Essays on Job Search and Retraining

by

Aicha Ben Dhia

Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Economics

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

September 2020

Author . . . .
Department of Economics
August 14, 2020

Certified by . . . .
Esther Duflo
Abdul Latif Jameel Professor of Poverty Alleviation and Development
Thesis Supervisor

Certified by . . . .
Frank Schilbach
Gary Loveman Career Development Associate Professor of Economics
Thesis Supervisor

Accepted by . . . .
Amy Finkelstein
John & Jennie S. MacDonald Professor of Economics
Chairman, Department Committee on Graduate Theses

Essays on Job Search and Retraining

by

Aicha Ben Dhia

Submitted to the Department of Economics
on August 14, 2020, in partial fulfillment of the
requirements for the degree of
Doctor of Philosophy in Economics

Abstract

This thesis comprises three essays in empirical labor economics. Broadly, the essays provide evidence on the existence and the effects of information barriers in situations of job search and retraining.

Chapter 1 (coauthored with Esther Mbih) begins from the observation that little is known about how job seekers decide to enroll in a training program. Decisions related to job training might be undermined by informational gaps, especially about program costs, enrollment procedures, and expectations of reemployment chances. The paper reports the results of a low-cost intervention aimed at testing for the existence of misinformation about training costs and returns, and its impact on enrollment. Partnering with the French Public Employment Services and the largest training provider in France, we sent 50,000 emails advertising training opportunities to job seekers in four regions of France in late summer 2016. We randomly added short messages on training costs, registration procedures, and training returns to the basic email template. A baseline survey reveals misperceptions about financial aspects of training participation among more than half of job seekers: they either believe that they need pay to participate in a training (45%) and/or that their unemployment benefits would be affected (30%). Further, half of respondents perceive enrollment procedures as complex or very complex. We find that receiving an email with a message emphasizing training returns in terms of employment more than doubles the likelihood that job seekers call back the training center. However, callback rates are low in absolute value (less than one percent) and we detect no impact on enrollment one to six months after the intervention. We provide suggestive evidence that increasing salience of basic information about training is driving the effects on callbacks rather than belief updating.

Chapter 2 (coauthored with Bruno Crépon, Esther Mbih, Louise Paul-Delvaux, Bertille Picard and Vincent Pons) shows the results of another large-scale randomized experiment to evaluate the impact of an online platform helping job seekers adopt effective job search strategies. The platform combines labor market data from the French public employment agency and personal data from individual profiles to recommend users occupations and areas with high employment chances and to give them concrete tips to improve their job search methods. The experiment was conducted in collaboration with the French public employment agency on a sample of 212 277 job seekers from April to November 2017. An encouragement design led to a take-up rate of 26.2% in the treatment

group and virtually zero in the control group. Following individual trajectories over 18 months after the intervention, we do not observe any impact on job seekers' search effort and search scope, whether occupational or geographical. We find modest effects on search methods: job seekers using the website are more likely to rely on personal networks and to use resources provided by public employment services. However, we do not find any effect on self-reported well-being and on employment outcomes, both in the short run or in the middle run, indicating that more intensive interventions are required to bring unemployment down.

Chapter 3 contributes to the debate on how to regulate the market of vocational training. Understanding the decision-making process of job seekers who benefit from public training is crucial to improve their matching with effective providers and increase competitive pressure on badly performing providers. The chapter reports the results of an online survey on job seekers in France who had participated in a training program between January 2017 and April 2018. The survey aimed at understanding what they knew of and how they selected a center among heterogeneous training providers. I find two main results. First, job seekers use very limited information when making their choices. Only a third of respondents compare different centers before choosing one and to find a training provider, almost all respondents use a single source of information, which for half of respondents is their caseworker. Second, job seekers take into account various factors beyond the probability of finding a job. Logistical considerations such as start date or distance to home play a more important role than provider characteristics such as employment performance or size and connections to firms. Taken together, these results may explain the low competitive pressure between job centers, which in turn may contribute to low value added.

Thesis Supervisor: Esther Duflo
Title: Abdul Latif Jameel Professor of Poverty Alleviation and Development

Thesis Supervisor: Frank Schilbach
Title: Gary Loveman Career Development Associate Professor of Economics

Acknowledgments

I often said these two pages would be the best chapter of my thesis - too bad it is also the shortest! I am very grateful to my advisors, Esther Duflo and Frank Schilbach, for their guidance and encouragements to complete this thesis. In addition to driving economic research closer to ground reality, Esther considers with the same respect a farm worker in India, a caseworker in France, or a policymaker in the US. This is a lesson of integrity and strength that I will not forget. Frank deserves not one but as many thank yous as the number of versions of my papers that he edited and of kind “Hi-how are you” I received from him throughout my Ph.D. This thesis benefited from the feedback of many other faculty members at MIT, CREST, and PSE. Special thanks to Abhijit Banerjee for sharing soothing teas and advice on economics and life. I also want to thank Eva Konomi and Gary King, who helped me take a sabbatical year – and return to my thesis afterwards – and the J-PAL Fellowship program for financial support.

I am grateful to my co-authors for these intellectual journeys together. I had the very great chance to work with Alicia Marguerie, a true co-author of research and friendship. I cannot count the many hours Esther Mbih spent in Pôle emploi database that kept our projects alive! Thanks to Louise Paul-Delvaux, the best partner in crime for unforgettable field visits under the bright sun of Marseilles. Arthur Contejean and Pierre-Louis Bithorel provided remarkable research assistance with a distinguished sense of graph coloring. Thanks to Audrey Rain and Elizabeth Lloyd Burkhalter for their great help with the writing of my papers.

In spite of Rida Laraki’s enthusiasm, this thesis is less a demonstration of my game theory skills than an undeniable proof of my mastering of the foot-in-the-door technique as well as my innate sense of endless conversations. A big thanks to all of those who kept the door open and responded to my questions: Bruno Crepon, Chantal Vessereau, Christophe Bonraisin and all the caseworkers and statisticians I met at Pôle emploi; Bob emploi and Jumia Jobs teams; managers and trainees I interviewed in Afpa and Ifocop training centers; and all the anonymous respondents to my numerous online surveys - I hope some of this research will help them, one way or the other!

I am grateful to my classmates for their support: Mateo Montenegro, Roman Zarate, Maddie McKelway, Ryan and Sarah Hill, Sarah Abraham, Cory Smith, Gustavo Joaquim, Mert Demirer.

Special gracias to Mateo-hermano, the only resident of Sid-Pac I know who would read Fernando Pessoa's poems in Portuguese to cope with our endless macro psets. Thanks to my other dear friends who made Cambridge winters warmer and fun : Augustin Bergeron, Francine Loza, Gabriel Koehler-Derrick, Rahima Bensaid, little queen Aida, Adil Ababou, Silvia Danielak, Rim Hariss and Anupama Tiwari. Big up to the MIT women soccer team, and above all to Alizee Deleris and Alice Dufresne, members of the corniche-wide famous Soccer Pastis Team. A final thanks to the brilliant and always supportive Camille Terrier, a true Virginia Woolf in E52.

I still think that God made up for all the first two year's psets by sending me for a summer in Delhi, and another one in Venice. A warm thanks to Arun Singh for his tenderness and support, to all my Delhi friends for laughs and political debates in k26 kitchen, and to Stephen Marglin for his inspiring authenticity. Embarking with Marie Ekeland on 2050 boat last year was another strike of fortune. A generous and (hence?) visionary captain, a skillful team, and a motto based on creativity and confidence, that made it possible for me to complete my Ph.D in the best conditions, and paves the way for exciting adventures.

Over these six years between Cambridge and Paris, I discovered a continent: North America where I lived and studied, and South America(ns) for soccer and aguardiente. I learned the intellectual power of a multicultural world and the complexity to find one's way in social institutions, whether it be in Pole emploi corridors or in MIT cubicles. More than anything, I forged an inner sense of freedom and joy, that I hope to never lose. Words will not be enough to express how grateful I am to all of those who supported and inspired me throughout these years. An immense thanks to the Four Mousquetaires of Happiness: Jessica Hoffmann, Guillaume Bonnet, Paul Sidoun and Michele Moulonguet, best known as Mimi. Many thanks to my very dear friends (who still listen to my existential digressions - I wonder who pays them). In order of appearance, a non comprehensive list includes Arthur Leblais, Mélanie Meslay, Mathilde Bouchard, Adrien Fulda, Charlotte Le Mouel, Corentin Morice, Alexis Jouan, Marguerite Jossic, Antoine Julia, Mathilde Poupée, Ana Larderet, Marie Zegierman-Gouzou and Margot Malpote. Lastly, I am infinitely grateful to my family, Hachmi, Anne-Sophie, Naim, Larissa and Leila Ben Dhia, the love foundations of my life.

Contents

List of Figures

List of Tables

Chapter 1

Do informational frictions affect enrollment in public-sponsored training programs? Results from an online experiment

Joint work with Esther Mbih*

1.1 Introduction

Government-sponsored vocational training plays a leading role in public policies used to combat structural unemployment and to mitigate the negative employment effects of business cycle downturns (McCall et al. (2016)). In France, more than 4 billion euros of public expenditure are devoted annually to training for the unemployed.[1] To maximize the impact of these investments, policymakers target public funding towards sectors perceived as having high labor demand and towards job-seekers most likely to benefit from the program. However, the decision to participate in a training program ultimately takes place at the individual level and remains in the job seeker's hands. Information gaps regarding the pecuniary and non-pecuniary costs and returns from training may hinder the ability of job seekers to make optimal decisions. The efficiency of the whole job training system hence relies heavily on job seekers having access to information.

*We are grateful for the guidance provided by Bruno Crépon, Esther Duflo, Vincent Pons and Frank Schilbach. We would also like to thank Cyril Nouveau, Audrey Perocheau and Chantal Vessereau at *Pôle emploi* and our partners at Afpa without whom this project would have not been possible.

[1]This is reported in the 2018 Draft Budget Bill.

While there is a growing literature on the determinants of enrollment in formal education, especially at the primary and secondary levels (Dynarski and Scott-Clayton (2008), Barr and Turner (2018), Abdulkadiroğlu et al. (2018)), much less is known about determinants of demand for vocational training (Barnow and Smith (2015)). As with other educational investments, participation decisions depend on individuals' beliefs about the pecuniary cost of the program and its expected returns in terms of future earnings and employment probabilities (Jacobson and Davis (2017)). Yet there are reasons to believe that these parameters are particularly hard to know in the context of job training. In France, as in other developed countries, public-sponsored training is a complex institutional system that involves many different stakeholders, including the public administration at both the national and regional levels, public employment services, and private training providers. This results in a highly diverse landscape of programs, funding opportunities, and training providers. Existing empirical evidence provides only estimates of average returns to large classes of training programs and highlights important heterogeneity across individuals and institutional settings (Card et al. (2018), Barnow and Smith (2015)).[2] Moreover, job seekers enter training programs in the course of their professional lives, at very different ages, in different labor markets, and with very different backgrounds. This vast heterogeneity can generate high levels of uncertainty for job seekers regarding the returns of different programs for their employment chances and take-home pay.

As public services increasingly rely on digital communication tools, it is important to understand the potential of these tools to help job seekers overcome informational challenges and to eventually improve their reemployment chances. Online websites and mobile applications provide a low-cost and highly scalable means to spread information, which can be continuously modified and individually tailored (Kuhn and Skuterud (2004), Kuhn and Mansour (2014), Autor (2009), Horton (2017)). However, digital communication also comes with limitations. Certain sub-groups of the population, especially among the unemployed, are not familiar with digital technologies and do not

[2]This is one of the important take-aways of Card et al. (2018) meta-analysis of active labor market policies and Barnow and Smith (2015) review of U.S. programs. For example, Andersson et al. (2016) look at returns to two major public programs of vocational training in the United States. Despite the many similarities between both programs, they find moderately positive returns for one program but no significant returns for the second one. This is all the more puzzling as many job seekers are eligible for both streams.

have access to online information. Online messages might also not be as convincing as a discussion with another individual. Given the importance of job training, the complexity of the programs, and uncertainty about the impact of training, online communications have the potential to decrease misinformation and other obstacles that might limit the effectiveness of job training programs.

This paper presents the results of an experiment testing the effects of information frictions on job seekers' training demand by measuring the impact of online information provision on enrollment decisions. The experiment took place in late summer 2016, in the context of a large-scale public investment increase in vocational training targeted at the unemployed. The French government sought to nearly double the number of trainees, amounting to an additional 500 000 job seekers enrolled within a year. We partnered with *Pôle emploi*, the French Public Employment Service, and *Afpa*, the largest training provider in France. We collaborated on a large-scale emailing campaign addressed to more than 50 000 job seekers, which advertised a list of 24 standard training programs in 4 regions of France, to boost enrollment and meet the government's target. Emails were sent on August 30 and 31, 2016. Reminders were sent ten days after and programs started within the following three weeks. The experiment built on a similar campaign run earlier in the year by our partners using its target sample, operational schedule and email template. In its basic version, the email contained the list of programs offered in the region, and interested recipients were directed to the webpage with full program information if they clicked on the link provided in the email. The email also included a phone number to call the training center for additional information and to enroll.

Our intervention slightly varied the content of the messages that were sent out. We randomly sampled a *Control group* that received no email at all, and formed 5 different treatment groups. The *Basic email group* received the template version of the email, allowing us to measure the impact of receiving an email on enrollment. To test for the existence of specific information barriers, the four additional treatment groups received emails based on the same template but augmented with short sentences emphasizing different key information about training participation. All emails also included a hyperlink leading to a webpage with more detailed information. Job seekers in the *Cost email group* were reminded that training participation was entirely subsidized and would entitle participants to a stipend. In the *Simplicity email group*, the additional sentences emphasised the simplicity of registration procedures, stressing the availability of assistance from call operators. Messages received by the *Returns email group* provided job seekers with information about the potential returns from training: a short sentence mentioned the numerous job opportunities opened up by the training, and a hyperlink led to a webpage with rich metrics on wages and recruitment rate for the relevant jobs. The last *All info email group* received an email combining all three additional sentences.

We tested the impact of the intervention on two main outcomes: callback rates to *Afpa* and enrollment in a training program within the six months following the experiment. Along with these variables, we measured intermediate outcomes, including whether recipients opened the email and clicked on one of the links. This helps us shed some light on the degree to which recipients interacted with the information provided. Furthermore, three days before sending the emails, we sent a short baseline survey to the entire sample in order to capture prior beliefs about training cost and registration procedures.[3] We also asked respondents to estimate their expected wages and employment probabilities over a period of six months with and without training.

The survey reveals important information gaps about basic aspects of training costs and suggests that many respondents are uncertain or skeptical about training returns.[4] One third of respondents believe their unemployment benefits will decrease or get suspended if they participate in a training program, while nearly half of respondents believe training is not fully subsidized, with 14% expecting to pay more than 1 000 euros out of pocket. In addition, half of respondents perceive the registration procedure to be complicated or very complicated, which may act as a strong deterrent when considering whether to participate in a program. Finally, 26% do not expect training to increase their re-employment probability and up to 68% do not report any difference in expected wage with or without training. These results provide motivation for the intervention.

While the overall rates are low (around 0.5% overall), our results concerning the callback rates confirm the importance of information provision. As expected given the design of the campaign,

[3]The delay between the survey and the intervention was imposed by our partners' logistical constraints and prevented us from sending reminders to increase the response rate.

[4]The response rate for this survey is relatively low (13%). However, those who responded to the survey are on average more educated than the rest of the sample, they have more work experience and benefit less from assistance from Pôle emploi. It is thus plausible that such misinformation among the remaining job seekers might be even *more* pronounced.

all callbacks came from email recipients, who were significantly more likely to call back training centers over the month that followed the intervention than job seekers of the *Control group* who did not receive any email. Our modifications to the information content of the emails highlight important heterogeneity in the nature of information. Emails emphasizing training returns had the highest impact and almost tripled the callback rate compared to the group that received the basic email. More precisely, the callback rates of the *Return email group* and the *All info email group* increased by 0.4 and 0.36 percentage points from a mean of 0.27% in the *Basic email group*, respectively, and these increases are significant at the 1% level. Receiving an email on registration simplicity also increased callbacks by 75% compared to the level in the *Basic email group*, and the effect is significant at te 5% level. Perhaps surprisingly, given the results from the baseline survey, we detect no additional impact of emails with messages on cost compared to the basic email.

Contrary to the results on callback rates, enrollment six months after the experiment was not affected by the intervention. Irrespective of the message sent, the campaign was not successful in converting callbacks into effective registration: over the entire sample, only 11 individuals eventually enrolled in one of the 25 listed programs. As individuals could have leveraged the information they received to look for other training programs, we measure enrollment in *any* public-sponsored program over the six months that followed the intervention. We observe no effect on either measure. This null effect is unlikely to be entirely due to mistargeting as enrollment in any training program six months after the intervention hovers around 6% in all groups, including the *Control group* who received no email. This indicates that training was an option that the targeted population of the campaign was considering. However, it is clear that the campaign did little to attract job seekers to the listed programs. While about half of the individuals opened the email they received, of which about half clicked on a hyperlink, less than one percent of those who opened the email called back *Afpa* centers. Such high attrition made it highly unlikely to find an impact on downstream outcomes, such as enrollment.

As discussed in Bleemer and Zafar (2018), information interventions may have an impact through two main mechanisms: (1) by updating people's beliefs, or (2) by making information more salient and acting as a reminder. Disentangling these two mechanisms is important as they have different policy implications. In the case of belief updating, the efficiency of interventions is determined by the precision with which uninformed individuals are targeted with tailored messages. On the contrary, if effects are mainly due to salience, no such targeting is needed as all individuals benefit from regular reminders.

Our data only allow us to provide suggestive evidence on these two mechanisms. We look for heterogeneous effects along individual observables that indicate individuals' misbeliefs. Following Bleemer and Zafar (2018), the rationale of these tests is that if the impact of the emails is due to belief updating, it should mainly affect individuals with wrong beliefs. On the contrary, under the salience scenario, emails could have an effect irrespective of individuals' initial beliefs. The highest impact would be obtained on individuals for whom the message is the most salient, that is, on individuals who pay most attention to their emails. Since the low response rate to the baseline survey prevents us from using baseline answers in heterogeneity analysis, we propose an alternative method leveraging callers in the *Basic email group* and survey respondents. The method relies on two assumptions. First we assume that individuals who call back in the *Basic email group* are the least misinformed and that additional messages convince marginally less informed job seekers to call back. Secondly, we assume that respondents to non-mandatory online surveys are individuals who pay most attention to their emails. In fact, responding to the baseline survey is highly correlated with opening the intervention email. This supports the interpretation that this variable is a sign of digital literacy and easiness to handle online communication with *Pôle emploi*. Under these assumptions, variables that correlate with callbacks in this group may be used as a proxy to identify misinformed individuals and responding to baseline may be used as a proxy for attention. We use these proxies in a standard heterogeneity analysis framework and we observe whether they increase or decrease the effect of the treatment. For example, in the updating scenario, characteristics of the callers in the *Basic email group* should decrease the effect of additional email messages, because additional messages convince less-informed job seekers.

We observe that callback rates in the *Basic email group* are correlated with having an educational degree higher than high school diploma (the *baccalauréat*), which generally corresponds to better informed individuals. When interacted with the treatment dummies, high education turns out to significantly reinforce the impact of receiving an additional message (it more than doubles the effect). The results are less consistent when we look at the impact of each additional message separately but the pattern is consistent with an increase of the effect of the messages on returns. As the education variable is correlated with many other individual characteristics, results should be taken with caution but suggest that the effects we observe are rather due to information salience among attentive readers rather than updating. Had it been mainly updating, we would expect the coefficients of the interactions to be negative. Running the same regressions with a dummy for responding to baseline as a proxy for attentive job seekers reveals that baseline respondents systematically react more to emails and to each email message separately. The incremental effect on the sub-group of baseline respondents is even larger than on the high education sub-group: the impact of receiving an additional message and the impact of receiving a message on returns are tripled in all regressions, with all coefficients being significant at the 1% level.[5] Adding further support to the salience hypothesis, we analyze click rates and find that only about half of those who call back click on one link. Thus at least half of the effect is driven by seeing generic messages rather than learning more specific information. These results all add support to the salience hypothesis.[6]

Overall, our findings suggest important information gaps that can deter job seekers from starting a training program. They reveal the existence of misinformation on very basic features of training programs and that marginal modifications of messages can affect at least some real-world behaviors, although the effects do not translate into increased enrollment in job training. These results offer several interesting takeaways from a policy perspective and for future research. Considering that baseline respondents are likely to be better informed than the average population, it encourages public services to improve on information systems, even to communicate simple basic messages. Our intervention also shows that message content matters, even when it is delivered in a very simple manner and that job seekers seem to be particularly sensitive to employment returns. Yet online messages alone do not have long-lasting effects on significant outcomes such as enrollment and they seem to work primarily on individuals that are most informed. It would be

[5] One caveat on this interpretation is the short delay between the survey and the campaign, that may have acted as a reminder. However, it is not clear why this reminder would have worked only on some messages.

[6] To see whether the intervention durably affected beliefs, we also sent out an endline survey two months after the intervention. We got a low response rate as with the baseline survey, with a slightly unbalanced attrition across groups. Along with an additional sample size reduction, this prevented us from measuring any potential change on individuals' beliefs due to the intervention.

interesting to build on this intervention to test the impact when online information is augmented with caseworker assistance or other offline messages. Overall, the intervention was inexpensive cheap and could be easily replicated and scaled. It offers an interesting example of collaboration between researchers and administrative services, making results directly policy-relevant as both the setting and the methodology are grounded in existing practices.

Related literature. As noted by Barnow and Smith (2015), despite the long tradition of evaluating training programs, there is only limited evidence on how information impacts training enrollment, with the notable exception of Barr and Turner (2018). Our paper starts to fill this gap. Barr and Turner (2018) finds that US unemployment insurance beneficiaries are four percentage points more likely to enroll in a community college program upon receiving a letter with information on the costs and returns of these programs. The authors attribute this strikingly large effect, a 40% increase relative to the baseline enrollment rate, to the efficient complementarity of well-coordinated institutional support and endorsement from the White House.

An important puzzle that emerges from the existing training literature is unexplained returns heterogeneity, both across sites and participants (see e.g. Andersson et al. (2016), or McCall et al. (2016) for a review). Jacobson and Davis (2017) dig further in that direction by exploiting a particularly rich dataset in Florida allowing them to compare training returns by training program and participant socio-demographic characteristics. Their findings show that women select higher-returns fields and suggest that there is considerable room to increase their gains by altering their choice of field. Such informational barriers might slow down desirable re-allocation and a follow-up to this study could improve on information targeting leveraging similar individual-level data as in Jacobson and Davis (2017). In Germany, Altmann et al. (2018) run a similar experiment to ours, sending a brochure to a vast sample of job seekers informing them of observed returns to job strategies and consequences of unemployment. While the intervention has no significant average effect, they also find that it increases employment and earnings for a specific group of individuals, namely those at higher risk of long-term unemployment. For this group, the brochure increases employment and earnings in the year after the intervention by roughly 4%, which is remarkable considering the low cost of the intervention.

Recent works on the impact of information on education investments (e.g. Dizon-Ross (2019) and Conlon (2018)) showed in different contexts that changing parents and students' beliefs about educational outcomes could change investment decisions. Our study is closest to the experiment reported in Bleemer and Zafar (2018), where the authors provide information on cost and returns to college education in two separate treatment arms. Measuring the impact on intended enrollment decision, they find the cost intervention to have no effect, while information on returns significantly increased reported intentions to enroll and with lasting effects on beliefs two months after the experiment. These results align well with the findings of our study.

From policy perspective, it is equally important to know whether such information interventions can be successfully implemented at scale and with low costs. While there have some remarkably successful pure-information interventions (e.g. Hoxby and Turner (2013), Altmann et al. (2018)), several papers underline the need to combine online messages with offline assistance (Castleman and Page (2015), Carrell and Sacerdote (2017)). Finkelstein and Notowidigdo (2018) confirm this complementarity in the context of SNAP enrollment, i.e. the food assistance program in the US. They find that delivering information on SNAP eligibility almost doubles enrollment but when complemented with assistance from public servants, information triples enrollment. Despite poor targeting properties of the interventions, their calculation suggests that these interventions are a cost-effective policy.

Finally our study relates to recent work on behavioral obstacles in job search (Della Vigna and Paserman (2005), Babcock et al. (2012), Spinnewijn (2015), Caliendo et al. (2015), McGee (2015), DellaVigna et al. (2020)). Babcock et al. (2012) argue that complex institutional systems might deter job seekers from optimal decisions, which is illustrated in the context of college enrollment in Dynarski and Scott-Clayton (2008). These barriers may be especially salient for the most vulnerable among the unemployed, resulting in an exacerbation of inequality in long-term outcomes (Bertrand et al. (2004), Schilbach et al. (2016)). Our paper adds some more evidence to this and suggests that reminders and repetition of simple information can help job seekers.

The rest of the paper is structured as follows. Section 2 gives background on how the French training system works. Section 3 explains the design of the experiment while section 4 provides an overview of our data. In section 5 we present our main results and we conclude in Section 6.

1.2 Background

Training to the unemployed is one of the main labor market policies in France. From 2014 to 2018, an average of 10% of all job seekers registered at *Pôle Emploi* participated in some form of training program, amounting to nearly 3 million trainees over the period. Pursuing this trend, in 2018, the French government launched a massive 5-year plan, channeling 15 billion euros into training towards uneducated youth and low-skilled job seekers. In this section, we briefly describe the French training system for the unemployed, and then provide more details on the context of the intervention and on our partners.

1.2.1 Public-sponsored training in France

The public-sponsored training system for the unemployed is managed by three main protagonists: administrative regions, the Public Employment Service (*Pôle emploi* hereafter), and the State.[7] These three players jointly account for more than 80% of the nearly 5 billion euros going annually to fund training to the unemployed.[8] Thus they play a crucial role in the type of programs and sectors where job seekers can train. Although *Pôle emploi* aims primarily at quickly reducing unemployment through short programs while regions fund longer training delivering professional qualifications, all three protagonists prioritize sectors with high labor demand in each region. The largest share of their subsidies are allocated by regions to group programs through a system of public auctions. This process allows them to set a number of requirements that training providers have to meet and to select providers that offer the best trade-off between price and program quality. The remaining share of their funding is available in the form of individual grants to fund individual training.

Training costs. When a program is funded by regions, *Pôle emploi* or the State, its direct cost is entirely covered. Additional grants may be given to cover transportation or housing. Only if

[7] Firms and other third-party institutions called "OPCO" are in charge of training for employed individuals and only play a minor role in job seekers' training.

[8] Out of the 4.91 billions euros spent in 2015 on training for the unemployed, 1.47 billion were contributed for by regions, 1.94 billion by *Pôle emploi*, 82 millions were spent by firms, 37 millions by the State and 31 millions by beneficiaries themselves. This is reported in the appendix of the 2018 Draft Budget Bill.

either the desired program is not a sponsored group program or if the job seeker does not obtain an individual grant must she pay the entire cost of the program out of her pocket. By definition such programs are outside of the list of subsidized programs and they are very rarely advertised by *Pôle emploi* or proposed by caseworkers. In total, only 6% of all training programs are paid by beneficiaries.[9] Job seekers are formally not allowed to complement the maximum stipend they can get with their own money to pay for a program.[10] Hence, by and large, participating to a public-sponsored training can be considered as free, aside from transportation and accommodation costs.

Upon enrollment, job seekers under unemployment insurance will keep the exact same amount of unemployment benefits. If their benefits exhaust before the end of the training, they get extended. Job seekers who are not eligible to unemployment benefits can also receive a special subsidy provided either by *Pôle emploi* or the State. Such subsidies vary across individuals but typically range between 300 and 500 euros per month. Enrolling in a training program can therefore only increase unemployment benefits.

Enrollment procedures. Enrollment processes vary across job seekers and largely depend on how they hear about the program, which is generally either on the Internet or by discussing with their *Pôle Emploi* caseworker.[11] If a job seeker wishes to enroll in a training program, her caseworker is asked to make sure that the training is consistent with her professional project. In order to avoid early drop out or disappointment, the caseworker also makes sure that the job seeker is committed enough to pursue training until the end, that she is fully aware of the practical and

[9] See appendix of the 2018 Draft Budget Bill.

[10] Field interviews in three different centers in the region of Paris revealed that this rule was not fully enforced. We met trainees who had paid a small complement of a couple of hundred euros, adding to a public subsidy in order to make up for the program cost. It is very easy for providers to bill this additional cost separately from the rest and it is in the interest of both the trainee and the provider to reach such an agreement rather than giving up on the training entirely.

[11] When a job seeker first registers at *Pôle Emploi*, she gets assigned to a caseworker that will assist her throughout her job search. One important mission of the caseworker is to make sure that the job seeker meets the requirements to receive her unemployment benefits by attending mandatory workshops and actively pursuing her job search. They also help job seekers navigate the administrative system, e.g to participate to a job training program. However, caseworkers get assigned to a minimum of 150 job seekers at the same time, which limits the assistance they can provide. A caseworker who assists the most autonomous job seekers (who are most at ease with the Internet and other job search tools) follows from 300 to 600 individuals at the same time.

logistical conditions of her enrollment, and that she has carried out sufficient research to make sure she would find accessible job opportunities after the training.

If these conditions are met, caseworkers often help job seekers to look for available and subsidized programs within their sector of interest. Caseworkers are told to prioritize group programs that are subsidized by regions or *Pôle emploi*. If the program they desire is not in this list, job seekers can request an individual grant by formally making a case to obtain the approval of the *Pôle emploi* agency committee. Individual grants generally cover around 1 000 to 1 500 euros but their availability varies a lot depending on years and time in the year, regional and local available budgets, as well as government directives and caseworkers leniency.[12]

If job seekers wish to enroll in a group training, they have access to a limited number of providers, which are the ones that have been selected through public auctions. Because of the requirements set in the public auction, program contents (at least from what job seekers can read on brochures and learn on websites) tend to be fairly similar across selected providers. Nonetheless, there can remain important differences across training centers in terms of size, staff or educational methods. Centers and contents are more diverse for those who obtain individual funding. Given the absence of standardized and easily understandable system of certification in France, it is quite complex to get a complete picture of the available training supply and to use reliable information to choose the best program and the most efficient provider.[13]

Once they have obtained funding and identified a potential training center, job seekers attend an informational meeting at the training center that gives them more detailed information about the program. They might also have to take some selection tests at entry. To complete their enrollment, they finally need to get an enrollment form with signatures from the training center and their caseworker. This back-and-forth process was digitized in 2016, which considerably simplified the procedure. Registrations are now recorded on an online platform that both training providers and caseworkers can access in real time.

[12]Note that 1 500 euros is not enough to pay for most training programs available on the market.

[13]In another study where we surveyed former trainees about their choice of training providers, we found that future enrollees tend to spend little effort comparing training centers and generally choose the one that is closest to their home or that starts its program the earliest. This suggests that the diversity of the supply is not really perceived as such by job seekers.

Information available on training. There is no centralized platform that aggregates all the information on training programs available to job seekers. Information is spread across different websites, often at the regional level. *Pôle emploi* hosts two websites that help job seekers look for training programs in their sector of interest and geographical area. Other institutions provide larger catalogs that are not limited to public-sponsored programs and also include training for workers.[14,15]

Importantly, none of these websites include precise information on training returns. On their own websites, providers often post quantitative performance rates but those do not come from any rigorous evaluation. Information websites generally include only the dates and location of the program, along with a short description of the program content.[16] At best, they may add hyperlinks to other webpages hosted by public employment agencies with information about jobs that the program can lead to.

1.2.2 Context of the intervention

The experiment took place within the *Plan 500 000*, a national program to massively increase training participation among job seekers. The program had set the ambitious goal to increase by 500 000 the number of job seekers enrolled in a training program, corresponding to a 50% increase compared with previous years.[17] This inevitably required an important effort of recruitment and advertising and it was crucial for training providers and public services to communicate intensively about available programs.

---

[14] In another study where we surveyed 2 000 trainees on their choice of training providers, we saw that among respondents who heard about their center on the internet, only 30% used general information websites. The remaining 70% used instead training center websites. As confirmed by our field visits at *Pôle emploi* agencies, 40% respondents relied on *Pôle emploi* caseworkers.

[15] In France, the most important one is the *Centre d'Animation des Ressources d'Information sur la Formation* (CARIF), a public institution that centralizes information about job training programs.

[16] In 2017, Ile-de-France regional offices launched an experiment called "*Anotéa*": they surveyed trainees one to six months after the end of the training to collect feedback and then displayed the average result on *Pôle emploi* information websites. An important challenge to the success and the replication of such an initiative is the ability to get a sufficiently high response rate to ensure statistical reliability. Programs often enroll no more than 30 trainees per year and there is no stable classification of programs over years that allows to easily sum up feedback over multiple cohorts.

[17] This target was reached, and the rate of job seekers enrolled in a training increased from 10% in 2015 to 15% in 2016.

Given the short timeline, public services focused on expanding existing supply rather than promoting new programs. They primarily funded programs that were similar to the ones funded before the *Plan 500 000* and collaborated with already existing providers. Thus, training programs advertised in emailing campaigns such as the one of this study were not different from standard programs. Thus, our experiment does not study programs with particularly high employment returns and low demand from job seekers. The context of the national plan, however, means that advertising campaigns were meant to recruit marginal job seekers, i.e. job seekers who would not have trained in absence of a national program. It also means that job seekers in all groups, including the *Control group*, were exposed to other, regular communication campaigns promoting training.

