Take-up of Social Benefits: Experimental Evidence from France

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Abstract

We report on two nationwide experiments with job seekers in France. We first show that a meeting with social services to assess eligibility and help with application to social benefits increased new benefit take-up by 31 %. By contrast, an online simulator that gave personalized information on benefit eligibility had no effect on take-up. Marginal treatment effects from the first experiment show that individuals who benefit the most from the meetings are the least likely to attend. Overall, our results suggest that transaction costs deter eligible people from applying to benefits and from accessing government's assistance to help them apply.

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1 Introduction

The efficacy of social policies throughout the world is limited by low benefit take-up. An estimated 25% of people eligible to the Earned Income Tax Credit (EITC), the largest federal means-tested cash transfer in the United States, do not claim it (Plueger, 2005). In France, the setting of our study, 36% of eligibles do not claim the out-of-work minimum income (Revenu de Solidarité Active socle), a figure that goes up to 68% for the in-work component (Domingo and Pucci, 2012).[1]

A key question is why poor households would forgo additional resources offered to them. Currie (2004) identifies three possible reasons. The first is the lack of information: potential claimants may be unaware of their eligibility to social benefits. The second is transaction costs, which are due to complex and lengthy application procedures. The third is welfare stigma, due to the conflict between eligibles' self-image and their negative perception of benefit claimants (Moffitt, 1983). These different explanations for non-take-up imply different policy responses: information or anti-stigma campaigns, or application assistance.

A natural follow-up question is whether alleviating barriers to benefit take-up will bring in poorer or richer benefit claimants, i.e. improve or worsen targeting. For example, complex application procedures may screen out households who need the benefits the least (Nichols and Zeckhauser, 1982; Kleven and Kopczuk, 2011; Alatas et al., 2016). Or they may discourage less educated, financially stressed households who need the most support (Deshpande and Li, 2019; Finkelstein and Notowidigdo, 2019; Linos et al., 2020). The specific design of interventions that facilitate benefit take-up, e.g. online or in-person, may also change their impact on targeting.

In this paper, we investigate the barriers to take up of 15 different social benefits using two nationwide experiments with job seekers in France.[2] In the first experiment, we show that job seekers who attended a meeting with social workers to inform them about their eligibility and help them apply to social benefits were 31% more likely to take up any new benefit. The effects are driven by family, housing and income benefits, for which social workers could directly help them with their application, and among them by benefits that have more complex application forms. By contrast,

[1]The recent review by Ko and Moffitt (2022) provides more international estimates: French take-up levels are well within the range of high-income countries.

[2]The benefits include family and housing related transfers, in- and out-of-work income supplements, and subsidized health insurance.

in a second experiment, job seekers learned about their eligibility from an online simulator: this treatment had a much smaller and statistically insignificant effect on take-up. These results suggest that the lack of information about one's own eligibility to benefits is not the main barrier to the take-up of social benefits. Instead, the evidence points towards the importance of transaction costs. Finally, we estimate Marginal Treatment Effects (Heckman, 2010), and find that the effectiveness of the meeting is higher for job seekers with a low propensity to attend it. This suggests that transaction costs which prevent job seekers from taking up social benefits also prevent them from accessing assistance.

In the first experiment, we evaluate the impact of a meeting with social workers (Rendez-vous des droits élargis, RDVDE). Within a target population of 60,000 job seekers with small or no claims to unemployment benefits, a randomly selected half was invited to a meeting with social services to discuss their potential eligibility to 15 different benefits and support them in starting the application process. Using a combination of administrative data sources, we evaluate the effect of the intervention on benefit applications and take-up. To shed some light on barriers to take-up, we leverage additional random variation in the type of meeting (face-to-face or phone) and in the content of the letter (anti-stigma or information flyer).

The policy was well implemented: 21% of job seekers who were invited had a meeting with social services, thus reaching the official target of 6,000 RDVDE. It was also successful at inducing benefit take up : the treatment group was 7% more likely to have received any new benefit six months after the intervention (the probability of taking up new benefits was 22% in the control). These effects were primarily driven by income support benefits (in- and out-of-work), for which take-up increased by 10%, and by family and housing benefits, whose take-up rose by 5% (the probability of taking up these benefits was 15 and 11% in the control). When we estimate the effect of the meeting instrumented by the invitation, the LATE is very large, +46% for income benefits and +23% for family and housing after six months.

The RDVDE is a bundled intervention, which provides both information and application assistance, and may even have alleviated welfare stigma, although this was not its explicit aim. The evaluation findings, however, point to the importance of application costs. Indeed, the intervention was effective for benefits administered by the social services who offered the meeting and who could follow up immediately with the application: family, housing, and income support benefits. By contrast, we find no effect on health benefits, which required an application with other social services. This suggests that the RDVDE's main role is to lower application costs: if the main effect of the RDVDE was through better information or lower stigma, one would expect it to increase health benefit take-up as well.[3] Within the group of family, housing and income benefits, we further show that the meetings had a stronger positive effect on take up of benefits whose application forms were more complex. Using random variation in the invitation letters, we also show that the information and the stigma fliers had no additional positive effect on meeting attendance or benefit take-up. However, this last piece of evidence is not very strong, since the qualitative evaluation suggests that job seekers did not remember the flyers.

In order to study the role of pure information provision on benefit take-up, we ran another nationwide experiment one year prior to the RDVDE experiment, on a sample of 40,000 job seekers targeted in the same way. We sent job seekers an email with an information leaflet similar to the one in the main experiment, with a direct link to the simulator, but without an invitation to any meeting. This email generated interest among recipients, as 67% of the treatment group clicked on the link and 18% completed a simulation immediately. However, we find that the email only had small and insignificant effects on benefit take-up. This evidence suggests that the lack of information is not the main barrier to benefit take-up.

It could be, however, that job seekers hold on average accurate beliefs but are still misinformed, some under-estimating, some over-estimating their eligibility. Within the RDVDE experiment, we combine survey data on applications made and administrative data on benefit receipt to show that the success rate of applications, which is about 90% in the control, fell slightly in the treatment group. This suggests that the intervention did not improve the quality of applications, as one would expect from better informed applicants, with more accurate knowledge of their entitlement. Instead, the result is consistent with a lowering of application costs, which may have induced lower quality applications.

In the last part of the paper, we ask two questions: whether the meeting induced poorer or richer claimants to take up social benefits, and whether the meeting attracted the job seekers who needed assistance the most. To answer the first question,

[3]The qualitative evaluation of the RDVDE experiment (Alberola et al. (2018)) emphasizes the importance of meeting social workers, who help benefit claimants to fill the form and to identify the required documents. Personal contact is also useful to focus the attention of benefit claimants and to answer their questions directly, which reduces their cognitive load.

we use data on the amounts received by benefit claimants, which we obtained on a pre-defined 58% subsample of the experimental sample. We find that while the meeting increased coverage of income benefits in this subsample, it had a negative effect on the value of benefits received by the marginal claimant. Together with our finding that the success of application declined slightly, this result suggest that the meeting did not improve the targeting of social benefits, i.e. it did not help poorer people deterred by transaction costs to access social benefits.

All the experimental results we document are effects on compliers, i.e. job seekers who come to the meeting when they are invited. One may wonder whether the treatment effects would be lower or higher for never-takers, i.e people who do not come to the meeting. For this, we use the Marginal Treatment Effect framework developed by Heckman and Vytlacil (2005). The Marginal Treatment Effect is the effect of the meeting as a function of the (observed and unobserved) cost of going to the meeting. For identification, we combine experimental variation in the invitation to the meeting and individual characteristics at baseline (e.g. distance to social services). We find that the marginal treatment effect on benefit take-up is increasing, which means that people with a lower unobserved probability to come to the meeting have a higher benefit from attending. We also find that individuals with observed characteristics related to higher effects of the treatment also have a lower predicted probability to attend the meeting. These results suggest that people who face high transaction costs in applying to benefits also face high costs in joining meetings that could help them apply. The differences between never-takers and compliers are large: we estimate that attending the meeting increases the probability to take up a benefit by 21 points among never-takers, which represents about 90 euros per month of additional benefits, against 6 percentage points and only 6 euros per month for compliers.

Our paper contributes to the literature which evaluates interventions aimed at increasing benefit take-up (see Ko and Moffitt (2022) for a more extensive review).

Some authors have found positive effects of information provision: e.g. Duflo and Saez (2003) on retirement plan take-up, and Kling et al. (2012) on Medicare plan choice by senior citizens in the United States. Bhargava and Manoli (2015) test several barriers on take-up of the EITC by varying the design and presentation of the letter sent to each treatment group. Their study finds that take-up is most affected by the salience and simplicity with which information is presented, while efforts to attenuate stigma or reduce perceived transaction costs do not have any effect. However, Linos et al. (2020) find that similar nudges targeted at first-time tax filers have no effect on take-up, implying that "outreach to hard-to-reach populations will often need to include higher-touch interventions that simplify the underlying processes".

Several (mostly non-experimental) papers evaluate interventions that changed application costs. For instance, Kopczuk and Pop-Eleches (2007) find that the introduction of electronic filing programs increased take-up of the EITC, because it increased the presence of tax preparers who are paid by the poor to help them apply. Similarly, Aizer (2007) finds that a toll-free number to assist claimants of Medicaid increases enrollment, and Herd et al. (2013) show that a plan to reduce administrative burden to claim Medicaid in Wisconsin increased take-up by about 10%. More recently, Ganong and Liebman (2018) show that several state-level policies that made it easier to apply to SNAP did improve take-up, and Deshpande and Li (2019) find that social security office closures in the US reduce applications to disability benefits.

Our paper belongs to the few papers which test the effect of information provision and application assistance in the same context, using an experimental method. Bettinger et al. (2012) show that coupling improved information with personalized assistance in filling out applications leads to increased financial aid applications and receipt, while information provision alone has no effect on applications. Targeting a population of low income elderly who were not currently enrolled into the SNAP income support program, Finkelstein and Notowidigdo (2019) find that an information letter doubled the probability of take-up, while application assistance tripled it.[4] One obstacle to generalize findings from the literature is that each study focuses on one particular type of benefit within an often narrowly defined target population. One contribution of our paper is to exploit two nationwide experiments which test an information and an application assistance intervention on take up of 15 different social benefits (including family, health and income benefits), implying a wide range of eligibility rules and potential beneficiaries. Our findings suggest that in our context information provision alone does not affect take-up, and that application assistance is essential.

We also contribute to the ongoing discussion about the effects of application costs on targeting. On the one hand, Kleven and Kopczuk (2011) argue that the complexity of the application process to social benefits may be a necessary by-product of optimal

[4] In a middle-income country context Gupta (2017) shows that while information helps literate women take up old age pensions in India, mediation is needed for illiterate women.

targeting. Consistent with this view, the targeting of social benefits in Indonesia improves when social services which provide application support are located further away (Alatas et al., 2016). On the other hand, Tempelman and Houkes-Hommes (2016) show that take up of a Dutch health-care allowance by eligible individuals decreases with income, but is very low for the lowest incomes. Similarly, Deshpande and Li (2019) show that social security office closures in the US reduce applications among people with better claims to disability benefits, and Gupta (2017) argue that application costs for old age pensions in India screen out the most vulnerable women.

A key issue in empirical studies to date is that their conclusions regarding targeting are only true for people who comply with the treatment, i.e. who access application support. But the same barriers that prevent poor people from applying to benefits may prevent them from accessing application support. Finkelstein and Notowidigdo (2019) find that the marginal applicant to SNAP induced by an information and assistance intervention has lower claims than average (suggesting good targeting), but also argue that non-applicants have lower education and may have higher claims (suggesting poor targeting). Our contribution is to formalize and test this idea empirically using Marginal Treatment Effects. On the one hand, the intervention induces higher take-up but not higher benefits per recipient. On the other, job seekers who receive assistance are not the ones who would benefit the most from it.

The psychological literature interpret this type of findings from a cognitive perspective, as the result of low executive functioning (Christensen et al., 2020). People who suffer from financial distress or poor health have a lower ability to plan and prioritize, they lose the overview of the benefits of their actions, and are less resilient to obstacles. This type of mechanisms has also been highlighted by the behavioral economics literature (Mani et al., 2013; Mullainathan and Shafir, 2013). This would explain why the most deprived people may also be those for whom administrative burden is felt more strongly, thus accessing benefits less often in spite of higher needs.[5]

Finally, our paper contributes to the literature devoted to the use of digital solutions to improve the functioning of social programs. Recent studies show that the use of

[5]Dubois (2003) in his sociological analysis of social workers in France offers a different interpretation: he argues that social workers provide less assistance to benefit claimants who are more socially distant from them (and hence poorer). However, this interpretation does not seem in line with the observation made in the qualitative evaluation of the RDVDE experiment (Alberola et al. (2018)) which reports that social workers were eager to help the most deprived individuals access benefits, and were disappointed by the fact that the people most in need did not attend the meetings.

IT in beneficiary identification and payments can reduce corruption and improve targeting, but may increase exclusions errors or payment delays (Muralidharan et al., 2016; Banerjee et al., 2020; Muralidharan et al., 2020). In a similar vein, we show that in contrast to phone or in-person meetings with social workers, an online simulator that provides personalized information on benefit eligibility had no effect on benefit take-up. Our results suggest that digital solutions may not improve access to social programs if they are unable to significantly reduce transaction costs for vulnerable populations (job seekers in our case).

The remainder of the paper is structured as follows. Section 2 outlines our conceptual framework. Section 3 presents the context and the interventions. Section 4 describes the data and the empirical strategy for the evaluation. The main evaluation results are presented in section 5. Section 6 discusses the reasons for low take-up, and Section 7 explores the implications for targeting. Section 8 concludes.

2 Conceptual framework

Set-up. We consider a stylized model of benefit take-up. An individual is eligible to an amount $b \geq 0$ of social benefits. Benefit application is an action $A \in \{0, 1\}$ equal to one if the benefits perceived with some error, $b + \varepsilon$, exceed application costs c and social stigma s :

$$A = \mathbb{I}\{b + \varepsilon - c - s > 0\}$$

Following Currie (2004), individuals eligible to a positive amount of social benefits $b > 0$ may not apply for three reasons. First, they may lack information and underestimate their eligibility: $\varepsilon < 0$ (Finkelstein and Notowidigdo, 2019). Second, they may face transaction costs c, which correspond to the non-monetary costs of going through the application process, e.g. due to its complexity and delays (Kleven and Kopczuk, 2011). Third, there may be a psychological cost s due to the stigma attached to claiming social benefit (Moffitt, 1983). In this paper, we evaluate the impact of different policy interventions that reduce the cost of application, provide information on benefit eligibility, and attenuate the stigma attached to benefit take-up.

Information. We first consider the effect of providing information to potential benefit claimants about their eligibility. This has an ambiguous effect on benefit application. To simplify the analysis of information provision, we drop the stigma s

that is formally analog to application cost c. Each individual is characterized by the value of the cost c and the error ε they make in the perception of the benefits they are entitled to. We denote with $F(x) = P(b < x)$ the cumulative distribution function of benefits b given c and ε. The probability of applying to benefits is:

$$P(A = 1) = P(b > c - \varepsilon) = 1 - F(c - \varepsilon)$$

Providing full information on benefit eligibility has the following effect on take-up:

$$\Delta(c, \varepsilon) = P(b > c) - P(b > c - \varepsilon) = F(c - \varepsilon) - F(c)$$

The effect of the treatment is thus positive for agents who underestimate their eligibility ($\varepsilon < 0$) and negative otherwise. The population effect sums $\Delta(c, \varepsilon)$ over the joint distribution of (c, ε). Therefore, providing information increases take-up if, under imperfect information, there are more individuals who would benefit from applying but do not apply because they underestimate their eligibility than individuals whose net benefit is negative but who apply because they overestimate their eligibility. Given our empirical results, it is useful to consider the reasons for why providing information may have no or very small effects on applications. First, it could be that *on average* agents have accurate beliefs, even though many of them are wrong about their own eligibility. Second, it could be that the effect of erroneous beliefs is small as compared to that of transaction costs, such that for most people with $F(c-\varepsilon) = 1$ (no take-up), we also have $F(c) = 1$. Finally, it could simply be that ε is uniformly zero, i.e. that there are no information frictions.

