number of days in unemployment during the last four years prior to registration. Interaction effects of controlling and supportive treatment with indicator of strong labor market attachment. The mean of the monthly share submitting activity reports in the control group reported in the last line. Standard errors in parentheses. ***, 0.01, **, 0.05, *, 0.1 denote significance levels at the 1%, 5%, and 10%, respectively. Primary outcomes are in bold.

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