The campaign of this study was run jointly by regional offices of *Pôle emploi* and *Afpa*, the largest training provider in France. *Afpa* has a unique status and history as a training provider in France: it was created in 1944 within the Ministry of Labor as a public institution in charge of professional training and it has played a central role in training job seekers since then. Although several recent reforms changed its status, to stimulate competition with other providers, *Afpa* has kept an important market share as well as massive equipment and numerous centers all over the territory with special connections with *Pôle emploi*. Not surprisingly, it participated actively in the *Plan 500 000* and ran several advertising campaigns to boost enrollment throughout the year 2016. The implementation of the campaign of this study followed the same procedure as earlier campaigns run by *Afpa* and *Pôle emploi*. In an emailing campaign launched in June 2016 in two regions, 37 000 emails were sent, resulting in 347 callbacks and 71 job seekers who agreed to register and participate to an information meeting. According to *Afpa* call operators, individuals who called back but who did not enroll were most often discouraged by the distance to the training center or said that the delay was too short for them to make their decision.

1.3 Design

To identify how informational barriers affect training enrollment, we conducted an online experiment during the last week of August and the first days of September 2016 among 63 000 job seekers. In this experiment, we varied the content of advertising emails by adding short messages on training costs, enrollment procedures, or expected labor market returns.

1.3.1 The campaign

The campaign was originally planned and designed by *Afpa* and *Pôle emploi*, as part of a larger advertising plan within the national training program *Plan 500 000*. It was targeted at four administrative regions.[18] In each region, a list of 5 to 7 programs were offered, which added up to 24 programs advertised in total. Messages were sent out on August 30 and 31, with reminder emails sent on September 9 and 10, 2016, for programs that were starting within the first three weeks of September. This timing was decided by our partners.

As our partners had run a similar emailing campaign three months before the experiment, they chose to reuse the same email template. This basic email is showed in Figure 1-1. It includes several motivational slogans about training and a short introduction sentence encouraging job seekers to enroll. It also lists the available programs in the region selected in the campaign.[19] Email recipients could click on one of the programs to open the *Afpa* webpage with more detailed information on the program and the jobs it may lead to. Finally, job seekers were provided with a phone number and were invited to call *Afpa* centers to get more detailed information and enroll.

1.3.2 The intervention

Our intervention consisted in introducing small variations to the basic email template. In collaboration with our partners, we designed three additional messages, all of which were not longer than a sentence or two, with a hyperlink to a webpage containing more detailed explanations.

Message on training costs. A first modification consisted in adding a message on training costs, which was sent to the *Cost email group*. More specifically, we added a short sentence that

[18] These regions are namely Auvergne-Rhône-Alpes, Centre, Hauts-de-France and Nouvelle-Aquitaine. They are geographically spread out over the French metropolitan territory and represent nearly one third of the French population.

[19] Each job seeker had been selected in the sample because she was searching in the same sector as one of the available programs. Displaying the entire list of available programs may have created some confusion, which in turn may have lowered the average response rate.

reminded job seekers that training participation was fully subsidized and that they could be entitled to benefits. This is shown in the top left panel of Figure 1-2. A hyperlink at the end of the sentence pointed to an external webpage hosted by *Pôle emploi* with more information about the type of benefits job seekers could be entitled to if they enrolled. The amount of the benefits could not be directly displayed in the email as it depended on each individual situation.

Message on registration procedures. In the emails to the *Simplicity email group*, we added to the basic template a sentence explaining that registration procedures had been simplified and that job seekers could get assistance from *Afpa* staff members. This is shown in the top right panel of Figure 1-2. The additional webpage provided detailed explanations on the different steps to enroll.

Message on training returns. This third type of message, sent to the *Returns email group*, was meant to convey high training returns, primarily in terms of reemployment. As it was difficult to provide detailed statistical information in the email, the sentence simply said that training would "lead to many job opportunities". In addition, job seekers could click to open a webpage from the *Pôle emploi* information website with several metrics including seasonal recruitment rates and average wages in the job as well as some indicators of market tightness based on *Pôle emploi* database. Wage and recruitment information were computed at the regional level and using observational data from employment administrative records. The email with information on returns is shown in the lower left panel of Figure 1-2. It shows an example for a program for a job of a network administrator. As is visible in the figure, providing job-specific information required that only one training be displayed in the email. It is possible that this may have made the email easier to read and more impactful, independently from the information on returns.

Message with all information. Lastly, we gathered the three messages into a single email, to test for possible complementarities. This is shown in the bottom right panel of Figure 1-2. If adding all three messages does not have crowd out the job seekers' attention, it is certainly the most policy-relevant email as it addresses all types of information gaps.

1.3.3 Eligibility criteria and sampling

Pôle emploi and *Afpa* established eligibility criteria in order to target job seekers with potential interest in the listed training programs. Using comprehensive unemployment records in the four regions of the experiment, we first sampled job seekers who had agreed to receive advertising emails from *Pôle emploi*. We then restricted the list to job seekers seeking jobs in a professional sector that was related to one of the campaign programs. More precisely, two types of job seekers were selected. First, we sampled job seekers for whom one of the campaign programs matched their desired job. Of those, we only kept individuals who had reported less than 3 years of experience when they registered at *Pôle emploi* as our partners considered that job seekers with more work experience would not be interested in getting trained.

We also sampled individuals who were seeking jobs in professional sectors that were close to the listed programs. As an example, a carpenter could be interested in getting some training in brick laying. This procedure was intended to help individuals complete their skill sets and expand the range of jobs they could apply to. Such individuals were selected only if they had reported more than 3 years of experience in their own professional sector. Finally, we removed from the list all job seekers who had ever participated in a training program since the beginning of their unemployment period. There were no specific criteria on the type of program and this may have left out many job seekers who had enrolled in a short job-search-related program and who would have been interested in a longer training with professional skill content. In total, 63 246 job seekers were sampled through this procedure, out of which 6.5% were looking for the same job as one of the listed programs.

1.3.4 Experimental design and randomization

As can be seen in Figure 1-3, we randomly assigned all job seekers to one of six groups. The first group served as a *Control group*: these individuals received no email at all. A second group (*Basic email group*) received the basic email showed in Figure 1-1. It was based on the template that our partners had used in their previous campaign, with the appropriate list of programs. We then formed four groups corresponding to our four different messages. The *Cost email group* received an email with the training cost message, while the *Simplicity email group* had an email with a message on training registration. Job seekers in the *Returns email group* received the email with a message on training returns. A last email combined all three messages (*All info email group*).

We created groups of equal sizes across treatment arms in each region. Due to logistical constraints and institutional differences across regions, we could not have all six groups in each region, which explains why treatment groups end up having different sizes. The distribution of the sample by treatment arm and region is summarized in Table 1.1, which shows that emails with information on training costs could only be sent in two regions.

We stratified the randomization at three levels: first, we split the sample by region as program lists were region-specific. Considering that listed programs were fairly heterogeneous, we also avoided imbalances across groups by stratifying the assignment by training program. The last strata was created based on whether job seekers were looking for the same job as the training.

We use the comparison between *Control group* that received no email to all other groups to study the effect of receiving an email, irrespective of its content. By comparing recipients of the basic email to job seekers in other email treatment groups, we can then identify which messages boost the impact of the basic email, thereby identifying potential information gaps.

1.4 Data and sample description

1.4.1 Data

This section provides a description of the data we use in the experiment.

Unemployment records

Administrative data from *Pôle emploi* provide individual socio-demographic characteristics including age, gender, education level and family situation. It also has detailed information on past and current unemployment spells. In particular, we can know the duration of the current unemployment spell as well as the job seeker's *assistance track*. As they register at *Pôle emploi*, job seekers have a first registration meeting with a caseworker who establishes the level of assistance they will need. The least intensive track generally concerns people who are familiar with digital tools and who are fairly autonomous in their job search. They mostly communicate with caseworkers by email. The most intensive track is for job seekers with very little autonomy in their job search, who might not be comfortable using a computer or writing down their resume. This characteristic might therefore be correlated with the impact of the intervention.

Unemployment records provide us with some information about job seekers' targeted jobs. A key variable for the experiment is the professional sector where the job seeker is searching for a job. Professional sectors are fairly narrow and correspond to 5-digit US occupational categories. Professional sectors are organized in a hierarchical way with lexicographical classification and links to similar sectors. As training programs are also matched with those 5-digit professional sectors, we could sample job seekers who were searching in the same professional sector or in a closely related sector as the training programs advertised in the campaign. We then created a variable indicating whether the job seeker was searching in the exact same sector as the training or in a closely related one.

We also have information on past work history. The data include the number of months of experience in the professional sector of interest as well as job seekers' professional category in their last job. There are seven possible categories, from unskilled worker to executive manager, that we grouped into four groups for tractability.

Finally, our data also include other variables capturing job seekers' preferences. Those variables need to be taken with caution as they are reported by job seekers only once at the time of registration and they rarely get cross-checked by caseworkers. Some of these variables such as desired wage or maximum acceptable distance to home are also recorded as point measures whereas one would want functions to describe indifference curves. Hence, in Table 1.2 we only keep two preference variables indicating if the job seeker said she was looking for part-time work or short-term contracts as we believe that these two variables are easier to interpret as stand-alone dummies and plausibly less likely to change with time as they tend to depend on family situation.

Email opening and click rates

Pôle emploi emailing software allows us to partially track job seekers' activity upon receiving an email. For each email recipient, we can see whether she opened the email, clicked on one of the hyperlinks in the email or if there was an error in the email address and the email bounced back.

Callback data

In the emails, job seekers were invited to call back *Afpa* training centers to get more information about training programs and enroll. The phone number was specific to this emailing campaign although not limited to the four regions included in the experiment. Call operators had to give some information about the programs, confirm job seekers' interest and invite them to participate to a first information meeting at the training center. At the end of the call, call operators had to indicate whether job seekers confirmed their interest in the program and whether they were available to this information meeting. Using names and first names we could match 269 names to our initial sample.[20]

Training enrollment

The last key variable in our data records job seekers' enrollment into a training program after the intervention. *Afpa* training centers provided us with a list of job seekers who had enrolled in one of the listed programs one month after the intervention. As emails might have raised interest in training more generally and boosted participation in programs outside the campaign, we also leverage unemployment records to measure enrollment in *any* training program. We focus on enrollment one and six months after the intervention.

[20]These data were manually recorded, with frequent typos, and many job seekers did not remember their unemployment ID, which is used as unique identifiers. This made the matching with our lists less precise and cumbersome. We manually corrected typos on names, first names, and unemployment IDs. We then tested the robustness of matching on names by comparing individuals' gender and region in both datasets and by using semi-automatic matching methods that did not rely on manual editing of typos. Only one observation belonged to the *Control group*. All other callbacks came from individuals in one of the email groups.

1.4.2 Sample description and balance checks

Table 1.2 provides key summary statistics of our sample and provides balances checks to assess whether the randomization was successful. Column 1 of Table 1.2 describes the sample along individual characteristics. The average age is 41, and about 60% of our sample are men. Just like jobs, training programs are highly segregated by gender and the gender imbalance in the sample is explained by the type of training programs in the campaign, which mainly attract men. At the time when we drew the sample, individuals had been unemployed on average for 13 months, although this average hides important dispersion that is typical of skewed duration distributions such as the ones of unemployment spells.

In line with *Pôle emploi* targets, most job seekers in the sample are low-skilled people, as indicated by the fact that 57% have less than a high-school diploma (baccalaureate). Yet 66% have a formal degree in their professional sector and individuals report an average of about 10 years of work experience in their desired jobs. Because of eligibility criteria (see previous section), only 6% are looking for a job in the same sector as one of the listed programs. Finally, only 13% of our sample benefit from intensive assistance from *Pôle emploi*, meaning that most individuals in our sample are considered to be fairly autonomous in their job search and familiar with online communication.

Panel II of Table 1.2 shows that only 43.5% of email recipients opened the email sent to them as part of our study. Columns 5 to 7 show how email openers differ from other email recipients. Not surprisingly, we see that they are more educated, as illustrated by a higher share with a formal degree in their job and an over-representation of employees and managers and educational levels higher than a high school diploma. Women were significantly more likely to open their emails, but this is likely to be driven by a strong correlation between gender and education in the list of advertised programs.[21]

Table 1.3 shows that the randomization was successful at balancing groups along most individual

[21] Interestingly, looking at the timing when emails got opened, we observed that almost all the action was concentrated during the first two days after we sent out the emails. This is a common feature of many emailing campaigns. This could either reflect individual heterogeneity, with a divide between individuals who read their emails as soon as they receive them and individuals who never read them. Alternatively, it means that old emails get quickly discarded, in which case the timing when they are sent is an important parameter of the intervention.

characteristics. Column 1 displays the mean value of each characteristic, along with its standard deviations in brackets. In columns 2 to 6, we report the β coefficients of several regressions of the following type:

$$X_i = \alpha + \beta G_i^j + \epsilon_i.$$

In these regressions, X_i is an observable characteristic (e.g. female gender), α is constant and G_i^j is a dummy for belonging to treatment group j. We run each regression only on individuals in the *Control group* and treatment group j with j ranging from 2 to 6, which means that the β coefficient is significant if and only if the *Control group* and the treatment group j are not balanced along X_i. We see in the table that only a few coefficients are significant, as should be statistically expected from the multiplicity of the tests we run on balanced groups.

1.4.3 Baseline survey

Three days before the intervention, we sent out a short online survey to measure existing misinformation about training. The complete questionnaire in both French and English is shown in Appendix 1.9.1. The survey was sent from *Pôle emploi* servers and had seven short questions related to the information gaps that the intervention targeted, asking the following:

- Question 1 asked job seekers whether they would have to pay to enroll in training programs "offered" by *Pôle emploi.* The wording of this question explicitly excluded supplemental costs such as transportation or housing, which are generally not subsidized.

- Question 2 asked whether enrolling in a training program has an impact on one's unemployment benefits. By default unemployment benefits remain unchanged if a job seeker receiving benefits enrolls in a professional training program.[22]

- Question 3 asked about people's perceptions of how easy it is to register. As described in section 2, the training system as a whole is hard to navigate. At the individual level, the

[22] Unemployment benefits can only get extended in case they are exhausted before training ends. This question only referred to the *amount* of benefits received.

main challenge is to identify a relevant training program, obtain funding and get one's caseworker's approval. However, once the program has been identified and validated, individual registration itself is fairly straightforward and often facilitated by caseworkers and training center staff members.

- Questions 4 to 7 aimed at capturing people's expectations of training returns. In questions 4 and 6, respondents had to estimate their re-employment probability within the following six months, with and without training, while in questions 5 and 7 they had to do the same exercise but for re-employment wages.

The response rate to the survey was 12.8%. While this response rate is low in absolute terms, it is fairly standard for such online surveys sent by *Pôle emploi*.[23] As can be seen in Panel I of Table 1.2, survey respondents are slightly older, more likely to be female, and significantly more educated than the rest of the sample (based on their highest formal degree and whether or not they have a degree in the job they search). They are also generally in the least intensive assistance track.[24] Interestingly, they seem to be selected along the same variables as people who opened the email (see Panel II of Table 1.2, discussed above). This evidence suggests that responding to baseline and opening emails do not depend on people's intrinsic interest in the message but rather their internet fluency and how easily they communicate with *Pôle emploi* by email.

Despite this fairly advantageous selection in terms of education and other covariates, responses to the survey reveal some important information gaps. The left panel of Figure 1-4 shows significant misinformation regarding direct training costs. About 45% of respondents to question 1 think training is not fully subsidized by *Pôle emploi*: 15% think that a 6-month training program "offered by *Pôle emploi*" will cost them more than 500 euros while 14% estimate this cost to be higher than 1000 euros. Such priors about training costs must be an important barrier to enrollment, even for job seekers who believe training to be relevant for their professional skills.

[23] This rate could have been pushed up had we been able to send reminders. However this was not possible due to the very short delay between the survey and the intervention itself.

[24] As explained in section 1.4.1, when they first register at *Pôle emploi*, job seekers get assigned to one of three assistance tracks that determines how closely their assigned caseworker will assist them, depending on their Internet fluency and how easily they handle their job search.

The right panel of Figure 1-4 shows further misinformation regarding how participation in the training impacts unemployment benefits. About 30% of respondents to question 2 think getting trained will modify their unemployment benefits. Among those, 27% think their benefits will decrease and 9% think they will entirely lose their benefits. Overall, more than half of respondents have incorrect priors about training financial costs and about a third think both that they will get less unemployment benefits and that they will have to pay to participate in a training program offered by *Pôle emploi*.

Turning to people's subjective perception of administrative procedures, more than half of respondents to question 3 report that registering to a training program is complicated or very complicated. Figure 1-5 shows that up to 14% choose the latter option. Finally, Figures 1-6 and 1-7 illustrate respondents' answers to questions 4 to 7 estimating people's expectations about training returns, which show a more complicated picture.[25] The mode value of answers to questions 4 and 6 is at 50%, which may reveal people's uncertainty about their baseline re-employment probability and future wage. It is nevertheless striking that 68% of respondents to questions 5 and 7 expect training to make zero difference in their future earnings. Respondents to questions 4 and 6 believe that training would increase their reemployment probability by 8 percentage points on average, but 26% of them expect no change at all and 18% think getting trained will instead lower their reemployment chances.

1.5 Results

1.5.1 Methodology

To estimate the impact of the intervention, we run several regressions. Equation (1.1) estimates the effect of receiving an email compared with no email:

$$Y_i = \beta E_i + \gamma' X_i + r_i + \epsilon_i \tag{1.1}$$

[25]Importantly, we do not know people's past work histories and returns are likely to vary across individuals, all the more given that we did not specify the training program respondents had to imagine.

where Y_i is the outcome dummy (e.g. callback or enrollment) and E_i is a dummy for having received any email.[26] The coefficient of interest is β. In other words, equation (1.1) compares individuals in the *Control group* to the pooled sample of all other groups. X_i are individual covariates including gender, age, assistance track at *Pôle emploi* and education levels. r_i are region fixed effects accounting for the fact that regions did not have the same number of treatment groups and did not have the same listed programs.[27] Lastly ϵ_i is a heteroskedastic error term.

To know if additional messages increased the impact of the basic email, we run a similar regression, simply separating individuals in the *Basic email group* from all other email groups. The corresponding regression is showed in equation (1.2). It is similar to (1.1) but we add a dummy M_i for having received any additional message in the email:

$$Y_i = \beta_1 E_i + \beta_2 M_i + \gamma' X_i + r_i + \epsilon_i \tag{1.2}$$

To compare email treatment groups and test which message is the most impactful, we remove the *Control group* and restrict the sample to emailed individuals only. The *Basic email group* is used as the reference group and we introduce one dummy per email group with additional message, as showed in equation (1.3) below:

$$Y_i = \beta^{cost} T_i^{cost} + \beta^{simp} T_i^{simp} + \beta^{ret} T_i^{ret} + \beta^{all} T_i^{all} + \gamma' X_i + r_i + \epsilon_i \tag{1.3}$$

where T_i^{cost}, T_i^{simp}, T_i^{ret} T_i^{all} are dummies for messages on training cost, registration simplicity, training returns and email with all information, respectively.

Finally, we report the results of the same regression for the sample of individuals who not only received an email but also opened it. Email subjects were identical across treatment groups, so the inference is still valid. Focusing on people who open the email might increase power since we remove individuals who did not even open the email, which just add noise to the regressions. At the same time, excluding who did not open their email can reduce the precision of the estimation

[26]Note that there is no constant term as it would be colinear with the four constant fixed effects.

[27]If we focus on the *Basic email group* alone, we observe that callback rates are different across regions which confirms the relevance of region fixed effects beyond differences in the number of treatment arms.

of the coefficients of covariates, which reduced statistical power.

1.5.2 Impact on callback rates

Our main outcome is whether job seekers called back *Afpa* training center. As it is the email's call-to-action and the first step to enroll, we interpret callback as evidence that the email raised interest in participating in a training program. Panel III of Table 1.2 shows that the average callback rate was overall very low, barely reaching half of a percentage point. This low number is of comparable magnitude to the previous campaign run in June 2016 by *Afpa* and *Pôle emploi*. Looking at individual characteristics, we observe that people who called back are significantly more educated than the rest of the sample - a selection bias that is twice stronger than for baseline respondents or email openers. Those who called back are also twice more often seeking jobs that directly match the employment opportunities of one of the campaign programs. Because of the eligibility criteria, they consequently have less professional experience.[28]

Table 1.4 shows the impact of the intervention on callback rates, using the regression specifications outlined above. Column (1) displays the results of regression (1.1). The mean in the *Control group* is virtually zero and confirms that all callbacks came from people who had received an email. This is not surprising given that the phone number used to track callbacks was specific to this email campaign. Hence, other job seekers had very little chance to call back on the same number. Column (2) corroborates these results adding a set of covariates including gender, age, dummies for assistance and education levels. Columns (3) and (4) show that the effect is amplified by additional messages. On average, the callback rate upon receiving an email with any additional message is more than twice as large than with a basic email, which is the case with and without covariates.

Columns (5) to (8) show that the largest impact on callback is obtained by giving information on returns. Emails emphasizing returns (with and without the other messages on cost and simplicity) more than double the callback rate. Messages on registration simplicity increase callback by 70%:

[28] As explained in section 1.4, individuals looking for jobs that corresponded to one of the listed programs were eligible only if they had less than 3 years of experience, whereas individuals in close sectors were eligible if they had more than 3 years of experience.

in columns (5) and (6), it adds 0.19 percentage points to a mean of 0.27 in the *Basic email group*. However, although we found visible information gaps on training costs in the baseline survey, emphasizing that training was free and could entitle participants to a stipend did not appear to significantly increase callback.[29]

Results are similar in columns (7) and (8), which report regressions using the restricted sample of individuals who opened the email they received. Since email subjects were the same in all treatment groups, restricted treatment groups remain statistically balanced. Out of the 6 503 individuals who opened the basic email they received, we read that 0.44% called back the training center. This percentage is not significantly higher for individuals who received and opened an email on training cost. However it increases by 81% among people who received the email on registration simplicity and it is multiplied by 2.5 for emails emphasizing training returns. These effects are of the similar magnitude in all regions even though regions had very different listed programs and callback rates upon receiving the basic email.

1.5.3 Call outcomes

As another intermediate outcome following a callback, we look at the outcome of the discussion during the callback to the *Afpa* center. During the call, *Afpa* operators were supposed to give basic information about the program, confirm the person's interest and finally schedule her participation to an "information meeting" in case she was interested.[30] *Afpa* operators had to indicate the outcome of the conversation. Four types of outcomes were recorded: (i) whether the individual was interested in the program and available for an information meeting, (ii) whether the individual was interested but not available, (iii) whether the individual was not interested, and finally (iv) whether the individual was interested in another program outside the campaign. Operators did not record if individuals eventually participated to a meeting.

Out of 269 callbacks, only 46 were found to be interested in an *Afpa* training program and

[29] Even though this treatment was done in regions 1 and 2 only, the null effect does not seem to be due to power limitation. In fact, running the same regression in these two regions only, we see that the effects of other email treatments remain statistically significant.

[30] Participating to this meeting before starting the program is useful for people to learn in greater detail what the program is about and for training centers to select who they let in. This is often required to enroll in a training program.

available to participate to an information meeting. 88 were interested but not available for the information session that was scheduled. Upon their discussion with the operator, 100 individuals turned out not to be interested in the program and 41 wished to participate to another training program. Overall, this means that less than a fifth of people who called back were ready to enroll in one of the listed programs. This high dilution effect considerably lowered the possibility of detecting any differential impact of the messages on enrollment. It also raises questions about the efficiency of such emailing campaigns to fill training programs within a short time.

1.5.4 Enrollment

The final outcome variable is training enrollment after the intervention. This outcome is the most policy-relevant of our study but also the one that is most difficult to change in the short run. We measure enrollment using two different sources of data. A first dataset created by *Afpa* operators lists all individuals who enrolled in one of the listed programs after calling back the center. This dataset only contains 11 individuals, a strikingly low number given that 269 individuals had called back.[31] It is hard to know whether this bad performance is due to usual low turnout for the programs advertised in the experiment, due to the timing of the campaign, or due to data misreporting, or for other reasons.

As most programs were to start only two weeks after the emails were sent, we could have missed individuals who needed more time to make their decision and finalize their enrollment. Hence we turn to the more comprehensive training dataset compiled by *Pôle emploi* to measure participation to *any* training program (in any *Afpa* center or in other training center). Emails could indeed have raised interest in training beyond the listed programs of the campaign. To allow for short- and long-term effects, we measure enrollment one month and six months after the intervention.

Panels A and B of Table 1.5 show that our intervention had no visible impact on training enrollment after one and six months. Receiving any email (compared to not receiving an email) had no effect on training participation, nor did any of the additional messages taken separately.

[31] However, this number is in line with previous performances of *Afpa* campaigns. In a previous campaign run by *Afpa* and *Pôle emploi* three months before our intervention, 60 job seekers had enrolled out of an initial sample of 37 000 email recipients.

Looking specifically at enrollment to programs in *Afpa* centers, which is measured with some noise by *Pôle emploi*, we find again no detectable impact of any of our treatments (see Table 1.9 in appendix section 1.9.3).

To further investigate the reasons for this null effect, we turn to Panel IV of Table 1.2, which shows the distribution of individual characteristics among job seekers who participated in a training within the six months that followed the intervention. We see that more than 6% of the sample enrolled in a training program, with two thirds of them participating to a program that was longer than two weeks. To correctly interpret this number, one must remember that the experiment took place in the middle of a vast national program to boost training participation among job seekers. Therefore, even job seekers in the *Control group* were exposed to multiple information campaigns promoting training.

The selection pattern of trainees in our sample, that we show in panel IV of Table 1.2, looks somewhat different than the one for email openers in panel II. Although both trainees and email readers are positively selected on education levels, trainees are more often male, slightly younger than non-trainees and in more intensive assistance tracks, with about a year less experience in the job they search. This selection is absent or reversed if we look at openers, who are more often female and slightly older than non-openers. These differences in selection may suggest that emailing does not reach the job seekers who are most likely to be interested in training. This mis-targeting might have contributed to the low impact of the intervention.

1.6 Heterogeneity

In the final part of our paper, we now explore whether the treatment had heterogeneous effects to shed some light on whether the impact on callbacks was due to increasing salience or to belief updating.[32] Following Bleemer and Zafar (2018), in a scenario where the main mechanism driving the effects on callback is information updating, then additional messages in the treatment groups should have encouraged marginally misinformed or less-informed job seekers to call back.

[32]Given that we could not observe any average impact on enrollment, we show heterogeneity tests for callback only. Similar tests on enrollment variables show no heterogeneous patterns.

A second possible mechanism that is considered in Bleemer and Zafar (2018) is that messages increase information salience. If additional messages work through updating, we should see a negative correlation between these variables and the treatment effects. In the salience scenario and with no specific assumption on the joint distribution of attention and beliefs, we do not predict any particular correlation between treatment effect and initial beliefs. Nevertheless, if attention is also a driving mechanism that explains why certain individuals call back upon receiving the basic email, then it would come as no surprise that similarly attentive recipients are also more sensitive to additional messages in the treatment groups and we would detect a positive correlation between treatment effects and variables that characterize callers in the *Basic email group*.

To apply this test, we could in principle use the answers from the baseline survey to identify less-informed job seekers. Unfortunately the low response rate to the survey only allows us to run regressions on a fifth of the initial sample. Using this reduced sample size does not ensure sufficient power to detect any heterogeneity pattern. We propose an alternative method. In the updating scenario, those who called back in the *Basic email group* are individuals who already had accurate information whereas additional callers in the other treatment groups were less informed. We can hence use variables that characterize callers in the *Basic email group* to identify well-informed job seekers.

Following this reasoning, we first characterize individuals who called back in the *Basic email group*. We do it in a similar fashion as we do for the whole sample in panel III of Table 1.2. We find that people who called back in the *Basic email group* are more educated and in less intensive assistance tracks, as is showed in Table 1.6. We interpret these two patterns as indicating a high education level. Hence we group the two first education levels to create a dummy for having at least a high school degree and we run the following regression:

$$Y_i = \beta_1 Z_i + \beta_2 T_i + \beta_3 T_i \times Z_i + \gamma_1' X_i + \gamma_2' X_i \times Z_i + r_i^1 + r_i^2 \times Z_i + \epsilon_i \tag{1.4}$$

In this equation, Z_i is a dummy for having an education level above the *baccalauréat*, T_i is any treatment dummy, X_i are the same covariates as in previous regressions and r_i^j are region fixed effects. The coefficient of interest is β_3, which will be positive if treatment effects are higher on people with high Z_i.

To test for heterogeneous effects of each additional message, we adapt equation 1.4 and look at coefficients on interactions between Z_i and four treatment dummies:[33]

$$Y_i = \beta_1 Z_i + \sum_j \beta_2^j T_i^j + \sum_j \beta_3^j T_i^j \times Z_i + \gamma_1' X_i + \gamma_2' X_i \times Z_i + r_i^1 + r_i^2 \times Z_i + \epsilon_i \qquad (1.5)$$

The results presented in Table 1.7 confirm that high education is associated with a higher impact of receiving an email on callback. This can be seen by looking at columns (1) and (2) of Table 1.7. Having a high school diploma almost doubles the impact of calling back after receiving an email. The effect of receiving an email remains significant for those with low education (the coefficient on the simple dummy of receiving an email is positive and significant). This result shows that the average impact found in Table 1.4 is not entirely driven by those with higher education. The same pattern remains when we split the *Basic email group* and additional message groups, as showed in columns (3) and (4).

When we look at each treatment separately, as we do in the four last columns of Table 1.7, high education appears to significantly increase the impact of both email treatments with information on returns. The interaction with the *Cost email group* also turns positive and significant in columns (5) and (7) although the effect does not remain once we add additional covariates. Not all these effects remain significant in the restricted sample of openers only.

A possible interpretation for such pattern is that educated people are more familiar with emails and internet communication, and thus more likely to react to interventions that are sent by email. Attention in this context might be strongly correlated with digital literacy. To further explore this hypothesis, we run the same regressions from equations (1.4) and (1.5), this time interacting the treatments with a dummy for responding to baseline. Answering to online surveys is indeed correlated with how familiar individuals are with online communication in professional contexts

[33] Equation 1.4 has twice more coefficients to estimate than equation 1.1 because of interacted terms. This might prevent us from detecting an effect on the variable of interest. As a robustness check, we also run the same regressions dropping the *Cost email group*. This allows us to remove region fixed effects along with their interacted terms as all regions have then the same number of treatment groups. Results are showed in Table 1.10 of appendix section 1.9.4. They are very similar to those with region fixed effects.

and formal institutions like *Pôle emploi*.[34] Results from these regressions are remarkably consistent and strong. Columns (1) and (2) of Table 1.8 show that responding to baseline predicts an effect of receiving an email almost three times larger than for non-respondents. It also largely improves the efficiency of each separate message, especially for the email treatment with information on returns only.

These results should naturally be taken with caution. In these simple heterogeneity tests, variables that are interacted with treatment dummies are correlated with many other individual characteristics. It is therefore impossible to rigorously identify one main driving factor. However, available evidence suggest that the effects on callbacks are due to a salience effect benefiting those who are most familiar with digital communication.

1.7 Discussion and conclusion

We provided with a low-cost intervention embedded in an advertising campaign for public-sponsored training programs. A baseline survey suggests that there exist important information gaps on training that might affect job seekers' enrollment. This might not come as a surprise if we consider the complexity of the training system: the high diversity of programs as well as participants' and providers' heterogeneity make it almost impossible for any job seeker to gather all the information she might need to make an optimal decision. The existing literature itself remains puzzled by the persistent heterogeneity of training effects across participants and institutional settings. Nevertheless, this study focused on arguably simple features of training participation which one would assume to be common knowledge among job seekers. Yet even for such basic information, the baseline survey reveals that a significant fraction of job seekers hold incorrect beliefs. Taken at face value, these biased beliefs would be sufficient to strongly deter individuals from enrolling. An important question for future research is to better characterize those who are misinformed and exploit this information to design targeted intervention.

This work also shows that very simple messages can modify people's behaviors. Treatments

[34] Answering to the baseline survey may also have raised individuals' attention to their emails, especially when they related to training programs and independently of pre-existing digital literacy. We cannot rule out this interpretation.

only consisted in adding one sentence and a hyperlink to standard emails. Such light modifications are virtually costless and prove that details can make a difference. As these email campaigns are daily routine for public employment services, such marginal and cheap improvements can help to significantly raise communication efficiency.