To distinguish between these alternative explanations, we can look at the effect of information provision on the success rate of applications:

$$P(b > 0|b > c) - P(b > 0|b > c - \varepsilon) = 1 - P(b > 0|b > c - \varepsilon) \geq 0$$

If many agents are wrong about their eligibility, even if they are right on average in the population, then the effect of providing information on the success rate of applications should be positive. This is because better information on eligibility improves self-selection into applying.

Application costs. We next investigate the impact of reducing application costs. We define c_0, as the application cost one faces when supported by a caseworker, i.e. when one receives treatment; on their own (untreated), agents face a larger cost $c \in [c_0, \infty[$ that is distributed in the population.[6] To simplify, we assume perfect information ($\varepsilon = 0$). Then, the effect of this treatment for any agent of type c is:

$$\Delta(c) = P(b > c_0) - P(b > c) = F(c) - F(c_0) \geq 0$$

The population effect sums $\Delta(c)$ over the marginal distribution of c, and is positive. Note that in this case, the probability to apply and the probability to obtain a benefit are identical; the latter is more easily observable. The effect is stronger on agents that face higher transaction costs in the absence of treatment:

$$\frac{\partial \Delta(c)}{\partial c} = f(c) > 0$$

To understand the effect of application costs on the targeting of benefits, we also consider the treatment effect on the benefit received by the average claimant:

$$\Delta^b(c) = E(b|b > c_0) - E(b|b > c) = \frac{\int_{c_0}^{\infty} bf(b)db}{1 - F(c_0)} - \frac{\int_{c}^{\infty} bf(b)db}{1 - F(c)}$$

For any agent of type c this quantity is negative if $c > c_0$ and zero otherwise. Integrating over c yields:

$$\Delta^b = \int_0^{\infty} \Delta^b(c)g(c)dc$$

If b and c are uncorrelated in the population, then Δ^b is negative: lowering application costs extends benefit eligibility to people with lower claims. This corresponds to the idea that application costs are ordeals mechanisms that improve targeting (Kleven and Kopczuk, 2011; Alatas et al., 2016). If b and c are sufficiently positively correlated however, i.e. if poorer people with higher claims also have higher costs, then Δ^b can be positive. In that case lowering application costs can attract poorer claimants (Deshpande and Li, 2019; Gupta, 2017).

Compliance. In practice, the intervention is a meeting with social services, to which potentially eligible people are invited, but may not attend. In addition to the decision to apply to benefits, we need to model the decision to comply with the treatment, as we can only identify treatment impact on compliers. Let us assume that

[6] Again, given that stigma and costs enter in the same way in the decision to apply for benefits, the effect of reducing stigma can be discussed in the same terms.

agents decide to attend the meeting by comparing the anticipated value of attending $(b - c_0)$ with the cost of attending which we denote with $\kappa > 0$. An agent complies with the invitation if:

$$b > c_0 + \kappa$$

In our analysis, we are interested not only in the average treatment effect, but also in the shape of the marginal treatment effect as a function of the cost of participation κ, following Heckman and Vytlacil (2005) and Heckman (2010). We define the marginal treatment effect (MTE) as the effect of the treatment on the compliers, conditional on a value of the cost of meeting participation, κ, for any individual with application cost c. The MTE is:

$$\begin{aligned} MTE(\kappa; c) &= P(b > c_0 | b > c_0 + \kappa; c) - P(b > c | b > c_0 + \kappa; c) \\ &= 1 - \frac{1 - F(c)}{1 - F(c_0 + \kappa)} \end{aligned}$$

if $\kappa < c - c_0$ and zero otherwise. Conditional on c, we have:

$$\frac{\partial MTE(\kappa; c)}{\partial \kappa} \leq 0$$

As κ grows, compliers are more and more selected towards higher benefits b; they find value in attending the meeting because it reduces their administrative burden, but the share that would apply even in the absence of the treatment grows, so that the treatment effect decreases. This is true for a given c. We take the integral over c to obtain the MTE for the whole population:

$$MTE(\kappa) = \int_{c_0}^{\infty} MTE(\kappa; c) g(c|\kappa) dc$$

When κ increases, every $MTE(\kappa; c)$ in the integral decreases. If c and κ are uncorrelated, then $MTE(\kappa)$ will also be a decreasing function of κ. But if c and κ are positively correlated, the MTE can be increasing in κ, since the large $MTE(\kappa; c)$ with large c receive a higher weight $g(c|\kappa)$. Claimants who face large transaction costs and would benefit the most from the meeting also face large costs of attending it.

This model provides a framework for the empirical MTE estimation that we develop in this paper. Call Z the invitation to the meeting that has been randomized, and M a dummy for participation to the meeting. Our model can be translated into a single index model for participation:

$$M^* = (b - c_0) + \beta Z - \kappa$$

with $M = 1$ if $M^* > 0$ and β represents an exogenous change in the propensity to attend the meeting that is generated by the invitation (and by nature is not present in the model). As shown by Heckman and Vytlacil (2005), spanning the different values of $P(M = 1|Z) = p(Z)$ is akin to spanning the different values of the unobserved cost to attend the meeting, here interpreted as κ.[7] We observe benefit take-up: call it $Y \in (0, 1)$, with counterfactual notations Y_0 and Y_1. In terms of the model, $E(Y_0) = P(b > c)$ and $E(Y_1) = P(b > c_0)$. Then $MTE(\kappa)$ is simply $E(Y_1 - Y_0|\kappa)$, and it is identified by taking the derivative of $E(Y|p(Z))$ with respect to $p(Z)$. We discuss the details of the empirical strategy and identification in Section 4.

3 Context and intervention

3.1 Institutional setting

Social security in France is organized around a set of agencies, or *Caisses*, with each agency responsible for a specific occupation and/or benefit type. Income, family and housing benefits are managed by the *Caisse nationale d'allocations familiales* (CNAF), and health benefits are managed by the *Caisse nationale d'assurance maladie* (CNAM). Unemployment benefits for all job seekers are managed by *Pôle Emploi*, a separate state-controlled agency which also maintains a large job search platform and carries out various labour market policies. These multiple agencies were created at different points in history and kept their autonomy (Direction de la Sécurité sociale, 2017). Procedures, amounts and eligibility rules differ substantially across agencies, which makes benefit applications more complex.

In this paper we will consider in total 15 benefits, which for simplicity we pool into three groups. The first group, which we call **family benefits**, includes benefits that are given to parents or people with disabilities, but also housing benefits, which are managed by the CNAF. The second group, which we call **health benefits**, includes benefits that subsidize access to complete healthcare insurance coverage, and are

[7] Strictly speaking, if agents differ in b, as they do in the data, κ is implicitly understood as the cost normalized to the benefit value.

managed by the CNAM. The third group, which we call **income benefits**, covers benefits that directly support low-income individuals, whether in or out of work, and are managed by the CNAF.[8]

3.2 Intervention

The issue of low benefit take-up has been discussed in France since at least the late 1990s (Warin, 2012). It received more public attention in the early 2010s when studies suggested that less than 50% of individuals eligible for the Revenu de Solidarité Active (the main income benefit managed by the CNAF) actually claimed it (Domingo and Pucci, 2012). In 2013, increasing take-up became part of the mission of the CNAF, which committed to implement the "Rendez-vous des droits" (RDVDD) (Decobecq, 2013). Launched in 2014, the RDVDD is a one-hour meeting with a social worker offered to potential claimants to assess their eligibility to social benefits. The aim is to cover *all* nationally-available social benefits, and not only those administered by the CNAF. It focuses on individuals who are known to the CNAF, either because they are themselves benefit recipients or because they are part of a recipient's household. It targets among them those who were flagged because they have likely experienced a recent negative income shocks.[9] Another feature of the RDVDD is to blend the work of two types of social workers: "travailleurs sociaux", who are used to appraise claimants' overall situation and offer solutions outside the CNAF system, and "techniciens conseils", who are better versed in the details and eligibility rules of the CNAF benefits. The two types of workers were trained for the RDVDD to deliver comprehensive information on the 15 social benefits, including non-CNAF benefits.[10]

This paper evaluates a new version of the RDVDD, known officially as the *Rendez-vous des droits élargi* (RDVDE), that was experimented during the last trimester of 2017. As compared to the RDVDD, the RDVDE has a broader targeting scope. The trigger-based strategy is carried forward, but is no longer limited to CNAF beneficiaries. Instead, the target population are low-income individuals who have registered as unemployed at Pôle Emploi and many of whom are not present in the CNAF's databases. Two types of individuals are considered. *Population 1* have

[8] See Appendix Table B1 for more detail about each benefit.

[9] The flags are 1) applying for out-of-work benefits, 2) reporting unfit housing or rent arrears, 3) registering as a lone parent, 4) reporting a family separation, and 5) reporting a birth.

[10] Although we cannot estimate the causal effect of meeting one type of social worker or the other, descriptively there is no differential effect on take-up, which is consistent with this convergence effort.

registered as job seekers between one and three months before the randomization date, and they didn't have a recent previous unemployment spell.[11] *Population 2* are job seekers whose rights to unemployment benefits (Allocation de Retour à l'Emploi) is due to expire less than three months before the randomization date. We removed from the sample the most affluent individuals by excluding those who received an unemployment benefit amount above a certain threshold (see Appendix D). Because the two populations just experienced an income shock, they may have become eligible to benefits and might benefit from the RDVDD.[12]

The structure of the RDVDE is the following. The meeting lasts about an hour. At the start of the RDVDE, the agent goes through a series of basic questions on the household composition, occupational status and income of the potential claimant. A decision-tree algorithm helps social workers determine which social benefits should be discussed, but they are free to overrule the algorithm's recommendations. The qualitative evaluation of the RDVDE experiment (Alberola et al. (2018)) highlights that the algorithm was a central and distinguishing feature of the intervention, which social workers found especially useful to assess eligibility to benefits that are not provided by CNAF, although it was not used systematically in practice. During the meeting, the worker can consult information on benefit criteria stored on the CNAF Intranet, as well as on the Internet, and perform simulations using online benefit calculators. Once a potential for new benefits has been identified, the worker instructs the person on the process to claim those benefits, which is often cumbersome, and usually involves a different requirement and paperwork for every single benefit. When the benefit is provided by CNAF (i.e. all family and income benefits), the worker can start the application process on the spot.[13] This is not possible however for health benefits, which are provided by another agency. Finally, social workers are prompted by the Intranet tool to fill out a questionnaire about the meeting, and to report which benefit they have discussed with the potential claimant.

[11] Specifically, a previous spell, if any, had to be closed for more than three months before randomization. This condition removes individuals from industries (e.g. the arts) characterized by frequent short unemployment spells and who are likely to be well aware of their benefit eligibility.

[12] The targeting strategy was chosen based on exploratory small-scale trials conducted by the SGMAP (Secrétariat Général pour la Modernisation de l'Action Publique) and members of the research team, in partnership with two CNAF agencies and Pôle Emploi in 2013.

[13] The qualitative evidence gathered from interviews with both potential applicants and social workers suggests that both groups consider this to have been one of the most useful aspects of the meeting.

3.3 Experimental design

This study uses an encouragement design. Half of the job seekers from the two populations received letters by mail inviting them to the RDVDE. The allocation of the meetings was managed by the CNAF. There were two ways for potential claimants to arrange a meeting. The first was to call the landline number provided on the invitation letter, in which case they were transferred to the CNAF's call-centre department, which then allocated meeting slots matching the local CNAF agency's availabilities with the individual's time and location preferences. Less than 10% of the meetings in our sample were allocated in this way. Most meetings were agreed when the call-centre directly called potential claimants, which it did systematically in the treatment group: social workers were instructed to call each individual up to three times.

The content of the invitation letter was randomized: there were six treatment groups following a 3 x 2 cross-cutting design. Appendix C presents the invitation letters. In the first dimension, there were three types of letters.[14] The *neutral* letter (see Figure C5) explains the reason for the invitation, the purpose of the meeting, invites recipients to call to fix a meeting and informs them that they may also be called by a CNAF employee. The *information* letter (see Figure C6) consists of the same invitation, with a second page listing several common life events that lead to negative income shocks and the corresponding social benefits. It also includes a flyer (see Figure C7) which describes four fictitious households, with the types and amount of benefits they are eligible to, and invites readers to visit the online simulator `mes-aides.gouv.fr` to obtain a personalized estimate of their eligibility. The *anti-stigma* letter contains the same invitation as the neutral letter, but includes a flyer which tells three real life stories of people who experienced negative income shocks: birth of a child and job loss, bereavement, and family separation (Figure C8). Each story presents social benefits as a safety net and benefit receipt as a legal right.

The second dimension of the design aims to create variation in the costs incurred by individuals in attending the RDVDE. For a randomly selected half of the treatment group, each of the earlier three type of invitation letter offer individuals a choice between participating in a face-to-face meeting with a social worker, or having the meeting over the phone. In the other half, face-to-face meeting is the only option offered in the letter. The phone group thus faces potentially lower meeting costs.

[14] All three types were printed on a joint CNAF/Pôle Emploi letterhead and posted using the same envelopes as used by the CNAF in its day-to-day contacts with the public.

3.4 Sampling

Randomisation and invitations happened in two separate waves. In Wave 1 a sample of job seekers registered as unemployed on May 31st 2017 received invitations from September 18th 2017 onward. In Wave 2 a sample of job seekers registered as unemployed on July 31st 2017 received invitations from November 2nd 2017 onward. In total we drew 90,000 individuals from administrative unemployment registers, 45,000 for each target population. They were informed of the fact that their data would be used for research purposes: about 2,000 individuals refused and were withdrawn from the sample. Due to logistical constraints from the CNAF, we had to select a smaller subsample of 60,000 individuals to participate in the study (24,000 for Wave 1 and 36,000 for Wave 2) with probability proportional to the number of job seekers registered in each *departement* within each population (1 and 2).

We allocated job seekers to six treatment and one control groups using a stratified random sampling procedure carried out by computer. The strata correspond to cells formed by all the possible combinations of three variables : CNAF branch to which the individual is affiliated, type of target population (1 and 2), and a variable indicating whether an individual's distance to the nearest CNAF center is smaller than the median in their *departement* (calculated over all the individuals in the base sample). Half of the sample (30,000) was allocated to the control group, who received no invitation at all, with the other half distributed equally across all the six treatment groups (5,000 per subgroup).

The research team set the minimum number of meetings needed to detect the effect of the intervention at 6,000 (20% of the treatment group of 30,000). In practice, the CNAF network used 6,000 as a target, and stopped arranging meetings once the quantitative objective set to each *departement* was achieved. We do not dispose of the list of job seekers who were called, hence we cannot use this information in our analysis. However, in a few *departement*s no meetings were planned for second wave individuals, because all available slots had been used for the first wave : we drop these wave $\times$ *departement* samples altogether. These are balanced by construction, since the randomization was stratified by wave$\times$*departement*. In the end the baseline sample is composed of 54,418 individuals. The baseline characteristics of the sample are presented in Table B2 (column (1)).

3.5 Additional experiment: online simulator

We carried out a second experiment which evaluates the online social benefits simulator mes-aides.gouv.fr. This online simulator was created by the Secétariat Général pour la Modernisation de l'Action Publique (SGMAP), a government agency responsible for modernising government services. It covers the same range of benefits as the RDVDE, and offers personalised information on benefit eligibility, including the amounts to which potential beneficiaries might be eligible.

The sampling for the additional experiment followed the same structure as in the main experiment: we selected job seekers from the two target populations (Population 1 and 2) in June 2016, using Pôle Emploi data, and randomly allocated each individual to one of two experimental groups. The treatment group received an e-mail containing generic information on the social benefits covered by the simulator, as well as a personalized link inviting them to visit the mes-aides.gouv.fr website. The control group did not receive any e-mail. To reduce the risk of information spillovers, we first randomized Pôle Emploi agencies, and allocated each agency to the control or the treatment group. In every agency, we then randomly drew 50 individuals from each of the two target populations. Job seekers registered to the same agency received the same treatment. The total sample size is 40,000 individuals, with 20,000 individuals in each treatment group (10,000 for each target population).

