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

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Abstract

Despite massive and increasing public spending in training for the unemployed, 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. This 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. 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 the effects on callbacks are driven by increasing salience of basic information about training rather than by belief updating. Overall, this suggests that public services need to invest in improving relevant knowledge among job seekers rather than relying exclusively on digital communication to modify behavior.

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.

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 in France. 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* (Agence nationale pour la formation professionnelle des adultes), 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. 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 a basic version of the email, allowing us to measure the impact of receiving an email on enrollment. This email contained the list of programs offered in the region, with hyperlinks directed to the webpage with full program information for each training program. The email also included a phone number to call the training center for additional information and to enroll. 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*, additional sentences

[1] See Annexe au Projet de Loi de Finances (2018) (Appendix of the 2018 Draft Budget Bill).

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.[2] 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.[3] 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. 14% expect 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, 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 70% compared to the level in the *Basic email group*, and the effect is significant at the 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.

[2]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.

[3]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.

Contrary to the results on callback rates, enrollment six months after the experiment in both our listed programs or any public-sponsored program was not affected by the intervention. 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.

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 observable characteristics 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.[4]

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.

Related literature. 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 (2017), 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. (2017); Barnow and Smith (2015)).[5] 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

[4]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.

[5]This is one of the important take-aways of Card et al. (2017) 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.

their employment chances and take-home pay.

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 (2017). Our paper starts to fill this gap. Barr and Turner (2017) find 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 a policy perspective, it is equally important to know whether such information interventions can be successfully implemented at scale and with low costs. Public services increasingly rely on digital communication tools, which 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 have access to online information. Online messages might also not be as convincing as a discussion with another individual. Several papers underline the need to combine online messages with offline assistance (Castleman and Page (2015); Carrell and Sacerdote (2013)). 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 computations suggest that these interventions are a cost-effective policy. However, 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.

Finally our study relates to recent work on behavioral obstacles in job search (DellaVigna 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, notably in the context of training enrollment. Insofar as people are not perfectly rational, barriers in terms of financing cost or administrative hassles 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.

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.

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. [6] These three players jointly account for more than 80% of the nearly 5 billion euros going annually to fund training to the unemployed.[7] Thus they play a crucial role in the type of programs and sectors where

[6] Firms and other third-party institutions called "OPCA" (Organisme Paritaire Collecteur Agréé) are in charge of training for employed individuals and only play a minor role in job seekers' training.

[7] 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 See Annexe au Projet de Loi de Finances (2018) (Appendix of the 2018 Draft Budget Bill).

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 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.[8] Job seekers are formally not allowed to complement the maximum stipend they can get with their own money to pay for a program. 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.[9] 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 and that she is committed enough to pursue training until the end.

If these conditions are met, caseworkers often help job seekers to look for available and financially supported programs within their sector of interest, prioritizing group programs that are subsidized by regions or *Pôle emploi*.

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

[8] See Annexe au Projet de Loi de Finances (2018)

[9] 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.

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. Given the absence of standardized and easily understandable systems 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.

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.

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.

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.

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.[10] 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.

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. For these reasons, 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.

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

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.

3. Design

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.[11] In each region, a list of 5 to 7 programs were offered, which added up to 24 programs advertised in total. Overall, these are certifying training programs, with an average duration of 6 months, aimed at making persons involved in those programs directly operational for low and medium-skilled occupations. 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 Appendix 9.1, Figure 1(a). 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.[12] 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.

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.

[11]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.

[12]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.

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 reminded job seekers that training participation was fully subsidized and that they could be entitled to benefits. This is shown in Appendix 9.1, Figure 1(b). 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 sent 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 Appendix 9.1, Figure 1(c). 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 Appendix 9.1, Figure 1(d). It illustrates an example for a program for a job of a network administrator. As 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 Appendix 9.1, Figure 1(e). 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.

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.