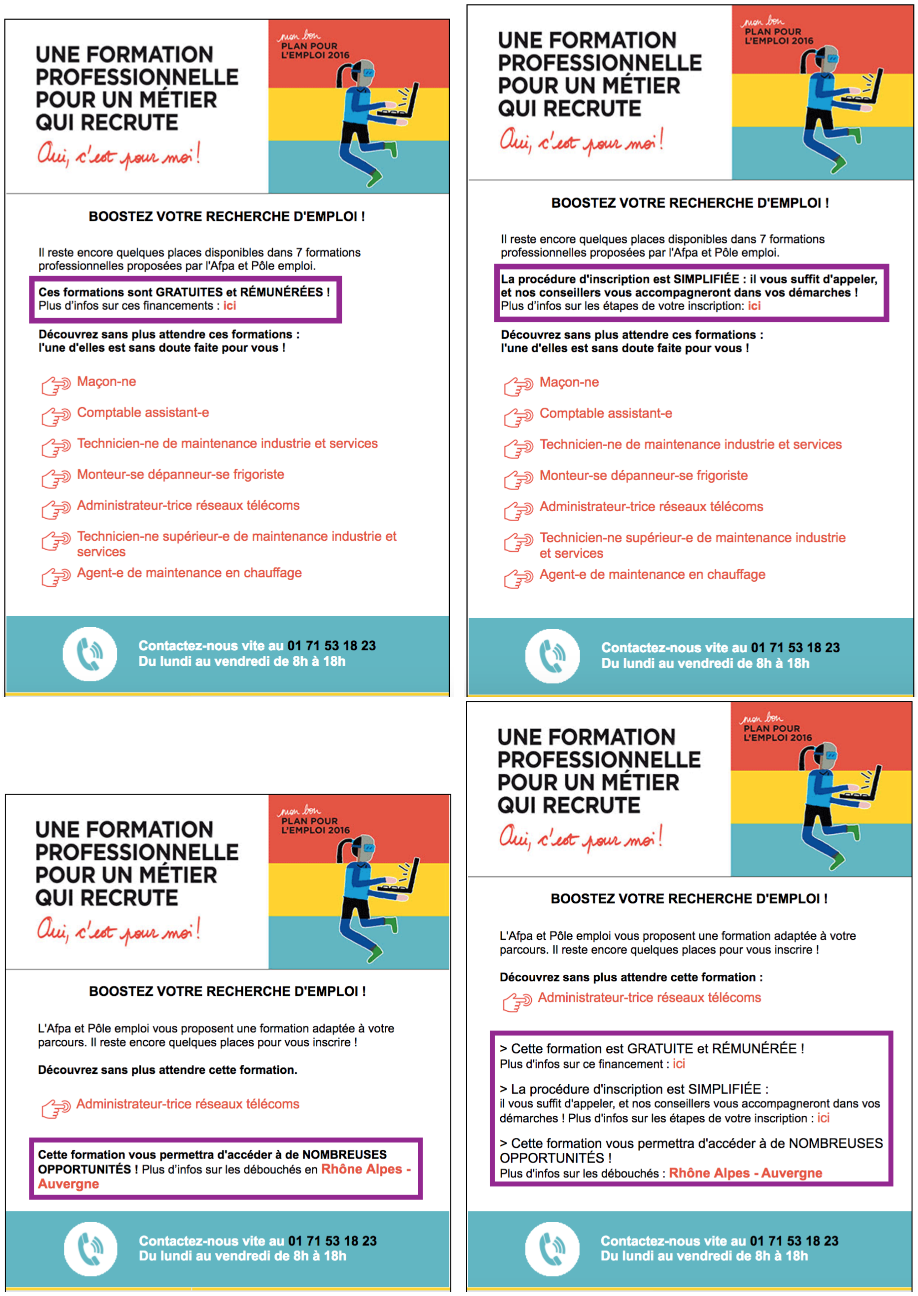
Yet, the intervention did not have any measurable impact on actual enrollment in training programs. While we face some statistical power constraints, we can rule out any large effect on training participation and the effects we obtain on callback are also very low in absolute value. As suggested by other studies on college enrollment (e.g. Carrell and Sacerdote (2017)), a fruitful avenue for future research and efficient policy could be to mix such online interventions with offline assistance and better targeting. The importance of caseworkers throughout the enrollment process suggests that information interventions can have a stronger impact if they are also targeted at *Pôle emploi* caseworkers themselves.

Overall, this study could be a first step to better understand the determinants of training participation. The inexpensive and policy-grounded aspect of our experiment makes it very easy to replicate, improve upon, and scale. More research could be undertaken to confirm the robustness of our results, by testing similar interventions on different samples, at different timings and advertising more specifically training programs with low demand and high returns. Similar messages could also be spread out through different communication channels to reach out to other types of job seekers.

Figure 1-1: Basic email

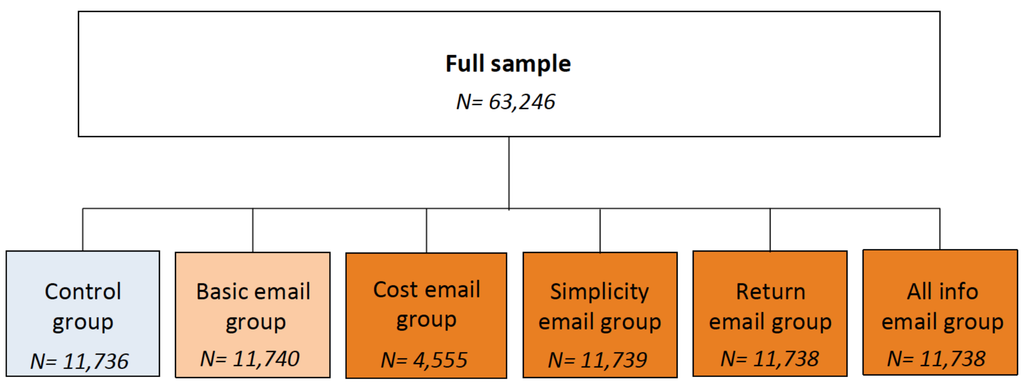
Notes: This figure shows the basic email that was sent to the *Basic email group*. This email served as template for all other emails. In the top left corner, the text says “A training program leading to many job opportunities: yes, it’s for me!”. The main text in the email says “BOOST YOUR JOB SEARCH! There are still some seats left in one of the 7 training programs offered by *Afpa* and *Pôle emploi*. Take a look: there surely is one for you!”. This text is followed by the list of programs in the region (here region 1). The bottom banner gives contact information to call *Afpa* centers.

Figure 1-2: Email types for each treatment group



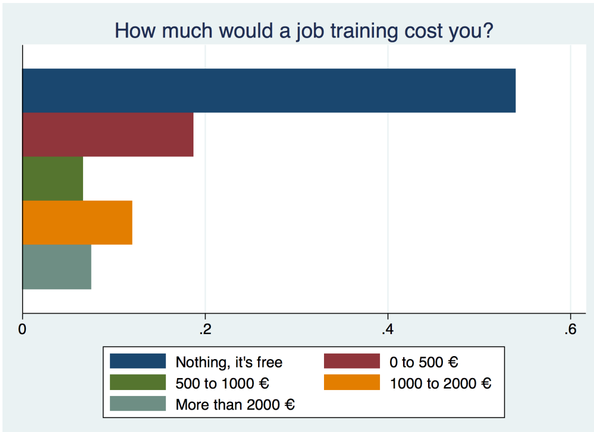
Notes: This figure shows the four emails in the additional message groups. The sections that differ from the basic email are in purple boxes. The first email on cost in the top left corner adds one sentence saying "This training is FREE and SUBSIDIZED! More info on funding options: here." The second email on simplicity adds one sentence saying "The registration procedure is SIMPLIFIED : you just need to call and our caseworkers will help you throughout the process ! More info on the steps towards enrollment : here." The third email at the bottom left is an example from the *Returns email group*. One can note that only one training program is showed, that is most adapted to the job seeker work trajectory. An additional sentence at the bottom of the email says "This training will help you get numerous job opportunities! More info on these opportunities in [REGION]." Finally the last email adds all these additional messages.

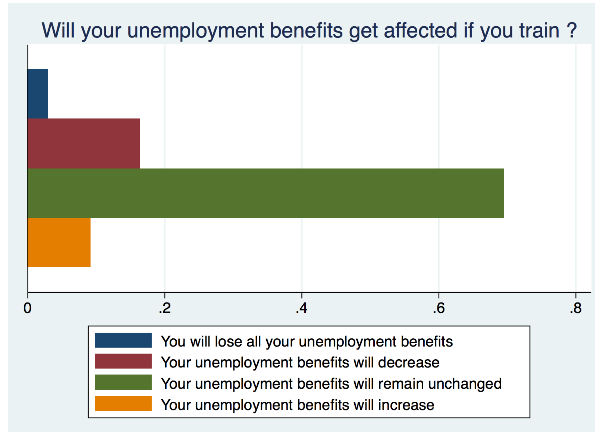
Figure 1-3: Randomization design



Notes: This figure illustrates the randomization design of the experiment, with the different treatment arms and their corresponding sample sizes.

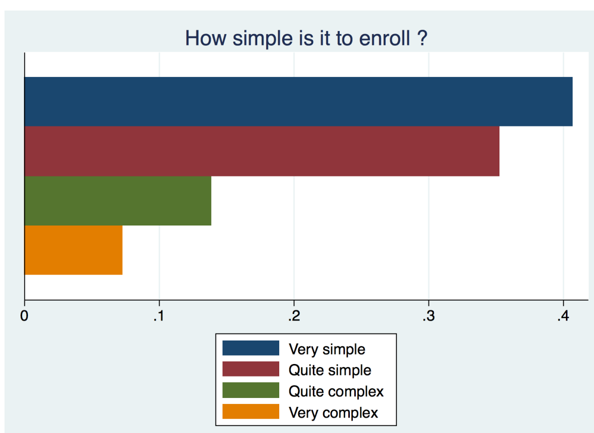
Figure 1-4: Answers to baseline questions on training cost





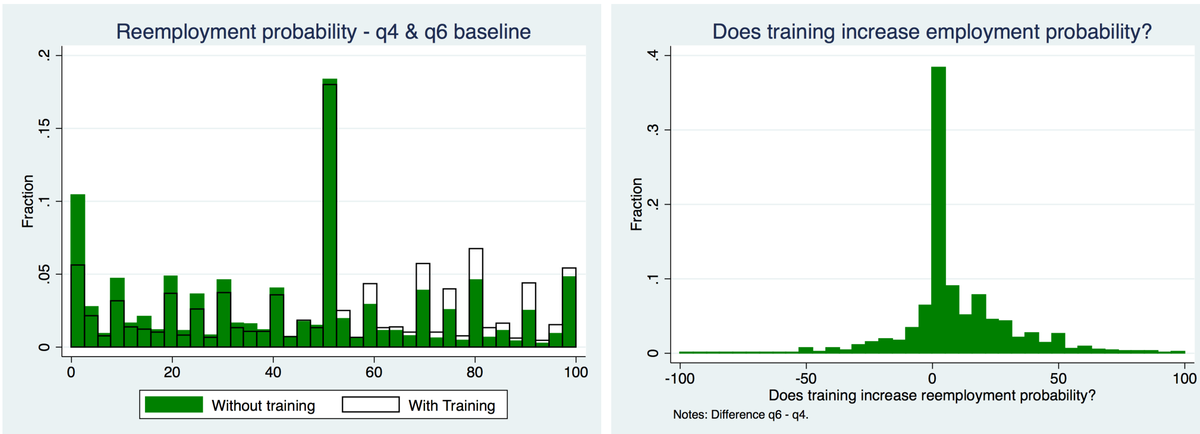
Notes: These histograms reflect the distribution of answers to the first two questions of the baseline survey. The horizontal axis shows the fraction of each answer from 0 to 1. While about half of respondents think training is free, the remaining fraction believe that it is costly. Similarly, while nearly seventy percent of respondents don't think unemployment benefits get affected upon enrollment in a training program, more than 20 percent either think that they decrease or get removed.

Figure 1-5: Answers to baseline question on enrollment simplicity



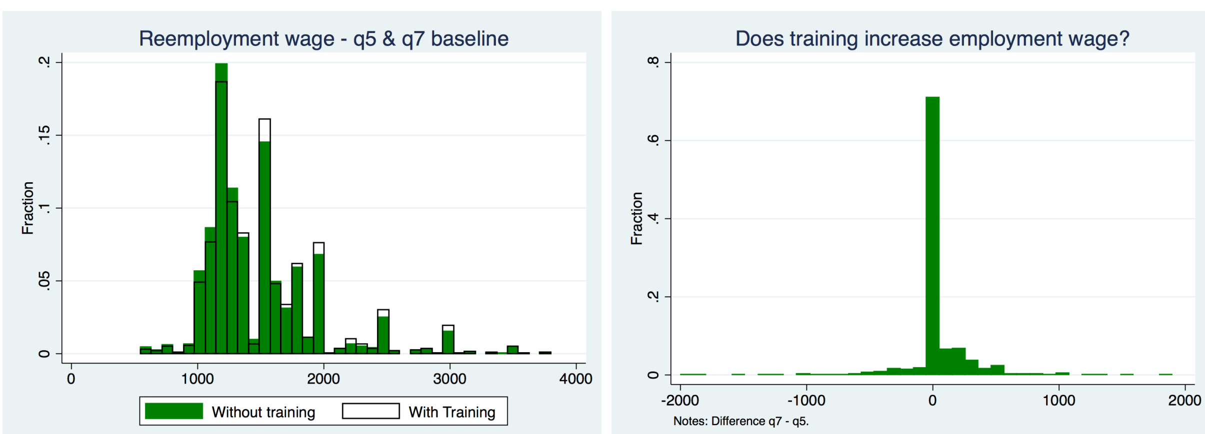
Notes: This histogram shows the distribution of answers to the third question of the baseline survey: "How simple is it to enroll to a job training?". The horizontal axis displays the fraction of each answer from 0 to 1. While about 40% respondents consider that it is quite simple to enroll, 35% view enrollment as quite complex and nearly 15% consider it to be very complex.

Figure 1-6: Expected reemployment likelihoods with and without training



Notes: These two graphs show the distribution of reemployment expectations with and without training. The first histogram reports the answers of both questions 4 and 6 of the baseline survey, asking respondents to estimate the probability of getting reemployed with and without training. The second graph computes the difference: a positive result means that the respondent believes training participation would increase her chances to get reemployed. Both histograms make it visible that respondents rather believe that training may help them getting reemployed, although nearly 40% expect training to make no difference on their reemployment chances.

Figure 1-7: Reemployment wages with and without training



Notes: These two graphs show the distribution of wage expectations with and without training. The first histogram reports the answers of both questions 5 and 7 of the baseline survey, asking respondents to estimate their future wage assuming they get reemployed within the following 6 months, with and without training. The second graph computes the difference: a positive result means that the respondent believes training would increase her future wage upon reemployment. Strikingly, more than 70% respondents expect training to make no difference for their future wage.

1.8 Tables

Table 1.1: Distribution of job seekers across regions and treatment arms

Region	Control group	Basic email group	Cost email group	Simplicity email group	Return email group	All info email group
Auvergne-Rhône-Alpes	2 870	2 871	2 870	2 870	2 870	2 870
Centre	1 684	1 685	1 685	1 685	1 685	1 685
Hauts de France	2 078	2 079	-	2 079	2 079	2 079
Nouvelle Aquitaine	5 104	5 105	-	5 105	5 104	5 104

Notes: This table shows the distribution of job seekers across treatment groups and regions. One can check that the sample size is similar across groups within each region. For administrative reasons, there was no *Cost email group* in Hauts de France and Nouvelle Aquitaine.

Table 1.2: Summary Statistics

	All	I. Baseline respondents			II. Opened the email			III. Called Afpa center			IV. Enrolled in a training		
		Non-resp	Resp	[3 - 2]	Not open	Open	[6 - 5]	No call	Call	[9 - 8]	No train	Train	[12 - 11]
	1	2	3	4	5	6	7	8	9	10	11	12	13
Female (%)	38.0	35.9	52.3	8.17***	34.7	41.4	6.77***	38.5	36.4	-2.09	38.4	32.8	-5.65***
Age	41.8	41.3	45.3	2.00***	41.3	42.3	1.06***	41.9	43.4	1.49**	41.9	40.5	-1.43***
Foreigner (%)	8.5	9.0	5.5	-1.75***	9.0	8.1	-0.84***	8.5	17.8	9.30***	8.5	8.6	0.07
Married (%)	50.6	49.8	55.9	3.05***	48.4	52.3	3.93***	50.6	46.3	-4.32	50.5	50.8	0.22
Number of children	1.0	1.0	1.0	-0.02***	1.0	1.0	-0.03**	1.0	1.0	-0.03	1.0	1.0	0.03*
Duration (months, capped at 18)	13.0	13.0	12.8	-0.13***	13.3	12.7	-0.60***	13.0	13.0	-0.02	13.1	11.5	-1.61***
Looking for short-term contract (%)	5.5	5.4	5.6	0.09	5.5	5.4	-0.10	5.5	5.4	-0.09	5.7	2.6	-3.05***
Looking for part-time work (%)	9.7	9.2	12.8	1.79***	9.1	10.5	1.42***	9.9	9.9	0.05	10.1	3.9	-6.18***
Formal training in desired job (%)	65.5	64.5	72.6	4.05***	61.9	68.2	6.36***	65.4	63.9	-1.56	65.3	69.3	4.05***
Programs match with desired job (%)	6.6	6.6	7.0	0.20	6.4	6.8	0.45**	6.6	12.3	5.71***	6.5	8.7	2.22***
Experience in desired job (months)	128.5	125.4	149.5	12.01***	123.9	132.2	8.25***	128.6	128.1	-0.57	129.3	117.6	-11.71***
Assistance track													
Low (%)	41.2	40.9	43.3	1.18***	41.3	41.6	0.31	41.5	37.7	-3.77	42.1	28.7	-13.43***
Moderate (%)	43.8	44.0	42.6	-0.65**	44.0	43.4	-0.57	43.6	44.8	1.13	43.6	46.6	3.00***
Intensive (%)	13.3	13.4	12.5	-0.45**	13.0	13.3	0.34	13.2	15.3	2.12	12.7	21.8	9.05***
Education level													
No degree (%)	12.8	13.4	8.4	-2.52***	15.1	11.3	-3.74***	13.0	5.6	-7.39***	13.1	8.1	-5.00***
Vocational degree (%)	43.3	44.7	33.8	-5.45***	47.4	39.5	-7.84***	43.0	30.6	-12.41***	43.4	41.8	-1.59**
High school diploma or GED (%)	23.5	22.7	29.0	3.18***	21.0	25.5	4.56***	23.5	34.3	10.84***	23.3	26.5	3.27***
Bachelor degree or more (%)	18.3	17.2	25.5	4.13***	15.0	21.0	6.07***	18.4	22.4	4.02	18.1	20.8	2.72***
Professional status													
Unskilled worker (%)	2.7	2.9	1.2	-0.87***	3.4	2.1	-1.32***	2.6	3.7	1.09	2.7	2.5	-0.17
Skilled worker (%)	32.6	34.4	20.6	-6.90***	37.4	28.7	-8.69***	32.5	24.3	-8.29***	32.7	32.4	-0.27
Employee (%)	55.1	53.4	66.5	6.55***	50.5	58.9	8.35***	55.2	60.1	4.84	54.7	60.8	6.06***
Manager (%)	3.2	2.8	5.8	1.50***	2.5	3.8	1.29***	3.2	1.9	-1.35	3.2	3.2	0.02
N =	63246	55175	8071		22362	29049		51242	268		59136	4110	
		87.2%	12.8%		43.5%	56.5%		99.5%	0.5%		93.5%	6.5%	

Notes: This table presents baseline summary statistics for outcome and control variables used in the main regression tables and the appendix tables, as well as other background variables mentioned in the paper. Column 1 displays the variable means in the whole sample. Panel I compares individuals who did not respond to baseline (column 2) to those who did (column 3). Columns 4 shows the coefficient we obtain by regressing a response dummy on the covariate of the row. Stars reflect the significance of the coefficient with robust standard errors. Panel II works similarly, comparing individuals who did not open the email they received (column 5) to those who did. This comparison is done among individuals who received an email, that is in all groups but the control group. Panel III compares individuals who called back Afpa and those who did not among people who received an email. Panel IV compares individuals who did and did not enroll in a training within the 6 months that followed the experiment in the whole sample. Variables are extracted from unemployment records (see appendix for table references). Formal degree in desired job means that the job seeker has a formal educational degree in the job he is looking for. No degree means that the individual has no high school diploma nor vocational degree. Email training in desired job means that the training that is advertized in the email that the job seeker receives leads to the same job as the one he is looking for. Past experience in the same job refers to job seekers who worked in the job that the offered training leads to. The two last rows refer to sample sizes and their percentage as a share of the relevant group of comparison (whole sample for panel I and IV, sub-sample of individuals who received an email for panel II and III). *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table 1.3: Balance table

	(1) Control (C) Mean	[S.D.]	(2) Basic C = Basic	(3) Cost C = Cost	(4) Simplicity C = Simp	(5) Returns C = Ret	(6) All info C = All
Female (%)	36.26	[48.08]	0.70	0.339	0.504	0.179	0.319
Age	41.62	[10.85]	0.20	0.084	0.192	0.167	0.171
Foreigner (%)	8.46	[27.83]	0.49	-0.138	-0.173	0.041	0.159
Married (%)	50.46	[50.00]	-0.14	0.428	-0.294	0.128	-0.247
Number of children	1.01	[1.23]	-0.01	-0.027	-0.017	-0.005	0.001
Duration (months, capped at 18)	13.06	[5.61]	0.00	-0.150	-0.089	-0.080	-0.020
Looking for short-term contract (%)	5.51	[22.81]	-0.44	0.014	-0.093	0.019	0.166
Looking for part-time work (%)	8.91	[28.48]	0.51	1.373*	0.600	0.373	0.304
Formal training in desired job (%)	66.08	[47.35]	-0.46	-0.624	-0.758	-1.769***	-0.269
Programs match with desired job (%)	6.49	[24.64]	0.08	-0.046	0.058	-0.018	-0.001
Experience in desired job (months)	127.96	[100.62]	0.38	2.286	0.760	0.194	1.238
Assistance track							
Low (%)	40.27	[49.05]	0.28	1.769*	0.961	1.084*	1.041
Moderate (%)	44.37	[49.68]	0.10	-1.129	-0.752	-0.928	-0.076
Intensive (%)	13.77	[34.46]	-0.44	-0.837	-0.395	-0.266	-1.025**
Education level							
No high school nor vocational degree (%)	12.18	[32.71]	1.17***	-0.354	0.729*	1.370***	0.458
Vocational degree (%)	44.94	[49.75]	-1.63**	-0.710	-0.719	-1.132*	-0.919
High school diploma or GED (%)	23.25	[42.25]	0.37	-1.038	-0.049	-0.651	-0.464
Bachelor degree or more (%)	17.70	[38.17]	0.10	2.147**	0.055	0.431	0.943*
N =	11736		11740	4555	11739	11738	11738

Notes: This table shows balance tests across treatment arms to check that the randomization was successful at creating statistically comparable groups. The first two columns show variable means in the control group that received no email, with standard deviations in brackets. Column (2) shows the coefficients of regressions testing the effect on each variable of belonging to the basic email group compared to the control group. Columns (3) to (6) proceed similarly for each treatment group. We use robust standard errors for all regressions and three stars indicate a p-value < 0.01; two stars indicate a p-value < 0.05; one star indicates a p-value < 0.1. We observe that the randomization was successful at balancing groups along observable characteristics. A few significant and small differences emerge, as is expected from such statistical procedure. The last row of the table shows the sample size in each treatment group. The cost group is smaller as it was only implemented in region Auvergne-Rhone-Alpes and Centre.

Table 1.4: Impact on callback

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed only	(6) Emailed only	(7) Openers only	(8) Openers only
Received any email	.496***	.45***						
	(.0321)	(.0322)						
Received basic email			.264***	.238***				
			(.0489)	(.0486)				
Received basic email and message			.565***	.514***				
			(.0387)	(.0388)				
- *Cost*					.0563	-.035	.132	-.0556
					(.132)	(.127)	(.218)	(.212)
- *Simplicity*					.196**	.194**	.359***	.305**
					(.0793)	(.0795)	(.138)	(.141)
- *Returns*					.409***	.352***	.699***	.546***
					(.0899)	(.0879)	(.154)	(.153)
- *All info*					.366***	.371***	.572***	.547***
					(.0878)	(.089)	(.148)	(.154)
Mean in the control group	.0085	.0085	.0085	.0085	-	-	-	-
Mean in the basic email group	-	-	-	-	.2726	.2726	.4459	.4459
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	58314	63246	58314	51510	47498	29049	26644

Notes: This table shows the effect of receiving emails on calling back *Afpa* center. All regressions use a callback dummy as their outcome. In column (1) we group all individuals who received an email and compare them to those who received no email (the control group) , as per equation (1.1). Column (2) adds to this regression a set of covariates including gender, age, assistance intensity at Pôle emploi and educational levels as covariates. Column (3) splits emailed individuals into two groups: the first explaining variable is a dummy for being in the basic email group and the second is a dummy for all other email groups, as per equation (1.2). Column (4) adds covariates. In column (5), we remove the control group and regress callback on five separate dummies for each email treatment group, using the basic email group as the reference group as per equation (1.3). Column (6) adds covariates. Finally columns (7) and (8) display the results of the same regression as (5) and (6) on a restricted sample with only individuals who opened the email they received. All regressions include region fixed effects. Means in the reference groups are computed separately. Standard errors are in parenthesis: *** p-value $<$ 0.01, ** p-value $<$ 0.05, * p-value $<$ 0.1.

Table 1.5: Impact on enrollment

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed Only	(6) Emailed Only	(7) Openers only	(8) Openers only
Panel A: Enrollment 1 month after the intervention								
Received any email	.0157	.0311						
	(.142)	(.152)						
Received basic email			.0845	.0935				
			(.183)	(.195)				
Received basic email and message			-.0049	.0123				
			(.145)	(.155)				
- *Cost*					-.135	-.146	-.0402	-.0171
					(.235)	(.254)	(.338)	(.368)
- *Simplicity*					-.085	-.09	.176	.196
					(.183)	(.195)	(.277)	(.295)
- *Returns*					-.144	-.152	-.191	-.188
					(.181)	(.193)	(.268)	(.285)
- *All info*					-.0252	.0151	.196	.307
					(.184)	(.197)	(.278)	(.299)
Mean in the control group	1.9598	1.9598	1.9598	1.9598	-	-	-	-
Mean in the basic email group	-	-	-	-	2.0443	2.0443	2.4912	2.4912
Panel B: Enrollment 6 months after the intervention								
Received any email	.282	.341						
	(.25)	(.265)						
Received basic email			.322	.386				
			(.321)	(.341)				
Received basic email and additionnal message			.271	.327				
			(.257)	(.273)				
- *Cost*					.12	.0221	.617	.485
					(.434)	(.464)	(.629)	(.673)
- *Simplicity*					-.0846	-.111	.108	.11
					(.324)	(.343)	(.48)	(.51)
- *Returns*					.129	.13	.141	.141
					(.326)	(.346)	(.48)	(.509)
- *All info*					-.246	-.217	-.0934	.0409
					(.322)	(.342)	(.477)	(.508)
Mean in the control group	6.3309	6.3309	6.3309	6.3309	-	-	-	-
Mean in the basic email group	-	-	-	-	6.6525	6.6525	8.1808	8.1808
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	58314	63246	58314	58314	47498	29049	26644

Notes: This table shows the effect of receiving emails on enrolling to a training program. It replicates the regressions and the format of table 3 with a different dependant variable. In panel A we measure enrollment 1 month after the intervention; in panel B we measure enrollment 6 months after the intervention. See more detailed explanations in the footnotes of table 1.4.

Table 1.6: Summary statistics in the basic email group

	All	Called Afpa center		
		No call	Call	[3 - 2]
	1	2	3	4
Female (%)	37.0	37.0	37.9	0.98
Age	41.8	41.8	44.3	2.50
Foreigner (%)	9.0	8.9	20.7	11.77
Married (%)	50.3	50.4	37.5	-12.85
Number of children	1.0	1.0	1.0	-0.04
Duration (months, capped at 18)	13.1	13.1	13.6	0.50
Looking for short-term contract (%)	5.1	5.0	13.8	8.75
Looking for part-time work (%)	9.4	9.4	13.8	4.38
Formal training in desired job (%)	65.6	65.6	56.7	-8.98
Programs match with desired job (%)	6.6	6.6	12.5	5.94
Experience in desired job (months)	128.3	128.3	147.8	19.55
Assistance track				
Low (%)	40.5	40.6	31.2	-9.32
Moderate (%)	44.5	44.4	53.1	8.68
Intensive (%)	13.3	13.3	15.6	2.30
Education level				
No high school nor vocational degree (%)	13.4	13.4	9.4	-3.99
Vocational degree (%)	43.3	43.3	34.4	-8.96
High school diploma or GED (%)	23.6	23.6	43.8	20.18**
Bachelor degree or more (%)	17.8	17.8	6.2	-11.58***
Professional status				
Unskilled worker (%)	2.7	2.7	3.1	0.45
Skilled worker (%)	33.2	33.2	18.8	-14.45**
Employee (%)	54.7	54.7	65.6	10.93
Manager (%)	3.3	3.3	0.0	-3.31***
N =	11740	11708	32	
		99.7%	0.3%	

Notes: This table presents summary statistics for the basic email group. It is structured in a similar fashion as table 1.2. Column 1 displays the variable means in the basic email group. Columns 2, 3 and 4 compare individuals who called back Afpa and those who did not. See more detailed explanations in the footnotes of table 1.2.

Table 1.7: Heterogeneous impact of having a high level of formal education on callback

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed only	(6) Emailed only	(7) Openers only	(8) Openers only
High educ	.242**	.206	.235**	.154	.428**	.292	.445	.722
	(.098)	(.188)	(.0978)	(.192)	(.17)	(.302)	(.276)	(.44)
Received any email	.318***	.299***						
	(.0372)	(.0376)						
Received any email X High educ	.412***	.394***						
	(.0672)	(.068)						
Received basic email			.195***	.17***				
			(.0582)	(.0574)				
Received basic email X High educ			.163	.194*				
			(.102)	(.104)				
Received basic email and message			.355***	.338***				
			(.0436)	(.0442)				
Received basic email and message X High educ			.484***	.451***				
			(.0811)	(.0819)				
- *Cost*					-.162	-.184*	-.293	-.331
					(.116)	(.11)	(.217)	(.206)
- *Cost X High educ*					.439*	.261	.755*	.478
					(.251)	(.243)	(.398)	(.387)
- *Simplicity*					.118	.141	.204	.216
					(.0897)	(.091)	(.168)	(.172)
- *Simplicity X High educ*					.179	.118	.321	.222
					(.167)	(.171)	(.278)	(.287)
- *Returns*					.236**	.228**	.456**	.423**
					(.0988)	(.0982)	(.188)	(.188)
- *Returns X High educ*					.406**	.312*	.511	.341
					(.191)	(.189)	(.313)	(.314)
- *All info*					.212**	.224**	.353*	.375**
					(.0974)	(.0977)	(.181)	(.187)
- *All info X High educ*					.355*	.364*	.451	.417
					(.185)	(.192)	(.299)	(.315)
Mean of calls in the control group	.0085	.0085	.0085	.0085	-	-	-	-
Mean of calls in the basic email group	-	-	-	-	.2726	.2726	.4459	.4459
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	59568	63246	59568	51510	48543	29049	27342

Notes: This table shows the heterogeneous effect of receiving emails on calling back *Afpa* center depending on education level. All regressions use a callback dummy as their outcome and include region fixed effects, as per equation (1.5). Columns are structured in a similar fashion as table 1.4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 1.4.

Table 1.8: Heterogeneous impact of having responded to baseline on callback

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed only	(6) Emailed only	(7) Openers only	(8) Openers only
Answered baseline	.13	.11	.12	-.0356	.304	.409	.111	-.137
	(.169)	(.351)	(.168)	(.353)	(.281)	(.527)	(.349)	(.763)
Received any email	.401***	.383***						
	(.0316)	(.0321)						
Received any email X Answered baseline	.767***	.695***						
	(.136)	(.135)						
Received basic email			.244***	.229***				
			(.0506)	(.0509)				
Received basic Email X Answered baseline			.156	.167				
			(.172)	(.183)				
Received basic email and message			.448***	.429***				
			(.0375)	(.0381)				
Received basic email and message X Answered baseline			.947***	.851***				
			(.168)	(.167)				
- *Cost*					-.0236	-.115	.0061	-.173
					(.128)	(.123)	(.23)	(.224)
- *Cost X Answered baseline*					.681	.636	.67	.706
					(.485)	(.474)	(.585)	(.576)
- *Simplicity*					.135*	.143*	.289**	.271*
					(.0789)	(.0805)	(.147)	(.153)
- *Simplicity X Answered baseline*					.49	.433	.366	.308
					(.323)	(.329)	(.394)	(.403)
- *Returns*					.25***	.218***	.458***	.363**
					(.0856)	(.0847)	(.156)	(.156)
- *Returns X Answered baseline*					1.32***	1.2***	1.32***	1.23**
					(.412)	(.418)	(.498)	(.501)
- *All info*					.289***	.33***	.469***	.528***
					(.0877)	(.0908)	(.157)	(.167)
- *All info X Answered baseline*					.647*	.488	.572	.361
					(.357)	(.364)	(.437)	(.446)
Mean of calls in the control group	.0085	.0085	.0085	.0085	-	-	-	-
Mean of calls in the basic email group	-	-	-	-	.2726	.2726	.4459	.4459
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	59568	63246	59568	51510	48543	29049	27342

Notes: This table shows the heterogeneous effect of receiving emails on calling back *Afpa* center depending on whether individuals responded to baseline. All regressions use a callback dummy as their outcome, with region fixed effects, as per equation (1.5). Columns are structured in a similar fashion as table 1.4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 1.4.

1.9 Appendix

1.9.1 Translation of the baseline questionnaire

Thank you for participating in this survey.

It will only take 3 minutes to answer !