4 Data and Empirical Strategy

4.1 Data sources

The data used in this paper is drawn from four administrative sources and one household survey.[15] The first data source was jointly provided by Pôle Emploi and CNAF. Pôle Emploi collects information on job seekers' characteristics, including age, sex, marital status, number of children, nationality, educational achievement, type of work sought, reason for registering as job seeker, length of the unemployment spell, and

[15] All data used in this paper is anonymized: Social Security numbers used for merging the administrative datasets were replaced with unique custom-generated strings used only for this study, and all other identifying information was removed prior to the data being accessed for research purposes. In accordance with French and European law, all individuals in the sample were informed about their privacy and data rights prior to the start of the study, and were able to request deletion of their data from the study dataset. The collection and processing of the data was approved by the French data and privacy regulator CNIL.

unemployment benefit amount and duration. The socio-demographic information is collected at the time of registration, and updated for every unemployment spell. The unemployment information is updated every month by job seekers; it is a condition for receiving unemployment benefits. Pôle Emploi transmitted this data to the CNAF, which added an indicator for individuals who had previously received a CNAF benefit either themselves or in their household.

The second dataset is extracted from the Répertoire National Commun de la Protection Sociale (RNCPS), a Social Security database which contains information on the benefit reception status of every individual holding a French Social Security number. This database is hosted by the CNAV, which maintains an interface accessible to other French Social Security agencies (Friconneau, 2014). Data for each wave was queried via this interface by the CNAF at baseline, three months and six months after the end of the intervention (February and May for the first wave and April and July for the second wave). The RNCPS provides cross-sectional individual-level social benefit receipt data, but does not contain information on benefit amounts. Information on benefits which are no longer received is only kept if the individual does not receive the same benefit again, and only up to one year. If a new claim is approved, all information about the old claim is overwritten.

We use RNCPS data to construct three different measures of benefit take-up. The first is a *stock* measure, defined as a dummy variable equal to 1 if an individual is registered as receiving benefits at month t, and 0 otherwise. The second, a *flow* measure, is a dummy variable equal to 1 if the individual is registered at month t as receiving a benefit that they did not receive at baseline, and 0 otherwise. Our *preferred* measure is a dummy variable equal to 1 if the individual is registered as having received a benefit that they did not have at baseline, at any point between baseline and month t, even if they do not receive it any more at time t.[16] While we focus on the preferred measure in most of our analysis, we also provide results using the other two measures for completeness. As per the Pre-Analysis Plan, we construct these measures for each benefit group (family, health and income, see section 3.1), and our baseline outcome is having received at least one benefit in the group.[17]

We supplement the RNCPS data using a third source, the CNAF's own internal

[16] Hence by construction, both the preferred and the flow measures are normalised to 0 at baseline.

[17] We also construct three measures of take-up (stock, flow and preferred) based on the *number* of benefits received by group and report the treatment effect on those outcomes too.

database, which provides information on family and income benefit amounts. These data are only available for individuals in the RDVDE sample who were known to the CNAF at baseline (58% of the full RDVDE sample), and do not cover health benefits.

The fourth data source provides detailed information about the RDVDE meeting. Social workers reported in an intranet tool developed for the experiment whether they held meeting over the phone or face to face, and which social benefits they discussed.[18] In addition to these data, a team of sociologists from CREDOC conducted qualitative interviews with social workers and meeting beneficiaries in four *departements*.

Finally, we conducted a phone survey with 10,000 individuals selected from the experimental sample.[19] The response rate was about 61% (16,489 individuals were called).[20] The survey collected information about benefit receipt by the household at the time of the survey: we use it to construct a self-reported *stock* measure of the number of benefits received. We report treatment effect on this measure for completeness. The survey also asked respondents about benefit applications they or someone from their family had made since September (1st wave sample) or November (2nd wave sample) that had not (yet) led to benefit receipt because they were either unsuccessful, pending, incomplete or abandoned.

For the additional experiment which evaluates the online simulator, we measure compliance outcomes, such as the email open rates and the simulator usage rate through electronic trackers. Because the randomization unit is geographical in that design, we also collected connexions by postal codes to asses differential take-up. The main outcome is benefits take-up, which we measure in the RNCPS at eight and twelve months after the start of the intervention, but not at baseline, three and six months as in the main experiment.

[18] The tool had a checklist for popular benefits and free text entry for the others. Many social workers reported all benefits discussed as a free text entry. We ran a regular expression search of the free text portions to search for benefit names, abbreviations and possible variant spellings, and completed the checklist data.

[19] The survey sample was drawn by stratified random sampling using the same strata as the randomization process. The sampling probabilities for individuals called were $\frac{1}{9}$ for each treatment group and $\frac{1}{3}$ for the control group. Therefore the treatment group was oversampled compared to the control group.

[20] Appendix table B4, compares the characteristics of survey respondents with those of the whole sample. Survey respondents are significantly more likely to be female, they are younger, less experienced, more educated and less likely to be married. These differences are however very small (less than 1% of the mean).

4.2 Randomization checks

We perform randomization checks by estimating the following equation:

$$X_i = \alpha + \sum_j \delta_j T_i^j + \pi_s + \varepsilon_i \tag{1}$$

where X_i is a characteristic of individual i at baseline, including take-up of benefits covered by the RDVDE, T_i^j are dummies for the different treatment groups and π_s are randomization strata fixed effects. A stratum is defined by the interaction of four dimensions: implementation wave, target population, distance to the nearest CNAF branch and *departement*. As randomization occurred at the individual level, standard errors are only adjusted for heteroskedasticity. Appendix Table B2 presents balance tests for socio-demographic variables (Panel A) and baseline benefit take-up (Panel B): there is good balance between the control and the two types of invitation (phone and in-person). Appendix Table B3 presents the same balance tests with the full set of six treatments (interacting the type of invitation with the flyer received with the invitation).[21]

4.3 Empirical strategy

In most of the paper, we pool all treatment groups into being invited to a meeting or not. Let Y_i denote the outcome of interest. Let T_i be a dummy variable equal to one for job seekers who were invited to the RDVDE. We estimate by OLS the Intention to Treat (ITT) effect of being invited to the RDVDE:

$$Y_i = \beta T_i + X_i \delta + \pi_s + \varepsilon_i \tag{2}$$

where π_s are strata fixed effects. Given the large number of variables available, we use the robust double lasso procedure (Belloni et al., 2014) to select controls X_i. Standard errors are robust to heteroskedasticity.[22].

In order to estimate the effect of attending the meeting itself, we also implement

[21] Appendix Table B5 reports balance tests for survey respondents: there are few significant differences across treatment groups. Appendix Table B19 presents balance tests for the evaluation of the online simulator, which is also balanced.

[22] When we estimate regressions on the online simulator experiment, standard errors are clustered at the employment agency level

an IV strategy, in which we regress outcomes on meeting attendance, and instrument attendance by the invitation. Formally, we use 2SLS with the following two equations to estimate the Local Average Treatment Effect (LATE) :

$$M_i = \beta_1 T_i + X_i \delta_1 + \pi_{1s} + u_i \tag{3}$$

$$Y_i = \beta_2 M_i + X_i \delta_2 + \pi_{2s} + \varepsilon_i \tag{4}$$

where M_i denotes meeting attendance, as measured in the RDVDE extranet module, and X_i are the same variables as in the OLS specification. The IV approach is only valid if the invitation has no effect on benefit take-up other than via meeting attendance. We will provide evidence in support for this hypothesis.

In the last part of the paper, we go beyond the LATE and estimate Marginal Treatment Effects (MTE) following Heckman and Vytlacil (2005) to study whether job seekers who attended the meetings were the ones who stood the most to gain from it. We estimate the MTE in two stages. The first stage estimates the probability of attending the meeting conditional on invitations to the meeting, observable characteristics and their interaction:

$$M_i = Z_i \beta_3 + X_i \times Z_i \gamma_1 + X_i \delta_3 + \pi_{3s} + u_i \tag{5}$$

where Z_i splits T_i into two indicators for the standard invitation and the phone invitation, which are randomized, and X_i are the controls chosen by lasso in the first stage. The second stage estimates a flexible relationship between the predicted propensity of attending and outcomes:

$$Y_i = a\, \widehat{p(X_i, Z_i)} + b\, \widehat{p(X_i, Z_i)}^2 + X_i\, c \times \widehat{p(X_i, Z_i)} + X_i \delta_4 + \pi_{4s} + \varepsilon_i \tag{6}$$

where $\widehat{p(X_i, Z_i)}$ is the propensity score computed in the first stage. Standard errors are based on 500 bootstrap replications of both stages. The MTE is the derivative of this equation with respect to the propensity score: it is equal to $a + X_i\, c + 2b\, \widehat{p(X_i, Z_i)}$. It has an observed and an unobserved component: if the vector c is not zero, the treatment effect varies with the observed variables X. This captures the traditional observed heterogeneity in treatment effects. The term $2b\, \widehat{p(X_i, Z_i)}$ measures the treatment effect heterogeneity along the unobserved dimensions that drive the decision to receive the treatment. Specifically, a positive coefficient b indicates that people with a higher unobserved cost to attend the meeting have higher treatment effects.

We follow Cornelissen et al. (2018) and Bhuller et al. (2020) and assume that the MTE is separable between observed and unobserved heterogeneity. In our specification, the MTE is linear in $p(X_i, Z_i)$. Non-parametric identification of the MTE is possible with a continuous instrument, but our instrument only takes three values (control, standard and phone invitation); further, as will appear when we provide the estimation of equation 5 (Appendix Table B18), phone invitations and standard invitations have almost the same effect on take-up. In practice, therefore, we should consider that we have a two-values instrument. Brinch et al. (2017) show that in that case, only a linear MTE can be identified.

5 Results

This section presents the main findings from the RDVDE experiment: the effects of invitations on meeting attendance, the effects of invitations on benefit take-up, and the effects of meeting attendance on benefit take-up.

5.1 Compliance

Appendix Table B6 presents the treatment effects on the probability of attending the RDVDE. As column 1 shows, in the control, the probability of attending a RDVDE is null. The average compliance rate for the treatment group is 21.3%, which corresponds to the official target of 6,000 meetings. Within the treatment group, participants who were given the choice between meeting in person and on the phone were only slightly more likely to attend than those who were only invited to a face-to-face meeting (column 2). We also test in column 3 whether the content of the invitation letter had any effect on the probability of attending the meeting. We find that beneficiaries who received an information and an anti-stigma flyer were significantly *less* likely to attend the meeting by 1.3 and 1.1 percentage points: the lack of information on eligibility or welfare stigma do not seem to act as barriers to meeting attendance.

Columns 4 to 6 in Table B6 present the estimated treatment effect on the probability of having a meeting on the phone. Nobody in the control group had a phone meeting, and 8.3% of the treatment group had a phone meeting, i.e. a bit more than a third of all RDVDE were held on the phone (column 4). As column 5 shows, the probability of having a phone meeting is 10.1% in the treatment group that was offered a phone meeting in the invitation mail, and 6.7% in the group that was only offered a face-to-face meeting initially. The deviation from the study protocol is due to the success of phone meetings, who were offered increasingly to participants of all treatment groups, even when they were not supposed to. Finally, there was little difference in phone meeting attendance between the groups that received different invitation letters.

In Appendix Table B7, we provide additional information on the content of the RDVDE meeting, based on administrative data from the Intranet tool used by social workers. Specifically, we estimate that 19% of job seekers who received an invitation to the meeting attended and discussed at least one social benefit they may be eligible to. When we break it down by benefit group, we find that 14% discussed at least one family benefit, 11% a health benefit and 16% an income benefit. These results suggest that over 90% of job seekers who attended the RDVDE meeting were potentially eligible to social benefits, including family, health and income benefits. We will next test whether this potential eligibility translated into actual take-up.

5.2 Effect of invitations on benefit take-up

We now use equation 2 to estimate the effect of the RDVDE invitation on our main outcome, the take-up of social benefits. Panel A in Table 1 presents the treatment effects on new benefit receipt during a period of six months since the intervention.[23] As column 1 shows, 26.7% of job seekers in the control group received a new benefit in these six months. As compared to the control, the treatment group was 1.74 percentage points more likely to have received any new benefit. The effects are driven by family and housing benefits (0.54 percentage points increase, column 2), and income support benefits (1.5 percentage points increase, see column 4), with no treatment effect for health benefits (column 3). Appendix Table B9 presents the results for each benefit. The rise in family benefits receipt is entirely due to increased housing benefit (AL) take-up. The positive effect on income benefits is present both for in- (PA) and out-of-work (RSA) benefits but stronger for the former.[24]

[23] Appendix Table B8 shows that the effects of the invitation on benefit take-up increased between three and six months after the intervention.

[24] The qualitative evaluation of the experiment (Alberola et al. (2018)) notes that the RDVDE attracted workers that alternate frequently between employment and unemployment. This population, which is often unknown to social services, is not very poor but is entitled to in-work (PA) benefits, often transitorily, with thus limited incentive to claim it. This is also consistent with the

Table 1: Effect of invitation and attendance on benefit amounts

	Any benefit (1)	Family benefit (2)	Health benefit (3)	Income benefit (4)
Panel A: Effect of receiving an invitation to the RDVDE				
Any invitation	0.0174	0.0054	0.0017	0.0150
	(0.0037)	(0.0027)	(0.0023)	(0.0031)
Control group mean	0.267	0.11	0.076	0.154
Num.Obs.	54418	54418	54418	54418
R2	0.106	0.043	0.049	0.070
Panel B: Effect of attending the RDVDE				
Attended meeting	0.0815	0.0255	0.0079	0.0704
	(0.0172)	(0.0126)	(0.0106)	(0.0144)
Num.Obs.	54418	54418	54418	54418
Control group mean	0.267	0.11	0.076	0.154
R2	0.109	0.044	0.050	0.072

Note: Panel A presents the ITT estimate of being invited to the RDVDE on benefit take-up. Panel B presents the 2SLS estimate of the LATE of the RDVDE meeting. In both panels, the outcome variable is the preferred measure of benefit take-up at six months after the start of the intervention. The preferred measure is a dummy variable equal to 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. In Panel B, we instrument meeting attendance with being assigned to any group receiving an invitation letter. Controls selected using the robust double lasso method, coefficients omitted for brevity. All specifications contain strata fixed effects. Standard errors are robust to heteroskedasticity.

In Appendix Table B10, we present the treatment effects on alternative outcome measures. In Panel A, we use the *stock* measure, i.e. all benefits received at endline (three or six months to the meeting), including those being already received at baseline. In Panel B, we use the *flow* measure, i.e. only benefits still open at endline, not including those that may have been closed between the intervention and the endline. The results in Panel B of that table are qualitatively similar to our preferred measure, but the treatment effects are smaller. This suggests that part of the benefit take-up generated by the RDVDE may have been temporary, e.g. to help transitions from unemployment to a paying job. When we use the *stock* measure (Panel A), we find even smaller effects, which is expected since the meeting would have a low effect on benefits at endline for households who already have one at baseline. Second,

observation that part of the effect of RDVDE is transitory – cf discussion of Table B10 below.

we estimate the treatment effects on the number of benefits instead of dummies for any benefit receipt: Appendix Table B11 shows that the intervention increased the number of benefits received, with stronger effects for our preferred measure than for the *stock* or *flow* measures.

We next turn to the effect of the different types of invitations to the RDVDE. Appendix Table B13 presents the results. In Panel A, we test whether being offered a phone meeting made a difference to the treatment effect on benefit take-up. The treatment group who was invited to a phone meetings had a slightly higher chance of opening new benefits, but the difference is not significant. In Panel B, we test whether the content of the invitation letters had any differential effect on benefits. We find that, as compared to the neutral invitation, the group who was sent the information flyer was more likely to receive new benefits after six months, but the difference is not significant. There is no evidence that the anti-stigma letter had any differential effect on take-up either. Overall, it does not seem that the type of invitation changed the effectiveness of the meeting in inducing benefit take-up.[25]

5.3 Effect of meeting attendance

The results presented so far have documented the effect of receiving an invitation to the RDVDE. We next estimate the effect of attending the meeting itself and estimate the IV specification of equation 3. The exclusion restriction needed for the IV strategy to be valid requires that invitations had no independent effect on take-up other than through RDVDE attendance. This hypothesis seems plausible, given that (i) we do not detect differential effects of the type of invitation letter on benefit take-up; (ii) less than 10% of the meetings were arranged by people who spontaneously contacted the CNAF upon receipt of the invitation letter; (iii) qualitative interviews suggest that beneficiaries who attended the meetings did not remember the letter.