3.4. Experimental design and randomization

As can be seen in Figure 2, 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 Appendix 9.1, Figure 1(a). 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, 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.

4. Data and sample description

4.1. Data

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

4.1.1. 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.

4.1.2. 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. 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.[13]

4.1.3. Training enrollment

One of the key variables 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.

4.1.4. Unemployment records

Finally, we use administrative data from *Pôle emploi*, as it provide individual socio-demographic characteristics such as age, gender, education level and family situation.

It also gather information on job seekers' past work history : this includes the professional category in their last job, and the number of months of experience in their professional sector of interest.

Administrative data give information about job seekers' current unemployment spells as well, such as the duration of unemployment, or the targeted jobs, which is a key variable for the experiment. Professional

[13] 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.

sectors are fairly narrow and correspond to 5-digit US occupational categories. They 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.

In addition, unemployment records contain information about the job seeker's *assistance track*, which corresponds to the level of assistance they need from the caseworker. It ranges from "low", which is the least intensive track concerning people who are familiar with digital tools and who are fairly autonomous in their job search, to "high" for job seekers with very little autonomy in their job search, who might not be comfortable using a computer.

Finally we have access to variables capturing job seekers' preferences, such as the type of contract the jobseekers is looking for, e.g. part-time work or short-term contracts.[14].

We use these administrative data to both sample job seekers as reported in subsection 3.3, and to describe the final sample, as these characteristics might be correlated with the impact of the intervention.

4.2. Sample description and balance checks

Table 2 provides key summary statistics of our sample and provides balances checks to assess whether the randomization was successful. Column 1 of Table 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 2 shows that only 56.4% 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

[14]Variables capturing job seekers' preferences 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 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.

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.

Figure 3 gives details on the conversion rate of our emails into calls (*i.e* the rate of phone calls after receiving emails) through the study of clicks rates. It indicates that among job seekers who opened our emails, only 22.35% clicked on at least one training link, and only 2.63% of clickers called the training center. However, when considering people who called the training center, 65% of them had also clicked on one of the training link, which suggests that overall, training in the type of programs we promoted was an option that the callers were at least considering.

Table 3 shows that the randomization was successful at balancing groups along most individual 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.

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 10. 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.[15]
- 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 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.

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

- 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*.[16] As can be seen in Panel I of Table 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.[17] Interestingly, they seem to be selected along the same variables as people who opened the email (see Panel II of Table 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 4(a) shows significant misinformation regarding direct training costs. About 45% of respondents to question 1 think training is not fully subsidized by *Pôle emploi*: almost 20% think that a 6-month training program "offered by *Pôle emploi*" will cost them up to 500 euros while another 20% 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.

The right panel of figure 4(a) 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, roughly 30% think their benefits will decrease and nearly 10% think they will entirely lose their benefits.

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 4(b) shows that up to 14% choose the latter option. Finally, Figures 4(c) and 4(d) illustrate respondents' answers to questions 4 to 7 estimating people's expectations about training returns, which show a more complicated picture.[18] 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.

[16] 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.

[17] As explained in section 4.1.4, 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.

[18] 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.

5. Results

5.1. Methodology

To estimate the impact of the intervention, we run several regressions. Equation (1) estimates the effect of receiving an email compared with no email:

$$Y_i = \beta E_i + \gamma' X_i + r_i + \epsilon_i \tag{1}$$

where Y_i is the outcome dummy (e.g. callback or enrollment) and E_i is a dummy for having received any email.[19] The coefficient of interest is β. In other words, equation (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.[20] 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 (2). It is similar to (1) but we add a dummy M_i for having received any additional message in the email:

$$Y_i = \beta_1 E_i + \beta_2 M_i + \gamma' X_i + r_i + \epsilon_i \tag{2}$$

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 (3) below:

$$Y_i = \beta^{cost} T_i^{cost} + \beta^{simp} T_i^{simp} + \beta^{ret} T_i^{ret} + \beta^{all} T_i^{all} + \gamma' X_i + r_i + \epsilon_i \tag{3}$$

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 of the coefficients of covariates, which reduced statistical power.