[q1] **In your opinion, if you decide to participate in a 6 month vocational training offered by Pôle emploi, how much would it cost you? (apart from indirect costs such as transportation or childcare expenses)?** *(Selon vous, si vous décidez de suivre une formation professionnelle de 6 mois proposée par Pôle emploi, combien cela vous coûtera-t-il (en dehors des frais indirects comme les transports ou la garde des enfants) ?)*

- Nothing, it's free
- Between 0 and 500
- Between 500 and 1000
- Between 1000 and 2000
- More than 2000

[q2] **In your opinion, if you participate in a 6 month vocational training offered by Pôle emploi:** *(Selon vous, si vous suivez une formation professionnelle de 6 mois proposée par Pôle emploi:)*

- You will loose all your unemployment benefits
- Your unemployment benefits will decrease
- Your unemployment benefits will remain unchanged
- Your unemployment benefits will increase

[q3] **In your opinion, the steps to enrol into a 6 month vocational training offered by Pôle emploi are:** *(Vous pensez que les démarches pour s'inscrire dans une formation professionnelle de 6 mois proposée par Pôle emploi sont:)*

- Very simple
- Quite simple
- Quite complex
- Very complex

[q4] **What are the chances that you find a full-time job within the next 12 months?**
Set a percentage between 0 and 100 using the cursor.
0 (very low) 100 (very high) *Quelles sont vos chances de retrouver un emploi à temps plein dans les 12 prochains mois? Indiquez un pourcentage entre 0 et 100 à l'aide du curseur. 0 (très faibles) 100 (très fortes))*

[q5] **If you find a full-time job in your professional sector or in a closely related one within the next 12 months, how much will be your net monthly wage?**
Set an amount between 0 and 100000 euros. *(Si vous obtenez un emploi à temps plein dans votre secteur d'activité ou dans un secteur proche dans les 12 prochains mois, de combien sera votre salaire mensuel net ? Indiquez un montant entre 0 et 100000 euros.)*

Imagine from now on that you have participated in a 6 month vocational training for a job in your professional sector or a closely related sector. *(Imaginez à présent que vous avez suivi une formation professionnelle de 6 mois dans un métier de votre secteur d'activité ou dans un secteur proche.)*

[q6] **What are the chances that you find a full-time job within the 12 months following the training ?**
Set a percentage between 0 and 100 using the cursor.
0 (very low) 100 (very high) *(Quelles sont vos chances de retrouver un emploi à temps plein*

dans les 12 mois qui suivent la formation? Indiquez un pourcentage entre 0 et 100 à l'aide du curseur. 0 (très faibles) 100 (très fortes))

[q7] **If you find a full-time job in your professional sector or in a closely related one within the 12 months following the training, how much will be your net monthly wage?**
Set an amount between 0 and 100000 euros. *(Si vous obtenez un emploi à temps plein dans votre secteur d'activité ou dans un secteur proche dans les 12 mois qui suivent la formation, de combien sera votre salaire mensuel net ? Indiquez un montant entre 0 et 100000.)*

1.9.2 Test of low bandwidth mechanism

A possible explanation of the differential impact of the email with information on returns is that emails are easier to read because they display only the relevant training program for the job seeker instead of a list. Job seekers with low bandwidth might have identified their program of interest more quickly than among a list of 4 to 6 other programs (see figure 1-2). In order to capture this low bandwidth effect, we had agreed with our partners to add one additional group in the smallest region of the experiment. In this group, emails were identical to the basic email and did not contain any additional message, but they only displayed the most relevant training program for the job seeker instead of a list of 5 programs as in figure1-1. Had return emails generated an increase in callback simply by raising attention to the most relevant training program, we would in theory have observed a similar effect in the target email group. Unfortunately, the sample sizes are likely too small to detect any significant effect and conclude on this hypothesis.

1.9.3 Impact on enrollment in *Afpa* centers

The intervention could have increased enrollment to programs in *Afpa* centers beyond the listed programs, possibly at the expense of other centers. Unemployment records contain a dummy variable that is meant to indicate whether job seekers who participated in a training enrolled in an *Afpa* center. Although we could not ensure the robustness of the variable, we use it to test whether the fraction of enrolled job seekers in an *Afpa* center increased upon receiving emails.

Looking at the fraction of enrolled job seekers is valid from a statistical inference point of view, as the intervention had no effect on the total number of job seekers enrolled. Results are showed in Table 1.9. We see no robust impact of any treatment on this outcome.

1.9.4 Heterogeneous effects without region fixed effects

As a robustness check, we show here the results of the same regressions as in Table 1.7 and 1.8 but without region fixed effects in order to reduce the number of coefficients. To balance sample sizes by treatment arms across regions, we remove the *Cost email group*. Results are presented in Table 1.10 and 1.11. We see no difference with the tables with fixed effects: both variables reinforce the effect of receiving an email and boost the effect of emails with messages on returns.

Table 1.9: Impact on enrollment at Afpa as a fraction of total enrollment

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed Only	(6) Emailed Only	(7) Openers only	(8) Openers only
Panel A: Enrollment 1 month after the intervention								
Received any email	-.0741	.148						
	(.831)	(.846)						
Received basic email			-.647	-.597				
			(1.02)	(1.04)				
Received basic email and message			.101	.378				
			(.857)	(.874)				
- *Cost*					1.31	1.74	1.33	1.9
					(1.58)	(1.78)	(1.97)	(2.25)
- *Simplicity*					.53	.895	.171	.379
					(1.01)	(1.05)	(1.25)	(1.33)
- *Returns*					.208	.454	.654	.902
					(.98)	(1.02)	(1.27)	(1.35)
- *All info*					1.22	1.21	.757	.496
					(1.06)	(1.09)	(1.29)	(1.32)
Mean in the control group	4.0480	4.0480	4.0480	4.0480	-	-	-	-
Mean in the basic email group	-	-	-	-	3.3382	3.3382	3.7895	3.7895
Panel B: Enrollment 6 months after the intervention								
Received any email	.189	.515						
	(1.42)	(1.46)						
Received basic email			-2.4	-2.15				
			(1.73)	(1.76)				
Received basic email and additionnal message			.984	1.34				
			(1.47)	(1.5)				
- *Cost*					2.66	2.57	1.08	.972
					(2.64)	(2.77)	(3.14)	(3.3)
- *Simplicity*					2.37	2.75	.813	1.18
					(1.72)	(1.76)	(2.11)	(2.18)
- *Returns*					3.62**	3.74**	3.77*	3.83*
					(1.75)	(1.8)	(2.19)	(2.26)
- *All info*					4.16**	4.02**	2.05	1.75
					(1.79)	(1.82)	(2.15)	(2.18)
Mean in the control group	12.8936	12.8936	12.8936	12.8936	-	-	-	-
Mean in the basic email group	-	-	-	-	10.3048	10.3048	11.7895	11.7895
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	3621	3357	3621	3357	3357	2738	2110	1939

Notes: Same regressions as in table 1.5, restricting the sample to enrollees only and replacing the enrollment outcome with the fraction of enrollees in an *Afpa* program. Standard errors are in parenthesis: *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table 1.10: Heterogeneous impact of having a high level of formal education on callback (regressions without region fixed effects)

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed only	(6) Emailed only	(7) Openers only	(8) Openers only
High educ	-.0149	-.256**	-.0149	-.316***	.143	-.181	.13	.0507
	(.0149)	(.111)	(.0149)	(.118)	(.1)	(.224)	(.166)	(.336)
Received any email	.322***	.305***						
	(.0373)	(.038)						
Received any email X High educ	.431***	.413***						
	(.0684)	(.0696)						
Received basic email			.196***	.172***				
			(.0581)	(.0573)				
Received basic email X High educ			.158	.189*				
			(.102)	(.104)				
Received basic email and message			.36***	.346***				
			(.0437)	(.0447)				
Received basic email and message X High educ			.508***	.476***				
			(.0824)	(.0835)				
- *Cost*					-.0638	-.0544	-.106	-.0994
					(.102)	(.0912)	(.192)	(.175)
- *Cost X High educ*					.627***	.456**	.969***	.695**
					(.231)	(.22)	(.367)	(.352)
- *Simplicity*					.118	.14	.203	.213
					(.0896)	(.0909)	(.168)	(.172)
- *Simplicity X High educ*					.184	.124	.327	.227
					(.167)	(.171)	(.278)	(.287)
- *Returns*					.235**	.228**	.453**	.418**
					(.0987)	(.0981)	(.188)	(.188)
- *Returns X High educ*					.41**	.321*	.5	.335
					(.191)	(.19)	(.313)	(.314)
- *All info*					.211**	.223**	.347*	.367**
					(.0973)	(.0977)	(.18)	(.186)
- *All info X High educ*					.36*	.37*	.451	.419
					(.185)	(.192)	(.299)	(.315)
Mean of calls in the control group	.0085	.0085	.0085	.0085	-	-	-	-
Mean of calls in the basic email group	-	-	-	-	.2726	.2726	.4459	.4459
Region FE	No	No	No	No	No	No	No	No
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	59568	63246	59568	51510	48543	29049	27342

Notes: This table shows the heterogeneous effect of receiving emails on calling back *Afpa* center depending on education level. All regressions use a callback dummy as their outcome, for the restricted sample where we remove the cost email group. This allows to remove region fixed effects. Columns are structured in a similar fashion as table 1.4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 1.4.

Table 1.11: Heterogeneous impact of having responded to baseline on callback (regressions without region fixed effects)

	(1) ITT	(2) ITT	(3) ITT	(4) ITT	(5) Emailed only	(6) Emailed only	(7) Openers only	(8) Openers only
Answered baseline	-.0097	-.285	-.0097	-.439*	.151	.0223	.0502	-.688
	(.0097)	(.242)	(.0097)	(.251)	(.172)	(.442)	(.218)	(.59)
Received any email	.413***	.397***						
	(.0321)	(.0328)						
Received any email X Answered baseline	.781***	.71***						
	(.138)	(.138)						
Received basic email			.244***	.229***				
			(.0506)	(.0509)				
Received basic Email X Answered baseline			.161	.175				
			(.172)	(.183)				
Received basic email and message			.463***	.447***				
			(.0381)	(.0389)				
Received basic email and message X Answered baseline			.964***	.868***				
			(.171)	(.17)				
- *Cost*					.168	.107	.329	.201
					(.116)	(.109)	(.211)	(.201)
- *Cost X Answered baseline*					.753	.702	.7	.687
					(.462)	(.458)	(.557)	(.554)
- *Simplicity*					.135*	.143*	.291**	.271*
					(.0789)	(.0805)	(.147)	(.153)
- *Simplicity X Answered baseline*					.496	.435	.377	.309
					(.323)	(.329)	(.394)	(.403)
- *Returns*					.251***	.22***	.452***	.358**
					(.0856)	(.0848)	(.157)	(.157)
- *Returns X Answered baseline*					1.32***	1.18***	1.32***	1.22**
					(.413)	(.419)	(.498)	(.502)
- *All info*					.289***	.331***	.464***	.523***
					(.0878)	(.0909)	(.157)	(.167)
- *All info X Answered baseline*					.642*	.481	.573	.362
					(.357)	(.364)	(.437)	(.447)
Mean of calls in the control group	.0085	.0085	.0085	.0085	-	-	-	-
Mean of calls in the basic email group	-	-	-	-	.2726	.2726	.4459	.4459
Region FE	No	No	No	No	No	No	No	No
Covariates	No	Yes	No	Yes	No	Yes	No	Yes
Sample	All	All	All	All	Emailed	Emailed	Openers	Openers
N =	63246	59568	63246	59568	51510	48543	29049	27342

Notes: This table shows the heterogeneous effect of receiving emails on calling back *Afpa* center depending on whether individuals responded to baseline. All regressions use a callback dummy as their outcome, for the restricted sample where we remove the cost email group. This allows to remove region fixed effects. Columns are structured in a similar fashion as table 1.4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 1.4.

Chapter 2

Can a Website Bring Unemployment Down? Effects of a French Online Platform on Job Search Efficiency

Joint work with Bruno Crépon[*], Esther Mbih[†], Louise Paul-Delvaux[‡], Bertille Picard[§] and Vincent Pons[¶*]

2.1 Introduction

Helping unemployed workers to search for jobs more effectively is central to the agenda of employment agencies. Job seekers face considerable challenges in gathering information on occupations and locations where they are most likely to find employment and in identifying useful search methods and tools.[6] These informational barriers can reinforce behavioral biases and dissatisfaction,

[*]CREST-ENSAE
[†]CREST-ENSAE
[‡]Harvard University
[§]Aix-Marseille University, CNRS, EHESS, Centrale Marseille, AMSE
[¶]Harvard Business School
[*]We are grateful to Pierre-Louis Bithorel, Quiterie Landèche and Marion Richard for great research assistance, and to our partners at Bayes Impact and Pôle emploi. We are also grateful to the foundation *La France S'Engage* for financial support.

[6]On the difficulty of identifying occupations and geographical areas with the highest chances of reemployment, see e.g. Şahin et al. (2014), Patterson et al. (2016), Papageorgiou (2014), and Herz and van Rens (2019) on occupational mismatch; Altmann et al. (2018) and Gee (2019) on perceived market tightness; Marinescu and Rathelot (2018) on geographical mismatch; Beaman and Magruder (2012), Belot et al. (2019), and Abel et al. (2019) on search methods; and Della Vigna and Paserman (2005), Caliendo et al. (2015), Spinnewijn (2015), and DellaVigna et al. (2020) on sub-optimal search effort.

generating a vicious circle in which bad search outcomes lower one's self-esteem, motivation, and search effort (Babcock et al. (2012)). By and large, evaluations of public programs providing job search assistance have not been conclusive on their impact (Card et al. (2018)). Over the last two decades, numerous private websites have been launched to complement the public sector effort by taking advantage of modern digital technologies. These websites primarily work as job boards, bringing together job seekers and job openings. They also frequently contain modules with pieces of advice to help job seekers improve their job search strategy. Even though these websites are increasingly used by job seekers, there is little evidence on their effectiveness. The appeal of online assistance services is that they can be easily implemented at scale, updated and customized to meet users' needs (Kuhn and Skuterud (2004), Kuhn and Mansour (2014), Autor (2009), Horton (2017)). On the other hand, digital tools also come with limitations. Certain sub-groups of the population, especially among the unemployed, are not familiar with digital technologies and may find it difficult to use these websites. Moreover, the content of the recommendations provided by websites may not be sufficiently tailored to each user.

In this paper, we study the impact of "Bob Emploi," an online platform dedicated to help job seekers in France overcome informational barriers and identify effective search strategies. Bob Emploi was launched in November 2016 by Bayes Impact, a private non-profit organization working with tech developers to analyze rich data collected by public administrations with state-of-the-art methods. To design Bob emploi, the tech NGO benefited from a collaboration with the French public service of employment, Pôle emploi. This innovative partnership sets Bob Emploi apart from other private sites. At its creation, Bob Emploi aroused much interest and enthusiasm and it benefited from continued support by policymakers as well as important financial aid. As the French minister of Labor of that time put it, it represented a promising opportunity for "innovators in the tech industry to contribute to social inclusiveness." Bayes Impact C.E.O. set high expectations by announcing that the website and its algorithm had the potential to lower the unemployment rate by 10%.

When job seekers first log into the Bob Emploi website, they are asked to provide socio-demographic information and to indicate their target job and the status of their current job search (e.g., the number of applications sent and the number of interviews obtained in the past weeks).

Bob Emploi then compares user profiles with job market data coming from two main sources. The first dataset contains information about a representative sample of job seekers registered at Pôle emploi on socio-demographic variables, career objectives, search methods, and employment outcomes. This dataset includes variables such as age, gender, and highest educational degree, desired job and geographic search zone, and dates and duration of unemployment spells. Second, Bob Emploi uses data on job postings on the Pôle emploi job board and results of an annual survey on employers' expected labor demand. Based on this data, the website provides users with an employment assessment which evaluates how likely their job strategy is to be successful and guides them towards economic sectors and geographic areas that may increase their chances to find a job. In addition, Bob Emploi groups together recommendations of search methods and tools from several job search and recruitment professionals. The website builds on this advice to propose a list of steps that can help users overcome the obstacles and bottlenecks they face in their job search.

We designed a randomized experiment to investigate whether using Bob Emploi modifies job seekers' search strategies and, in turn, accelerates re-employment. The impact evaluation is based on a large-scale encouragement design implemented in 254 Pôle emploi local agencies. Our sample consists of 212 277 job seekers whose registration at Pôle emploi had begun less than a year before the intervention. Randomization was conducted at the individual level, and about half of the sample was assigned to a treatment group. Individuals in this group were invited to attend an information session at their local agency. During this session, Pôle emploi caseworkers introduced them to Bob Emploi.[7] In addition, all job seekers in the treatment group received three advertising emails encouraging them to create an account on Bob Emploi.

We follow the trajectories of individuals in our sample over 18 months after the intervention. We rely on administrative records from Pôle emploi to measure the rate of reemployment, socio-demographic variables, interactions between job seekers, and the use of Pôle emploi's support services (e.g. meeting with a caseworker, attending a job training program or a group resume editing session). The Pôle emploi data also record job seekers' online applications. In addition, we

[7] The meetings occurred at a rate of approximately one per week in each agency between late April and July 2017. Each session lasted approximately an hour and a half and was attended by 13 job seekers on average.

sent an online survey 6 months after the end of the intervention. In this survey, job seekers were asked about their search scope in terms of occupation and geographic area, the search websites they used, and the time spent on job search every week. The survey also contained questions on job seekers' self-assessed well-being, whether they felt support or loneliness in their search, and activities outside of the job search to evaluate their life balance. The response rate to the main questions in the survey was slightly above 15%, which is fairly high for such online surveys among job seekers.[8]

We measure a small but statistically significant differential attrition between control and treatment groups.

Our (binary) measure of take-up of the treatment is whether the job seeker created a Bob Emploi account and/or attended an information meeting conducted by Pôle emploi. Take-up reached 26.2 % in the treatment group, and remained close to zero in the control group.[9] In itself, participation in the information meetings gave job seekers an opportunity to find out about other resources at Pôle emploi and to interact with caseworkers. However, we find that the differential rate of usage between the treatment and the control groups was more than 25 points higher for Bob Emploi than for any other resources presented in the meeting, which indicates that the main effects we measure are driven by Bob Emploi.

Overall, Bob Emploi had limited effects on job seekers' search strategy and reemployment outcomes. We define a modification of a job search strategy as a change in target (i.e. of the desired job or geographic search zone), in the search methods used (e.g. type of contacts solicited in one's social network, internet sites used, etc.), or in the search effort, as measured by the time spent searching and the number of applications sent. First, we find no effect on search effort, as measured by the time spent searching and the number of applications sent. Analyzing the characteristics of the applications sent, we also see no change of job seekers' search scope, whether in terms of occupations or geographic locations.

[8] Out of an eligible sample of 86 673 job seekers, DellaVigna et al. (2020) obtain a response rate of less than 8%. In comparable online surveys sent by Pôle emploi, the response rate usually hovers around 10%.

[9] This percentage can be broken down into three types of participation: in the treatment group, 6.6 % of job seekers only created a Bob Emploi account, 14.07 % only attended a meeting, while the remaining 6.53 % did both. As a robustness check, we compare this measure with an alternative definition that also includes survey respondents who reported using Bob Emploi, and we find a correlation between the two take-up measures of 0.78.

Secondly, we detect treatment effects along some of the variables measuring the methods job seekers use to search for jobs. Survey respondents in the treatment group report being 1.3 percentage points more likely to rely on their personal network, which is significant at the 5% level. They do not behave differently than the control group regarding other types of networking. Treated job seekers are not more likely to use best practices for the job search frequently recommended by the website (e.g. adapting one's resume to the job listing before applying) either, but Bob Emploi does increase the number of websites they use to look for jobs by 3%. Taking into account the innovative collaboration with Pôle emploi, we pay special attention to the website's impact on job seekers' use of public job search assistance services. The positive effect on the number of websites used is entirely driven by the increased usage of websites that are either created by or closely connected to Pôle emploi. Moreover individuals in the treatment group are 2.4 percentage points (4%) more likely to engage in conversations with their caseworker within the 6 months that followed the intervention, which is significant at the 1% level.[10] The website did not lead to a higher rate of participation to other search assistance programs, such as group workshops or training programs. Consistent with the close partnership between Bob Emploi and Pôle emploi, these results suggest that Bob Emploi acts as a complement to rather than a substitute of public job search services.

Third, to provide a more complete picture of the potential mechanisms by which Bob Emploi may increase the effectiveness of the job search, we turn to survey variables related to job seekers' well-being and life balance. Unemployment spells can be stressful periods for job seekers, especially when they feel isolated, poorly advised, and poorly equipped (Krueger and Mueller (2012)). The failure to obtain interviews and finding a job can generate important psychological costs (McKee-Ryan et al. (2005), Ridley et al. (2020)), drain energy out of social activities, and decrease future search efforts. In spite of Bob Emploi's user-friendly design, uplifting messages, and recommendations to participate in activities outside of the job search, survey respondents in the treatment group do not report increased well-being than in the control group, nor do they feel more supported in their job search. They are also not more likely to engage in sport, cultural, or volunteering activities.

[10] An important caveat is that these two effects may be partly driven by the participation in the information meetings.

Fourth, and most importantly, we investigate whether the modest effects on upstream variables related to the job search translated into effects on employment. As we observe all consecutive exits and entries out of and in unemployment, we can detect whether individuals experienced some employment episodes following the intervention and compute the total number of days spent in unemployment. For any exit out of unemployment, we know whether the individual finds a job and the type of contract she obtains. We do not measure any impact of Bob Emploi on any employment outcome and at any time horizon. Bob Emploi did not increase the likelihood of experiencing some employment episode, irrespective of the form of employment we look at, and did not reduce the duration in unemployment. Considering the upper bound of the 95 percent confidence interval, we can reject any effect higher than 0.5 percentage points on experiencing some employment episode within the 18 months following the intervention. To shed light on the cost-benefit analysis of this type of assistance program, we also consider cumulative unemployment benefits received by job seekers and observe no significant difference between the control group and the treatment group.

Unfortunately, the results of the experiment reported in this paper show that Bob Emploi did not fulfill its promise of a breakthrough in job search assistance, in spite of its close partnership with the public employment services and access to the latter's rich data. Beyond the performances of this particular website, this raises questions about the potential of online labor market intermediaries that aim to orient job seekers by applying modern data analysis techniques.

Related literature. A large body of economic work on job search motivates this study. Babcock et al. (2012) insist on the potential for and the existence of information gaps as a source of occupational and geographic mismatch. Biases in perceptions of job arrival rates can also lead individuals to make suboptimal decisions in terms of search intensity (see e.g. Della Vigna and Paserman (2005), Spinnewijn (2015), Caliendo et al. (2015), McGee (2015), Krueger et al. (2011)). Although the advent of digital technologies in the late nineties gave job seekers access to many online resources with information and advice on labor markets, Kuhn and Mansour (2014) do not observe significant returns from using the Internet to search for jobs. In fact, finding what will be most relevant and choosing the most appropriate method of search certainly remains a challenge. Even when accurate information is available, Babcock et al. (2012) also mention the difficulty in moving from information to action, particularly during job search periods associated with self-deprecation and demotivation (Falk et al. (2006), Krueger and Mueller (2012)). In this context, finding useful tools and methods of search may be highly valuable and can help job seekers avoid vicious circles in which failures fuel demotivation which then lowers effort and reemployment chances.

Yet, in their meta-analysis, Card et al. (2018) find that most of public programs providing job search assistance have shown only limited impact, and where some effect is demonstrated, it is not long-lasting. As pointed out by Belot et al. (2019), such programs generally bundle many different forms of assistance, often coupling monitoring and sanctions (e.g. Blundell et al. (2004)). Hence, available empirical evidence is often not sufficient to identify the main bottlenecks in the job search and the precise mechanisms by which interventions could help the beneficiaries.

Our paper adds to a recent strand of the literature that tests more narrowly-designed interventions aiming to solve specific challenges faced during the job search. Addressing the question of the link between misinformation and suboptimal search effort, Altmann et al. (2018) studies the impact of sending a standardized brochure with generic information on job search returns and employment. Belot et al. (2019) follows a sample of job seekers who can use a new search algorithm with recommendations of jobs to expand one's occupational scope of search. Addressing the challenge of moving from information to action, Abel et al. (2019) help job seekers stick to their intentions by writing down and committing to a search plan. The research question we address in this paper is the impact of information provision (as in Belot et al. (2019) and Altmann et al. (2018)) when combined with actionable advice on search methods (as in Abel et al. (2019)). To detect the impact of Bob Emploi platform, we use a particularly rich set of variables that allows us to document multiple aspects of job search, covering search effort, employment outcomes and detailed information on the search methods used by job seekers. This variety is unusual in a randomized experiment and more common in descriptive studies such as Krueger et al. (2011).

In the three aforementioned randomized experiments, the involvement of the researchers in the design of the program and the intervention itself allows them to reach a high level of precision in the program content, sometimes departing from the real conditions of job search and partly losing external validity. Besides, in most existing studies on private labor market intermediaries, the goal is to compare the provision of counseling services by traditional government agencies and by new entrants from the private sector (e.g. Behaghel et al. (2014), Krug and Stephan (2013)). Instead, this paper studies a platform based on the cooperation between the public services and a private labor market intermediary. As Bob Emploi was developed by Bayes Impact, a non-profit tech start-up, and leveraged data and logistical capacity of Pôle emploi, we explore the potential complementarities between both types of actors.

To the best of our knowledge, this is also the first randomized study of a private website that is entirely dedicated to job search assistance that does not couple it with matching functionalities of job boards. This connects our paper to studies on the potential of online platforms and to the related discussion on profiling in active labor market policy (see, e.g., Berger et al. (2001) for a comprehensive review). In the last two decades, the rise of the Internet offered a promising opportunity to lower labor market search costs. However as pointed out by Horton (2017), this first wave turned out to have only limited impact (Kuhn and Skuterud (2004)). Horton (2017) suggests that the reason is that providing searchable listings of jobs or applicants does not create enough value-add to significantly improve the functioning of the labor markets. In this paper, as in Horton (2017) and Belot et al. (2019), we test the potential of a more sophisticated website that uses the power of digital technology not only to gather and make information available for free but also to process and analyze this information, and to deliver customized advice at scale. Therefore we contribute to the literature on how algorithms and digital technologies can solve market frictions (Resnick and Varian (1997); Adomavicius and Tuzhilin (2005); Varian (2010), Dinerstein et al. (2018)).

2.2 Setting

2.2.1 Online resources for job seekers in France

France's unemployment system is characterized by a high degree of centralization. The Public Employment Service, Pôle emploi, is not only in charge of distributing unemployment benefits and monitoring job search, but also of providing job search assistance to job seekers and of helping them match with employers. With the rise of the internet in the end of the nineties, new entrants emerged as alternative labor marker intermediaries. The first generation of these private websites were often basic job boards such as CareerBuilder or Monster (Kuhn and Skuterud (2004)). A second wave leveraged the power of social networks: famous international examples include LinkedIn and Viadeo. These online websites may be only at a national scale (e.g. RegionsJob in France) or sector-specific (e.g. dice.com in the United States for tech sector or emploi-environnement.com in France for jobs in environmental fields). At the same time, Pôle emploi started to digitize its traditional services, creating its own platform, called Emploi Store, to gather new online services along with recommendations to private websites that help job seekers in their search.

In fact, private websites frequently augment their basic services with additional resources offered to job seekers. Most often, these resources are free articles presenting what the website considers best practices to search for a job.[11] These extra resources sometimes also include paid services (e.g. getting help from a coach). The advent of artificial intelligence and machine learning over the last decade opened up new opportunities to address job seekers' and recruiters' needs. Leveraging large datasets, these techniques work by selecting and analyzing relevant information, allowing for better and customized recommendations. Among labor market intermediaries, many private actors attempt to improve the matching between employers and applicants (e.g. training algorithms on past hiring and résumés to help employers sort applications). However, fewer solutions have been designed to optimize the job search from the perspective of the job seeker. In fact, the business models of these websites are often based on monetizing employers' postings. As a consequence, most advanced services are oriented towards employers (e.g. boosting job listings to a pool of pre-identified profiles). Moreover, as in other fields of application, big data algorithms are only as good as their number of users and the quality of their data, and it is a significant challenge for all websites to obtain data about job search techniques.

From that perspective, Bayes Impact occupies a unique position: its website is dedicated to improving the job search (and not the matching) and can leverage datasets on job seekers' trajectories thanks to its partnership with Pôle emploi. At the time of its creation and to the best of our knowledge, no other private website offers job search assistance with similar technical capacity.

[11] For example, RegionsJob in France makes recommendations in dedicated posts such as "how to write a good résumé?" or "8 recommendations to perform during an interview".

2.2.2 Description of Bayes Impact organization

Bayes Impact is a non-profit organization created in 2014 by a small group of developers with the goal of leveraging advanced data analysis techniques to help resolve important public policy challenges. The organization is based in Paris and San Francisco and recruits primarily within the world of tech start-ups. Its approach consists of building partnerships with public institutions in order to access relevant data to find solutions to a public issue. As an example, the organization launched in 2017 a project to document the management of the police's use of force in partnership with the police services in California. Results of the analyses of these data with artificial intelligence techniques are then shared with citizens via user-friendly, simple interfaces. As stated on its website, Bayes Impact aims to "modernize public services in a transparent manner".[12] Bayes Impact is financed almost exclusively through public funding. In keeping with this positioning, the organization has taken on a culture of transparency and sharing, notably by keeping the developed codes open-sourced (even though the data generally are not).

2.2.3 Description of Bob Emploi website

In France, Bayes Impact formed a partnership with Pôle emploi in 2016 to obtain data on the trajectories of job seekers and the hiring conditions on the job market. Bayes Impact aimed to apply the techniques of big data analysis in order to identify, for each profile of job seekers, the best strategy for rapid re-employment. In November 2016, the organization launched the online platform, Bob Emploi, cross-referencing Pôle emploi data with user profiles to propose an active strategy for job search and to identify priority actions to augment their chances of employment.

More precisely, Bob Emploi cross-referenced two sources of data coming from Pôle emploi and from the users themselves. At the moment of initial login, a user creates her profile with information about standard socio-demographic variables (gender, age, highest educational degree, municipality of residency). The user indicates her desired job, as well as her target geographic zone and salary. Finally, Bob Emploi questions the user about the metrics of her job search over the course of the past weeks: the number of job opportunities identified each week, the number of applications sent,

[12] See Bayes Impact website https://www.bayes.org/fr/focus/spc

the number of interviews and job offers obtained. The ratio of one step to the next is then used by the site to orient recommendations on specific steps that are proving to be the greatest obstacles in the job search.

Bob Emploi then cross-references each user profile with data on the job market obtained via Pôle emploi. The algorithm uses principally three databases from Pôle emploi:[13]

- a representative sample from a database called Fichier historique, which follows, over the last 10 years, the trajectories of persons enrolled in Pôle emploi throughout their unemployment periods. This base contains the basic socio-demographic information of the job seekers (gender, age, highest educational degree, municipality of residency), as well as the principal job desired (target job) at the moment of enrollment (see details in section 2.3) and all the accompanying activities carried out via Pôle emploi in which the job seeker participated (e.g. meetings with caseworkers, job training program). The principal limitations of this base are that it contains little information about periods of employment following unemployment (occupational title of the job, salary obtained, location) and that it does not contain information on the methods used during the job search.

- a database of job offers placed on the Pôle emploi job board by employers. Each observation in the database contains a job title and description as well as the number of applications to the job posted on the job board at the moment of data collection. The compilation of this information allows for the construction of a proxy of market tightness for each career in a given locale. This also permits, by semantic analysis, to retrieve the requirements of job listings.

- data from the annual Pôle emploi survey of a sample of 500 000 employers, called Besoins en Main d'Oeuvre, to know their hiring needs in the six months following the study, and the skills they deem particularly difficult to recruit. This survey allows Pôle emploi to measure market's labor demand and orient job seekers towards high-demand sectors.

[13] Details about the databases used are published by Bayes impact in the github https://github.com/bayesimpact/bob-emploi/blob/master/data_analysis/data/README.md#bmo-bmo-besoin-en-main-doeuvre

Bob Emploi relies on these assessments to suggest modifications of user's search targets, in exploring related careers in demand or in widening the geographic zone of her search. Though Bob Emploi's initial aim was to implement machine learning techniques over the collected parameters affecting job search, the data available limit the predictive power of the algorithm, notably because of the lack of information about reemployment trajectories in the *Fichier historique* (occupation obtained, type of contract) and the absence of precise data on the applications sent and the methods used during the job search. For lack of this information, Bob Emploi relies on recommendations gathered over the course of a series of qualitative interviews with multiple professional recruiters (Pôle emploi caseworkers, recruiters, career coaches) who wish to support the project.

In a broad outline, the recommendation algorithm is based on the following modeling of standard recruitment paths as a funnel. The funnel corresponds to the different stages of the trajectory of a job seeker who manages to find a job : she first defines her search perimeters (occupational and geographical scope of search), identifies offers, applies, and attends an interview to secure, in the end, an offer of employment. The algorithm is based on ratios of reference, arbitrarily defined by the site designers, in relation to which are compared the performance metrics stated by a user during his first login. For example, if the number of applications reported by a user is superior to the algorithm's reference number for the number of sent applications, but the number of interviews is inferior to the algorithm's reference number, Bob Emploi concentrates its recommendations on the improvement of the job seeker's application file, e.g. advising the user to contact professional career coaches to help her prepare for interviews. In the long run, the platform aims to enrich its data through user feedback.

The user landing on the Bob Emploi website begins by creating her account and giving information about her profile and job search activities up until that moment. In return, the site sends him a "diagnostic" of employability in the form of a numerical score deconstructed along different axes (labor demand for the target job, competition, match between the user's profile and the job requirements). The ensuing suggestions of key actions are presented in a hierarchical manner. Actions can be to widen one's search perimeters if the user reported not being able to find many job offers but the data indicate the market is less tight in surrounding municipalities, or to edit one's resumé if the ratio between the number of applications sent and the number of invitations to interview is particularly low. Each recommendation is matched with concrete advice and signals appropriate external resources, most often online.

Finally, the tone and format used by the interface are deliberately inviting and simple, intentionally distinct from the often austere and complex appearances of administrative sites. A significant portion of the site's design was created by professional web designers to find ways to render quantified information easily digestible.

2.3 Data

Our analysis draws on three data sources: the administrative data collected by Pôle emploi, the user data on connections to the Bob Emploi site shared by Bayes Impact and outcomes collected through online surveys, administered to both control and treatment groups six months after the intervention.

2.3.1 Administrative data from Pôle emploi

The administrative data from Pôle emploi cover information about job seekers from their registration at Pôle emploi until the end of their unemployment period.

Unemployment records

The Pôle emploi unemployment records, called Fichier historique, contain several standard socio-demographic characteristics including gender, age, municipality of residency and education level along with their unemployment duration at the time the intervention is implemented, and afterwards. They report the individual monthly unemployment benefits, which allows us to compute the cumulative amounts job seekers received over the 18 months following the intervention.