The IV estimates are presented in Panel B of Table 1. As expected given the relatively low compliance rate, the LATE is much larger than the ITT. Attending the meeting increases the probability of receiving any new benefit by 8.15 percentage points, a 31% increase as compared to the control group (column 1). As column 4 shows, the meeting was particularly successful at inducing income benefit take-up : the meeting increased the probability of receiving a new income benefit by 46% over

[25]Naturally, the attendance rates to the meeting is different for each of those incentives, but scaling the effects to that rate (i.e. estimating LATEs) does not change this general finding.

six months (7.04 percentage points increase). The meeting also increased the take-up of any new family benefit over six months by 23% (2.55 percentage points increase, see column 2). By contrast, there is no significant effect on health benefit take-up, and the point estimate is 10% of the control mean. The very large effects of the meetings suggest that the take-up of social benefits is far from 100% in the population of job seekers we study: among compliers, *at least* 8% do not receive benefits they are entitled to. We will show in Section 7 that job seekers who are the least likely to attend the RDVDE are also the ones for which the benefit of the treatment is highest: this suggests that benefit take-up could be even lower in the non-complier population.

6 Barriers to take-up

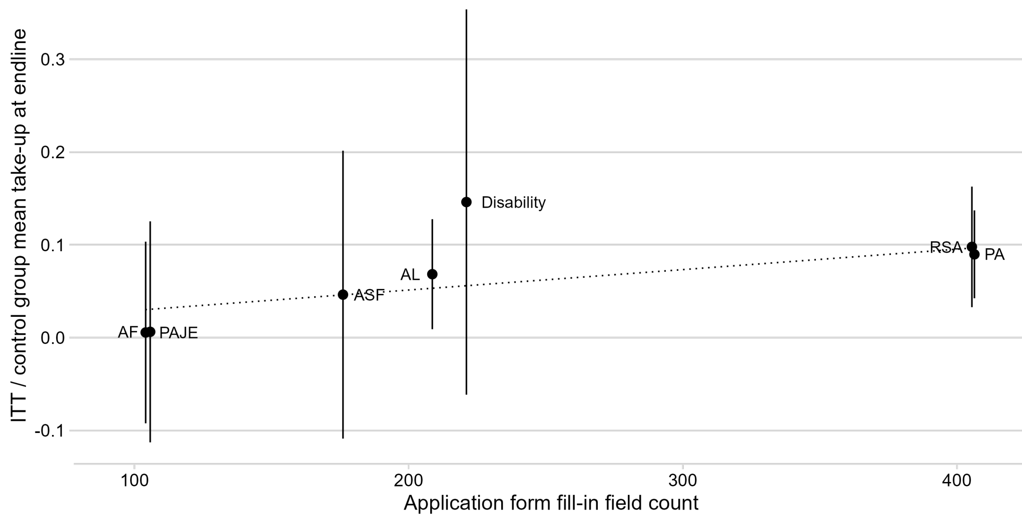
6.1 Evidence from the RDVDE experiment

The RDVDE is a bundled intervention: it provides information on benefit eligibility as well as assistance for applications to family and income support benefits. It could also alleviate welfare stigma although this was not the explicit aim of the meeting between social workers and beneficiaries. A natural question is which of these components were the most important to generate the increase in benefit take-up following the RDVDE.

In the previous section, we showed that in contrast to family and income benefits, the RDVDE had very little effect on the take-up of health benefits (Appendix Table B8). This is despite the fact that 11% of the treatment group (and half of the job seekers who went to meetings) discussed their eligibility to health benefits during the meeting (Appendix Table B7). The main difference between health benefits and the other types of benefits is that they are provided by a different agency and the CNAF social workers in charge of the RDVDE could not offer application assistance for them. This result suggests that the reduction of transaction costs is an essential part of the RDVDE, i.e. that providing information or alleviating welfare stigma alone are not enough to induce take-up in this context.

To provide more evidence on the role of the RDVDE in reducing transaction costs, we can exploit the varying degrees of complexity of the application to the different benefits covered in the meeting. Specifically, we focus on family and income benefits, which are delivered by the CNAF, and count the number of fields that need to be filled in the application forms of each benefit. We then plot the treatment effect of the invitation to the meeting on the take-up of each benefit against this measure of complexity of the application process. As Figure 1 shows, the RDVDE was more effective in raising benefit take-up for benefits that are harder to apply to. This confirms that the meetings contribute to the reduction of application costs.

Figure 1: Treatment effect on take-up and complexity of application for each benefit



Note: The figure presents the relationship between the effect of the invitation to the meeting with social services and the number of fields to fill in the application form for each benefit. We exclude health benefits and focus on benefits that are directly managed by the CNAF social services that held the meetings. AF, PAJE and ASF are family benefits, AL are housing benefits, and RSA and PA are income support benefits. 90% confidence intervals plotted.

The only direct evidence on the role of welfare stigma we have comes from the anti-stigma invitation letter, which we showed had no effect on meeting attendance or benefit take-up. This finding suggests that welfare stigma may not be important in our context, but stigma could also be so deeply entrenched that a simple letter may not be sufficient to alleviate it, so that the evidence is inconclusive. In a similar vein, we can test the role of information through the information leaflet, which provided general information about the existence of benefits and amounts received by households in different situations. We showed that it did not have any impact on meeting attendance or benefit take-up. This test is however a weak one, because the information in the leaflet may have been too general to induce take-up.

Table 2: Effect of the invitation to the online simulator

	Any benefit received over eight months			
	Any benefit	Family	Health	Income
Any invitation	0.0029	−0.0041	0.0058	0.0039
	(0.0064)	(0.0059)	(0.0048)	(0.0045)
Control group mean	0.494	0.39	0.129	0.164
Observations	40,000	40,000	40,000	40,000

Note: This table presents the ITT estimates of being invited to use the online simulator on receiving any benefit measured eight months after the start of the intervention. Controls selected using the double-lasso method, omitted for brevity. Standard errors robust to heteroskedasticity and clustered at the unemployment agency level. All specifications contain strata fixed effects.

6.2 Evidence from the online simulator

To test the role of information more precisely, we leverage the second nationwide experiment, which invited a random sample of job seekers to evaluate their personal eligibility to social benefits using an online simulator provided by the government. The simulator provided customized and detailed information at a small cost, but did not provide any help in benefit application. We see this as a good test of the effect of "pure" information content delivered in a credible and neutral manner. From the 20,000 job seekers in the treatment group, 67% opened the invitation email, 39% clicked on the link in the email to start a simulation immediately, and 18% completed a simulation in the same session. By contrast, simulator usage in the control was negligible: since randomization took place at the level of unemployment agencies, we can use geo-localized connection data at the zip code level to check that there were almost no connections in control areas. Indeed, at the time of the experiment, only social workers used the simulator, which was not publicized until 9 months later. The net compliance rate to this treatment is thus comparable to that of the main experiment. Out of the 18% completed simulation, the logs of the computations run indicate that most job seekers were eligible to some benefit (16% of the total sample), and in particular to health benefits (14%). These results suggest that the information was well received, and well targeted: in fact these numbers likely underestimate simulator usage, since we can only track the first connections.

As Table 2 shows, however, the information only had a very small effect on benefit take-up: the estimate for any benefit is a very small and insignificant 0.29 percentage points (0.6%) eight months after the emails were sent, the point estimate is even negative for family benefits.[26] Appendix Table B20 confirms this result using the number of benefits as an alternative outcome. Taken together, these results suggest that providing personalized information, and only information, to job seekers who are likely eligible to benefits does not have a large effect on take-up overall.[27] The possibility remains that information could be more credibly or more clearly provided to job-seekers during the meetings with social workers than online, but we cannot test this in our context. Yet, to the extent that the platform provides reasonably understandable information, we find no evidence here that people's lack of information is a major obstacle to take-up. As we discussed in the model section 2, it could be that: (i) job seekers make mistakes individually but on average, at the population level, they have accurate beliefs about their eligibility; or (ii) job seekers systematically underestimate their eligibility, but the bias is small relative to transaction costs, so that correcting them does not visibly change the decision to apply.

6.3 Success Rate of Applications

If job seekers are misinformed about their eligibility but do not on average over-estimate or under-estimate them, then providing information about potential benefit eligibility would not increase the probability of applying to benefits, but might make them more successful (because better information on eligibility improves self-selection into applying, see section 2). Based on the phone survey in the RDVDE experiment, we can test this using information on benefit applications which did not lead to the receipt of benefits, either because they were rejected, pending, incomplete or abandoned. We construct the application success rate by dividing the number of benefits opened according to the administrative data after three months (i.e. around the time of the survey) by the sum of the number of benefits opened and the number

[26] The only outcome we have for the online simulator is identical to the *stock* measure defined for the main experiment, rather than the *preferred* one. The corresponding effects of the RDVDE are 0.68 percentage points (1.4%) for any benefits and 0.82 percentage points (2.3%) for family benefits (Appendix Table B10).

[27] We can assume there are job seekers eligible to benefits because: (i) the log of simulations indicates it; and (ii) this is a similar population as in the main experiment, where we find large effects on take-up.

of rejected applications according to the survey.[28]

Table 3 presents the treatment effect on the success rate of applications. Note that the baseline rate is high, about 90%, which implies that control individuals do not strongly overestimate their eligibility, and runs counter the idea that people lack information.[29] Further, the success rate is not improved by the treatment: social workers increased applications, but not selection into applying, as would be predicted if the meetings improved the accuracy of information on benefit eligibility. If the job seekers who came to the meetings tended to have lower claims than individuals in the control group who applied for benefits, but were encouraged to apply by the lower transaction cost, this would even explain the small decrease in success rates. The next section provides evidence of such an adverse selection of compliers.

Table 3: Effect of the invitation on application success rates

	Application success rate			
	All benefits (1)	Family benefits (2)	Health benefits (3)	Income benefits (4)
Any invitation	−0.0218	−0.0819	0.0083	−0.0305
	(0.0137)	(0.0283)	(0.0390)	(0.0200)
Control group mean	0.8950	0.9070	0.9220	0.8790
Observations	2552	1017	541	1606

Note: This table presents the ITT estimates of being invited to the RDVDE meeting on the success rate of benefit applications. The success rate is computed as the number of new benefits received based on the preferred measure from the RNCPS divided by the sum of the number of new benefit received and of the number of unsuccessful applications measured using the phone survey. The preferred measure is a dummy variable equal to 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. The total number of new benefits is the sum, within benefit group, of the preferred measures for each benefit in that group. Controls selected using the double-lasso method. Standard errors robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity.

[28]By construction, this measure is missing for people who did not apply, thus the number of non-missing observations in Table 3 is 2,225 (for all benefits), although survey respondents are 9,969.

[29]Finkelstein and Notowidigdo (2019) who find significant information effects, also observe higher rejection rates (23% compared to 10% here).

7 Targeting

Our findings so far suggest that many eligible job seekers do not take up social benefits to a large extent because of transaction costs, and that these costs can be lowered by meetings with social services, with large positive effects on take-up. The key question that remains is whether the intervention improves the targeting of social benefits, i.e. whether it helps people with higher claims to apply. Guided by the model in Section 2, we tackle this question in two ways: we first estimate treatment effects on benefit amounts received to assess the eligibility of marginal claimants, and we then estimate Marginal Treatment Effects to evaluate the targeting of the meeting itself.

7.1 Amounts

We first test whether the marginal claimants induced by the lowering of transaction costs in the meetings had lower or higher claims (i.e. whether they were richer or poorer than the average claimant). Our discussion in Section 2 suggests that, by decreasing transaction costs, we also decrease the threshold of benefit amounts for which one is ready to apply. Thus, in general, the average value of claims should decrease with treatment.

We were able to collect information on benefit amounts for only 58% of our sample, composed of the job seekers who were already known to social services because they had taken up family or income benefits at some point prior to the baseline.We estimate the ITT effect of being invited to the RDVDE on this population, on three outcomes: the probability of receiving any new benefit since the invitation, the amount received conditional on receiving a benefit and the total amount received, giving zero to people who did not receive any benefit. Panel A in Table 4 shows that, in this subsample, the effect of the invitation on take-up of benefits is slightly smaller but quite comparable to that in the full sample (0.0155 vs. 0.0174, see Table 1). In Panel B, we report the effect of the invitation on average amounts received conditional on claiming them, which are negative, as expected if the treatment reduces transaction costs. In other words, the sample in Panel B is made of infra-marginal claimants, who receive benefits in the absence of the intervention, and marginal claimants who take-up benefits only if they get invited (and subsequently attend the meeting). The control group mean in Panel B provides the average value of amounts received by the infra-marginal claimants (158 euros monthly). Those who receive benefits in the treatment group receive 5.7 less euros on average; because this group is composed of infra-marginal and marginal individuals, we can recover the average amounts received by the marginal claimants: it is 91 euros less than the infra-marginal individuals (bootstrapped 95% confidence interval [-317;28]).[30] Thus, the intervention attracts potential beneficiaries with substantially lower claims, which they probably wouldn't think worth claiming if they did not receive some assistance. This finding is very consistent with Finkelstein and Notowidigdo (2019)'s findings on SNAP recipients.

Table 4: Effect of receiving an invitation to the RDVDE on monthly benefit amounts

	Any benefit	Family benefit	Income benefit
Panel A: Benefits take-up			
Any invitation	0.0155	0.0012	0.0154
	(0.0048)	(0.0031)	(0.0043)
Control group mean	0.231	0.08	0.172
Observations	31444	31444	31444
R2	0.060	0.024	0.065
Panel B: Amounts received conditional on take-up			
Any invitation	-5.7192	-10.5496	-1.9391
	(3.9593)	(6.6148)	(4.0261)
Control group mean	158.46	171.392	133.329
Observations	7556	2526	5713
R2	0.079	0.170	0.092
Panel C: Benefits amounts received			
Any invitation	1.2589	-0.6397	1.8737
	(1.1963)	(0.7109)	(0.9113)
Control group mean	36.606	13.661	22.944
Observations	31444	31444	31444
R2	0.045	0.022	0.047

Note: This table presents the ITT estimates of being invited to the RDVDE on three different measures of benefit take-up. The measures are the preferred dummy variable take-up measure (Panel A), the monthly amount of benefits received conditional on the benefit being newly received as per the preferred measure (Panel B), and the total amounts of benefits received attributing a zero value to non-claimants (Panel C). Regressions estimated on the sample known to services at baseline for which benefit amount data is available. All outcome variables are measured six months after the start of the intervention. Controls for the specifications selected using the robust double lasso method, coefficients omitted for brevity. All specifications contain strata fixed effects. Standard errors are robust to heteroskedasticity.

[30] Call B the value of benefits, use loose notation I for infra-marginals and M for marginals, and $p(k)$, $k = I, M$ their proportion in the population. Then, the control group mean in Panel B is $E(B|I, B > 0)$ and the coefficient is $p(I)E(B|I, B > 0) + p(M)E(B|M, B > 0) - E(B|I, B > 0)$, from which we recover $E(B|M, B > 0)$ using the estimates of $p(I)$ and $p(M)$ in Panel A.

Finally, in Panel C we estimate the effect of being invited to the meeting on benefits received attributing zero values to non-claimants, which combines the two previous effects. We find that each invitation resulted in an unprecisely estimated 1.26 euros of new income benefits per month, which reflects a higher take-up of lower amounts. Rescaled by the share who attended the meeting, this ITT effect corresponds to a gain of 6 euros *per meeting*, which is equal to about 15% of the control group mean. More precise figures are estimated for income benefits, where the gain per meeting would be about 39% compared to the control group. Hence overall, the analysis of benefit amounts confirms some positive average effects on benefits, but they are limited by the strong negative targeting of marginal beneficiaries.