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

[20] 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.

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 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.[21]

Table 4 shows the impact of the intervention on callback rates, using the regression specifications outlined above. Column (1) displays the results of regression (1). The mean in the *Control group* is virtually zero and confirms that all callbacks came from people who had received an email. 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.[22] Messages on registration simplicity increase callback by 70%: 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.[23]

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

[21] As explained in section 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.

[22] 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). 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 Figure 1(a). 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.

[23] 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.

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.

5.3. 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.[24] It is hard to know whether this bad performance is due to usual low turnout for the programs advertised in the experiment, or to the timing of the campaign, 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 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.

To further investigate the reasons for this null effect, we turn to Panel IV of Table 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 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.

Finally, as our interventions 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

[24] 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, 71 job seekers had enrolled out of an initial sample of 37 000 email recipients.

variable that is meant to indicate whether job seekers who participated in a training enrolled in an *Afpa* center. 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 6. We see no robust impact of any treatment on this outcome.

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.[25] 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.

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 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 7. 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:

$$Y_i = \beta_1 Z_i + \beta_2 T_i + \beta_3 T_i \times Z_i + \gamma_1' X_i + \gamma_2' X_i \times Z_i + r_i^1 + r_i^2 \times Z_i + \epsilon_i \tag{4}$$

[25] 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.

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 4 and look at coefficients on interactions between Z_i and four treatment dummies:[26]

$$Y_i = \beta_1 Z_i + \sum_j \beta_2^j T_i^j + \sum_j \beta_3^j T_i^j \times Z_i + \gamma_1' X_i + \gamma_2' X_i \times Z_i + r_i^1 + r_i^2 \times Z_i + \epsilon_i \tag{5}$$

The results presented in Table 8 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 8. 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 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 8, 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 (4) and (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 and formal institutions like *Pôle emploi*.[27] Results from these regressions are remarkably consistent and strong. Columns (1) and (2) of Table 9 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

[26] Equation 4 has twice more coefficients to estimate than equation 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 11.1 and 11.1 of appendix section 11. We find very similar results are to those with region fixed effects.

[27] 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.

with digital communication.

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 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. Finally, this study 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.

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8. Tables

Table 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 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 high school nor vocational 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. 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 3: Balance table

	(1) Control (C)		(2) Basic	(3) Cost	(4) Simplicity	(5) Returns	(6) All info
	Mean	[S.D.]	C = Basic	C = Cost	C = Simp	C = Ret	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	$1.373^{\star}$	0.600	0.373	0.304
Formal training in desired job (%)	66.08	[47.35]	-0.46	-0.624	-0.758	$-1.769^{\star\star\star}$	-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	$1.769^{\star}$	0.961	$1.084^{\star}$	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	$-1.025^{\star\star}$
Education level							
No high school nor vocational degree (%)	12.18	[32.71]	$1.17^{\star\star\star}$	-0.354	$0.729^{\star}$	$1.370^{\star\star\star}$	0.458
Vocational degree (%)	44.94	[49.75]	$-1.63^{\star\star}$	-0.710	-0.719	$-1.132^{\star}$	-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	$2.147^{\star\star}$	0.055	0.431	$0.943^{\star}$
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 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). 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 (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 (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 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 additionnal 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 4.

Table 6: 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 additionnal 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 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 7: 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 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 2.

Table 8: 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 additionnal message			.355***	.338***				
			(.0436)	(.0442)				
Received basic email and additionnal 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 (5). Columns are structured in a similar fashion as table 4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 4.

Table 9: 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 additionnal message			.448***	.429***				
			(.0375)	(.0381)				
Received basic email and additionnal 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 (5). Columns are structured in a similar fashion as table 4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 4.