These records also contain information on the job search strategy that the job seeker adopts at the beginning of her unemployment spell, including labor sector, skill level of the desired occupation (e.g. manager), target wage.[14] As these variables are entered only once, at the beginning of the

[14] Labor sector is recorded as a 5 digit occupation code based on Pôle emploi's lexicographical classification of

unemployment spell, they cannot be used to detect changes in the job search strategy following the intervention.

Employment outcomes

Our main employment outcome captures whether job seekers get reemployed after the intervention. To identify employment episodes, we rely on the ICT01 employment indicator of Pôle emploi.[15] As described in section 2.4.1 and shown in figure 2-1, we drew our sample over three consecutive months in order to ensure that the lists of job seekers who would be invited to information meetings were as up-to-date as possible.[16] We compute all our employment outcomes starting from the sampling date of each wave – April 1st for the first wave, May 1st for the second wave and June 1st for the third one. As the intervention ended two months after the sampling date, the employment outcome one month after the end of the intervention for an individual in the first wave is measured by looking at whether she had any employment episode between April 1st and July 1st.

To better characterize these episodes, in terms of duration and stability, we look at two complementary outcomes. First, we look at employment episodes in long-term jobs, that is, whether individuals obtained any work contract of more than 6 months (CDI or CDD longer than 6 months) over the period of interest.[17] Second, we create a variable that counts the cumulative number of days in unemployment since the sampling date.

occupations (called Répertoire ROME) that is an equivalent of the US O*NET classification. A 5-digit occupation corresponds to a narrow type of job, such as nurse-anesthetist.

[15]This indicator mainly draws on: (i) employer declarations at the time of hiring and (ii) job seekers' self-reported declaration of joining the labor force. This second category is necessary to be able to track job seekers who become self-employed.

[16]Individuals from the first wave were drawn and added to the sample on April 1st and were invited to attend meetings happening between late April and late May. Individuals from the second wave were sampled on May 1st for meetings between late May and late June while individuals in the third wave were sampled on June 1st and attended meetings between late June and late July.

[17]Employment episodes fall into one of the following categories: long-term contract (called Contrat de Durée Indéterminée, or CDI), short-term contract (called Contrat de Durée Déterminée, or CDD) for more than 6 months, short-term contract of 1 to 6 months, and part-time work.

Use of Pôle emploi assistance services

Having access to both Pôle emploi and Bayes Impact data, we can investigate potential substitution effect between the use of Bob Emploi and Pôle emploi services. Pôle emploi database records all jobsearch actions completed by the job seekers: one-on-one meetings with their caseworkers, participation in workshops at Pôle emploi (e.g. resume writing classes), individual assistance program (e.g. skill assessment) or any information meeting such as the ones that were organized within this experiment. From these records, we create two variables: (i) the number of meetings with caseworker and (ii) the total number of workshops, programs and meetings that the job seeker attends following the intervention.

Online applications from Pôle emploi job board

We use data about online applications on the job board administrated by Pôle emploi. This job board website is one of the most popular search platforms in France. According to Skandalis (2019), it covers about one third of job creation and "vacancies posted on the website are mostly for low-skilled positions but also tend to offer more permanent contracts and relatively better-paid jobs than the average job created in France in the period."

Since 2014, job seekers can apply online to these job listings, which gives us access to micro-level data on job applications. From this, we can recover the total number of applications and break down into different sub-categories depending on whether the application was initiated by the job seeker herself, by his caseworker or by the employer.

The data also include some of the job listing features such as the job title and its geographical location. Based on this information, we create:

1. a dummy variable capturing the geographical scope of the job search. It is equal to 1 if the job seeker applies to vacancies outside her own municipality and 0 otherwise.

2. a dummy variable capturing the occupational scope of the job search scope. It is based on Pôle emploi's lexicographical nomenclature of occupations that is equal to 0 if the individual only applies to jobs within a single 5-digit category and 1 otherwise.

2.3.2 Data from Bayes Impact

We use data from Bayes Impact to see who created an account on the Bob Emploi website among the job seekers in our sample.[18]

2.3.3 Survey data

To collect additional information on job search behavior, we administered an online survey to the entire sample, 6 months after the end of the intervention.[19] This survey allows us to shed some light on:

i) job search effort, as measured by the number of hours spent searching over a normal week, along with the frequency at which job seekers send unsolicited applications.

ii) job search methods, including:

   - relying on social networks: we ask whether job seekers rely on their personal network and on other types of contacts.[20]
   - search websites used. We compute (i) the number of websites created by Pôle emploi that the job seeker used, and (2) the number of other private websites used.[21]
   - geographical scope of job search, that is whether the respondent looks for jobs in his municipality, department or region of residency.
   - job search best practices, that are frequently recommended by Bob Emploi to increase search efficiency.[22]

[18]For privacy reasons, Bayes Impact could not directly share its list of users' email addresses, nor could we share email addresses of individuals in our sample from Pôle emploi database. Therefore we used a anonymized matching algorithm to match email addresses from both sources.

[19]The complete questionnaire is available in appendix 3.8.

[20]We asked about 7 different social groups, such as friends, former colleagues or neighbors. Personal networks included friends, friends of friends, former classmates and colleagues. Other contacts included Pôle emploi caseworkers, volunteers from non-governmental organizations, shopkeepers in one's neighborhood and other job seekers.

[21]We provided a list of 9 websites, including Bob Emploi, for which respondents had to say whether they knew of or used them, and whether they found them useful.

[22]Bob Emploi often suggests to adapt one's CV or cover letter by reusing words or expressions of the job listing, or to call back to check in with an employer after an interview.

iii) well-being: we asked respondents had to choose numbers on a Cantrill scale from 0 to 10 to show their general well-being level, their motivation, and the extent to which they felt supported in their job search. To know about their life balance, respondents were asked if they participated in any sport, art or community activity at least once a month.

iv) employment expectations: we also asked job seekers when they expected to find a job and create a dummy variable for whether individuals expect to find a job within the following 3 months.

2.4 Design

2.4.1 Experimental procedure

Encouragement design

Our experimental protocol follows an encouragement design. First, we invited the treatment group to information sessions at their local agency. These information meetings gave an introduction to Bob Emploi.[23] Each introduction to Bob Emploi session was hosted by one or two Pôle emploi caseworkers and lasted approximately 1 hour and 30 minutes. It began with an introduction to the challenges associated to job search and the usefulness of widening one's search methods, notably through personal and professional networking. The caseworkers then presented the Emploi Store, the Pôle emploi platform that brings together digital job search services, which references Bob Emploi. The session concluded with a presentation of the Bob Emploi website using screenshots and, some time, a real-time simulation. These information meetings occurred at a rate of approximately one meeting per week in each participating branch, between April 20 and July 31, 2017.

We reinforced the encouragement design by sending advertising emails to the entire treatment group encouraging job seekers to visit the Bob Emploi site.[24] Three such emails were sent out: the

[23]Such information sessions are frequently organized by Pôle emploi caseworkers to introduce free tools and programs to job seekers.

[24]In fact, when nearly two-thirds of the information sessions had occurred, a comparison between the list of invited participants and the creation of Bob Emploi accounts had indicated that only a small percentage of participants actually created an account on the website. The reminder emails differed slightly depending on whether the person had attended an information session or not.

first in the month of July, the second after summer vacation, on September 28th, and the last on November 13th.

Sampling and timeline of the intervention

Selection of agencies 254 Pôle emploi local agencies were selected to participate in the experiment. The goal of this selection was to use only universal branches with the capacity to implement weekly information sessions throughout the 3 months of the experiment, and which were also as representative as possible of the totality of agencies in terms of size and geographic location.

Eligibility criteria The eligibility criteria were defined by a working group of Pôle emploi caseworkers involved in the design and follow-through of the experiment. The objective was to target people deemed the most likely to benefit from Bob Emploi and to be interested in the site. Eligible job seekers registered in participating branches at the moment of the drawings met all of the following criteria:

- has been unemployed for a year or less at the moment of the sampling, and in this respect likely to have not yet explored the entirety of search methods offered by Bob Emploi;[25]
- does not work part-time hours exceeding half of full-time employment per month and has declared herself available to begin work immediately. This criterion allows for the protocol to exclude those undergoing a training program or an internship and those not actively searching for employment due to health reasons;
- demonstrates strong self-sufficiency related to job search.[26] The working group of Pôle emploi caseworkers who was involved in the design determined that the Bob Emploi tool was more likely to benefit those already capable of browsing the internet with ease;

[25] Duration of unemployment was defined as the duration of unemployment at the moment of the experiment, meaning from their latest registration at Pôle emploi. It is thus possible that an individual in the sample was unemployed at other periods before the drawing.

[26] The degree of self-sufficiency was estimated based on a variable that indicates the intensity of Pôle emploi assistance. At the time of registration, caseworkers assess job seekers' level of autonomy and assign them to one of three main assistance tracks. Job seekers that are considered to easily handle their job search benefit from a more distant monitoring and assistance from caseworkers.

- had already defined their job search strategy, especially with a target job in an acceptable geographic zone. Bob Emploi was considered more relevant to reacting to and redirecting a previously defined strategy than to helping users produce initial career objectives;
- and, has a valid email address and agreed to receive emails with information from Pôle emploi.

Randomization and timeline The implementation of meetings occurred over a 3 month period, in April, May, and June 2017. The sampling protocol was thus staggered in 3 waves: each month, a sampling was executed in each branch, in order to draw eligible individuals in the treatment group and in the control group. The reference was to form lists of 40 people invited per session, mirrored by 40 people as control group.[27] This resulted in a final sample composed of 212 277 individuals: 119 525 in the treatment group and 92 752 in the control group. Figure 2-1 provides a schematic representation of the timeline of the experiment. Note that for logistical reasons and contrary to information meetings, the reminder emails and the online survey were sent all at once to the entire sample.

2.4.2 Description of the sample

Descriptive statistics and balance checks

Table 2.1 provides descriptive statistics of our sample along with balance tests. Column (1) shows the mean of pre-intervention variables for individuals in the control group and column (2) displays the results of regressions to detect differences in means with the treatment group. This latter column shows that no coefficients are significantly different from zero, which confirms that the randomization was successful at balancing the treatment and the control groups. Thus the composition of the control group reveals the composition of the whole sample.

The composition of the sample is consistent with the eligibility requirements. All job seekers in the sample have less than a year of seniority in unemployment. A majority has been registered at

[27] Small branches sometimes could not fill in 40 eligible job seekers in the control group and hence compiled reduced lists, which explains why the experiment resulted in a slightly higher number of individuals in the treatment group than in the control group.

Pôle emploi for more than 6 months (53.20 %) and only about one quarter has been unemployed for less than 3 months. Most individual are aged 26 to 55 although more than a quarter of the sample is in their twenties, which is also in line with the eligibility condition on unemployment seniority. The requirement on self-sufficiency in one's job search partly explains that more than 80% of the sample earned a formal education degree, with 30.60 % having a vocational degree and as many as 34.55% who own a university degree. The sample is balanced on gender and targeted sectors of search.

Survey respondents and attrition

Table 2.2 shows that there was low but significant unbalanced attrition on survey response between the treatment and the control groups. Response rate to the survey is 0.4 percentage points lower in the treatment group than in the control group. Respondents were more likely to be male than non-respondents, as shown in table 2.3. They are significantly older and are 12 percentage points more likely to have earned a university degree than non-respondents. They also have been unemployed for longer at the time of the survey.

2.4.3 Empirical strategy

Intent-to-Treat estimation

In the empirical analysis in Section 2.5, we aim at identifying the causal impact of Bob Emploi on job search strategy and employment outcomes of unemployed individuals. Given the randomized design of the experiment with balanced treatment and control groups, we primarily rely on Intent-to-Treat estimations. We estimate equations of the following kind :

$$Y_{it} = \alpha_1 + \beta_1 T_i + \mu_{i1}^{a,m} + \epsilon_{it1} \tag{2.1}$$

where Y_{it} measures the outcome of interest, e.g., job search effort or employment, for individual i at time t. T_i is the dummy equal to one if the individual i is in the treatment group. To account for this multi-local multi-timing sampling, we add fixed effects $\mu_{i1}^{a,m}$ for each strata defined by the month m of inclusion in the sample and the local Pôle emploi agency a of individual i.[28] As explained in the previous subsection, we drew job seekers to invite to information meetings from monthly lists of local Pôle emploi agencies over three consecutive months. In small agencies and to respect a standard procedure for the meetings, we had to sample more individuals in the treatment group than in the control group. At the aggregate level, this implies that treatment and control groups are not balanced across agencies, with an over-representation of small agencies in the treatment group.

T_i is a dummy for treatment status and β_1 thus captures the average effect of the treatment on outcome Y in month t after treatment.[29] Throughout the analysis, we cluster the robust error term ϵ_{it1} by local agency.

In section 2.5, we mainly present results for $t = 6$ months after the intervention and we show similar results for $t = 18$ months in the appendix. The model lends itself to graphical presentations which we use to document treatment effects over time on employment outcomes.

Definition of take-up and LATE estimation

To estimate the individual effects of Bob Emploi on compliers, we turn to a LATE. Considering the experimental protocole, compliers are individuals who either created an account on Bob Emploi website or participated in an information meeting. Information meetings were an opportunity for job seekers to find out about other resources at Pôle emploi and to interact with their caseworker (who was often the person who invited them to the meeting). We cannot rigorously rule out that meetings had an effect on our outcomes in and by themselves and therefore we count as treated those who participated in a meeting. The list of created accounts is obtained by matching email addresses of Bob Emploi users recorded by Bayes Impact with email addresses of individuals in our sample. Survey data give us a second possible measure of Bob emploi usage on the sample of respondents, which we use as a robustness check on survey outcomes.[30]

[28] $\mu_{i1}^{a,m}$ is a dummy variable equals to 1 if the individual i is in the agency a and included in the sample at month m (either April, May or June)

[29] Controlling for agency and month strata, the random treatment assignment ensures that T_i is orthogonal to ϵ_{it1}. Hences β_1 identifies the average causal effect of the treatment even without controlling for any other covariate.

[30] Among other websites, respondents were asked whether they knew, used and liked Bob Emploi (the complete questionnaire is available in appendix 3.8). Hence we can count respondents who reported that they used Bob

Table 2.4 compares the two take-up definitions, either including survey responses on Bob Emploi usage or not, to verify that they are consistent and that our results are robust to using either one. Column (1) shows the take-up differential between the treatment and the control group based on meeting participation and Bob Emploi connection only. Column (2) runs the same regression restricting the sample to survey respondents, which allows a comparison with column (3) where we count as additional takers respondents who report to use Bob Emploi. We see in column (1) that the experiment was successful at creating a substantial take-up differential of 26.8 % while keeping a participation in the control group to virtually zero. Reassuringly, take-up differentials among respondents only, showed in columns (2) and (3), are very close and highly correlated, although the mean in the control group is slightly higher when using survey responses.[31]

In tables of section 2.5, we therefore choose to show the results of LATE estimation defining as takers individuals who either attended an information meeting or connected to Bob Emploi website. The equation is similar to 2.1:

$$Y_{it} = \alpha_2 + \beta_2 \widehat{B}_i + \mu_{i2}^{a,m} + \epsilon_{it2} \tag{2.2}$$

where Y_{it} measures the outcome of interest for individual i at time t. $\widehat{B}_i$ is a dummy for Bob Emploi take-up status, which we estimate by the appropriate first stage regression with T_i the treatment dummy :

$$B_i = \gamma + \delta T_i + \mu_{i0}^{a,m} + \eta_i \tag{2.3}$$

This ensures that controlling for $\mu_{i2}^{a,m}$, $\widehat{B}_i$ is orthogonal to the heteroskedastic error term ϵ_{it2}, so that β_2 captures the individual effect of the treatment on outcome Y.

Emploi as takers of the treatment.

[31] In column (2), the differential increases to 42%. This is very close to what we obtain when we add survey responses in the take-up definition in column (3). Yet we see in column (3) that this definition identifies 9% of the control group as takers. The correlation between column (2) and (3) is 0.78. Discrepancies could be due to individual mistakes in survey responses (Bob Emploi being easily confounded with any Pôle emploi website). Job seekers might also have used a different address to connect to Bayes Impact. Finally, the 9% takers in the control group in column (3) might reflect a natural contamination due to information spreading during the three months when information meetings where organized in local agencies.

Description of takers

Table 2.4 shows that the take-up differential between the treatment and the control groups is 26.8 %. As the take-up is virtually zero in the control group, this take-up is only made of compliers of the treatment group. Compliers fall into three distinct categories: 14.07 % only attended an information meeting, 6.6 % did not attend a meeting but created an account on Bob Emploi, and the last 6.53 % both attended a meeting and created an account. The take-up regularly increased over time during the three months when the meetings were organized, followed by discrete bumps corresponding to the days when reminder emails were sent out. Using survey data, we observe that overall the intervention led to a significantly higher differential in Bob emploi usage compared to other websites presented during the meetings.[32] Thus we can interpret our results as mostly driven by Bob Emploi usage.

Table 2.5 shows the selection pattern of compliers along the same variables as table 2.1. Perhaps surprisingly, take-up markedly increases with age. Take-up is highest for job seekers above 50 years old, and lower for the youngest job seekers. This is not only due to older job seekers participating more to meetings as the correlation still holds if we restrict to creations of Bob Emploi accounts only. While younger job seekers tend to be more familiar with digital technologies and could have used Bob Emploi more easily, it seems on the contrary that the website rather appealed to older job seekers.

2.5 Results

In this section, we summarize the main results of our experiment. We first consider the effects of Bob Emploi on job search strategy, namely search effort, target and methods. We then analyze the treatment effects on employment and well-being. All regressions in this section show the results of regressions on outcomes measured 6 months after the intervention both in ITT (panel A) and LATE (panel B). They are replicated with outcomes 18 months after the intervention in the appendix.

[32] We measure that the differential in Bob emploi usage between the respondents in the treatment group and the control group reached 28%, whereas it is only 4% for the main other website that was presented during information meetings, called Emploi Store.

2.5.1 Effort and scope of job search

Table 2.6 shows how Bob Emploi affected job seekers' reemployment expectations, search effort and target. Column (1) first presents the impact of Bob Emploi on individuals' expectations of reemployment as reported in the survey. On one hand, Bob Emploi advice can improve job seekers' search strategy, and thus increase their effort and expectations. On the other, information may make them realize that their reemployment chances are low, which could either make them look harder or discourage them. In the control group, we see that 57% respondents believed that they would find a job within three months after the date of the survey. There appears to be no significant difference in the treatment group. This rules out a predominant demotivation impact which is a legitimate worry in any information intervention.

By identifying reachable targets and providing concrete methods of search, Bob Emploi may also increase the returns to time spent on the job search. Combined with a general motivation effect, this can lead users to increase the number of hours spent searching. In extreme cases, if it sufficiently increases the returns to each unit of effort, providing good information can in theory have a net decreasing impact on search effort. To measure job seekers' search effort, we compute both the total number of hours respondents declare to spend on job search (column (2)) and the number of online applications they usually send (columns (3) and (5)). Column (2) shows that people invited to use Bob Emploi do not spend significantly more time searching for a job, nor do they send more online applications.[33]

If Bob Emploi does not affect time spent on job search, it may improve its efficiency. As a first lever to make the job search more efficient, Bob Emploi may lead the job seeker to widen the scope of her search by providing new information on labor demand. In fact, when the algorithm identifies that the job seeker aims for an occupation or a region where the labor market is particularly tight, it encourages her to consider jobs in neighboring regions or to apply to related occupations. We test for such widening effects using survey questions on applications as well as observations of

[33]In the control group, respondents declare to spend on average 8.5 hours per week searching, which is in line with other surveys (e.g. Krueger and Mueller (2012), DellaVigna et al. (2020)) and half of them report to send an unsolicited application every week. This translates in only 2.4 applications on average according to the administrative application dataset. Apart from reporting mistake, this discrepancy may reflect the selection bias among respondents or the limited coverage of Pôle emploi application website.

applications in the administrative dataset from Pôle emploi job board. More precisely, column (4) shows whether treated respondents apply more often to jobs outside their municipality. In column (6) we investigate for an effect on the total number of observed applications outside of one's municipality. Both variables yield no significant impact. Similarly, in column (7) we see no change in the occupational scope of search, as measured by the number of observed online applications in different occupations.

2.5.2 Job search methods

Bob Emploi not only offers a diagnosis of reemployment perspectives but connects the information with actionable advice of how to improve one's search strategy. Table 2.7 leverages our particularly rich set of variables to explore the extent to which using Bob Emploi modified job seeker's search techniques. These variables reflect frequent recommendations given on the website, such as leveraging one's personal and professional networks or adapting one's application to the specifics of each job listing. Bob Emploi also sorted online tools that it often referred job seekers to and it is therefore interesting to know if this led job seekers to use more assistance websites. These different techniques are analyzed using survey data and presented in columns (1) to (7) of table 2.7. We see in column (1) that Bob Emploi increases the reported use of personal network by 1.3 percentage point in the ITT estimation, and by 3.1 in the LATE estimation. However, the effect does not hold for professional network. Looking at columns (3) to (5), we detect no significant effect on either of the most frequent suggestions made by Bob Emploi (adapting one's CV or cover letter to the job listing, or calling back a recruiter after an interview). Turning to columns (6) and (7), we see that Bob Emploi significantly increases the number of websites used, although the effect is limited to websites created and hosted by Pôle emploi. This effect is perhaps not surprising as Bob Emploi often refers to less well-known Pôle emploi websites and so did caseworkers who led the information meetings. Finally, considering the connections between Bob Emploi and Pôle emploi, it is of particular interest to take a closer look at the impact of Bob Emploi on job seekers' interactions with Pôle emploi. In fact, one may worry that such private website discourage job seekers from engaging in conversations with their caseworkers or from participating in the many programs that are offered by Pôle emploi. If anything, we observe the opposite effect. Column (8) shows that Bob Emploi increases the fraction of job seekers who had had at least one interaction with their caseworker within the 6 monhs that followed the intervention, and column (9) shows that there is no impact on the number of attended programs and workshops at Pôle emploi. These last results might be partly driven by participation to information meetings by itself.[34]

2.5.3 Well-being and life balance

Table 2.8 shows the impact of using Bob Emploi on job seekers' reported well-being. Job seekers' demotivation and dissatisfaction is an important concern in economics as well as in sociology or psychology.[35] Along with a direct loss of material wealth and the unrewarding aspect of job search, unemployment is often associated with social bounds dislocation, loss of meaning and of social identity.[36] With time, individuals who stay unemployed may get discouraged and drop out of the labor market. This can become a vicious circle when job search itself does not lead to positive outcomes because of a bad job search strategy: demotivation and self-deprecation lower individuals' energy and might preclude them from grasping promising job opportunities. This in turn would lead to more disappointments and unsuccessful searches. By providing actionable advice to optimize one's search effort in a user-friendly manner, Bob Emploi has the potential to give job seekers a feeling of purpose, support and achievement. If this leads to better search outcomes, it can increase motivation and optimism.

Columns (1), (2) and (3) report the answers to the three questions of the survey that relate to well-being. In these questions, respondents had to provide a number between 0 and 10 to indicate their level of overall well-being, of motivation during their search and the extent to which they felt supported. Means in the control group reveal that respondents estimate to have an average

[34] During the meetings, caseworkers put an emphasis on the main platform called 'Emploi Store' where Pôle emploi gathers several search websites including Bob Emploi and other less well-known search online tools created by Pôle emploi. Besides, the data on recorded interactions with caseworkers did not allow us to properly distinguish with interactions that were only due to invitations to the information meetings.

[35] The importance of discouragement and unhappiness is supported by empirical evidence: while remaining longer unemployed, workers express more dissatisfaction and unhappiness with their lives and are particularly sad during episodes of job search (Krueger et al. 2011).

[36] Psychological studies show that unemployed individuals have lower psychological and physical well-being (see McKee-Ryan et al. 2005 for a meta-analysis).

well-being but feel significantly more motivated than they feel supported. This would provide a rationale to create more advice tools such as Bob Emploi. Nevertheless, as can be seen from both panels, Bob Emploi does not seem to affect positively any of these measures.

Column (4) of table 2.8 summarizes three questions asking about job seekers' activities outside of job search, namely whether they take part in sport, arts, culture or community activities at least once a month. In fact, Bob Emploi often recommends to its users that they keep a balanced life and get involved in some social activities such as volunteering. Column (4) shows the maximum value of the three dummies, so that a respondent who engages in either sport, arts or community activities is counted as 1. As with previous outcomes, we don't observe any significant difference between the treatment and the control groups.

2.5.4 Employment outcomes

The main objective of Bob Emploi is to help job seekers get reemployed faster. To identify any difference between the treatment and the control groups, in particular any acceleration in reemployment, we look at individual paths in and out of unemployment over time.

As explained in section 2.3, we first measure whether job seekers experience any employment episode over the months that follow the intervention. As this variable does not take into account the quality of the job obtained, especially regarding their stability, we also look more specifically at episodes in long-term jobs. This is represented in figure 2-2. Panel (a) shows the fractions of individuals who have an employment episode over the 18 months following the intervention: the top graph indicates the levels in both treatment and control groups, while the bottom graph represents the treatment effect, along with confidence interval. The concave pattern of the evolution of levels is in line with models of job search frictions and other empirical evidence. Strikingly, 18 months after the end of the intervention, still 40% of the sample have no employment episode at all. Yet, as can be seen clearly from the bottom graph, the treatment appears to have no effect on the likelihood to have an employment episode faster. Panel (b) works similarly for long-term jobs. Consequently, levels are lower than in panel (a) and only reach 25% after 18 months. We do not see any impact of the treatment on this variable either. Considering the upper bound of the 95 percent confidence interval, we can reject any effect higher than 0.5 percentage points on experiencing some employment episode within the 18 months following the intervention.

To detect marginal changes in the duration of employment, we complement these variables by counting the cumulative number of days in unemployment, that is, the number of days registered at Pôle emploi. Structured in a similar fashion as the previous figures, panel (a) of figure 2-3 shows that this does not enable us to see any effect on Bob Emploi.[37]

Lastly, from a cost-benefit analysis, it may be interesting to see if Bob Emploi leads to any reduction in the total amount of unemployment benefits. This is what we show in panel (b) of figure 2-3, plotting the mean cumulative benefits received by job seekers in the sample. Mean levels hide some heterogeneity, as about 30% of job seekers in the sample receive no benefits. Once again, the bottom graph makes it clear that being invited to use Bob Emploi made no difference in the final amount of benefits received.

2.6 Conclusion

This evaluation offers a unique opportunity to measure the potential of private websites dedicated to job searches. Partnering with the French public employment services, we implemented a large-scale randomized experiment to test the impact of Bob emploi, a private website using data analysis to provide job search assistance. We find that the website did not have any significant impact on employment. The limited effects we detect on some search variables appear not to be sufficient to augment job seekers' likelihood of reemployment. The large sample included in the experiment and the significant take-up rate ensure that these null effects are precisely estimated and cannot be the result of the intervention not taking place.

Our findings provide evidence that the standard job search assistance services offered by private websites are unlikely to help job seekers. Bob emploi, which was designed by experienced tech developers and benefited from significant public funding and rich administrative data on labor markets, was in a better position than the majority of existing private websites to have a positive

[37] Due to a scaling effect, levels are virtually identical in the treatment and control groups all over the period of observation.

impact. We cannot rule out that better data algorithms coupled with matching functionalities may be more effective. Yet, the story of Bob emploi makes it clear that these improvements cannot be considered low-hanging fruit. Overall, our results suggest that the enthusiasm around digital platforms like Bob emploi may have to be toned down.

2.7 Tables

Table 2.1: Difference in means between the treatment and the control groups

	Control group mean (1)	Regression coefficient on treatment dummy (2)
Female	50.72	-0.128
	[50.00]	(0.236)
Age		
Less than 25 years old	23.11	-0.286
	[42.15]	(0.173)
Between 25 and 39 years old	45.42	0.265
	[49.79]	(0.216)
Between 40 and 54 years old	23.33	0.068
	[42.29]	(0.192)
More than 55 years old	8.14	-0.047
	[27.35]	(0.127)
Seniority in unemployment		
Less than 3 months	25.25	0.100
	[43.44]	(0.181)
Between 3 and 6 months	21.31	-0.207
	[40.95]	(0.179)
More than 6 months	53.20	0.105
	[49.90]	(0.223)
Education Level		
No high-school diploma	16.19	-0.022
	[36.83]	(0.169)
Vocational degree	30.60	-0.123
	[46.08]	(0.190)
High school diploma	18.66	0.146
	[38.96]	(0.163)
University degree	34.55	-0.000
	[47.55]	(0.198)
N	92752	212277

Notes: This table reports summary statistics for the control group as well as the results of balance checks testing equal means in the treatment and the control groups over a set of pre-intervention covariates. Column (1) shows average values of characteristics for the control group, with standard deviations in brackets. Column (2) reports regression coefficients on treatment group dummy with standard errors in parentheses. All regressions include agency x month fixed effects. Standard errors are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.2: Survey Respondent Attrition

	Survey Respondent
	(1)
Treatment	-0.004
	(0.002)**
Control Mean	0.149
N	212277

Notes: This table shows the coefficient of a regression where the dependent variable is a survey response dummy and the independent variable is a treatment group dummy, reflecting the differential attrition to the survey between the treatment and the control groups. The regression includes agency x month fixed effects. Standard errors, in parentheses, are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.3: Difference in means between survey respondents and non-respondents

	Non-respondent mean (1)	Regression coefficient on respondent dummy (2)
Female	49.38	8.590
	[50.00]	(0.308) ***
Age		
Less than 25 years old	24.35	-9.147
	[42.92]	(0.270) ***
Between 25 and 39 years old	46.01	-4.529
	[49.84]	(0.372) ***
Between 40 and 54 years old	22.09	9.122
	[41.48]	(0.338) ***
More than 55 years old	7.55	4.554
	[26.43]	(0.237) ***
Seniority in unemployment		
Less than 3 months	25.51	-2.263
	[43.59]	(0.279) ***
Between 3 and 6 months	21.53	-1.015
	[41.10]	(0.266) ***
More than 6 months	52.73	3.241
	[49.93]	(0.309) ***
Education Level		
No high-school diploma	16.89	-4.819
	[37.47]	(0.233) ***
Vocational degree	31.86	-6.319
	[46.59]	(0.294) ***
High school diploma	18.99	-1.466
	[39.22]	(0.226) ***
University degree	32.26	12.601
	[46.75]	(0.312) ***
N	181072	212277

Notes: This table reports some summary statistics for individuals who did not respond to the survey as well as the results of tests of equal means with the respondents over a set of pre-intervention covariates. Column (1) shows average values of characteristics for non-respondents, with standard deviations in brackets. Column (2) reports regression coefficients on survey response dummy with standard errors in parentheses. All regressions include agency and month fixed effects, weighted by the empirical probability of being assigned to the treatment group for each agency x month strata. Standard errors are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.4: First Stage

	Bob account	Attended meeting	Bob account + Attended meeting	Bob account + Attended meeting	Bob account + Attended meeting + Reported Use
	(1)	(2)	(3)	(4)	(5)
Treatment	0.130	0.205	0.269	0.421	0.441
	(0.002)***	(0.006)***	(0.006)***	(0.007)***	(0.007)***
Control Mean	0.002	0.000	0.002	0.004	0.091
N	212052	212052	212052	31171	31171
Correlation	0.662	0.851	1.000	1.000	0.788
Sample	All	All	All	Survey	Survey

Notes: This table reports regression coefficients on a treatment group dummy estimating equation 2.3, for different types of take-up. The dependent variables in columns (1) to (3) are dummy variables for the creation of a Bob emploi account, the attendance to an information meeting, the creation of a Bob emploi account or the attendance to an information meeting, respectively. The sample used in columns (1) to (3) is the whole sample. Column (4) shows the results of the same regression as (3) on the restricted sample of survey respondents. Column (5) uses as dependent variable a dummy for having either created a Bob emploi account, attended a meeting or reported using Bob emploi in the survey. The correlation coefficient shows the correlation with the dependent variable used in column (4). Means in the control group are computed separately. All regressions include agency x month fixed effects. Standard errors in parentheses are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.5: Difference in means between takers and non-takers in the treatment group

	Non-taker mean (1)	Regression coefficient on taker dummy (2)
Female	50.40	1.204
	[50.00]	(0.359) ***
Age		
Less than 25 years old	25.13	-11.345
	[43.38]	(0.342) ***
Between 25 and 39 years old	46.28	-5.503
	[49.86]	(0.413) ***
Between 40 and 54 years old	21.78	9.044
	[41.27]	(0.357) ***
More than 55 years old	6.81	7.804
	[25.20]	(0.278) ***
Seniority in unemployment		
Less than 3 months	25.38	-1.117
	[43.52]	(0.347) ***
Between 3 and 6 months	21.51	-1.111
	[41.09]	(0.317) ***
More than 6 months	52.88	2.197
	[49.92]	(0.396) ***
Education Level		
No high-school diploma	16.24	-1.012
	[36.88]	(0.281) ***
Vocational degree	31.15	-0.472
	[46.31]	(0.381)
High school diploma	19.35	-2.256
	[39.51]	(0.298) ***
University degree	33.25	3.740
	[47.11]	(0.425) ***
N	94906	119525

Notes: This table reports some summary statistics for individuals in the treatment group who did not take up the treatment as well as the results of tests of equal means with the takers over a set of pre-intervention covariates. Column (1) shows average values of characteristics for non-takers, with standard deviations in brackets. Column (2) reports regression coefficients on take-up dummy with standard errors in parentheses. All regressions include agency and month fixed effects, weighted by the empirical probability of being assigned to the treatment group for each agency x month strata. Standard errors in parentheses are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.6: Impact on reemployment expectations, effort and scope of job search