7.2 Marginal Treatment Effect

Our results so far are driven by the effects on compliers, i.e. people who decided to attend the RDVDE. Since compliers are self-selected, the effects of the meeting could be different among those more likely to attend the meeting than in the wider population. As an illustration, Appendix Table B14 and Table B15 present heterogeneous effects of the invitation on attendance and benefit take-up respectively, along four dimensions which we pre-specified in the pre-analysis plan. Take job seekers known to social services (CNAF beneficiaries, column 3): we already noticed in the previous section that they had smaller effects of the invitation on benefit take-up than the rest of the population; they also have a higher propensity to attend the meeting. Therefore, the effect of the meeting to them (the ratio of the two, or the Wald estimator) is smaller. This is a situation where those most likely to attend the meeting have a lower benefit to it. We find a similar pattern for job seekers who were at the end of unemployment benefits (Population 2, column 2), which were less likely to attend the meeting but had higher treatment effect on benefit take up (the latter difference is not statistically significant).

To explore this issue more systematically, in this section we apply the marginal treatment effect (MTE) method to estimate how the effect of the meeting varies with the propensity to attend. We first estimate the propensity score using specification 5 (Appendix Table B18). Variables selected by the lasso to be part of the propensity score are the main socio-demographic characteristics (citizenship, gender, age, and marital situation) and variables that capture the reasons for and the duration of unemployment, as well as unemployment benefits received, salary in previous jobs, and the type of work sought. We interact these variables with two treatment dummies: any invitation and phone invitation. Several interactions are significant: for instance, invitations increase attendance more for women, older individuals, foreigners, those with lower unemployment benefits and lower pre-unemployment salaries, those who have re-entered the labour market after an illness or maternity leave, and those who were previously known to social services. All of them thus have a lower net cost of attending the meetings and are more likely to belong to the complier population. By contrast, none of the interaction terms with the phone invitation dummy are significant, so that in practice the phone invitation treatment does not generate additional variation in the propensity score. As Figure A1 in Appendix shows, the combination of the instrument and observable characteristics induces large variations in the predicted propensity to attend the meeting, from 0 to about 50%. We can thus estimate the MTE on a rather large support of individuals with low to high cost of attending the meetings.

Table 5: Marginal treatment effect on benefit take-up

	Any new benefit take-up over six months			
	Any benefit	Any family benefit	Any health benefit	Any income benefit
Propensity score	0.0234	0.0893	0.0574	−0.1403
	(0.0809)	(0.0564)	(0.0509)	(0.0639)
Propensity score^2	0.3901	0.0015	−0.0819	0.5944
	(0.2141)	(0.1638)	(0.1415)	(0.1842)
Strata fixed effects	Yes	Yes	Yes	Yes
Observations	54,418	54,418	54,418	54,418
R^2	0.1000	0.0411	0.0452	0.0659

Note: This table presents the MTE estimates on benefit take-up, where take-up is measured at six months after baseline using the preferred measure. The preferred measure is a dummy variable which is 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention, and that benefit was not received at baseline, and 0 otherwise. The propensity score is obtained by a linear probability model of meeting attendance, based on treatment assignment and controls selected by double lasso. The estimates of the first stage are presented in Appendix Table B18. Standard errors are obtained through 500 bootstrap replications of both stages. All specifications include strata fixed effects and controls used in the propensity score interacted with treatment probability.

We then estimate the second stage, following specification 6, which regresses measures of benefit receipt on the propensity score, controls and the propensity score interacted with controls. Table 5 presents the coefficients on the propensity score (coefficients a and b of equation 6). We find positive and significant coefficients on the square of the propensity score for cumulated benefits, and for income benefits.

We start with the unobserved component of the MTE: Figure 2a presents the corresponding MTEs as a function of the propensity score, which are the derivative of the second stage equation with respect to the propensity score: they represent $a + \bar{X}\,c + 2b\,p\widehat{(X_i, Z_i)}$, where the intercept is estimated at the average sample value of the X's. This derivative identifies how the treatment effect varies as the unobserved cost of attending the meeting increases (or the probability to be a complier decreases). In the theoretical model we denoted this cost as κ (see Section 2). The MTE is positively sloped, it increases from a treatment effect of about zero for those with the lower attendance costs, to an effect of about 0.40 for those with the highest costs. In comparison, we estimated in Section 5 a LATE of 0.08 on the take-up of any benefit, which suggests that there is strong heterogeneity in the treatment effect along the attendance cost dimension. For income benefits, the slope is stronger, with even more significant effects (see Appendix Figure A2b).

To get a sense of the range of monetary gains from the meeting, we can use the subsample known to social services, for which we observe amounts received. We reproduce the MTE, using as outcome the monthly average of the cumulative amount received from benefits not present at baseline, with zeros when there was no new benefit received. Appendix Figure A3 shows that attending the meeting can increase benefits received by up to about 90 euros per month for the least likely to attend, which is large in comparison to the control group mean of 36 euros and the LATE effect of about 6 euros derived in the previous section.[31]

We next turn to the observed component of the MTE: its identification is more direct than the unobserved component. We first compute the average treatment effect for each individual with observed characteristics X_i as $(X_i\,c)$ from equation 6. We then compute the propensity score for every individual, and correlate the two. To present this graphically, we have formed 50 bins of the propensity score (of 0.01 points width), and computed the average $(X_i\,c)$ in each bin. Figure 2b, shows a mostly negative pattern: individuals who are most likely to attend the meeting based on their observed characteristics have smaller predicted treatment effects.[32] The effects

[31] Appendix Figure A4 shows that the MTE results on take-up for the subsample known to social services are similar to the whole sample (Figure 2).

[32] Values of the MTE in that figure are negative because they are relative effects, not accounting

are even more pronounced for income benefits (Appendix Figure A2d). Hence the observed component of the MTE tells the same story as the unobserved component.

The MTE can further be used to compute weights to estimate policy parameters of interest such as the Average Treatment Effect (ATE), the Average Treatment on the Treated (ATT) and the Average Treatment on the Untreated (TUT) (Heckman and Vytlacil, 2005). Table 6 shows the estimates of those parameters. First notice that the LATE reconstructed from those weights is close to our 2SLS estimate.[33] Most interestingly, the ATT is much lower than the TUT, which confirms that individuals attending the meeting are not those who would benefit most from it. Orders of magnitude are large: while the meeting increases the probability to take a benefit by 6 percentage points on average among those who attended, it would be increased by 21 points among those who didn't. The ATE is also much larger than our initial LATE estimate, at about 18 percentage points.

Table 6: Aggregated treatment effect parameters for benefit take-up

	Any new benefit take-up over six months				
	ATE	ATT	TUT	LATE	2SLS
Attended meeting	0.1815	0.057	0.2143	0.0649	0.0815

Note: This table presents the Average Treatment Effect (ATE), the Average Treatment on the Treated (ATT) and the Average Treatment on the Untreated (TUT) and the LATE, computed using our estimate of the linear MTE. Column (5) also recalls the 2SLS estimator from Table 1.

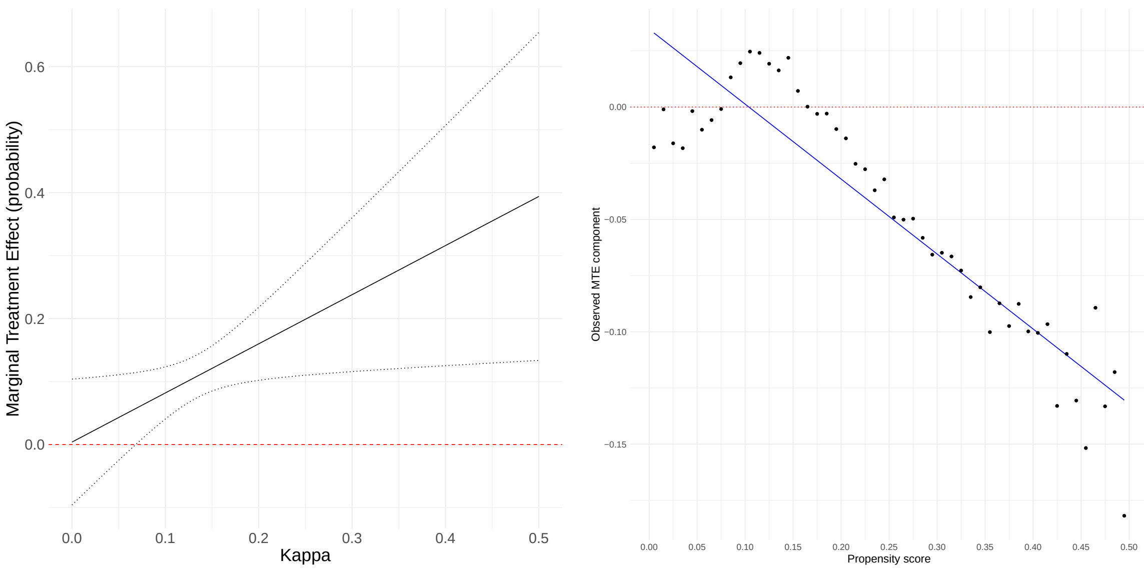
Overall, our findings suggest that claimants with higher costs of attending the meeting would gain the most from it. Through the lens of our representation of compliance and take-up, job seekers who have a high cost of attending the meetings only attend if they anticipate large benefits relative to the transaction costs of application. In that case, they would tend to claim the benefit even without support from social workers, and the treatment impact would be low for them. Our results point to another likely feature: that job seekers with high costs of attending also have high transaction costs to claim benefits on their own, and thus tend not to apply without the treatment.

for the constant and the term in $2b\,\widehat{p(X_i, Z_i)}$.

[33] They should not be identical because they are based on somewhat different specifications, and the weights computation introduces measurement error.

Figure 2: Marginal treatment effect (MTE) on benefit take-up

(a) Unobserved Component (b) Observed Component



Note: Panel (a) presents the relationship between the marginal treatment effect on any new benefit take-up and the unobserved cost from participating to the meeting with social services. The x-axis measures the cost of attending the meeting. Take-up is measured using the preferred measure, a dummy variable which is 1 if the individual received any benefit at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. It is measured over six months since the start of the intervention. The linear relationship is obtained from the estimates in Table 5, evaluated at the mean of the relevant sample covariates. 90% confidence intervals plotted. Standard errors are obtained through 500 bootstrap replications of the first and second stages. See Section 4 for more details. Panel (b) presents the relationship between the observable component of the MTE (i.e the treatment heterogeneity based on observables) and the propensity to attend the meeting (x-axis). The observed component of the MTE is computed as $X_i\,c$ from equation 6. We have formed 50 bins of the propensity score (of 1 points width), and computed the averages $X_i\,c$ in each bin, which are represented by the black points. The blue line is a linear adjustment.

When they do attend the meeting, application assistance is particularly effective for them. Our results for job seekers known to social services support this interpretation. They have lower costs of going to the CNAF agency, which explains their higher compliance rate; they also face lower transaction costs in applying to benefits, which explains that the meetings are less useful for them. Our MTE results suggests that this holds more generally: the distribution of compliance and transaction costs is such that self-selection tends to bring the wrong people to the meeting.

8 Conclusion

This paper reports on two nationwide randomized experiments aimed at increasing social benefits take-up in France. The first invited job seekers to a meeting with social services to evaluate their eligibility to a wide range of social benefits and to help them apply. The impact of the meetings on compliers was large : benefits opened within 6 months of the meeting increased by +46% for income benefits and +23% for family and housing benefits. By contrast, a companion experiment which provided personalized eligibility information via an online benefits simulator had no impact on take-up. Based on this second experiment and on additional results from the first, we argue that application costs rather than lack of information seem to be the main driver of non take-up in our context. In the final part of the paper, we show that the marginal claimant induced by the lowering of application costs had slightly lower claims, which suggests that among compliers application costs may worsen targeting. However, when we estimate the Marginal Treatment Effect, we find that the people who stood the most to gain from the meeting were less likely to attend it, which implies that the same costs that deter eligible individuals from applying to social benefits also prevents them from accessing assistance to help them apply.

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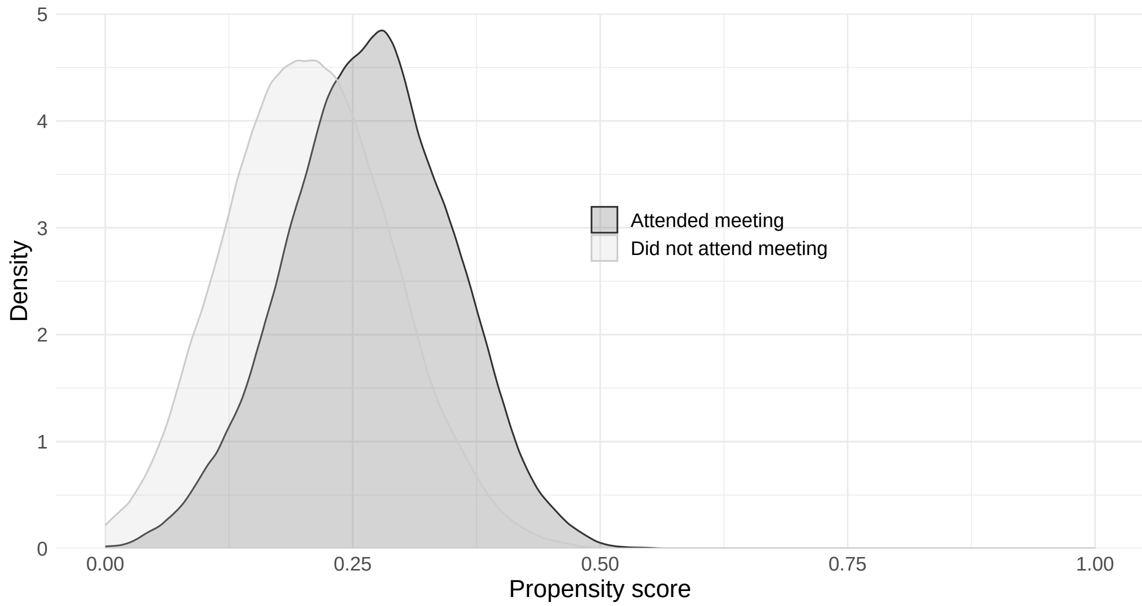
A Appendix Figures

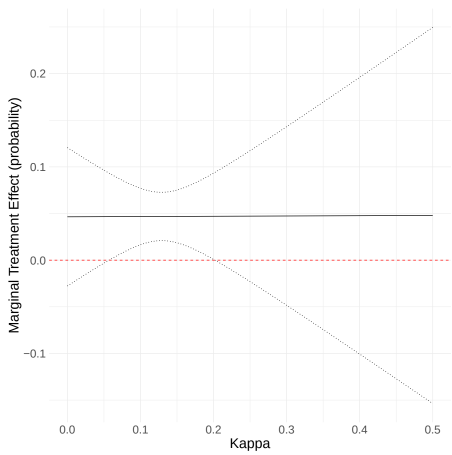
Figure A1: Propensity Score



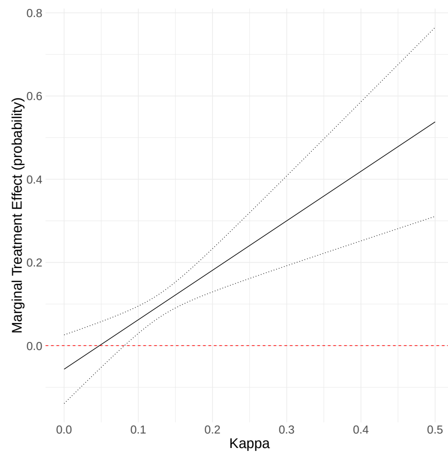
Note: The figure presents the distribution of the propensity score, which is the prediction of meeting attendance based on treatment assignment and control variables chosen by the double lasso. The estimates are presented in Appendix Table B18. See Section 4 for more details.