9. Figures

9.1. Email types for each treatment group

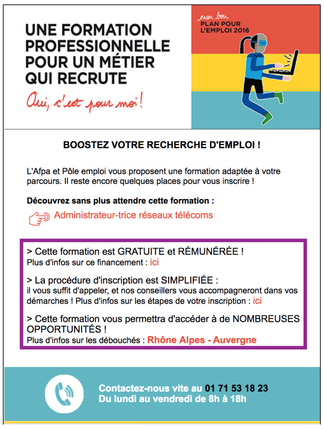
Figure 1: Email types for each treatment group

(a) Basic email

(b) Cost email

(c) Simplicity email

(d) Returns email

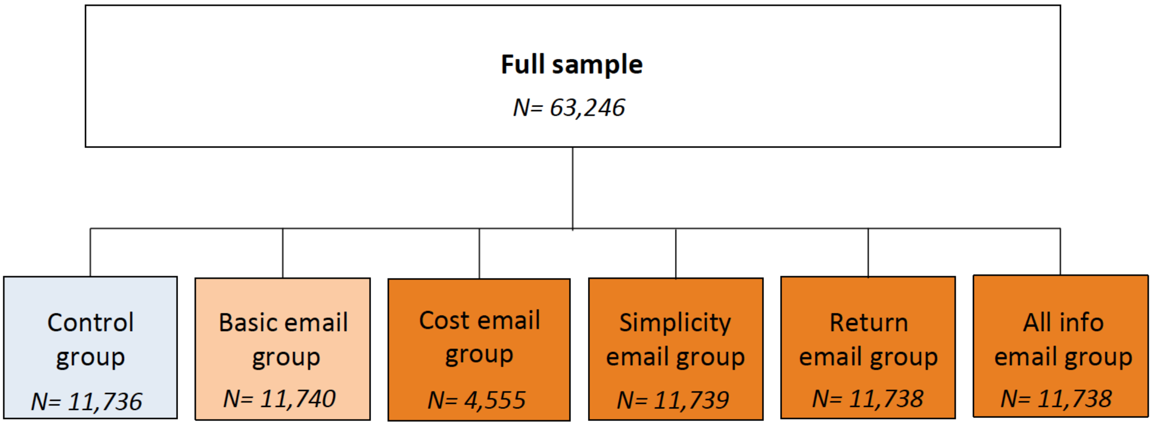


(e) All info email

Notes: This figure shows the five emails that have been sent to jobseekers belonging to our treatment groups. Figure 1(a) displays 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.
Figures 1(b) to 1(e) shows the four emails in the additional message groups. The sections that differ from the basic email are in purple boxes:

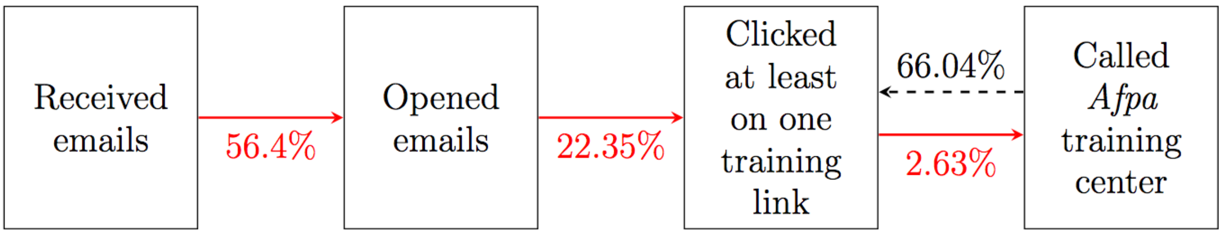
- The email on cost (Figure 1(b)) adds one sentence saying "This training is FREE and SUBSIDIZED! More info on funding options: here."
- The email on simplicity (Figure 1(c)) adds one sentence saying "The registration procedure is SIMPLIFIED : you just need to call and our caseworkers will help you throughout the process ! More info on the steps towards enrollment : here."
- Figure 1(d) is an example of the returns email sent to 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 email sent to the *All info email group* adds all these additional messages (see as an example figure 1(e)).