	Survey Data:				Administrative Data:		
	(1) Expects to find a job in less than 3 months	(2) Hours per week spent on job search	(3) At least one unsolicited application per week	(4) Applies to job beyond own municipality	(5) Number of online applications	(6) Number of online applications beyond own municipality	(7) Number of online applications beyond past occupation
Panel A: ITT							
Treatment	-0.001	-0.097	0.005	-0.001	0.023	0.034	-0.012
	(0.007)	(0.098)	(0.006)	(0.006)	(0.067)	(0.063)	(0.033)
Panel B: LATE							
Taker	-0.002	-0.231	0.011	-0.003	0.086	0.125	-0.044
	(0.015)	(0.229)	(0.014)	(0.013)	(0.247)	(0.234)	(0.122)
Control Mean	0.576	8.517	0.456	0.523	2.439	2.117	2.057
N	19523	31171	31171	31171	212052	212052	212052

Notes: This table reports regression coefficients on a treatment group dummy estimating ITT equation 2.1 in panel A and LATE equation 2.2 in panel B. Means in the control group are computed separately. Columns (1) to (4) use dependent variables from survey data, columns (5) to (8) use dependent variables from administrative data which are available for the entire sample. All regressions include agency x month fixed effects. Standard errors in parentheses are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

Table 2.7: Impact on job search methods

	Survey Data:							Administrative Data:	
	(1) Uses personnal network	(2) Uses professional network	(3) Adapts CV to job listing	(4) Adapts cover letter to job listing	(5) Follows up with recruting firms	(6) Number of used Pôle emploi websites	(7) Number of used private websites	(8) At least one meeting with caseworker	(9) Number of workshops attended at Pôle emploi
Panel A: ITT									
Treatment	0.012	0.006	0.009	-0.004	0.011	0.070	-0.009	0.024	-0.001
	(0.006)**	(0.006)	(0.007)	(0.006)	(0.006)*	(0.009)***	(0.011)	(0.003)***	(0.010)
Panel B: LATE									
Taker	0.029	0.013	0.020	-0.008	0.026	0.167	-0.022	0.090	-0.004
	(0.014)**	(0.014)	(0.015)	(0.012)	(0.014)*	(0.022)***	(0.026)	(0.012)***	(0.036)
Control Mean	0.691	0.571	0.633	0.760	0.430	1.446	0.855	0.572	0.653
N	31171	31171	22970	23106	23044	31171	31171	212052	212052

Notes: This table reports regression coefficients on a treatment group dummy estimating ITT equation 2.1 in panel A and LATE equation 2.2 in panel B. Means in the control group are computed separately. Columns (1) to (7) use dependent variables from survey data, columns (8) use dependent variables from administrative data which are available for the entire sample. All regressions include agency x month fixed effects. Standard errors in parentheses are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

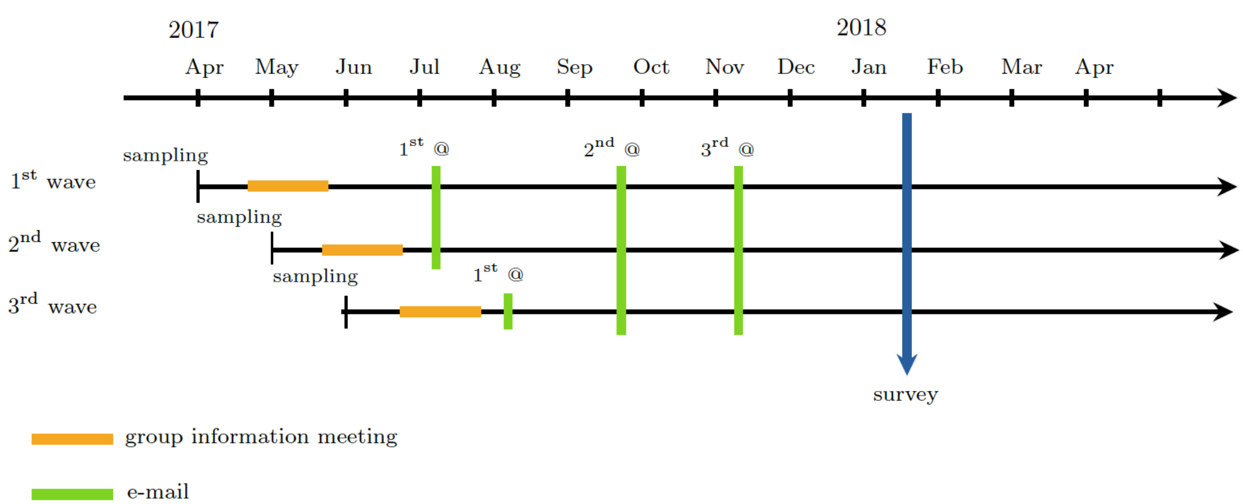
Table 2.8: Impact on well-being, motivation and life balance during job search

	Overall well-being	Feels motivated during jobsearch	Feels supported during jobsearch	Partici-pates in non-job related activities
	(1)	(2)	(3)	(4)
Panel A: ITT				
Invité	0.026	-0.036	0.051	0.008
	(0.025)	(0.027)	(0.033)	(0.005)
Panel B: LATE				
Participant	0.061	-0.085	0.121	0.018
	(0.059)	(0.064)	(0.078)	(0.011)
Control Mean	5.182	7.069	3.884	0.779
N	31171	31171	31171	30276

Notes: This table reports regression coefficients on a treatment group dummy estimating ITT equation 2.1 in panel A and LATE equation 2.2 in panel B. Means in the control group are computed separately. All columns use dependent variables from survey data. All regressions include agency x month fixed effects. Standard errors in parentheses are clustered at the agency level and ***, **, and * indicate significance at 1, 5, and 10% respectively.

2.8 Figures

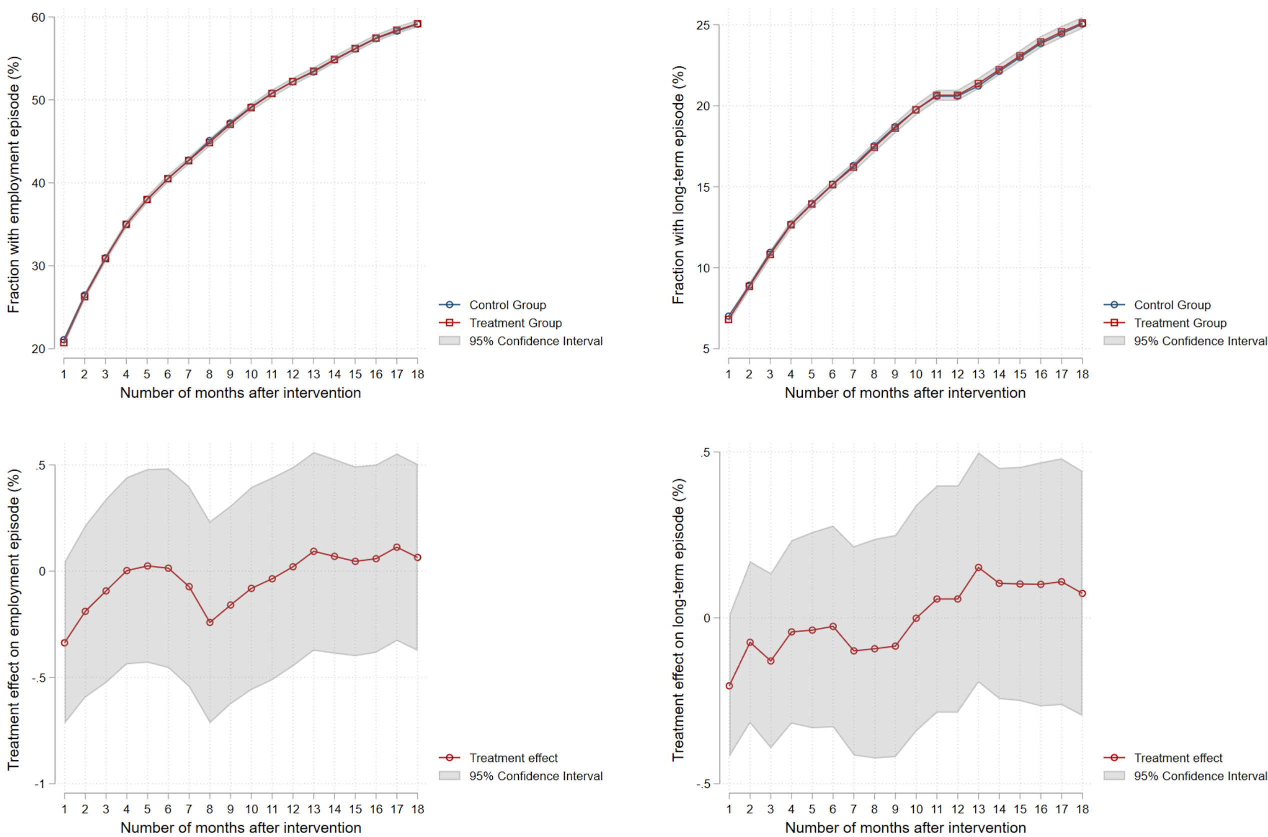
Figure 2-1: Timeline of the experiment



Notes: This figure shows the timeline of the experiment. The sample was drawn over three consecutive waves at the beginning of April, May and June. Information meetings for each wave started within the month of sampling and last one month. Three reminder emails were sent out in late July, late September and early November. The online survey was sent six months after the last wave of information meetings in January 2018.

Figure 2-2: Average treatment effects on employment episodes

(a) Fraction with employment episode (b) Fraction with long-term employment episode

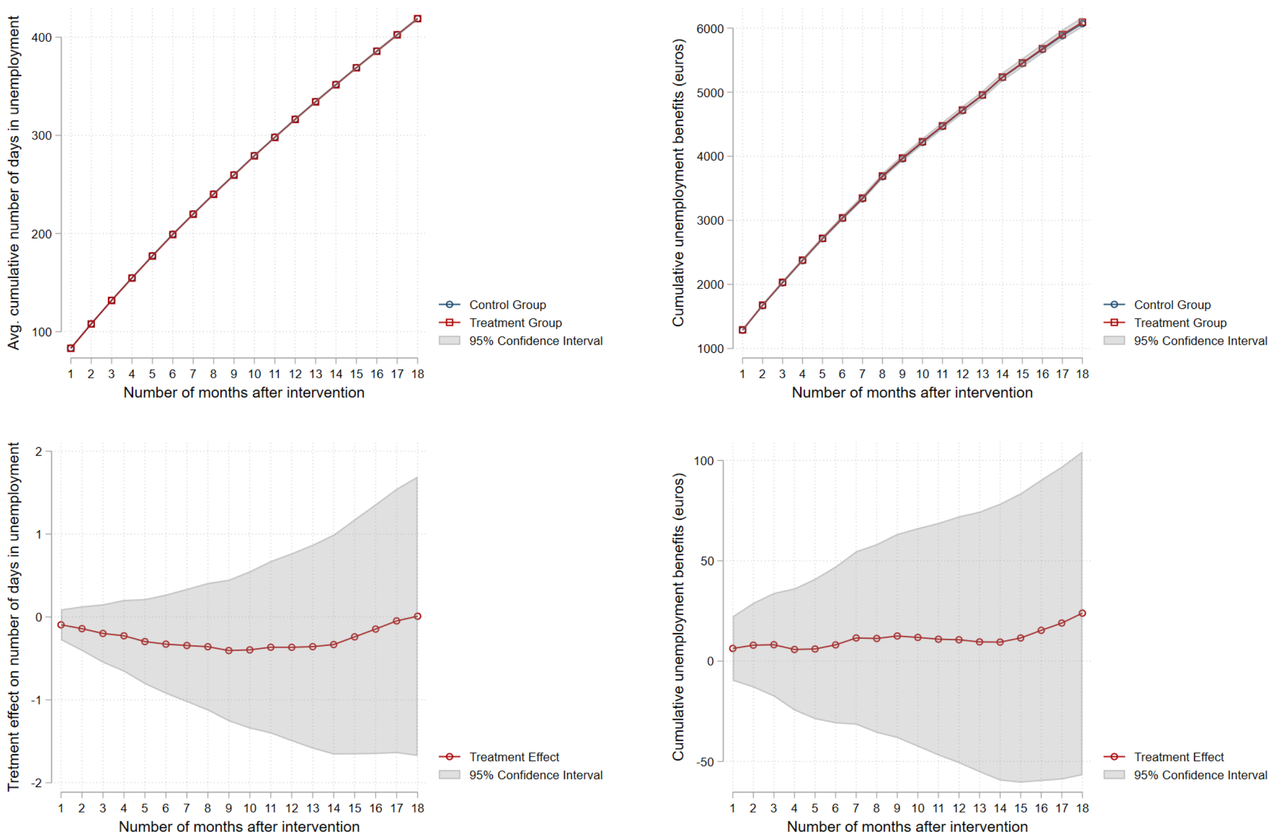


Notes: Figure (a) in the left panel shows the fraction of individuals who experienced an employment episode over the period. The top graph plots the evolution in levels over time, in blue and red for the control group and the treatment group, respectively. The bottom graph shows the ITT treatment effect as estimated by equation 2.1. Figure (b) in the right panel works similarly for the fraction of individuals with an employment episode corresponding to a long-term contract. Shadded grey areas correspond to symmetric 95% confidence intervals around values in the treatment group. For all graphs, the x-axis represents the number of months since the individual was included to the experimental sample.

Figure 2-3: Average treatment effects on cumulative number of days in unemployment and unemployment benefits

(a) Cumulative number of days in unemployment

(b) Cumulative unemployment benefits



Notes: Figure (a) in the left panel shows the mean cumulative number of days in unemployment over the period of observation. The top graph plots the evolution in levels over time, in blue and red for the control group and the treatment group, respectively. The bottom graph shows the ITT treatment effect as estimated by equation 2.1. Figure (b) in the right panel works similarly for the mean cumulative unemployment benefits received over the period. Shadded grey areas correspond to symmetric 95% confidence intervals around values in the treatment group. For all graphs, the x-axis represents the number of months since the individual was included to the experimental sample.

2.9 Appendix

2.9.1 Survey questionnaire

In this section, we report the survey questionnaire. Note that the first question was directly included in the email that invited job seekers to respond to the survey, while the other questions were gathered on a dedicated webpage and accessible via a hyperlink in the email. The questions were adapted to the employment status of the respondent, and asked employed individuals to refer to their latest job search period. We do not report the exact format of each question (e.g. whether respondents could fill an empty box or move a cursor to pick a number).

[q1] **What is your current employment status?**
Note : The "employment" category includes all types of contracts (short term/ seasonal (CDD), long-term/ salaried (CDI), temporary work and subsidized employment contracts).

- I am unemployed and searching for a job
- I am employed but I am still searching for another job
- I am employed and I am not currently searching for a job
- I intend to create my own business or I am self-employed
- I am doing an internship or a job training
- Other answer (please specify)

This survey takes on average 5 minutes. Your responses are kept anonymous and will never be shared with your caseworker. Your responses are very useful! They help us improve our support services!

The following questions are about your job search.
[q2] **During a typical week, over 7 days, how many days are you active in your job search?**
[q3] **On an average day that you look for work, how much time do you spend on your job search?**

- Between 0 and 30 minutes
- Between 30 minutes and 1 hour
- Between 1h and 2h
- Between 2h and 4h
- More than 4h
- I would prefer not to say

[q4] **In your opinion, what is the most useful action to move forward in your job search?**

- Responding to job offers online
- Networking
- Sending unsolicited applications
- I would prefer not to say

[q5] **Over the past month, how many times have you been invited to interview for a job?**

[q6] **In general, you send unsolicited applications...**

- Multiple times per week
- Once per week
- 1-3 times per month
- Less than once a month
- I have never sent an unsolicited application
- I would prefer not to say

The following 4 questions are about your global outlook on daily life.

[q7] **On a scale of 1-10, where would you put your life right now?**

Note: 0 indicates that you feel you are living the worst possible life and 10 indicates the best possible life for you. You may slide the curser to select your response.

[q8] **On a scale of 1-10, how motivated do you feel in your job search?**

Note: 0 represents a total absence of motivation and 10 a very strong motivation. You may slide the cursor to select your response.

[q9] **On a scale of 1-10, how much support do you feel you have during your job search?**

Note: 0 indicates a total lack of support and 10 a robust support system. You may slide the cursor to select your response.

[q10] **For the following sites, say if: you've never heard of it / you've heard of it / you've used it and find it useful / you've used it and don't find it useful**

- La Bonne Boîte
- L'Emploi Store
- Le Bon Coin
- LinkedIn
- Bob Emploi
- Jobi Joba
- La Bonne Formation
- CV Designer
- Jobeggs
- Pôle-emploi.fr

[q11] **Think back to your latest job applications. How frequently did you do the following? Systematically / Often / Sometimes / Rarely / Never**

- You added key words from the job offer to tailor your resumé
- You modified your cover letter to fit the job offer
- You followed-up with the recruiter a few days after sending your application

[q12] **Do you rely on the following persons to help you in your job search? Absolutely / More or less / Not really / Not at all**

- Friends and family
- Friends of friends
- Former classmates or college alumni
- Former coworkers
- Your Pôle emploi caseworker
- Volunteers from organizations that assist job seekers
- Local business owners
- Persons found online (via LinkedIn, etc.)

[q13] **What was your monthly salary (after taxes) from your latest job?**

[q14] **You are searching a job...**

- In your municipality and in neighboring municipalities
- Throughout your county
- Throughout your region
- Throughout the country
- I would prefer not to say

[q15] **What is your current monthly salary (after taxes)?**

Chapter 3

How do job seekers select training providers? Results from an online survey in France

3.1 Introduction

Government-sponsored training is a prominent policy tool to address structural unemployment and to help workers transition from declining to booming sectors. An extensive literature spurred by (LaLonde 1986) investigates the effectiveness of training programs and finds positive average effects on reemployment in the medium- and long-term but also highly heterogeneous impacts across sites and participants (Card et al. 2018).[1] As in primary and secondary education systems (Walters 2018), the quality of the supply side and the allocation of trainees to efficient providers are possible, yet poorly understood, determinants of training quality.

This paper provides descriptive evidence contributing to the emerging debate on the potential of demand-driven mechanisms to increase quality among training suppliers (Barnow and Smith

[*]This paper owes very much to the many discussions and field investigations I undertook with Louise Paul-Delvaux. Arthur Contejean provided excellent research assistance in the analysis part and Pierre-Louis Bithorel contributed to the implementation of the protocol. I am grateful to Esther Duflo and Frank Schilbach for their guidance, and to Chantal Vessereau at Pôle emploi who supported this project.

[1]In their article describing the US training system and the lessons from previous research, Barnow and Smith (2015) note that "The existing evidence makes it clear that some programs (in particular, the adult funding stream of the Workforce Investment Act program) have positive impacts on labor market outcomes sufficient to justify their costs, while many others do not. Explaining the differences in impacts among programs (and between funding streams within programs) remains an important topic for future research. In addition, we argue that the literature should shift its focus somewhat from research that estimates the impacts of program participation to research on how to better operate existing programs."

2015).[2] This approach aims at facilitating trainee choice so that poor-performing centers are incentivized to improve quality to attract demand or get driven out of the market. In the context of public-sponsored training, decisions to enroll also take place at the individual level, as job seekers are most often free to choose the provider they prefer within the list of subsidized programs. To leverage these demand forces and improve the efficiency of the training system, the French government embarked over the last decade on several structural reforms. These reforms culminated in November 2019 with the launch of a mobile application whereby workers and job seekers can directly use the funding to which they are individually entitled to select and enroll in the program of their choosing.

The success of such demand-driven mechanisms relies heavily on two assumptions. First, job seekers need to have access to accurate information on providers' quality (Hipp and Warner 2008). A second and often implicit assumption is that job seekers' preferences are primarily focused on immediate reemployment. That is, conditional on being well-informed, job seekers would prefer providers with the highest reemployment rates.

There is only limited evidence whether these conditions are both met. Several studies on job search and labor mobility have shown that job seekers may value other objectives than short-term reemployment (Jacobson and Davis 2017; Autor et al. 2014; Acemoglu and Restrepo 2020). Moreover, even policymakers only have scarce information on provider performance.[3] In addition, training providers in most developed countries are characterized by a high degree of heterogeneity, ranging from large community colleges to small for-profit organizations, which may further complicate job seekers' decision-making (Barnow and Smith 2015; Crépon et al. 2018; Ba et al. 2017).

In France, program contents come with virtually no requirements on staff and educator recruitment. This gives providers the liberty to determine optimal combinations of, e.g., on-the-job

[2]In their review, Barnow and Smith (2015) underline the need for more research to understand "how participants use information in making choices and on the effects of additional types of information on choices and outcomes, represents a logical next step."

[3]In the absence of randomized evaluations, the large variation in programs, participant profiles, and labor market conditions poses a serious challenge to measuring training center performance with observational data only. In many countries like France, this problem is further accentuated by constraints on data collection and sharing regarding trainees' professional trajectories and training center characteristics (McCall et al. 2016).

expertise with pedagogical skills or classroom teaching with on-the-job practice, to make arbitrary choices of investment in equipment and staff recruitment, and to set their own criteria for the selection of participants. With funding coming from several different sources, providers can have very different business models and organizational structures. Their performance will likely depend on their connections with firms in local labor markets, which are not easily observed by prospective trainees. The diversity of these characteristics and the multiplicity of performance factors can induce high levels of uncertainty for prospective trainees, while it creates room for heterogeneity in quality across providers.

In this paper, I report the results of an online survey with job seekers about their decision-making process regarding their choice of a training provider. Using unemployment records from the French public employment service, I surveyed 2 073 trainees who had started their program after January 2017 in one of the thirteen largest labor market sectors from the five largest administrative regions of France.[4] Sectors range from healthcare and financial services to transportation and security.[5] The administrative records provide me with standard socio-demographic variables about the trainees, as well as information about the sector, funding stream, dates and duration of their program.

My first main result is that job seekers gather remarkably little information before choosing a training center. Only 30% of job seekers reported comparing different centers before selecting one and only 27% of the respondents report having visited the center before beginning their program. Only half of the latter visited more than one training center, and the other half visited only the center in which they finally enrolled. This cannot be entirely explained by limited choice sets as only half of the respondents who knew of at least two accessible centers for the program they wanted said they compared different providers before making their choice.

When seeking information, trainees only consult a limited numbers of sources. Only 11% of the respondents cross-checked information from multiple sources and nearly half respondents only relied on their caseworker, who is also the person who informed them about the center. A third

[4] The response rate to the entire survey was slightly above 13% and reached 21% for the first questions of the questionnaire, a relatively high response rate for online surveys. In a comparable study with job seekers in Germany, DellaVigna et al. (2020) survey 6 877 individuals out of an initial sample of 86 673, which translates into a response rate of nearly 8%.

[5] The complete list of sectors can be found in appendix 3.8.2.

of the respondents relied not on their caseworker but on the internet, but among those, 62% only looked up the website of the training provider they selected.

My second main finding is that job seekers' preferences are not limited to maximizing re-employment chances. I find that the top two reported criteria for choosing a center used by the respondents are whether the center was the closest to their home (40%) or that the starting dates of the program were the most convenient (36%). Besides, contrary to a common assumption of theoretical models and an important rationale for the recent policy reforms in France, only a third of the respondents declare that the primary goal of training was to get reemployed faster. Nearly half of the respondents declare that they enrolled in a program either to get a job that better matches their preferences (29%) or to embark on a new career path (11%). These observations on preferences may partly explain the low effort exerted in collecting information.

Overall, these results suggest that the ongoing reforms to give more freedom to job seekers may fall short of their targets to improve the quality of the training supply if the level of information on providers does not increase at the same time. The study points at concrete actions that job seekers could be encouraged to undertake to better explore the available supply even outside of any structural reform. It also reveals the limits of a fully demand-driven approach if it overlooks job seekers' real preferences and important parameters of the current institutional setting that largely shape the supply and the job seekers' experience.

Related literature. This work is motivated by empirical evidence provided in recent economic literature that in the context of structural changes, enabling workers to switch occupations and move to growing sectors is essential to the design of effective active labor market policies (Walker 2013; Autor et al. 2014; Hyman 2018). The meta-analysis of Card et al. (2018) shows from a large number of studies across countries that training is the most long-lasting and impactful policy to help job seekers. It provides them with valuable human capital that can facilitate the transition to new jobs and set them on stable career paths (Kambourov et al. 2020). However, the impact of training is heterogeneous, not only across countries and institutional settings, but even within studies across programs and funding streams (Andersson et al. 2016; McCall et al. 2016). Barnow and Smith (2015) and McCall et al. (2016) conclude that there is a need for more research to understand not just whether training works but also how to make training work better.

This study contributes to this agenda by looking at the supply side of training systems. Suppliers' quality has been shown to be an important determinant of performance in the schooling system. Looking at the U.S. Head Start program, Walters (2018) finds that the variation of the program performance due to center quality is larger than standard estimates of teacher effects. Barnow and Smith (2015), Ba et al. (2017) and Crépon et al. (2018) point at the multiplicity and heterogeneity of operating training providers in France and in the United States, which suggests that there is also room for heterogeneity in training quality among training providers. Yet, outside of high-level policy reports that show a few aggregate figures such as the total number of operating training providers, I know of no study in either country that provides detailed information on the state of the training supply.

In this paper, I question the potential of leveraging demand forces to increase competition among suppliers and improve the quality of the training programs. This mechanism has been widely discussed in the context of schooling and more generally of the provision of public services (Plank and Sykes 2003). One strand of the literature has been to test the impact of spreading information to make citizens more aware and more able to sort through providers, not only on individuals' outcomes but also on market-level parameters. The results thus far are mixed. The experiments reported in Andrabi et al. (2017) and Hastings and Weinstein (2008) were successful at increasing quality among suppliers, sometimes even lowering prices. However Banerjee et al. (2010) and Mizala and Urquiola (2013) don't see any effect of information provision, perhaps because the additional information provided was not sufficiently accurate or relevant for each individual decision. This suggests that empowering job seekers to choose their training providers may not necessarily lead to better outcomes if individuals are not enabled to observe reliable indicators of centers' quality. This condition may in turn be hindered by the absence of statistically robust metrics of reemployment performance and on the lack of informative observable characteristics.[6]

Demand forces in vocational training systems are often discussed in experiments that test the impact of vouchers (e.g. Rinne et al. 2008). Such studies can only propose reduced-form estimates on individual employment outcomes and rarely speak about the effect on the supply side. One

[6]In his analysis of Head Start centers, Walters (2018) finds that characteristics often mentioned to explain performance are not the most predictive of performance of centers delivering Head Start programs.

exception is Hipp and Warner (2008) who discuss the implications of voucher systems in Germany and in the United States. They conclude that contrary to voucher proponents' arguments, vouchers tended to reinforce information asymmetries between prospective trainees and training centers, as well as market concentration among centers, driving average quality down. Overlooking caseworkers in the matching of trainees to centers may be sub-optimal, as suggested by evidence on schooling decisions of individuals' low scrutiny of badly performing schools (Abdulkadiroğlu et al. 2018) and on the beneficial role of counselors (Carrell and Sacerdote 2017).

The existing literature on job search and job loss provides many examples where job seekers' preferences are not limited to maximizing their reemployment chances. Using data from Florida, Jacobson and Davis (2017) show that trainees do not enroll in fields with highest reemployment rates and wages. Several empirical studies on job loss also reveal a low degree of spatial and occupational mobility among job seekers, which suggests that job seekers make their decisions not only based on reemployment probabilities, but also by focusing on location (Autor et al. 2014; Acemoglu and Restrepo 2020; Menezes-Filho and Muendler 2011). In the related literature on primary and secondary schooling, Abdulkadiroğlu et al. (2018) provides striking evidence where parents do not only value school performance as measured by test scores.

The rest of the paper is structured as follows. Section 2 gives some background on how the French training system works, which helps interpret job seekers' answers. Section 3 briefly describes the questionnaire and section 4 provides an overview of the sample of respondents and the data I use. Section 5 discusses the main results. Section 6 offers a conclusion and suggestions for future research.

3.2 Supply and demand in the French training system

This section provides a description of how professional training for the unemployed is organized in France. After a general overview, I present the different funding streams and the existing procedures of quality control of providers. Lastly, I show some aggregate numbers describing the supply side and I conclude with the information currently available to prospective trainees about training providers.

Central policy. Subsidized training to the unemployed is a cornerstone of labor market policies in France. Over the past decade, the consecutive governments argued that vocational training was an essential and effective firewall against the downside effects of trade, labor market liberalization, and technological changes.[7] In France, nearly five billion euros are spent yearly on government-sponsored training for job seekers, enrolling on average 750 000 individuals every year over the last four years. Trainees are 35 years old on average, with 30% under 26 and 13% above 50.[8]

There is a vast and diverse landscape of training programs. The main sectors in terms of number of programs per year include healthcare, financial services, transportation, security and food services.[9] Programs last on average nine months, which represents about 500 hours of training in total. This average hides important variation, with some programs lasting only a week (e.g. first degree of forklift driver) while other programs may last two years (e.g. nurse training). Training programs in healthcare and social services generally last longer and deliver professional degrees, while training programs in transportation and security can be much shorter, often responding to short-term local labor demand shocks. Programs almost always include some classroom teaching and some on-the-job practice conducted in private firms.

Funding streams. In France, vocational training for the unemployed is funded by two main actors: firstly, administrative regions, that primarily target young job seekers and subsidize certified training programs lasting more than several months; and secondly, regional offices of the Public Employment Services ("Pôle emploi") that sponsor shorter training programs aiming for quick transitions back into employment.[10]

[7]Running for the 2017 presidential elections, Emmanuel Macron declared in a meeting: "We do not say that we need to protect jobs of the past, we say that we will enable you to go through these [structural] changes, and transform yourself to get access to new jobs."

[8]2018 Draft Budget Bill

[9]I include in the survey the 13 largest sectors in terms of number of programs. The complete list is available in appendix 3.8.2. The French unemployment records count programs of "personal development" as vocational training, e.g. workshops where job seekers learn how to manage their interviews or get a skill assessment. The largest number of programs are of this type. I exclude them from this study to focus on programs that only provide job seekers with professional skills related to a specific occupation.

[10]The federal state intervenes only by giving general economic orientations or by launching one-time national programs. Out of the 4.91 billion euros spent in 2015 on training for the unemployed, 1.47 billion were contributed

Regions fund about half of all public-sponsored programs through a process of public auctions. Every two to four years, regional administrations identify a set of priority sectors, based on national orientations from the federal government on local labor market conditions and on forecasts of future labor demand.[11] Within each sector, they list the programs that they wish to subsidize with requirements on the duration and content of the programs, and possibly on targeted beneficiaries or geographical coverage. Training providers then apply to the public auction for one or multiple programs in the list. In spite of the requirements, providers have a lot of room to design their program, e.g., regarding the participant selection procedure, the staff to recruit or the mix of on-the-job vs. classroom training.[12] Based on these applications, regional administrations select one to three providers by program that become eligible for regional subsidies until the next public auction. Through this procedure, regions can negotiate low prices by contracting on a minimal number of slots, while giving providers some insurance to cover their fixed costs as well as some reputational advantage. Pôle emploi regional offices run similar auctions to buy multiple slots in advance from a set of selected providers in sectors that are considered to be top priority.

As a complement, Pôle emploi local agencies can grant subsidies to accommodate for individual needs, which represent about a third of the government-sponsored programs to the unemployed. Job seekers who request individual funding need to get their caseworker's approval by showing the program is consistent with their professional project. They must demonstrate that the program has not been funded within the regional group programs and that they have undertaken some minimal market analysis to choose a provider.[13]

Therefore, caseworkers are told to encourage job seekers to consider group programs over individual grants. Job seekers who want to enroll in a group program have an easier access to funding,

---

by regions, 1.94 billion by Pôle emploi, 820 million were spent by firms, 370 million by the state and 310 million by beneficiaries themselves. This is reported in table 6 of the appendix of the 2018 Draft Budget Bill.

[11]In 2017, regions started to organize some auctions at a higher frequency to better respond to local labor demand variations.

[12]Field interviews and observation of the applications to the 2018 Paris region public auction revealed that applications vary greatly on prices, partly reflecting the diversity of providers' business models. Auctions tend to favor large providers, with sufficient administrative staff to handle the application procedure and with enough centers. To meet the requirements on geographic coverage, providers may apply in groups with a leading provider but they do not need to make their programs uniform.

[13]Job seekers are in principle requested to show cost estimates from at least two different providers. From my observations during field interviews, it was not clear that this rule was enforced in practice.

but within a limited set of providers, than those who enroll in individual programs.

Quality controls. The procedure for any organization to get registered as a training provider in France is remarkably straightforward and does not include any thorough quality assessment.[14] For group programs, public auctions work as a form of ex-ante quality control. Programs through which job seekers obtain formal degrees and certificates must also be approved by a third party, which imposes some requirements on the program content. Providers may obtain additional labels that are most often sector-specific and relevant for advanced hard skill training.

The multiplicity of providers and the absence of consolidated data on trainees' outcomes and center characteristics make it much harder for public institutions to undertake ex-post quality controls and to investigate providers that do not apply to public auctions. Routine control procedures include random visits of public agents and online surveys of former trainees. The aforementioned data constraints limit the possibility of using of performance-based payments, as is done in other countries.

Since the promulgation in 2015 of a federal decree called "*Decret qualité*", the public institutions providing funding for job seekers' training programs are considered responsible for the quality of the programs. Funders are asked to make sure that eligible providers can "deliver programs of high quality" by looking at several standardized criteria (e.g. staff qualification) before allocating funding. Yet, in spite of this attempt to formalize quality criteria, the decree did not provide additional means to strengthen public oversight of providers. Thus, in 2019, the government launched a massive reform with the stated goal to "remove intermediaries" and to give more freedom to job seekers to use the amount of funding to which they are entitled and to select the programs and the providers in which they are interested.[15]

[14] The procedure notably allows firms for which training only represents a small fraction of their business to register as a training center. In Germany, on the contrary, providers need to obtain a certification. Crépon et al. (2018) observe that there are consequently more than 60,000 approved training providers in France, most of them being individual firms, while there are less than 5,000 certified providers in Germany.