Figure A2: Marginal treatment effect (MTE) on benefit take-up

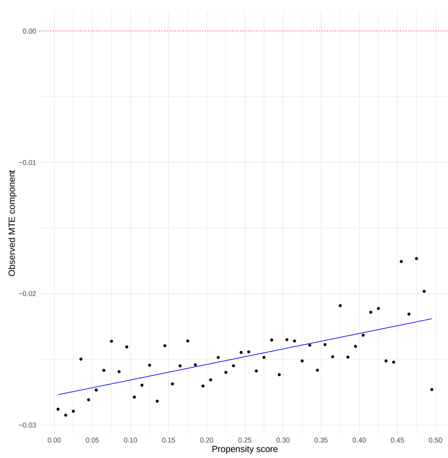
(a) Unobserved Component,Family Benefits



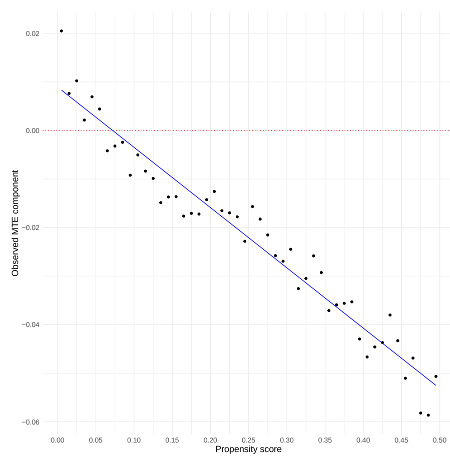
(b) Unobserved Component, Income Benefits



(c) Observed Component, Family Benefits



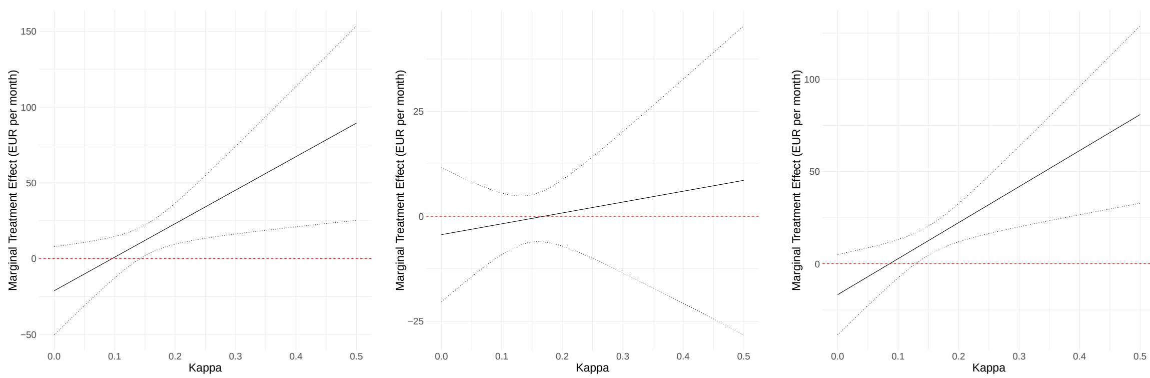
(d) Observed Component, Income Benefits



Note: The figures a, and b present the relationship between the marginal treatment effect on any new benefit take-up and the unobserved cost from participating to the meeting with social services. Take-up is measured using the preferred measure, a dummy variable which is 1 if the individual has was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. It is measured over six months since the start of the intervention. Panel (a) presents the results for family benefits, and Panel b for income benefits only. The linear relationship is obtained from the estimates in Table 5, evaluated at the mean of the relevant sample covariates. 90% confidence intervals plotted. Standard errors are obtained through 500 bootstrap replications of the first and second stages. See Section 4 for more details. The figures c and d presents the relationship between the observable component of the MTE (i.e the treatment heterogeneity based on observables) and the propensity to attend the meeting for family benefits (Panel c) and income benefits (Panel d). The observed component of the MTE is computed as $X_i\,c$ from equation 6. We have formed 50 bins of the propensity score (of 1 points width), and computed the averages $X_i\,c$ in each bin, which are represented by the black points. The blue line is a linear adjustment.

Figure A3: Marginal treatment effect (MTE) on total value of benefits, sample known to social services at baseline

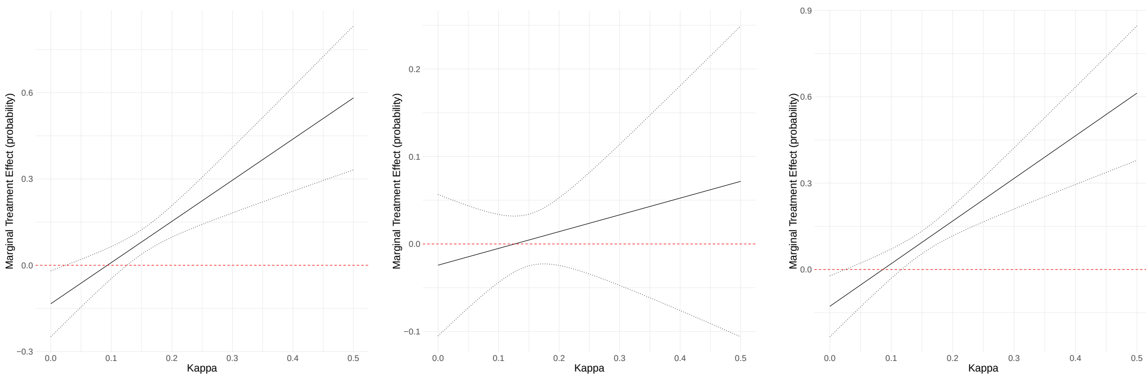
(a) All benefits (b) Family Benefits (c) Income Benefits



Note: This figure presents the relationship between the marginal treatment effect on the monthly amount of benefits received (including zeros when no new benefit was received) and the unobserved cost from participating to the meeting with social services, estimated on the sample of individuals known to social services at baseline. Panel (a) presents the results for all benefits, Panel (b) for family benefits, and Panel (c) for income benefits only. The linear relationship is obtained from the estimates in Table B16, evaluated at the mean of the relevant sample covariates. 90% confidence intervals plotted. Standard errors are obtained through 500 bootstrap replications of the first and second stage. See Section 4 for more details.

Figure A4: Marginal treatment effect (MTE) on benefit take-up, sample known to social services at baseline

(a) All benefits (b) Family Benefits (c) Income Benefits



Note: The figure presents the relationship between the marginal treatment effect on new benefit take-up and the unobserved cost from participating to the meeting with social services, estimated on the sample of individuals known to social services at baseline. New benefit take-up is measured using the preferred measure, a dummy variable equal to 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. Panel (a) presents the results for all benefits, Panel (b) for family benefits, and Panel (c) for income benefits only. The linear relationship is obtained from the estimates in Table B17, evaluated at the mean of the relevant sample covariates. 90% confidence intervals plotted. Standard errors are obtained through 500 bootstrap replications of the first and second stage. See Section 4 for more details.

B Appendix Tables

Table B1: List of social benefits included in the study

Benefit Type	Name of the Benefit	Caisse / Agency
Panel A: Family Benefits		
Family	Allocation familiale (AF)	CNAF
Family	Allocation de soutien familial (ASF)	CNAF
Family	Prestation d'accueil du jeune enfant (PAJE)	CNAF
Family	Allocation de rentrée scolaire (ARS)	CNAF
Housing	Allocation personalisée au logement (APL)	CNAF
Housing	Allocation de logement familiale (ALF)	CNAF
Housing	Allocation de logement sociale (ALS)	CNAF
Disability	Allocation adulte handicapé (AAH)	CNAF
Disability	Allocation d'éducation de l'enfant handicapé	CNAF
Disability	Allocation supplémentaire d'invalidité (ASI)	CNAM
Panel B: Health Benefits		
Health Insurance	Aide à l'acquisition d'une couverture maladie complémentaire (ACS)	CNAM
Health Insurance	Couverture maladie universelle complémentaire (CMU-C)	CNAM
Panel C: Income benefits		
Out-of-work	Revenu de solidarité active (RSA)	CNAF
In-work	Prime d'activité (PA)	CNAF

Table B2: Baseline characteristics and balance tests

Variable	Control (1)	Phone (2)	In person (3)	F-test p-value (4)	N (5)
Panel A: Socio-demographic variables					
Married	0.3283	0.0012	0.0011	0.960	54418
Number of children (Pole Emploi)	0.7312	0.0094	-0.0156	0.220	54418
Age in years	35.2655	-0.0345	-0.0460	0.922	54418
Female	0.4798	0.0109	0.0062	0.113	54418
Education: High school or less	0.1764	-0.0045	-0.0032	0.489	54418
Education: Missing	0.0054	-0.0007	0.0007	0.296	54418
Education: CAP/BEP	0.3531	0.0050	0.0013	0.615	54418
Education: Bac general	0.2463	-0.0044	0.0006	0.588	54418
Education: Higher education	0.2242	0.0039	0.0013	0.690	54418
French national	0.8497	-0.0004	0.0006	0.974	54418
EU national	0.0386	0.0036	0.0019	0.195	54418
Non-EU European national	0.0056	-0.0001	-0.0007	0.673	54418
Rest of the world	0.1051	-0.0032	-0.0023	0.559	54418
Years of experience in target job	5.7094	0.0359	-0.0100	0.864	54418
Unemployment duration in months	11.6144	-0.1709	-0.3205	0.188	54418
Monthly unemployment benefit (€)	456.7166	6.8806	5.0500	0.351	54418
CNAF beneficiary	0.5761	-0.0012	-0.0022	0.912	54418
Log daily ref. salary (euros)	3.8524	0.0067	0.0029	0.569	54418
Panel B: Baseline benefit usage					
Any benefit at baseline	0.4760	0.0057	-0.0029	0.374	54418
Any family benefit at baseline	0.3655	0.0054	-0.0005	0.541	54418
Any health benefit at baseline	0.1623	0.0027	-0.0039	0.358	54418
Any income benefit at baseline	0.1689	0.0061	0.0015	0.329	54418
AF at baseline	0.1540	0.0019	-0.0036	0.463	54418
ASF at baseline	0.0358	-0.0002	-0.0003	0.986	54418
Any AL at baseline	0.3182	0.0045	-0.0008	0.614	54418
AAH at baseline	0.0121	-0.0016	0.0001	0.348	54418
AEEH at baseline	0.0089	0.0002	0.0009	0.663	54418
PAJE: Prime à la naissance	0.0150	-0.0006	-0.0009	0.753	54418
PAJE: Allocation de base	0.0655	0.0015	-0.0019	0.551	54418
PAJE: Prestation partagée d'éducation de l'enfant	0.0042	-0.0002	0.0001	0.941	54418
PAJE: Complément de libre choix du mode de garde	0.0014	-0.0001	-0.0004	0.538	54418
ACS at baseline	0.0017	-0.0001	0.0001	0.964	54418
CMUC at baseline	0.1614	0.0025	-0.0039	0.370	54418
ASI at baseline	0.0004	0.0001	0.0002	0.737	54418
RSA at baseline	0.0758	0.0040	0.0007	0.375	54418
PA at baseline	0.1067	0.0033	0.0004	0.610	54418

Note: The second column presents the corresponding control group mean for the row variable. The penultimate column shows the p-value associated with a joint test of equality between the treatment group that received an invitation for a phone meeting, the treatment group that received an invitation for an in-person meeting, and the control group.

Table B3: Balance tests, full sample

Variable	Control	Neutral face-to-face	Neutral phone	Information face-to-face	Information phone	Stigma face-to-face	Stigma phone	P-value joint F-test	N
Panel A: Socio-demographic variables									
Married	0.326	0.001	-0.003	0.007	0.007	-0.004	0.000	0.900	54418
Number of children (Pole Emploi)	0.734	-0.024	0.014	-0.010	0.025	-0.013	-0.011	0.497	54418
Age in years	35.270	0.122	0.072	-0.183	0.016	-0.079	-0.197	0.835	54418
Female	0.472	0.018	0.009	-0.002	0.016	0.003	0.008	0.187	54418
Education: High school or less	0.179	-0.010	-0.005	0.006	0.001	-0.006	-0.009	0.339	54418
Education: Missing	0.006	0.002	-0.002	0.001	0.001	-0.001	-0.001	0.065	54418
Education: CAP/BEP	0.350	0.007	0.004	-0.001	-0.002	-0.003	0.014	0.650	54418
Education: Bac general	0.249	-0.001	-0.003	-0.003	-0.006	0.006	-0.004	0.887	54418
Education: Higher education	0.222	0.004	0.005	-0.002	0.007	0.002	-0.001	0.927	54418
French national	0.850	0.008	0.005	-0.004	-0.009	-0.002	0.003	0.373	54418
EU national	0.036	0.001	-0.001	0.001	0.006	0.004	0.005	0.314	54418
Non-EU European national	0.006	-0.001	0.001	0.000	0.000	-0.001	-0.001	0.839	54418
Rest of the world	0.107	-0.009	-0.005	0.003	0.002	-0.001	-0.006	0.367	54418
Years of experience in target job	5.677	0.076	0.039	-0.071	-0.049	-0.036	0.119	0.884	54418
Unemployment duration in months	11.992	-0.304	-0.318	-0.529	0.359	-0.121	-0.557	0.111	54418
Monthly unemployment benefit (€)	435.544	3.287	9.897	4.401	12.258	7.588	-1.762	0.657	54418
CNAF beneficiary	0.579	-0.005	-0.002	0.005	-0.002	-0.007	0.000	0.947	54418
Panel B: Baseline benefit usage									
Log daily ref. salary (euros)	3.848	0.018	0.007	-0.002	-0.002	-0.008	0.014	0.290	54418
Any benefit at baseline	0.476	-0.003	0.008	-0.004	0.009	-0.002	0.000	0.845	54418
Any family benefit at baseline	0.364	-0.003	0.001	0.003	0.010	-0.002	0.005	0.892	54418
Any health benefit at baseline	0.163	-0.004	0.002	-0.002	0.003	-0.006	0.003	0.890	54418
Any income benefit at baseline	0.167	0.009	0.009	-0.003	0.006	-0.003	0.003	0.484	54418
AF at baseline	0.155	-0.009	0.001	0.003	0.009	-0.004	-0.005	0.342	54418
ASF at baseline	0.036	0.003	-0.003	-0.001	0.002	-0.003	0.001	0.658	54418
Any AL at baseline	0.317	-0.002	0.000	0.002	0.007	-0.003	0.006	0.940	54418
AAH at baseline	0.013	0.002	-0.002	-0.001	-0.001	0.000	-0.001	0.736	54418
AEEH at baseline	0.008	0.000	-0.001	0.001	0.003	0.002	-0.001	0.543	54418
PAJE: Prime à la naissance	0.016	-0.002	-0.001	0.000	0.002	-0.001	-0.003	0.701	54418
PAJE: Allocation de base	0.065	-0.006	-0.003	0.004	0.007	-0.004	0.001	0.212	54418
PAJE: Prestation partagée d'éducation de l'enfant	0.004	0.001	0.001	0.000	0.000	0.000	-0.001	0.677	54418
PAJE: Complément de libre choix du mode de garde	0.002	-0.001	0.000	-0.001	0.000	0.000	0.000	0.885	54418
ACS at baseline	0.002	0.000	0.000	0.001	-0.001	-0.001	0.000	0.224	54418
CMUC at baseline	0.163	-0.004	0.001	-0.002	0.004	-0.005	0.003	0.894	54418
ASI at baseline	0.000	0.001	0.000	0.000	0.000	0.000	0.000	0.011	54418
RSA at baseline	0.072	0.005	0.005	0.000	0.002	-0.003	0.005	0.613	54418
PA at baseline	0.108	0.005	0.007	-0.005	0.002	0.000	0.001	0.647	54418

Note: The first column presents the control group mean for the row variable. Each subsequent column presents the difference between the corresponding treatment group mean and the control group mean. The penultimate column presents the p-value of the joint F-test under the null of equality between the means of the control and all treatment groups.