Figure 2: Randomization design



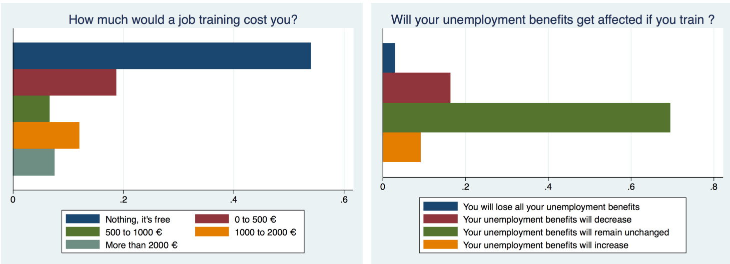
Notes: This figure illustrates the randomization design of the experiment, with the different treatment arms and their corresponding sampl

Figure 3: Conversion rate of our emails into calls

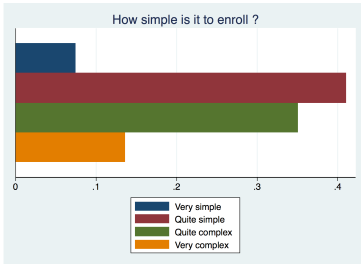


Notes: This figure illustrates the conversion rate of our emails into calls. Starting from the left, it appears that among job seekers who received our emails, 56.40% opened our emails. Out of these 56.40%, 22.35% clicked on at least one of the hyperlink redirecting to the associated training page. The last figure in red indicates that among those who clicked, only 2.63% decided to call the training center. Symmetrically, the percentage in black, above the dashed arrow, indicates that among those who called, 66.04% clicked on at least one training link.

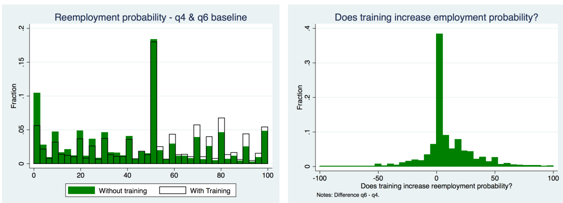
Figure 4: Answers to baseline questions



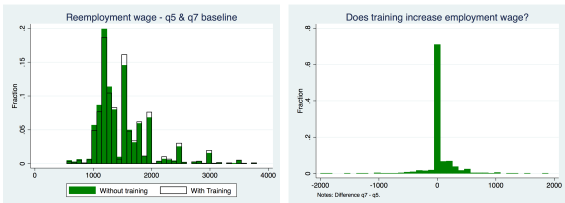
(a) Answers on training cost



(b) Answers on enrollment simplicity



(c) Reemployment wages with and without training



(d) Expected reemployment likelihoods with and without training

Notes:

- This figure shows the distribution of answers to our baseline survey about training programs promoted by *Pôle emploi*. Figure 4(a) displays the distribution of answers to the first two questions about the cost of these programs. 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 4(b) shows the distribution of answers to the third question about the registration process. The horizontal axis on these two figures 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 4(c) presents the distribution of wage expectations with and without training. The first histogram reports the answers of both questions 4 and 6, 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.
- Figure 4(d) shows the distribution of reemployment expectations with and without training. The first histogram reports the answers of both questions 5 and 7, 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.

Appendix

10. 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.)*

11. Heterogeneous effects without region fixed effects

Table 11.1: 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 additionnal message			.36***	.346***				
			(.0437)	(.0447)				
Received basic email and additionnal 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 4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 4.

Table 11.2: 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 additionnal message			.463***	.447***				
			(.0381)	(.0389)				
Received basic email and additionnal 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 4. All variables are interacted with the high education dummy. See more detailed explanations in the footnotes of table 4.