[15] The reform also applies to employed workers who previously needed to obtain the approval of public organizations run by workers' unions and employer organizations. This reform is planned to be followed by a re-design of the certification system that would make the French system closer to the German one, with fewer providers and several private certification agencies.

Current state of supply. According to the 2018 Draft Budget Bill, there are roughly 68 500 active training centers in France for total revenues of about 14 billion euros, which includes training delivered to employed workers. Since I know of no study quantitatively analyzing the distribution of training providers in France in greater detail, I leverage data from unemployment records tracing training programs that job seekers enroll in.[16] These records include a variable that indicates the establishment number of the training provider.[17] Looking at programs from January 2013 to July 2017, I count more than 24,000 different numbers. This figure goes down to 15,000 when I remove on-the-job training programs conducted as internships at private firms. The market is highly skewed, with some establishments having no more than one training participant over the period whereas others register more than 1,000 entries. The largest training providers based on the number of recorded programs include public or semi-public training centers that deliver training in many different sectors.

Available information for prospective trainees. Information on training providers is currently spread across multiple websites and can be considered as incomplete. The largest websites aggregating information on centers are hosted by the inter-regional training office, called CARIF-OREF, and the public employment service, Pôle emploi.[18] Beyond logistical details on location and dates as well as some description of the program content, websites generally provide limited reliable information to assess the quality of the training, especially regarding the trainees' employment outcomes after the program. In 2017, the Paris region launched an experiment, called "Anotéa", whereby they surveyed former trainees and displayed their average feedback on Pôle emploi information websites. An important challenge to the success and the replication of such an initiative is the ability to get a sufficiently high response rate to ensure statistical reliability. Programs often enroll no more than 30 trainees per year, and program contents continuously evolve which makes it difficult to design consistent metrics over multiple cohorts.

[16] The research center called CEREQ provide qualitative studies on small samples, while policy reports generally present only high-level aggregated figures. For a number of years, the statistical services of the Ministry of Labor, called DARES, relied on a form that training centers had to fill out every year, reporting their total revenues and the number of trainees. The most recent report I could find using this data dates back to 2014.

[17] This variable may be partly deceiving due to changes in establishment IDs or data misreporting.

[18] Pôle emploi runs two platforms, called "Trouver ma formation" and "La bonne formation" that draw information from the inter-regional catalog of the CARIF-OREF which include public-sponsored collective programs.

3.3 Survey description

To shed light on the decision-making process of job seekers when they choose a training provider, I designed an online survey sent to a large sample of trainees from the public employment services. This section provides a high-level overview of the survey questionnaire and of the sampling protocol. The exact phrasing is presented in the Appendix 3.8.1. I then explore the sample to whom the survey was sent, the response rate, and the demographics of respondents and non-respondents to analyze the dimensions of selection into responding.

3.3.1 Survey questionnaire

The survey covers two broad topics: (i) the information that the trainees collected as part of their search for training provider and (ii) their preferences about providers. A first set of questions inquires about different concrete actions that the respondents could undertake to evaluate the quality of the training center and make their choice, including comparing different centers or paying a visit in person. Respondents are also asked about the ways they first heard about centers and from whom they obtained useful information.

To interpret these answers, I also ask about the employment situation at the time of the survey and the available choice set of suppliers, as perceived by respondents. Responses to this latter question are endogenous to respondents' level of effort in seeking information about centers: job seekers who search more intensively will know more centers. Nevertheless, in the absence of existing studies on providers' spatial distribution, it provides a useful lower bound of the real size of choice sets faced by prospective trainees.

In light of the government reforms of the training system, I pay special attention to providers' reemployment rates, and I ask respondents whether they have access to that information, find it useful and used it. Lastly, respondents partly reveal their preferences by reporting the criteria they used to choose providers (e.g. center size, reputation, location) and their main objective with the training.

3.3.2 Sampling procedure

The survey was administered to two sets of individuals: (i) current job seekers and (ii) formerly unemployed individuals who had participated in a training program that started after January 2017. In order to minimize regional disparities in terms of program supply, I sampled trainees from the five largest regions of France in terms of number of trainees, namely Ile de France, Hauts de France, Grand Est, Occitanie and Provence-Alpes-Cote-d'Azur. Considering the high diversity of training programs, I also limited my study to programs in one of the thirteen largest professional sectors so that the sample sizes by sectors would not be too small for statistical inference. The complete list of sectors is available in the Appendix Section 3.8.2.

To focus on training programs whereby job seekers acquire new professional skills, I removed all training programs related to "personal development," e.g. workshops to learn how to prepare an interview or to write a job application. I stratified the sampling by region and sectors, as well as by starting date of the training. More precisely, I imposed that a third of each strata would still be training at the time of the survey. In total, 9 752 trainees were included in the sample, with about 750 in each sector, equally distributed across regions.

3.3.3 Sample description

Figure 3-1 shows the distribution of individuals in the sample along education, gender, and age across sectors. We see that the sample is heterogeneous along these characteristics, with markedly different profiles depending on the sector. The fraction of trainees with less than a high school diploma or a vocational degree varies from 5% in financial services to 40% in the material handling sector. Across sectors, the share of female ranges from less than 10% to nearly 90% while the share of young under 30 ranges from 22% to 60%. This gender pattern is particularly striking: there are less than 10% of women in electrotechnical and material handling sectors, while more than 80% of trainees are women in financial services, healthcare or secretarial services.[19] The typical trainee in healthcare is an educated young woman participating in a group training program funded by

[19] This selection pattern is commonly observed in the context of occupation and training choices (e.g. Jacobson and Davis (2017) in Florida) and may partially explain heterogeneity of returns to training observed in other studies.

the administrative region. Conversely, the typical trainee in the material handling industry is a low-education man following a group program funded by the public employment services.

Figure 3-2 similarly splits the sample across funding streams. 38% of the sample had participated or was participating in a group program funded by administrative regions. 24% are in a group program funded by the public employment services. 15% were granted an individual funding from the public employment services and 12% participated in an on-the-job training likely funded by the firm. The remaining 10% were enrolled in other specific programs, either funded by their municipalities, by the federal state or by themselves. Age, gender and education are fairly balanced across training streams.

3.3.4 Response rate and respondents characteristics

The survey was sent by email on April 9, 2018 to all individuals in the sample. Emails were sent from Pôle emploi servers, with a standard email address dedicated to surveys, and responses were received on the same servers. 2 073 individuals answered at least one question of the questionnaire, which corresponds to a response rate of 21 %. This rate goes down to 13 % (1 295 respondents) if we only consider those who responded to all questions of the survey. Such a response rate is fairly high for online surveys in general and standard for surveys among job seekers sent by the Pôle emploi public employment services.[20]

To shed light on the external validity of the study, Table 3.1 shows respondents' profiles and how they differ from non-respondents. Overall, both groups look fairly similar across socio-demographic characteristics. Respondents are aged 16 to 63 years, with more than a third of the sample being less than 25 and nearly 13% older than 50. This distribution and the average age of 36 are fairly representative of unemployed trainees.[21] There are 58% women in the sample of respondents, which is also comparable to the population of trainees but 10 percentage points larger than among all job seekers registered at Pôle emploi. As mentioned in the previous subsection, women are

[20] In a comparable study with job seekers in Germany, DellaVigna et al. (2020) sent SMS messages to a sample of 86 673 job seekers. 7 797 respondents consented to participated and 6 877 were included in the study, which translates into a response rate of nearly 8%.

[21] However, given the large share of young unemployed who benefit from vocational training, respondents significantly younger than the average job seeker registered at Pôle emploi. Perhaps surprisingly, the fraction of trainees older than 50 is also larger than in the entire job seeker population by about 4 percentage points.

highly unevenly distributed across sectors. Table 3.1 shows that the fraction of respondents with no high school or vocational degree is only slightly lower than among non-respondents, which is somewhat unusual given that online surveys generally come with a strong selection on education levels.

Trainees who participated in longer programs were more likely to respond to the survey. 27% of all respondents participated in a program of less than 200 hours while 37% enrolled in a program of more than 800 hours (approximately 25 weeks). The distribution of funding streams remains rather unaffected, although certain funding streams are characterized by longer programs. 22% of respondents were still training at the time of the survey, which is 6 percentage points higher than among non-respondents and is easily explained by the closer connection with Pôle emploi.[22] Looking at complementary questions from the survey, we see that 60% of the respondents had not worked in the sector in which they trained and more than a third had found a job at the time of the survey.

As showed in Figure 3-9 in Appendix Section 3.8.3, response rates markedly vary across sectors from 14% to 34% and this heterogeneity seems to largely explain the selection pattern of survey respondents. Not surprisingly, trainees in material handling, electrotechnical engineering, hospitality, food services and transportation respond less than trainees in financial and secretarial services and there is no difference on education level between respondents and non-respondents within sectors.[23]

[22]Emails were sent out from Pôle emploi servers with a Pôle emploi generic e-mail address that is used for online surveys.

[23]As trainees' profiles vary markedly across sectors, I run report another t-test in column 3 of Table 3.1, this time adding sector fixed effects to the regression. This exercise suggests that half of the selection bias on women is driven by the heterogeneous female fraction across sectors, as does the education level. To complement this exercise, we also look at differences between respondents and non-respondents within each sector. Table 3.2 in Appendix Section 3.8.4 shows that respondents are significantly older in all sectors, but the selection bias on education, female share and training duration disappear in most sectors.

3.4 Results

3.4.1 Information-seeking

The main finding of the study is that job seekers search for and use little information before choosing a training center. The first question of the survey looks at whether job seekers undertake some minimal investigation of the training market by comparing providers before choosing one. Answers reveal that 70% of the respondents simply opt for the first center they identify that offers the program they want to pursue, while only 30% compare different centers before selecting one. Figure 3-3 shows the percentages of individuals who compared centers by funding stream, revealing that slightly more than half of the trainees who obtained an individual grant compared centers whereas this percentage goes down to 20% for collective programs paid by the Public Employment Service and for on-the-job training in firms. The response rate to this question is 21 %, which is particularly high due to its position in the questionnaire.

Next, I investigate whether trainees went in person to the center in order to obtain some first-hand information before enrolling. Even though some aspects of center performance cannot be assessed with a visit, first-hand observations can be useful to compliment the public information available on websites with more informal feedback, either from other trainees or from staff members. Only 27% of the respondents declare that they went on site before the beginning of their program. Half of those visited more than one training center, while the other half visited only the center they ended up going to. 73% of the respondents made no visit at all.

To shed more light on job seeker's information-seeking behavior, the survey contained two questions related to the sources of information used. Trainees were asked from whom they first heard about their center and from whom they obtained useful information to make their choice. I allowed for multiple answers to the second question, within a list containing the caseworker, former colleagues, relatives or friends, as well as generalist search websites and the private websites of the providers. I find that the vast majority of the respondents only use information from the person or the online platform where they first hear about the center, and only 11% of the respondents cross-check information from multiple sources. Figure 3-4 merges the results of both questions by grouping all trainees who used a source of information and looking at their first information source.

We see that respondents rarely rely on other sources that the one they first learnt about the center they went to. As an example, among all job seekers who reported that they used information from Pôle emploi caseworkers, it was the case for 70% of them that they also found out about their center through their caseworker. On the contrary, once job seekers find about a center on the internet, many of them do not get additional information from anybody else.

Looking at individuals who only rely on one source of information, I find that nearly half respondents rely only on their caseworker, from whom they found out about the center. A third of the respondents rely instead on the internet, but among those, 62% only looked up the website of the training provider they selected. Such private websites often provide partial and possibly distorted information about the centers.

3.4.2 Preferences

A second set of questions aims at eliciting aspects of the respondents' utility function regarding their training and the providers they select. Most theoretical models and public policies work under the premise that outside of material constraints, job seekers will make decisions with the sole goal of increasing their re-employment prospects. Instead, I find that respondents who seek to enroll in a training program do not only seek to maximize their short-term reemployment chances but they put high weight on several logistical considerations such as proximity from their home.

First, I asked respondents about the criteria they used to select their center. They could select multiple criteria, including the post-training reemployment rate of the center or the quality of its staff and equipment. The list of possible answers also contained criteria related to logistical aspects of the program, namely, the distance of the center to the trainee's home and the starting date of the program. More than 60% of the respondents selected a center based on a logistical criteria whereas only 42% relied on some of the center's characteristics that may provide information about its quality and thus the job seekers' chances of re-employment. More precisely, Figure 3-5 shows that the top two criteria used by the respondents are that the center is the closest one to their home (40%) and that the starting dates of the program were the most convenient (36%).

In addition, respondents were asked about the main objective that motivated their decision to enroll in a training program. Respondents were only allowed for one answer to this question. Only a third of respondents declare participating in training with the main objective of increasing their employment chances. In contrast, 29% seek a job that better matches their preferences and 11% wish to enter a new career. Another 10% want to get a more stable job. Only 1% say that they train to get reemployed no matter the job. As shown in Figure 3-6, this pattern holds across almost all training sectors. Taken together, these responses are consistent with the fact that only 15% of the respondents used the reemployment rate posted by the center as a selection criterion.

3.4.3 Possible explanations

I explore possible explanations of these results. As this is a standard implicit assumption in models of provider selection (Crépon et al. 2018), I check that job seekers' decision-making process is sequential. That is, these models assume that job seekers first choose a program that they are interested in (e.g., 2-year training program to work as a carpenter) and then select a provider that proposes the program. I find that the majority of respondents indeed report pursuing such a two-step procedure. Only 22% declare that they choose a program after selecting a training center.

Next, I investigate whether the reported low efforts to gather information about centers can be explained by limited supply (Lovenheim and Walsh 2018). Training centers are not equally distributed across geographic locations and it is possible that most prospective trainees who have settled on a given program do not have more than one option within reach. As far as I know, there is no available study on the geographic distribution of training centers in France.[24] Hence, I asked respondents to report the number of training centers that proposed the program they were interested in and that they considered to be accessible. Even though it is endogenous to people's level of effort in collecting information, this measure nevertheless provides a lower bound of the real supply and therefore can only lead me to overestimate the importance of limited supply on information-seeking. I find that among respondents who declare that there are two such centers, as many as 44% of them do not compare centers before choosing the one they will go to. Respondents

[24] My reading of the literature is that there is no such study in other countries either. Besides, to be informative about the experience of prospective trainees and reveal the relevant choice sets they face, such maps should be done program by program.

could also choose as a selection criteria that the center they went to was the only one offering the program they were interested in. Only 30% of the respondents selected this criterion (Figure 3-5).

Lastly, I undergo some systematic heterogeneity analysis across age, gender, education, sectors, and funding streams. As explained in Section 3.2, funding may be allocated on an individual basis or on a group basis, coming mainly from administrative regions or regional offices of the public employment services. Group programs correspond to sectors that the public administrations consider to have high labor demand and wish to prioritize. Providers are selected once every two to four years through public auctions so that slots are negotiated in advance. Trainees who want to enroll in such programs are virtually certain to obtain funding within the limited set of selected providers. Conversely, individual grant can be allocated on a case-by-case basis by the local agency when the job seeker wishes to enroll in a different program.

While respondents' answers do not seem to differ much across socioeconomic characteristics, we find marked differences of behavior across funding streams and some differences across sectors. Trainees in group programs funded by the public employment services appear to spend the lowest effort in investigating training providers. This may be partly due to the limited number of providers selected during public auctions and eligible for group programs. Figures 3-7 and 3-8 show that they rely more on caseworkers to select a center: 69% of them directly followed the recommendation of their caseworker, compared with 25% of trainees that had individual funding. They are also less likely to cross-check the information with other sources and 30 percentage points less likely to compare several centers before choosing. This is only partly explained by a more limited choice set of centers, as 40% of respondents in Pôle emploi group programs report that there was only one center available for the program they wanted. This is 20 percentage points higher than for trainees in individual programs.

3.5 Conclusion and discussion

This paper presents the results of an online survey conducted in April 2018 among job seekers who participated in a training program. The survey sheds light on the decision-making process by which job seekers choose a training provider. Understanding this process is essential to the design of effective policies that can leverage demand forces to put competitive pressure on providers and increase quality. My main finding is that job seekers put little effort into collecting information to choose a provider. They rarely visit or compare multiple centers before selecting one, and do not cross-check information from multiple sources. This behavior does not seem to be due to limited available supply, but is visibly correlated with the type of funding stream people enroll in. Additionally, they mainly value logistical aspects, namely the distance from the center to their home and the timing of the program, and do not exclusively prioritize reemployment prospects. Although descriptive, this study nevertheless provides useful evidence to start looking into the "black box" of training systems. So far, the existing literature on vocational training had given little attention to the supply side of the system, although it is known in other contexts (notably primary and secondary schooling) that providers are a key component of performance. Even in the realm of policy and administrative reports, virtually nothing is known about training providers and about the market structure of the supply. In France, data constraints add to the intrinsic complexity of the training system that involves many different stakeholders and multiple heterogeneous providers.

The results of this work raise doubts regarding the effectiveness of stand-alone demand-driven policies to improve the quality of public-sponsored systems. Although such policies are increasingly promoted in France, my findings may be considered as a warning from a quick implementation of important institutional changes without preliminary improvements of the informational environment of job seekers. A promising avenue for future research would be to further explore data on providers from the unemployment records, possibly merging them with employment and firm-level data. A simple objective could be to map the operating providers by training sector to understand the accessibility constraints that job seekers currently face in each training sector.

The study also points at concrete actions that job seekers could be encouraged to undertake to spur competition between providers. Such actions include visiting centers before enrolling, cross-checking the information from providers' private websites or chatting with former trainees and potential future employers about the quality of the program. In the absence of reliable quantitative indicators of provider reemployment performance, these recommendations seem to be low hanging fruits to raise job seekers' awareness about training quality and to put pressure on low performing providers.

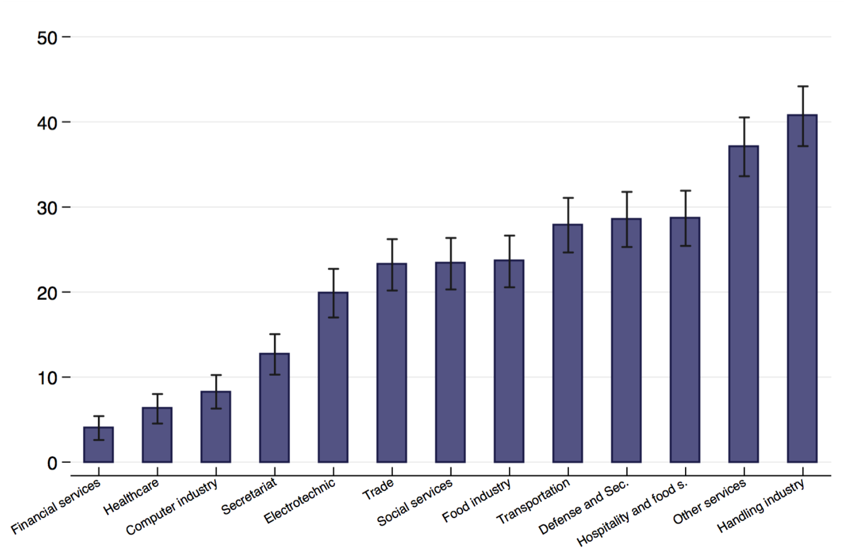
Lastly, the study suggests that job seekers might benefit from more hands-on support in making training decisions. Barr and Turner (2018) and Bergman et al. (2019) provide two recent examples in the contexts of postsecondary education and of housing decisions in the United States where well-coordinated and customized assistance significantly increases the impact of information intervention and voucher distribution to increase participation in and benefits from government programs.

Such support may be particularly important for job seekers suffering from loneliness and demotivation. As this survey underlines the pivotal role played by caseworkers in the diffusion of information, it should motivate researchers and policymakers alike to design interventions that include or directly target these key intermediaries.

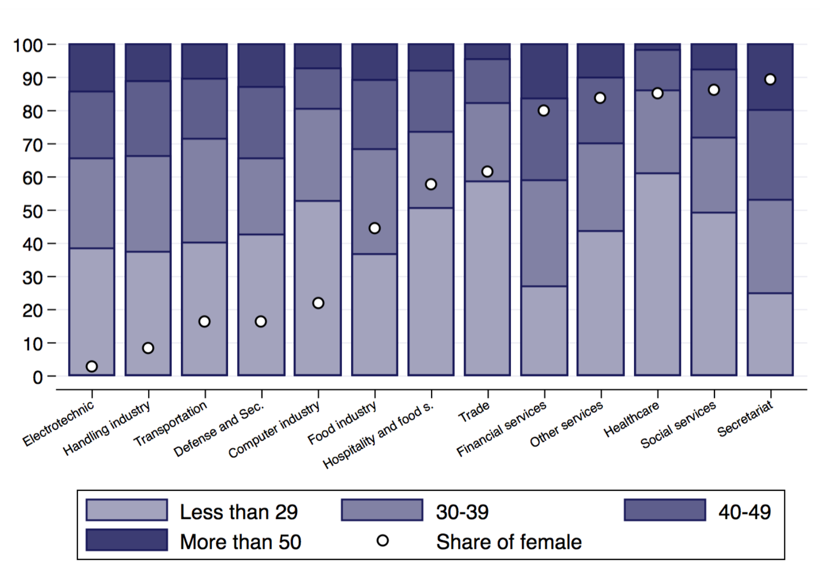
3.6 Figures

Figure 3-1: Distribution of characteristics across training sectors (whole sample)

(a) Share of low-education trainees



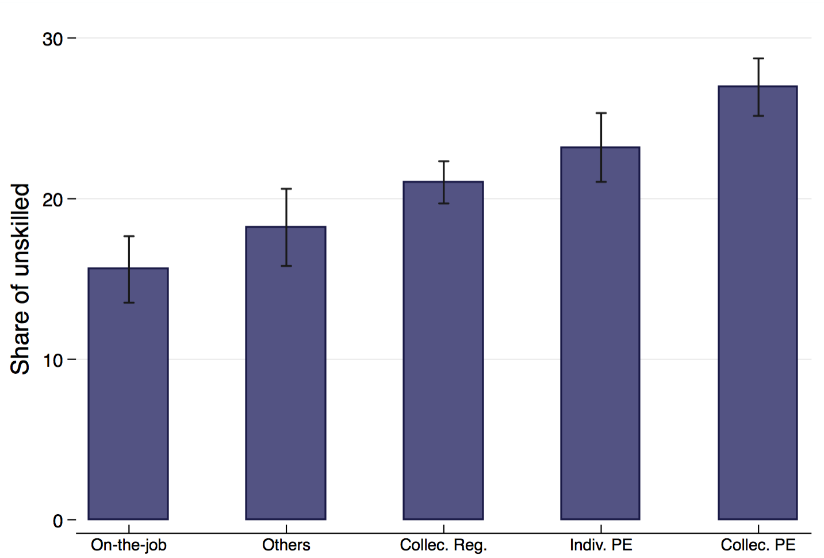
(b) Age categories and share of female



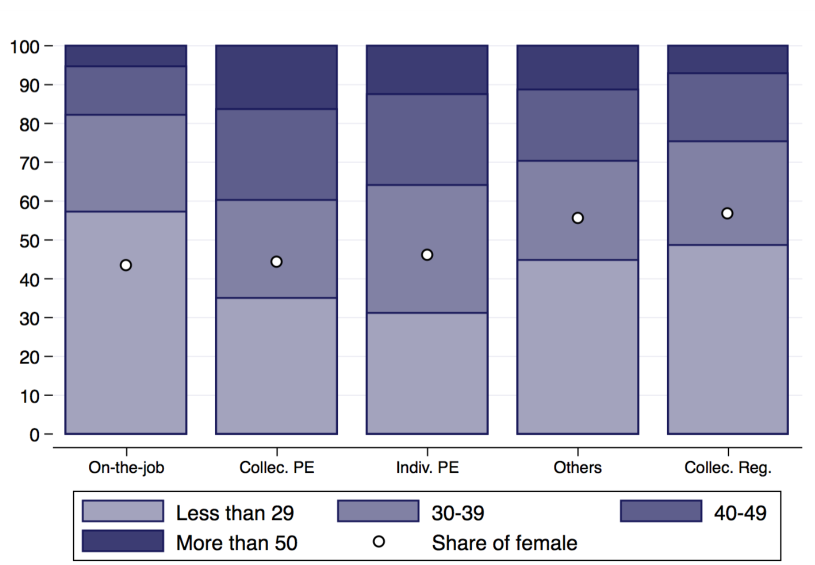
Notes: Figure (a) shows the percentage of low-education trainees in each sector, with standard deviations on top of each bar. Low-education is defined by grouping education levels of less than a high school diploma or a vocational degree. Figure (b) shows the percentages of age groups in each sector, along with the fraction of female represented with dots. The sample contains all individuals that the survey was sent to and the sample size is 9 752.

Figure 3-2: Distribution of characteristics across funding streams (whole sample)

(a) Share of low-education trainees



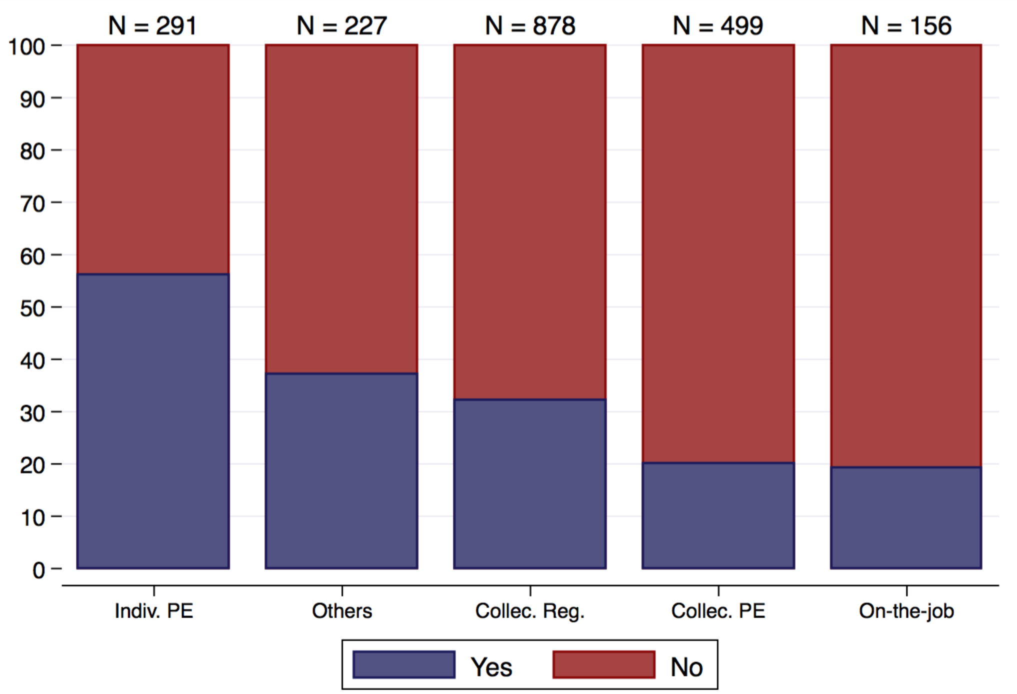
(b) Age categories and share of female



Notes: Figure (a) shows the percentage of low-education trainees in each funding stream, with standard deviations on top of each bar. On-the-job corresponds to on-the-job training programs in firms; Collec. Reg. corresponds to collective programs paid by the regions. Indiv. PE corresponds to individual grants attributed by the Public Employment Service and Collec. PE corresponds to group programs paid by the Public Employment Service. Low-education is defined by grouping education levels of less than a high school diploma or a vocational degree. Figure (b) shows the percentages of age groups in each funding stream, along with the fraction of female represented with dots. The sample contains all individuals that the survey was sent to and the sample size is 9 752.

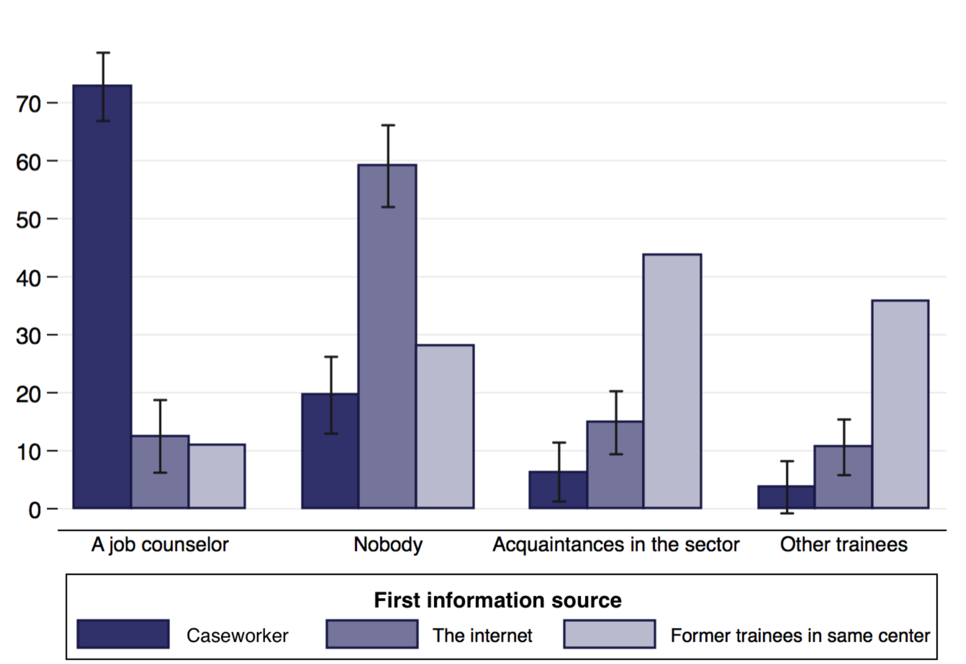
3.6.1 Survey results

Figure 3-3: Fraction who compared centers before choosing, by training stream



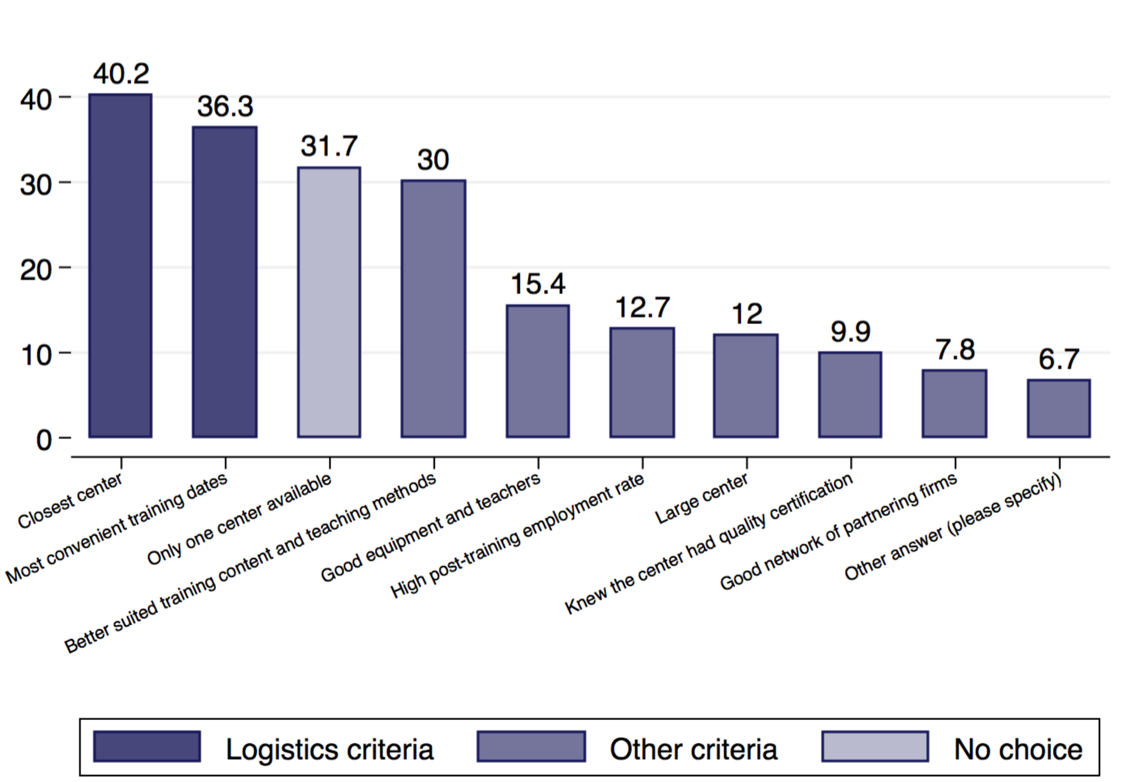
Notes: This figure shows the fraction of respondents who declared that they compared several centers before choosing the one they went to, in each funding stream. On-the-job corresponds to on-the-job training programs in firms; Collec. Reg. corresponds to collective programs paid by the regions. Indiv. PE corresponds to individual grants attributed by the Public Employment Service and Collec. PE corresponds to group programs paid by the Public Employment Service. The sample size is 2 073.