Table B4: Balance tests, survey sample v full sample

Variable	Full sample	Survey sample	Difference	P-value
Panel A: Socio-demographic variables				
Married	0.327	0.318	-0.009	0.040
Number of children (Pole Emploi)	0.733	0.730	-0.003	0.815
Age in years	35.268	35.080	-0.187	0.080
Female	0.475	0.485	0.009	0.056
Education: High school or less	0.178	0.178	0.000	0.920
Education: Missing	0.005	0.007	0.001	0.053
Education: CAP/BEP	0.351	0.348	-0.003	0.449
Education: Bac general	0.248	0.243	-0.004	0.239
Education: Higher education	0.223	0.230	0.007	0.063
French national	0.850	0.852	0.002	0.586
EU national	0.037	0.034	-0.003	0.115
Non-EU European national	0.006	0.006	0.000	0.624
Rest of the world	0.106	0.106	0.000	0.964
Years of experience in target job	5.692	5.512	-0.180	0.005
Unemployment duration in months	11.819	11.751	-0.069	0.655
Monthly unemployment benefit (€)	445.214	444.675	-0.539	0.909
CNAF beneficiary	0.578	0.580	0.002	0.689
Panel B: Baseline benefit usage				
Log daily ref. salary (euros)	3.850	3.859	0.009	0.101
Any benefit at baseline	0.476	0.473	-0.003	0.510
Any family benefit at baseline	0.365	0.363	-0.002	0.724
Any health benefit at baseline	0.163	0.162	-0.001	0.802
Any income benefit at baseline	0.168	0.167	0.000	0.929
AF at baseline	0.155	0.154	-0.001	0.891
ASF at baseline	0.036	0.038	0.002	0.237
Any AL at baseline	0.318	0.317	0.000	0.931
AAH at baseline	0.012	0.012	-0.001	0.514
AEEH at baseline	0.009	0.008	0.000	0.695
PAJE: Prime à la naissance	0.015	0.015	-0.001	0.538
PAJE: Allocation de base	0.065	0.068	0.003	0.253
PAJE: Prestation partagée d'éducation de l'enfant	0.004	0.005	0.001	0.236
PAJE: Complément de libre choix du mode de garde	0.001	0.001	-0.001	0.103
ACS at baseline	0.002	0.002	0.000	0.857
CMUC at baseline	0.162	0.161	-0.001	0.819
ASI at baseline	0.000	0.000	0.000	0.477
RSA at baseline	0.074	0.075	0.001	0.562
PA at baseline	0.107	0.106	-0.002	0.543

Note: This table presents the results of a balance test between the full sample and the sample selected for a phone survey. The second and third columns present the group means of the variable indicated in the first column for the full and survey samples respectively. The fourth column presents the difference between the full sample mean and the survey sample mean. The fifth column presents the p-value associated with the test that the difference between the two samples is zero.

Table B5: Balance tests, survey sample

Variable	Control	Neutral face-to-face	Neutral phone	Information face-to-face	Information phone	Stigma face-to-face	Stigma phone	P-value joint F-test	N
Panel A: Socio-demographic variables									
Married	0.347	-0.019	0.011	-0.008	0.016	-0.022	-0.026	0.238	9969
Number of children (Pole Emploi)	0.767	-0.018	0.062	0.019	0.064	0.020	-0.056	0.173	9969
Age in years	35.994	-0.357	-0.232	-0.160	0.356	-0.374	-0.735	0.454	9969
Female	0.499	0.017	0.040	0.002	0.035	0.015	0.006	0.235	9969
Education: High school or less	0.160	0.003	-0.016	0.020	-0.005	0.002	-0.010	0.407	9969
Education: Missing	0.007	-0.001	0.000	-0.003	0.000	-0.004	-0.003	0.553	9969
Education: CAP/BEP	0.353	0.009	0.008	0.016	-0.005	-0.024	0.004	0.575	9969
Education: Bac general	0.243	-0.002	0.004	-0.021	0.006	0.027	0.010	0.290	9969
Education: Higher education	0.244	-0.009	0.005	-0.015	0.005	-0.005	-0.005	0.915	9969
French national	0.869	0.000	-0.003	-0.014	-0.018	-0.002	-0.002	0.762	9969
EU national	0.024	0.003	0.000	0.011	0.003	0.007	0.005	0.650	9969
Non-EU European national	0.005	-0.002	0.001	-0.001	0.003	-0.002	-0.001	0.673	9969
Rest of the world	0.100	-0.001	0.001	0.002	0.013	-0.002	-0.002	0.922	9969
Years of experience in target job	5.731	0.058	-0.086	-0.237	0.323	0.262	0.092	0.574	9969
Unemployment duration in months	12.437	-0.990	-0.325	-1.347	0.226	-0.611	-1.094	0.140	9969
Monthly unemployment benefit (€)	439.777	27.136	-11.288	11.490	13.044	20.506	-8.608	0.400	9969
CNAF beneficiary	0.602	0.000	-0.003	0.011	0.004	-0.018	-0.036	0.308	9969
Panel B: Baseline benefit usage									
Log daily reference salary (euros)	3.862	-0.012	0.020	-0.028	0.003	0.003	-0.001	0.635	9969
Any benefit at baseline	0.477	0.022	0.029	0.022	0.029	-0.003	-0.025	0.066	9969
Any family benefit at baseline	0.376	0.004	0.032	0.012	0.027	-0.002	-0.013	0.268	9969
Any health benefit at baseline	0.158	0.007	0.005	0.028	-0.009	0.003	-0.017	0.129	9969
Any income benefit at baseline	0.162	0.029	0.015	0.014	0.010	0.008	-0.009	0.262	9969
AF at baseline	0.167	-0.011	0.017	0.006	0.008	-0.003	-0.019	0.323	9969
ASF at baseline	0.037	0.005	-0.002	0.013	0.008	0.003	0.004	0.629	9969
Any AL at baseline	0.324	0.008	0.031	0.011	0.027	-0.005	-0.006	0.314	9969
AAH at baseline	0.014	0.004	-0.002	-0.001	0.000	-0.007	0.000	0.269	9969
AEEH at baseline	0.008	0.002	0.002	0.004	0.002	0.007	-0.003	0.257	9969
PAJE: Prime à la naissance	0.018	0.000	-0.005	0.002	0.003	-0.006	-0.003	0.479	9969
PAJE: Allocation de base	0.073	-0.003	-0.005	0.003	0.009	-0.011	-0.003	0.712	9969
PAJE: Prestation partagée d'éducation de l'enfant	0.006	0.004	-0.002	0.002	-0.002	-0.004	-0.004	0.038	9969
PAJE: Complément de libre choix du mode de garde	0.001	-0.001	0.001	0.001	0.002	0.001	0.000	0.114	9969
ACS at baseline	0.002	0.002	0.001	0.003	0.000	-0.002	0.000	0.009	9969
CMUC at baseline	0.158	0.005	0.003	0.027	-0.008	0.003	-0.018	0.159	9969
ASI at baseline	0.000	0.000	0.000	0.001	0.001	0.000	0.001	0.694	9969
RSA at baseline	0.073	0.009	0.006	0.010	0.004	-0.004	-0.011	0.404	9969
PA at baseline	0.101	0.025	0.011	0.004	0.003	0.011	0.002	0.463	9969

Note: The first column presents the control group mean for the row variable. Each subsequent column presents the difference between the corresponding treatment group mean and the control group mean. The penultimate column presents the p-value of the joint F-test under the null of equality between the means of the control and all treatment groups.

Table B6: Treatment compliance

	Attended any RDVDD			Attended phone RDVDD		
	(1)	(2)	(3)	(4)	(5)	(6)
Any invitation	0.2134			0.0834		
	(0.0025)			(0.0017)		
Invited to phone meeting		0.2218			0.1010	
		(0.0037)			(0.0027)	
Invited to face to face meeting		0.2055			0.0671	
		(0.0035)			(0.0022)	
Stigma reduction letter			0.2109			0.0800
			(0.0044)			(0.0030)
Neutral letter			0.2210			0.0873
			(0.0044)			(0.0030)
Enhanced information letter			0.2078			0.0826
			(0.0043)			(0.0030)
Num.Obs.	54418	54418	54418	54418	54418	54418
R2	0.171	0.172	0.171	0.085	0.089	0.086
P-value Phone = In person	-	0.001	-	-	0	-
P-value Neutral = Info	-	-	0.036	-	-	0.271
P-value Info = Stigma	-	-	0.688	-	-	0.538
P-value Stigma = Neutral	-	-	0.095	-	-	0.087
Control group mean	0	0	0	0	0	0

Note: Controls selected using the double-lasso method. Standard errors robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the effects of receiving an invitation letter on the probability of attending the RDVDE (columns 1 thorough 3) and on attending a RDVDE by phone, estimated using a linear probability model. Columns 1 and 4 show the effect of having received any invitation. Columns 2 and 5 show the effects separately by type of RDVDE individual was invited to (choice between phone or face-to-face meeting vs face-to-face meeting only). Columns 3 and 6 show the effect by type of invitation letter. Rows labelled "Phone = In person", "Neutral = Info", "Info = Stigma" and "Stigma = Neutral" present the p-value of a Wald test of equality between the corresponding coefficients.

Table B7: Alternative measure of compliance: RDVDE meeting attendance and discussion of potential eligibility to social benefits

	Attended and benefit discussed			
	Any benefit (1)	Any family benefit (2)	Any health benefit (3)	Any income benefit (4)
Any invitation	0.1940	0.1394	0.1072	0.1568
	(0.0024)	(0.0021)	(0.0019)	(0.0023)
Control group mean	0	0	0	0
Observations	54,418	54,418	54,418	54,418

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effects of being invited to the meeting on a dummy variable equal to 1 for people who attended and whose potential eligibility for any benefit was discussed (Column 1), estimated using a linear probability model. Columns 2, 3 and 4 present the effect by benefit group.

Table B8: Effect of invitations to the RDVDE on benefit take-up

	Any new benefit take-up			
	Any benefit (1)	Family benefit (2)	Health benefit (3)	Income benefit (4)
Panel A: Over three months				
Any invitation	0.0140	0.0024	0.0011	0.0142
	(0.0034)	(0.0024)	(0.0019)	(0.0029)
Control group mean	0.267	0.11	0.076	0.154
Observations	54418	54418	54418	54418
Panel B: Over six months				
Any invitation	0.0174	0.0054	0.0017	0.0150
	(0.0037)	(0.0027)	(0.0023)	(0.0031)
Control group mean	0.267	0.11	0.076	0.154
Observations	54418	54418	54418	54418

Note: Controls selected using the double-lasso method. Standard errors robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. Panels A and B present the average treatment effect of receiving an invitation letter on the preferred measure of benefit receipt, three and six months after baseline, estimated using a linear probability model. The preferred measure is a dummy variable which is 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention, and that benefit was not received at baseline, and 0 otherwise.

Table B9: Effect of the invitation on individual benefit receipt

Benefit	Any invitation	Std. Error	N
Family benefits			
Basic family benefit (AF)	0.00012	(0.00119)	54418
Additional family benefit (ASF)	0.00055	(0.00094)	54418
Housing benefit (AL)	0.00522	(0.00231)	54418
Disability support (AAH)	0.00032	(0.00059)	54418
Child disability support (AEEH)	0.00063	(0.00047)	54418
Childcare support (PAJE)	0.00016	(0.00143)	54418
Health benefits			
Health insurance complement (ACS)	-0.00151	(0.00124)	54418
Basic health insurance (AF)	0.00368	(0.002)	54418
Disability supplement (ASI)	-0.00004	(0.00013)	54418
Income benefits			
Guaranteed income (RSA)	0.00634	(0.00215)	54418
Negative tax (PA)	0.0102	(0.00274)	54418

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effect on the probability of having individual benefits six months after the start of the treatment, estimated using a linear probability model.

Table B10: Effect of the invitation on alternative measures of benefit take-up

	Benefit obtained six months after			
	Any benefit	Family benefit	Health benefit	Income benefit
	(1)	(2)	(3)	(4)
Panel A: Stock measure				
Any invitation	0.0068	0.0082	0.0009	0.0069
	(0.0036)	(0.0034)	(0.0033)	(0.0033)
Control group mean	0.495	0.36	0.201	0.2
Observations	54,418	54,418	54,418	54,418
Panel B: Flow measure				
Any invitation	0.0110	0.0043	0.0039	0.0063
	(0.0035)	(0.0026)	(0.0020)	(0.0027)
Control group mean	0.225	0.1	0.058	0.116
Observations	54,418	54,418	54,418	54,418
Panel C: Preferred measure				
Any invitation	0.0174	0.0054	0.0017	0.0150
	(0.0037)	(0.0027)	(0.0023)	(0.0031)
Control group mean	0.267	0.11	0.076	0.154
Observations	54,418	54,418	54,418	54,418

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effects of receiving the invitation on benefits take-up six months after the start of the intervention measured in three different ways, using a linear probability model. The stock measure is a dummy variable equal to 1 if an individual is registered as receiving any benefit at endline, and 0 otherwise. The flow measure is a dummy variable equal to 1 if the individual is registered as receiving any benefit at endline which was not received at baseline, and 0 otherwise. The preferred measure is a dummy variable which is 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention, and that benefit was not received at baseline, and 0 otherwise.

Table B11: Effects of the invitation on the number of benefits opened

	Total number of benefits obtained six months after			
	Any benefit	Family benefit	Health benefit	Income benefit
	(1)	(2)	(3)	(4)
Panel A: Stock measure				
Any invitation	0.0191	0.0085	0.0015	0.0086
	(0.0090)	(0.0062)	(0.0034)	(0.0038)
Control group mean	1.013	0.586	0.207	0.22
Observations	54,418	54,418	54,418	54,418
Panel B: Flow measure				
Any invitation	0.0172	0.0065	0.0040	0.0069
	(0.0057)	(0.0037)	(0.0021)	(0.0031)
Control group mean	0.312	0.127	0.059	0.126
Observations	54,418	54,418	54,418	54,418
Panel C: Preferred measure				
Any invitation	0.0256	0.0071	0.0021	0.0163
	(0.0067)	(0.0039)	(0.0024)	(0.0038)
Control group mean	0.401	0.143	0.079	0.178
Observations	54,418	54,418	54,418	54,418

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effect of receiving the invitation on the number of benefits received six months after the start of the treatment. The various measures are dummy variables defined at the individual benefit level, and summed together by benefit group. The stock measure is a dummy variable equal to 1 if an individual is registered as receiving the specific benefit, and 0 otherwise. The flow measure is a dummy variable equal to 1 if the individual is registered as receiving the specific benefit which was not received at baseline, and 0 otherwise. The preferred measure is a dummy variable which is 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention, and that benefit was not received at baseline, and 0 otherwise.

Table B12: Effect of the invitation on the self-reported number of benefits received, by benefit group

	Total number of benefits obtained			
	All (1)	Family (2)	Health (3)	Income (4)
Any invitation	0.0287 (0.0279)	0.0019 (0.0175)	−0.0012 (0.0092)	0.0284 (0.0109)
Control group mean	1.294	0.781	0.227	0.286
Observations	9,969	9,969	9,969	9,969

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effect of receiving the invitation on the self-reported total number of benefits received based on survey responses. The dependent variable is the number of benefits reported as being received by the respondent or by someone in their household at the time of the survey (approximately three months after the intervention start).

Table B13: Effect of the different types of RDVDE invitations on benefit take-up

	Any new benefit take-up over six months			
	All benefits (1)	Family benefits (2)	Health benefits (3)	Income benefits (4)
Panel A: Phone or face-to-face invitation				
Invited to phone meeting	0.0187	0.0056	0.0037	0.0184
	(0.0047)	(0.0034)	(0.0029)	(0.0039)
Invited to face to face meeting	0.0160	0.0052	−0.0001	0.0121
	(0.0045)	(0.0033)	(0.0027)	(0.0038)
By phone = Face to face	0.621	0.915	0.248	0.173
Control group mean	0.267	0.11	0.076	0.154
Observations	54,418	54,418	54,418	54,418
Panel B: Type of invitation letter				
Neutral letter	0.0160	0.0050	0.0004	0.0147
	(0.0053)	(0.0038)	(0.0032)	(0.0045)
Enhanced information letter	0.0218	0.0051	0.0060	0.0162
	(0.0053)	(0.0039)	(0.0033)	(0.0045)
Stigma reduction letter	0.0140	0.0061	−0.0010	0.0142
	(0.0054)	(0.0039)	(0.0033)	(0.0045)
Neutral = Information	0.269	0.729	0.119	0.711
Information = Anti-stigma	0.219	0.983	0.069	0.765
Anti-stigma = Neutral	0.891	0.747	0.781	0.946
Control group mean	0.267	0.11	0.076	0.154
Observations	54,418	54,418	54,418	54,418

Note: Controls selected using the double-lasso method. Standard errors robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. Panels A and B present the average treatment effect of receiving an invitation letter on the preferred measure of benefit receipt, six months after baseline, estimated using a linear probability model. The preferred measure is a dummy variable which is 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention, and that benefit was not received at baseline, and 0 otherwise. Panel A presents the effect of receiving an invitation to a phone or a face-to-face meeting. Panel B presents the effect of the different letter types: neutral letter, letter with an information flyer and a link to an online simulator, letter with an anti-stigma message. Rows labelled "By phone = Face to face", "Neutral = Information", "Information = Anti-stigma" and "Anti-stigma = Neutral" present the p-value of a Wald test of equality between the corresponding coefficients.