Figure 3-4: First information source, by information sources used



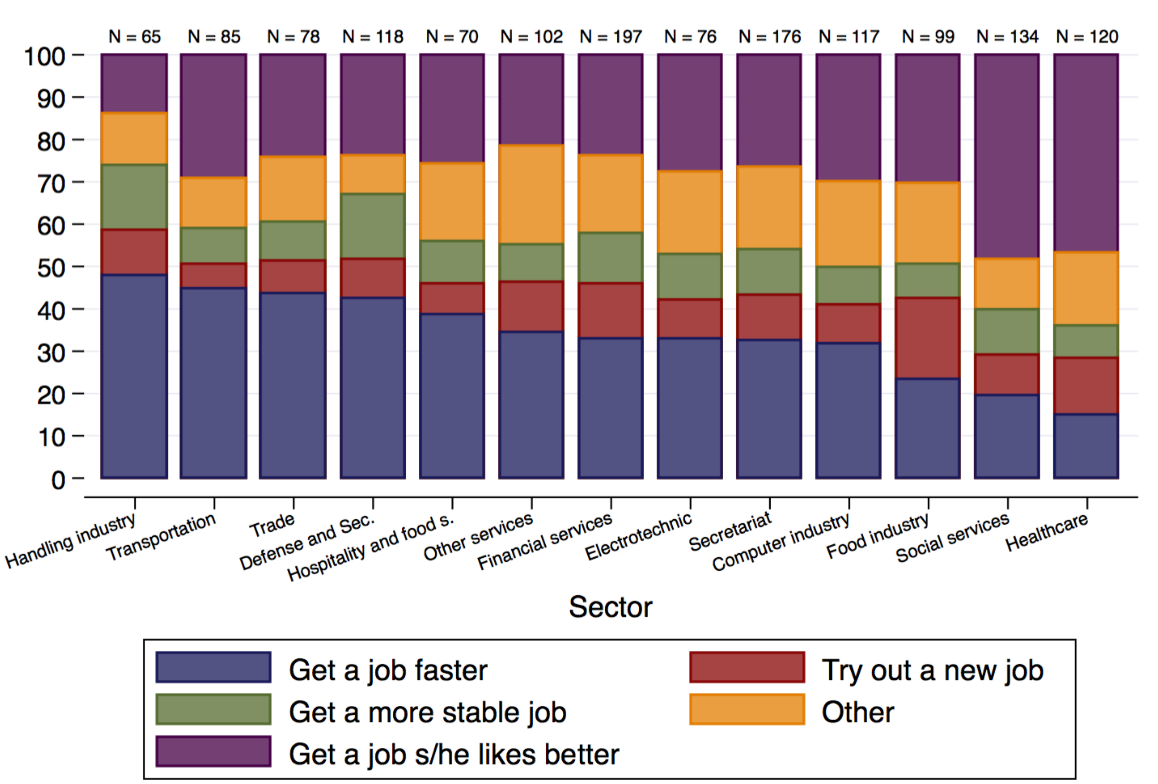
Notes: This figure shows respondents' first information source, that is, from whom or where they first heard about their center, by information sources used. For each information source used, we indicate the confidence interval of the difference with the reference group of respondents who heard about their center from former trainees. The sample size is 1 303.

Figure 3-5: Selection criteria



Notes: This figure shows the criteria respondents used to select their centers. The question allowed for multiple answers, and I use dark blue bars to show the two criteria related to the logistics of the program. The sample size is 1 437.

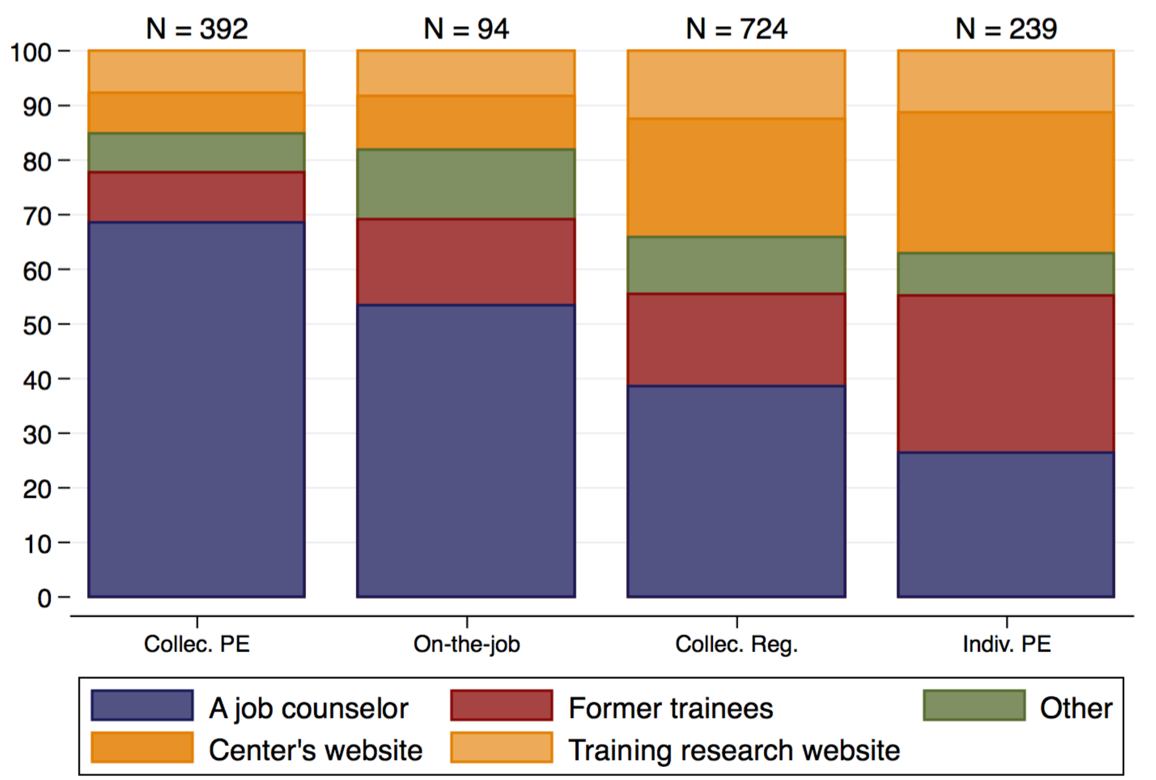
Figure 3-6: Main objective with the training, by training sector



Notes: This figure shows respondents' main objective with the training, by sector. The sample size is 1 437.

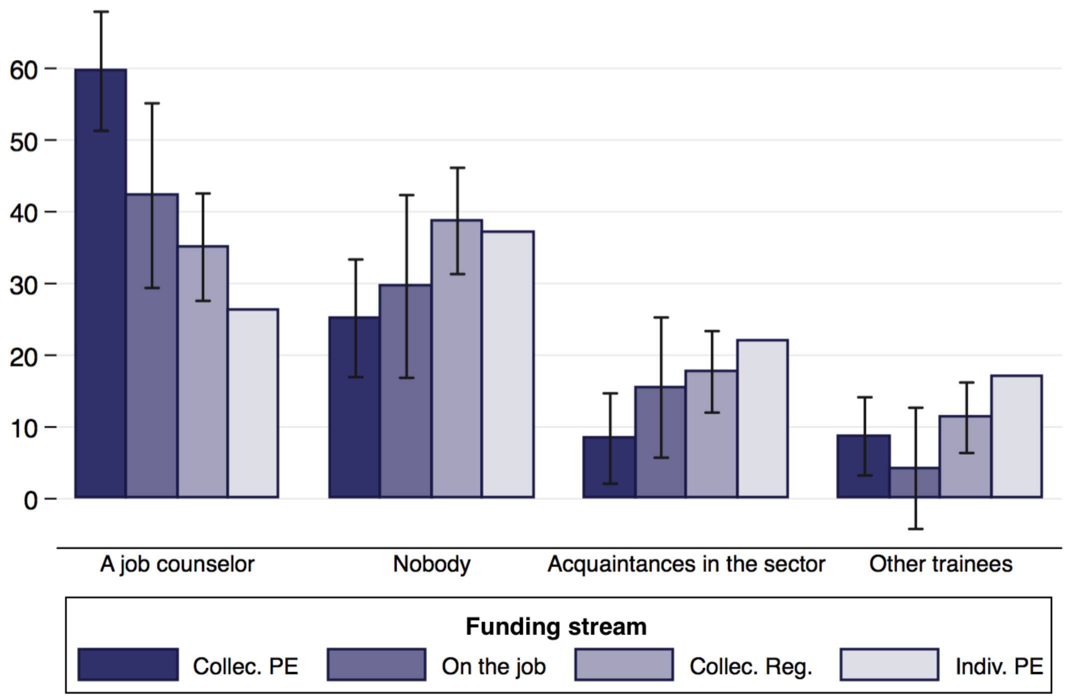
3.6.2 Heterogeneity by funding stream

Figure 3-7: First information source, by funding stream



Notes: This figure shows respondents' first information source, that is, from whom or where they first heard about their center, by funding stream. On-the-job corresponds to on-the-job training programs in firms; Collec. Reg. corresponds to collective programs paid by the regions. Indiv. PE corresponds to individual grants attributed by the Public Employment Service and Collec. PE corresponds to group programs paid by the Public Employment Service. The sample size is 1 449

Figure 3-8: Information sources used, by funding stream



Notes: This figure shows the different information sources used by respondents, by funding stream. On-the-job corresponds to on-the-job training programs in firms; Collec. Reg. corresponds to collective programs paid by the regions. Indiv. PE corresponds to individual grants attributed by the Public Employment Service and Collec. PE corresponds to group programs paid by the Public Employment Service. On top of each bar is indicated the standard error of the difference with the individual funding stream for each information source separately. The sample size is 1 266.

3.7 Tables

Table 3.1: Balance table comparing respondents with non-respondents

	Mean non-respondents	Diff (no control)	Diff (sector FE)	Mean respondents
Panel 1 : Whole sample (N = 9750)				
Age	**33.0**	3.4***	3.1***	**36.4**
Female	**48%**	10.2***	4.1***	**58%**
Low education	**22%**	-2.2**	0.1	**20%**
Collec. Reg. training	**37%**	6.1***	4.7***	**43%**
Collec. PE training	**24%**	-0.1	0.5	**24%**
Indiv. PE training	**16%**	-1.2	-0.9	**14%**
Ongoing training	**16%**	6.1***	4.7***	**22%**
Training duration (hours)	**727.2**	88.2***	43.5**	**815.5**
Panel 2 : Only respondents				
Past work experience in the training sector (N = 1408)				
No experience				**60%**
Less than 3 years				**21%**
More than 3 years				**19%**
Employment status at the time of the survey (N = 1277)				
Employed				**36%**
Still looking for a job				**33%**
Has not begun to search				**32%**

Notes: This table describes the sample of respondents. Panel 1 in the table shows the distribution of characteristics among non-respondents and respondents (in columns 1 and 4, respectively). Column 2 shows the difference between both means for each variable, while column 3 shows the same difference controlling for training sector. ***, **, and * indicate significance at 1, 5, and 10% respectively. On-the-job corresponds to on-the-job training programs in firms; Collec. Reg. corresponds to collective programs paid by the regions. Indiv. PE corresponds to individual grants attributed by the Public Employment Service and Collec. PE corresponds to group programs paid by the Public Employment Service. Low-education is defined by grouping education levels of less than a high school diploma or a vocational degree. Panel 2 shows the distribution for additional variables that are only available for respondents.

3.8 Appendix

3.8.1 Survey questionnaire

[q1] **Did you compare training centers before choosing your center?** (*Avez-vous comparé votre centre de formation à d'autres centres avant de le choisir ?*)

- Yes
- No
- Other answer (please specify) (*Autre (précisez)*)

[q2] **Did you visit training centers before choosing your center?** (*Avez-vous visité des centres de formation avant de choisir votre centre?*)

- Yes, I visited several centers.
- Yes, I visited only the center I chose.
- No.
- Other answer (please specify)

[q3] **Have you chosen your training before choosing your training center?** (*Aviez-vous choisi votre formation avant de choisir votre centre ?*)

- Yes
- No
- Other answer (please specify)

[q4] **How many other centers were accessible for the same training?** (*Combien d'autres centres étaient accessibles pour la même formation?*)

- 0

– 1

– 2

– 3

– 4

– 5 or more

– I do not know

[q5, only one answer allowed] **How did you find your training center?** (*Comment avez-vous connu votre centre de formation ?*)

– Directly on the center's website (*Directement par le site internet du centre*)

– On the website La Bonne Formation (*Par le site La Bonne Formation*)

– On the website Trouvemaformation (*Par le site Trouvermaformation*)

– On another trainings research website (*Par un autre site de recherche de formations*)

– You knew someone who had already been to this center (*Vous connaissiez quelqu'un qui était déjà allé dans ce centre*)

– Your job counselor have talked yo you about it (*Votre conseiller Pôle emploi vous en a parlé*)

– Other answer (please specify)

[q6] **Have you compared training centers according to the post-training employment rate of their trainees?** (*Avez-vous comparé les centres de formation selon les pourcentages de retour à l'emploi de leurs stagiaires ?*)

– Yes

– No

– Other answer (please specify)

[q7, multiple answers allowed] **You chose this training center because ...? (Multiple answers are allowed, please select all those applying to your situation)** (*Vous avez choisi ce centre parce que.... Plusieurs réponses possibles :sélectionnez toutes celles qui s'appliquent dans votre cas.*)

– It was the closest center to your home. (*C'était le centre le plus proche de chez vous*)

– It was the only center proposing this training (*C'était le seul centre qui proposait cette formation*)

– It had the most convenient training dates (*Les dates de la formation vous convenaient le mieux*)

– It was a center with a very high post-training employment rate (*C'était un centre avec un très bon taux de retour à l'emploi*)

– The center had a quality certification that you knew (*Le centre avait un label de qualité que vous connaissiez*)

– It was a large center training a lot of trainees (*C'était un grand centre qui forme beaucoup de stagiaires*)

– The center had a good network of partnering firms (*Le centre avait un très bon réseau d'entreprises*)

– The center had good technical equipments and a good team of teachers (*Le centre avait d'importants équipements techniques et une très bonne équipe de formateurs*)

– The center had better suited training content and teaching methods (*Le contenu de la formation et la pédagogie du centre vous convenaient le mieux*)

– Other answer (please specify)

[q8, multiple answers allowed] **Who gave you information to choose your training center?** (*Qui vous a donné des informations que vous avez utilisées pour faire votre choix de centre ?*)

- Acquaintances working in the sector (*Des connaissances qui travaillent dans le même secteur professionnel*)
- Other trainees who went to the same center (*D'autres stagiaires qui sont allés dans le même centre*)
- Other trainees who left their opinion on the internet (*D'autres stagiaires qui ont laissé des avis sur internet*)
- A job counselor (*Votre conseiller Pôle emploi*)
- Employers in the sector (*Des recruteurs et employeurs dans le même secteur professionnel*)
- Nobody (*Personne*)
- Other answer (please specify)

[q9] **According to you, what is the share of trainees who have found a job 6 months after having followed the same training as you, in the same training center? Please choose a percentage from 0 to 100. If you do not know or do not want to answer, you can choose the answer 0%.** (*Selon vous, quelle est la part de stagiaires qui ont retrouvé un emploi 6 mois après avoir suivi la même formation que vous, dans le même centre de formation que vous ? Choisissez un pourcentage entre 0 et 100. Si vous ne savez pas ou préférez ne pas répondre, choisissez la réponse 0%.*)

[q10, only one answer allowed] **To choose a training center, the post-training employment rate is ...?** (*Pour choisir un centre de formation, le pourcentage de retour à l'emploi des stagiaires est...?*)

- A useful and available piece of information (*Une information utile et disponible*)

- A useful but currently unavailable piece of information (*Une information utile mais indisponible actuellement*)
- A piece of information to be taken with caution carefully, but which could potentially influence your choices (*Une information à prendre avec précaution mais qui influencerait vos choix*)
- A useless piece of information (*Une information inutile*)
- Other answer (please specify)

[q11] **Had you already worked in the same sector as your training?** (*Aviez-vous déjà travaillé dans le même secteur professionnel que votre formation ?*)

- Yes, more than 3 years
- Yes, less than 3 years
- No
- Other answer (please specify)

[q12, only one answer allowed] **Your main objective to do this training was to ...?** (*Votre objectif principal pour faire une formation était de ... ?*)

- To get a job faster (*Retrouver un emploi plus rapidement*)
- To get a job that you like better (*Retrouver un emploi qui vous plaise*)
- To try out a new job (*Découvrir un nouveau métier*)
- To get a job closer to your home (*Retrouver un emploi plus proche de chez vous*)
- To get a more stable job (*Retrouver un emploi stable*)
- To get a job no matter the job (*Retrouver un emploi quel qu'il soit*)

- To earn a better living (*Gagner un meilleur salaire*)
- To regain self-confidence (*Reprendre confiance en vous*)
- Other answer (please specify)

[q13, only one answer allowed] **What is your current situation?** (*Quel a été votre parcours depuis la fin de votre formation ?*)

- Your are still in training (*Vous êtes encore en formation*)
- You have found a job (*Vous avez retrouvé un emploi*)
- You have had one or several job interviews but have not found any job yet (*Vous avez eu un ou plusieurs entretiens d'embauche mais pas encore d'emploi*)
- You have applied for jobs but have not had any job interviews yet found a job (*Vous avez envoyé des candidatures mais n'avez pas eu d'entretiens*)
- You have not found any adapted job offer (*Vous n'avez pas trouvé d'offres adaptées pour envoyer votre candidature*)
- You have not done any job search (*Vous n'avez pas effectué de recherche d'emploi*)
- Other answer (please specify)

3.8.2 Glossary

Funding streams:

- Collective training program funded by Pôle Emploi (in French, *AFC - Action de formation conventionnée*): programs proposed by Pôle Emploi to job seekers targeting sectors that are considered to face a high labor demand with recurring hiring needs. Providers are generally selected through public auctions for periods of several years. Slots for the programs are

- Collective training program funded by administrative regions: similar to Pôle Emploi collective programs, these programs target specific sectors and occupations in high demand, and providers are selected through public auctions by French administrative regions.

- Individual training program funded by *Pôle Emploi* (in French, *AIF - Aide Individuelle à la formation*): individual grants allocated by Pôle Emploi on a case-by-case basis. Eligible job seekers must demonstrate that the program fits their career plans and that it is not covered in any collective stream.

- On-the-job training (in French, *AFPR - Action de formation préalable au retour à l'emploi*, or *POEI - Préparation opérationnelle à l'embauche individuelle*): programs funded by Pôle Emploi that allow job seekers to work and get trained in private firms. Such programs are often a first step towards a stable job in the firm.

French translation of training sectors:

- Computer industry: *Informatique*
- Defense and Security: *Défense Prévention Sécurité*
- Electrotechnic engineering: *Electrotechnique*
- Financial services: *Gestion financière*
- Food industry: *Agroalimentaire*
- Handling industry: *Manutention*
- Healthcare: *Santé Secteur Sanitaire*
- Hospitality and food services: *Hôtellerie Restauration*
- Other services: *Services Divers*
- Secretariat: *Secrétariat Assistanat*

– Social services: *Action sociale*

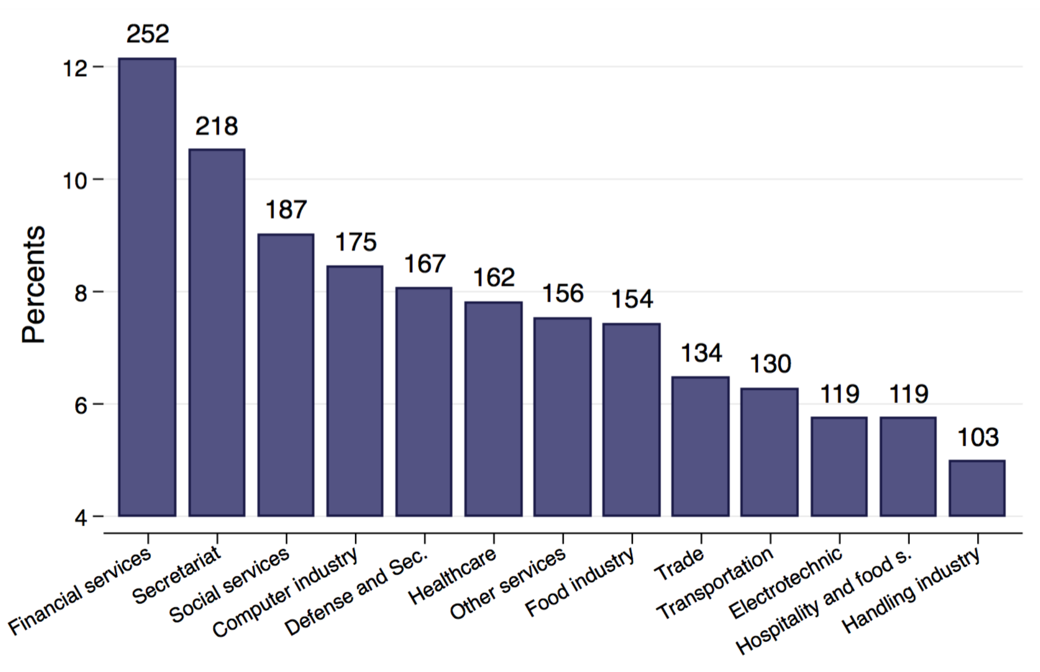
– Trade: *Commerce*

– Transportation: *Transport*

3.8.3 Appendix figures

Description of respondents

Figure 3-9: Distribution of training sectors among respondents



Notes: This figure shows the number of individuals who responded to at least one question of the survey in each sector. The total number of respondents is 2 073 .

3.8.4 Appendix tables

Table 3.2: Balance table of differences between respondents and non-respondents within sectors

	Handling	Elec.	Hosp.	Transp.	Trade	Food	Others	Health	Def.	Comput.	Social	Secret.	Financial
Response rate	13.7%	15.9%	15.9%	17.3%	17.9%	20.5%	20.8%	21.6%	22.3%	23.3%	24.9%	29.1%	33.6%
Differences													
Age	3***	5***	4***	4***	3***	2**	4***	4***	3***	2***	3***	2*	3***
Female	8% ***	2%	-2%	-4%	15% ***	4%	-1%	6% **	-0%	10% ***	0%	3%	10% ***
Low education	-4%	5%	5%	2%	4%	1%	2%	3%	-1%	-5% **	-5%	-4%	1%
Collec. Reg.	3%	-4%	15% ***	4%	7%	9% **	2%	-0%	8% *	5%	1%	12% ***	-0%
Collec. PE	8%	10% **	-1%	4%	8% **	-8% **	1%	-1%	-2%	-3%	3%	-3%	-2%
Indiv. PE	-13% ***	-0%	-5% *	0%	1%	-0%	2%	4%	-2%	-0%	-0%	-1%	-1%
Ongoing training	-1%	4%	9% ***	5% **	2%	14% ***	6% **	5%	0%	3%	1%	8% ***	3%
Training duration	38	49	155***	6	51	140**	25	9	-4	60	-3	53	19

Notes: This table shows the mean differences between respondents and non-respondents within each training sector. Sectors are ordered by response rate, which is indicated in the first row of the table. As in table 3.1, the coefficients in other rows correspond to differences between the means of each variable for respondents and non-respondents. ***, **, and * indicate significance at 1, 5, and 10% respectively. The total sample size is 9 750, with 750 observations in each sector.

References

Abdulkadiroğlu, A., P. A. Pathak, and C. R. Walters (2018). Free to choose: Can school choice reduce student achievement? *American Economic Journal: Applied Economics 10*(1), 175–206.

Abel, M., R. Burger, E. Carranza, and P. Piraino (2019). Bridging the intention-behavior gap? the effect of plan-making prompts on job search and employment. *American Economic Journal: Applied Economics 11*(2), 284–301.

Acemoglu, D. and P. Restrepo (2020). Robots and jobs: Evidence from us labor markets. *Journal of Political Economy 128*(6), 2188–2244.

Adomavicius, G. and A. Tuzhilin (2005). Toward the next generation of recommender systems: A survey of the state-of-the-art and possible extensions. *Knowledge and Data Engineering, IEEE Transactions on 17*, 734–749.

Altmann, S., A. Falk, S. Jäger, and F. Zimmermann (2018). Learning about job search: A field experiment with job seekers in germany. *Journal of Public Economics 164*, 33–49.

Andersson, F., H. J. Holzer, J. I. Lane, D. Rosenblum, and J. A. Smith (2016). Does Federally-Funded Job Training Work? Non-experimental Estimates of WIA Training Impacts Using Longitudinal Data on Workers and Firms.

Andrabi, T., J. Das, and A. I. Khwaja (2017). Report cards: The impact of providing school and child test scores on educational markets. *American Economic Review 107*(6), 1535–63.

Autor, D. (2009). Studies of labor market intermediation : Introduction to "studies of labor market intermediation". *Studies of Labor Market Intermediation*.

Autor, D. H., D. Dorn, G. H. Hanson, and J. Song (2014). Trade Adjustment: Worker-Level Evidence *. *The Quarterly Journal of Economics 129*(4), 1799–1860.

Ba, B. A., J. C. Ham, R. J. LaLonde, and X. Li (2017). Estimating (Easily Interpreted) Dynamic Training Effects from Experimental Data. *Journal of Labor Economics 35*(S1), 149–200.

Babcock, L., W. J. Congdon, L. F. Katz, and S. Mullainathan (2012). Notes on behavioral economics and labor market policy. *IZA Journal of Labor Policy 1*(1), 1–14.

Banerjee, A. V., R. Banerji, E. Duflo, R. Glennerster, and S. Khemani (2010). Pitfalls of participatory programs: Evidence from a randomized evaluation in education in india. *American Economic Journal: Economic Policy 2*(1), 1–30.

Barnow, B. S. and J. Smith (2015). Employment and training programs. (21659).

Barr, A. and S. Turner (2018). A letter and encouragement: Does information increase postsecondary enrollment of ui recipients? *American Economic Journal: Economic Policy 10*(3), 42–68.

Beaman, L. and J. Magruder (2012). Who gets the job referral? evidence from a social networks experiment. *American Economic Review 102*(7), 3574–93.

Behaghel, L., B. Crépon, and M. Gurgand (2014). Private and public provision of counseling to job seekers: Evidence from a large controlled experiment. *American economic journal: applied economics 6*(4), 142–74.

Belot, M., P. Kircher, and P. Muller (2019). Providing advice to jobseekers at low cost: An experimental study on online advice. *The review of economic studies 86*(4), 1411–1447.

Berger, M. C., D. Black, and J. A. Smith (2001). Evaluating profiling as a means of allocating government services. In M. Lechner and F. Pfeiffer (Eds.), *Econometric Evaluation of Labour Market Policies*, Heidelberg, pp. 59–84. Physica-Verlag HD.

Bergman, P., R. Chetty, S. DeLuca, N. Hendren, L. F. Katz, and C. Palmer (2019). Creating moves to opportunity: Experimental evidence on barriers to neighborhood choice. Working Paper 26164, National Bureau of Economic Research.

Bertrand, M., E. Shafir, and S. Mullainathan (2004). A behavioral economics view of poverty.

Bleemer, Z. and B. Zafar (2018). Intended college attendance: Evidence from an experiment on college returns and costs. *Journal of Public Economics 157*(C), 184–211.

Blundell, R., M. C. Dias, C. Meghir, and J. V. Reenen (2004). Evaluating the Employment Impact of a Mandatory Job Search Program. *Journal of the European Economic Association 2*(4), 569–606.

Caliendo, M., D. Cobb-Clark, and A. Uhlendorff (2015). Locus of control and job search strategies. *The Review of Economics and Statistics 97*(1), 88–103.

Card, D., J. Kluve, and A. Weber (2018). What works? a meta analysis of recent active labor market program evaluations. *Journal of the European Economic Association 16*(3), 894–931.

Carrell, S. and B. Sacerdote (2017). Why do college-going interventions work? *American Economic Journal: Applied Economics 9*(3), 124–51.

Castleman, B. and L. C. Page (2015). Summer nudging: Can personalized text messages and peer mentor outreach increase college going among low-income high school graduates? *Journal of Economic Behavior Organization 115*(C), 144–160.

Conlon, J. (2018). Major malfunction: A field experiment correcting undergraduates' beliefs about salaries.

Crépon, B., M. Ferracci, G. Jolivet, and G. J. van den Berg (2018). Information shocks and the empirical evaluation of training programs during unemployment spells. *Journal of Applied Econometrics 33*(4), 594–616.

Della Vigna, S. and M. D. Paserman (2005). Job search and impatience. *Journal of Labor Economics 23*(3), 527–588.

DellaVigna, S., J. Heining, J. F. Schmieder, and S. Trenkle (2020). Evidence on job search models from a survey of unemployed workers in germany. (27037).

Dinerstein, M., L. Einav, J. Levin, and N. Sundaresan (2018). Consumer price search and platform design in internet commerce. *American Economic Review 108*(7), 1820–59.

Dizon-Ross, R. (2019). Parents' beliefs about their children's academic ability: Implications for educational investments. *American Economic Review 109*(8), 2728–65.

Dynarski, S. and J. Scott-Clayton (2008). Complexity and targeting in federal student aid: A quantitative analysis.

Falk, A., D. B. Huffman, and U. Sunde (2006). Self-Confidence and Search. Technical report.

Finkelstein, A. and M. Notowidigdo (2018). Take-up and targeting: Experimental evidence from snap. (11558).

Gee, L. K. (2019). The more you know: Information effects on job application rates in a large field experiment. *Management Science 65*(5), 2077–2094.

Hastings, J. S. and J. M. Weinstein (2008). Information, school choice, and academic achievement: Evidence from two experiments. *The Quarterly Journal of Economics 123*(4), 1373–1414.

Herz, B. and T. van Rens (2019). Accounting for Mismatch Unemployment. *Journal of the European Economic Association*.

Hipp, L. and M. Warner (2008). Market forces for the unemployed? training vouchers in germany and the usa. *Social Policy Administration 42*, 77 – 101.

Horton, J. J. (2017). The effects of algorithmic labor market recommendations: Evidence from a field experiment. *Journal of Labor Economics 35*(2), 345–385.

Hoxby, C. and S. Turner (2013). Expanding College Opportunities for High-Achieving, Low Income Students.

Hyman, B. (2018). Can displaced labor be retrained? evidence from quasi-random assignment to trade adjustment assistance. *Working paper*.

Jacobson, L. and J. Davis (2017). The relative returns to workforce investment act–supported training in florida by field, gender, and education and ways to improve trainees' choices. *Journal of Labor Economics 35*(S1), S337–S375.

Kambourov, G., I. Manovskii, and M. Plesca (2020). Occupational mobility and the returns to training. *Canadian Journal of Economics/Revue canadienne d'économique 53*(1), 174–211.

Krueger, A. B., A. Mueller, S. J. Davis, and A. Şahin (2011). Job search, emotional well-being, and job finding in a period of mass unemployment: Evidence from high frequency longitudinal data [with comments and discussion]. *Brookings Papers on Economic Activity*, 1–81.

Krueger, A. B. and A. I. Mueller (2012). Time use, emotional well-being, and unemployment: Evidence from longitudinal data. *American Economic Review 102*(3), 594–99.

Krug, G. and G. Stephan (2013). Is the contracting-out of intensive placement services more effective than provision by the pes? evidence from a randomized field experiment. (7403).

Kuhn, P. and H. Mansour (2014). Is internet job search still ineffective? *The Economic Journal 124*(581), 1213–1233.

Kuhn, P. and M. Skuterud (2004). Internet job search and unemployment durations. *American Economic Review 94*(1), 218–232.

LaLonde, R. J. (1986). Evaluating the econometric evaluations of training programs with experimental data. *The American Economic Review 76*(4), 604–620.

Lovenheim, M. F. and P. Walsh (2018). Does choice increase information? Evidence from online school search behavior. *Economics of Education Review 62*(C), 91–103.

Marinescu, I. and R. Rathelot (2018). Mismatch unemployment and the geography of job search. *American Economic Journal: Macroeconomics 10*(3), 42–70.

McCall, B., J. Smith, and C. Wunsch (2016). *Government-Sponsored Vocational Education for Adults*, Volume 5 of *Handbook of the Economics of Education*.

McGee, A. D. (2015). How the perception of control influences unemployed job search. *ILR Review 68*(1), 184–211.

McKee-Ryan, F., Z. Song, C. R. Wanberg, and A. J. Kinicki (2005). Psychological and physical well-being during unemployment: a meta-analytic study. *Journal of applied psychology 90*(1), 53.

Menezes-Filho, N. A. and M.-A. Muendler (2011). Labor Reallocation in Response to Trade Reform.

Mizala, A. and M. Urquiola (2013). School markets: The impact of information approximating schools' effectiveness. *Journal of Development Economics 103*, 313 – 335.

Papageorgiou, T. (2014). Learning Your Comparative Advantages. *The Review of Economic Studies 81*(3), 1263–1295.

Patterson, C., A. Şahin, G. Topa, and G. Violante (2016). Working hard in the wrong place: A mismatch-based explanation to the uk productivity puzzle.

Plank, D. N. and G. Sykes (2003). Choosing choice: School choice in international perspective.

Resnick, P. and H. R. Varian (1997). Recommender systems. *Commun. ACM 40*, 56–58.

Ridley, M., G. Rao, F. Schilbach, and V. Patel (2020). Poverty, depression, and anxiety: Causal evidence and mechanisms. *Working paper*.

Rinne, U., A. Uhlendorff, and Z. Zhao (2008). Vouchers and caseworkers in public training programs: Evidence from the hartz reform in germany. *Institute for the Study of Labor (IZA), IZA Discussion Papers*.

Schilbach, F., H. Schofield, and S. Mullainathan (2016). The psychological lives of the poor. *American Economic Review Papers and Proceedings 106*(5), 435–40.

Skandalis, D. (2019). Breaking news: The role of information in job search and matching. *Working paper*.

Spinnewijn, J. (2015). Unemployed but optimistic: Optimal insurance design with biased beliefs. *Journal of the European Economic Association, Vol. 13, No 1, pp. 130-167*.

Varian, H. R. (2010). Computer mediated transactions. *American Economic Review 100*(2), 1–10.

Walker, W. R. (2013). The Transitional Costs of Sectoral Reallocation: Evidence From the Clean Air Act and the Workforce. *The Quarterly Journal of Economics 128*(4), 1787–1835.

Walters, C. R. (2018). The demand for effective charter schools. *Journal of Political Economy 126*(6), 2179–2223.

Şahin, A., J. Song, G. Topa, and G. L. Violante (2014). Mismatch unemployment. *American Economic Review 104*(11), 3529–64.