Table B14: Heterogeneous effects of the invitation on compliance

	Attended meeting			
	Distance to CNAF agency (1)	Population 2 (2)	CAF beneficiary (3)	Log daily reference income (4)
Any invitation	0.2223	0.2184	0.1725	0.2509
	(0.0050)	(0.0037)	(0.0037)	(0.0167)
Het. variable x Any invitation	−0.0333	−0.0101	0.0707	−0.0098
	(0.0069)	(0.0053)	(0.0051)	(0.0043)
Invited to phone meeting	0.0166			
	(0.0074)			
Het. variable x Invited to phone meeting	−0.0008			
	(0.0102)			
Observations	54 418	54 418	54 418	54 418
Control group mean	0.0000	0.0000	0.0000	0.0000

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents a heterogeneity analysis of the effect of having received any invitation on meeting attendance, estimated using a linear probability model. For column 1, the heterogeneity variable is a binary indicator for whether the individual lives in a postcode whose mean distance to the nearest CNAF branch is higher than the median for her stratum. In column 2, the heterogeneity variable is a binary indicator for the individual being less than three months away from the end of their rights to unemployment benefits. In column 3, the heterogeneity variable is attending a meeting with a certified social worker, rather than a social benefits adviser. In column 4, the heterogeneity variable is already having been registered as a beneficiary of CNAF administered benefits at least once. For column 5, the heterogeneity variable is the log of the daily reference income used in determining unemployment benefit amounts.

Table B15: Heterogeneous effects of the invitation on benefit take-up

	Any new benefit received over six months			
	Distance to CNAF agency (1)	Population 2 (2)	CAF beneficiary (3)	Log daily reference income (4)
Any invitation	0.0208	0.0130	0.0210	0.0619
	(0.0065)	(0.0049)	(0.0045)	(0.0248)
Het. variable x Any invitation	−0.0093	0.0087	−0.0063	−0.0115
	(0.0090)	(0.0068)	(0.0071)	(0.0063)
Invited to phone meeting	−0.0014			
	(0.0078)			
Het. variable x Invited to phone meeting	0.0079			
	(0.0109)			
Control group mean	0.2670	0.2670	0.2670	0.2670
Observations	54 418	54 418	54 418	54 418

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents a heterogeneity analysis of the effect of having received any invitation on meeting attendance, estimated using a linear probability model. For column 1, the heterogeneity variable is a binary indicator for whether the individual lives in a postcode whose mean distance to the nearest CNAF branch is higher than the median for her stratum. In column 2, the heterogeneity variable is a binary indicator for the individual being less than three months away from the end of their rights to unemployment benefits. In column 3, the heterogeneity variable is attending a meeting with a certified social worker, rather than a social benefits adviser. In column 4, the heterogeneity variable is already having been registered as a beneficiary of CNAF administered benefits at least once. For column 5, the heterogeneity variable is the log of the daily reference income used in determining unemployment benefit amounts.

Table B16: Marginal Treatment Effect on benefit amounts, sample known to social services

	EUR per month, any new benefit received over six months		
	Any benefit (1)	Family benefit (2)	Income benefit (3)
Propensity score	−13.8318	22.5738	−36.4056
	(21.5397)	(11.5492)	(16.2170)
Propensity score^2	109.4168	15.3563	94.0604
	(54.1795)	(30.5539)	(40.8109)
Strata fixed effects	Yes	Yes	Yes
Observations	31,444	31,444	31,444
R^2	0.0384	0.0220	0.0410

Note: This table presents the MTE on the monthly amount of newly claimed (not present at baseline) benefits received. Estimation covers the sample of individuals already known to social services at baseline. The propensity score is obtained by a linear probability model of meeting attendance, based on treatment assignment and controls selected by the double lasso procedure. Standard errors are obtained through 500 bootstrap replications of both stages. All specifications include strata fixed effects and controls used in the propensity score interacted with treatment probability. Control coefficients omitted for brevity.

Table B17: Marginal Treatment Effect estimates on benefit take-up, sample known to social services, measured using RNCPS data

	Any new benefit received over six months			
	Any benefit (1)	Any family benefit (2)	Any health benefit (3)	Any income benefit (4)
Propensity score	−0.0202	0.0620	0.1001	−0.1134
	(0.0914)	(0.0658)	(0.0609)	(0.0827)
Propensity score^2	0.4250	0.0317	−0.0180	0.5458
	(0.2197)	(0.1737)	(0.1551)	(0.2012)
Strata fixed effects	Yes	Yes	Yes	Yes
Observations	31,444	31,444	31,444	31,444
R^2	0.0328	0.0242	0.0312	0.0356

Note: Benefit take-up is measured using the preferred measure, a dummy variable equal to 1 if the individual was registered as receiving any benefit from the corresponding benefit group at any point since the start of the intervention and that benefit was not received at baseline, and 0 otherwise. The propensity score is obtained by a linear probability model of meeting attendance, based on treatment assignment and controls selected by lasso. Standard errors are obtained through 500 bootstrap replications of both stages. All specifications include strata fixed effects and controls used in the propensity score interacted with treatment probability. Control coefficients omitted for brevity.

Table B18: Marginal Treatment Effect first stage estimates

Variable	Main effect	Variable X Any invitation	Variable X Phone invitation
Female	0.002	0.018	0.004
	(0.001)	(0.008)	(0.011)
Higher Education	0.000	−0.013	0.006
	(0.001)	(0.009)	(0.013)
Age category 16 to 25	0.000	−0.083	0.024
	(0.001)	(0.010)	(0.015)
Age category 25 to 35	−0.001	−0.029	−0.004
	(0.001)	(0.009)	(0.014)
Age category 55 plus	0.001	0.054	−0.038
	(0.001)	(0.016)	(0.023)
Foreign national	0.007	0.021	−0.009
	(0.001)	(0.011)	(0.016)
Divorced or separated	−0.002	0.015	0.029
	(0.001)	(0.014)	(0.021)
Married	−0.002	0.006	0.025
	(0.001)	(0.009)	(0.013)
Seeks employment in Performing Arts ROME sector	0.006	−0.065	0.045
	(0.003)	(0.029)	(0.046)
Unemployment reason - Termination (Other)	0.001	0.007	0.030
	(0.001)	(0.013)	(0.019)
Unemployment reason - End of contract	0.000	−0.013	−0.007
	(0.001)	(0.009)	(0.013)
Unemployment reason - End of temporary worker assignment	0.004	−0.027	−0.012
	(0.001)	(0.012)	(0.017)
Unemployment reason - End of internship	0.006	0.035	−0.014
	(0.002)	(0.020)	(0.028)
Unemployment reason - End of illness or maternity	0.003	0.054	−0.028
	(0.002)	(0.020)	(0.028)
Unemployment reason - Released from prison	−0.003	−0.103	−0.037
	(0.005)	(0.049)	(0.067)
Monthly unemployment benefit amount - First quartile	−0.013	0.067	−0.026
	(0.001)	(0.012)	(0.017)
Monthly unemployment benefit amount - Second quartile	−0.014	0.036	−0.021
	(0.001)	(0.011)	(0.016)
Monthly unemployment benefit amount - Third quartile	−0.014	0.049	0.001
	(0.001)	(0.011)	(0.017)
Monthly unemployment benefit amount - Fourth quartile	−0.009	0.038	−0.009
	(0.001)	(0.012)	(0.017)
Seasonal contract sought	0.002	−0.069	0.034
	(0.002)	(0.019)	(0.029)
Temporary work sought	−0.003	0.032	−0.002
	(0.001)	(0.013)	(0.019)
Annual salary in previous job - Second quartile	−0.002	0.035	−0.016
	(0.001)	(0.009)	(0.013)
Annual salary in previous job - Fourth quartile	0.002	−0.013	0.001
	(0.001)	(0.009)	(0.014)
Known to social services at baseline	−0.002	0.043	0.018
	(0.001)	(0.007)	(0.011)
Length of unemployment spell - Second quartile	0.001	0.002	0.010
	(0.001)	(0.009)	(0.013)
Any invitation	0.181		
	(0.013)		
Phone invitation	−0.005		
	(0.019)		
Dist. (km) city of residence to closest CAF office		−0.001	0.000
		(0.001)	(0.001)
Individual at end of unemployment benefit rights		−0.015	0.011
		(0.008)	(0.012)
Num.Obs.	54418	54418	54418
R2	0.185	0.185	0.185
Std.Errors	Heteroskedasticity-robust	Heteroskedasticity-robust	Heteroskedasticity-robust
Strata fixed effects	Yes	Yes	Yes

Note: This table presents estimates of a linear probability model predicting compliance (attendance at the meeting), estimated on the full sample. Coefficients reported in all columns are part of the same regression. We use as instruments a dummy for the treatment group, and a dummy for the phone meeting group. We also include socio-demographic controls selected by the double robust lasso procedure. We interact the instruments with each of the controls, as well as with two stratification variables (distance to nearest CNAF office and nearing the end of unemployment benefit rights). The first column presents coefficients for each control variable, the second column presents coefficients on each control variable interacted with a dummy for the treatment group, the third column presents coefficients on each control variable interacted with a dummy for the phone invitation group. The specification also includes randomization strata fixed effects. Standard errors are calculated using a bootstrap with 500 replications.

Table B19: Balance tests for the online simulator

Variable	Control group mean	Invitation	Difference vs Control	P-value	Observations
Married	0.412	0.407	-0.004	0.529	40 000
Number of children	0.797	0.797	-0.001	0.965	40 000
Age	33.617	33.771	0.154	0.275	40 000
Female	0.506	0.508	0.002	0.756	40 000
Unfinished high-school or less	0.123	0.123	-0.001	0.872	40 000
Professional baccalaureate	0.306	0.318	0.012	0.154	40 000
General baccalaureate	0.258	0.256	-0.002	0.677	40 000
Higher education	0.229	0.224	-0.005	0.551	40 000
Foreigner	0.125	0.123	-0.001	0.882	40 000
ZUS resident	0.080	0.082	0.003	0.811	40 000
Years of experience in profession sought	5.328	5.268	-0.06	0.461	40 000
Unemployment duration in months	9.775	9.653	-0.122	0.340	40 000
Maximum benefit duration left (in days)	338.667	333.428	-5.238	0.091	30 597
Daily net benefit amount (in euros)	16.342	16.030	-0.312	0.104	40 000

Note: This table presents the average of each variable by treatment group, the difference between the invitation group and the control group and the p-value for the test under the null of no-difference. The unit of observation is the individual. The coefficients are obtained by regressing the row variable on the treatment variable (1=treatment group, 0=control group). For example, the first line shows that the proportion of married individuals in the control group is 41%, the same as in the treatment group, and that the difference between the two groups is not statistically significant.

Table B20: Effect of the invitation to the online simulator on the number of benefits opened

	Total number of benefits obtained over eight months			
	All benefits	Family benefits	Health benefits	Income benefits
Any invitation	0.0076	−0.0024	0.0059	0.0042
	(0.0154)	(0.0100)	(0.0048)	(0.0050)
Control group mean	0.908	0.602	0.129	0.178
Observations	40,000	40,000	40,000	40,000

Note: Controls selected using the double-lasso method. Standard errors are robust to heteroskedasticity. All specifications contain strata fixed effects. Control coefficients omitted for brevity. This table presents the treatment effects of being invited to use the online simulator on the total number of benefits obtained eight months after the start of the intervention.

C Letters

Figure C5: Neutral invitation to face-to-face meeting

Madame, Monsieur,

Vous étiez inscrit à Pôle Emploi au JJ MM AAAA. Vous avez peut-être droit à des prestations sociales et familiales dont vous ne bénéficiez pas encore.

Nous vous invitons à prendre contact, dès aujourd'hui, avec votre Caf en appelant le

<<Téléphone>>

Pour quoi faire ?

Bénéficier d'un rendez-vous personnalisé avec un professionnel de votre Caf pour étudier vos droits de manière détaillée.
Vous informer sur les prestations sociales et familiales auxquelles vous pouvez prétendre, mais dont vous ne bénéficiez peut-être pas encore.

Comment faire ?

Rien de plus simple.
En appelant le <<Téléphone>>, vous convenez d'un rendez-vous et obtenez immédiatement toutes les informations utiles pour vous y rendre.

Pour vous permettre d'accéder à l'ensemble de vos droits aux prestations familiales et sociales, appelez dès maintenant le **<<Téléphone>>** !

Nous sommes à votre service pour vous présenter les prestations sociales et familiales qui correspondent à votre situation. Vous êtes susceptible d'être contacté par un agent de la Caf à partir du numéro **<<Téléphone>>** pour vous donner toutes les informations utiles au déroulement du rendez-vous proposé.

Dans l'attente de votre appel, recevez, Madame, Monsieur, nos salutations respectueuses.

<<Prenom Nom>>
<<Fonction>>
<<Signature de Pôle Emploi>>

La proposition de rendez-vous personnalisé avec la Caf fait l'objet d'un traitement informatique de données à caractère personnel afin que la Caf dispose de la liste de personnes susceptibles de prendre ce rendez-vous. Conformément à la loi du 6 janvier 1978 modifiée, vous disposez d'un droit d'accès, de rectification et d'opposition pour des motifs légitimes, qui s'exerce par courrier électronique auprès du correspondant informatique et libertés à Pôle Emploi à l'adresse suivante: courriers-cnil-cada.00247@pole-emploi.fr

Figure C6: Invitation to face-to-face meeting with information

pôle emploi

ALLOCATIONS FAMILIALES

Madame, Monsieur,

Vous étiez inscrit à Pôle Emploi au JJ MM AAAA. Vous avez peut-être droit à des prestations sociales et familiales dont vous ne bénéficiez pas encore.

Nous vous invitons à prendre contact, dès aujourd'hui, avec votre Caf en appelant le

<<Téléphone>>

Pour quoi faire ?

Bénéficier d'un rendez-vous personnalisé avec un professionnel de votre Caf pour étudier vos droits de manière détaillée.
Vous informer sur les prestations sociales et familiales auxquelles vous pouvez prétendre, mais dont vous ne bénéficiez peut-être pas encore.

Comment faire ?

Rien de plus simple.
En appelant le <<Téléphone>>, vous convenez d'un rendez-vous et obtenez immédiatement toutes les informations utiles pour vous y rendre.

Pour vous permettre d'accéder à l'ensemble de vos droits aux prestations familiales et sociales, appelez dès maintenant le **<<Téléphone>>** !

Nous sommes à votre service pour vous présenter les prestations sociales et familiales qui correspondent à votre situation. Vous êtes susceptible d'être contacté par un agent de la Caf à partir du numéro **<<Téléphone>>** pour vous donner toutes les informations utiles au déroulement du rendez-vous proposé.

Au **dos de ce courrier,** découvrez quelques exemples de prestations susceptibles de vous concerner.

Dans l'attente de votre appel, recevez, Madame, Monsieur, nos salutations respectueuses.

<<Prenom Nom>>
<<Fonction>>
<<Signature de Pôle Emploi>>

… ➜

…

Connaissez-vous vos droits ?

Les prestations sociales et familiales qui peuvent s'appliquer à votre situation touchent tous les domaines de la vie : santé, famille, retraite, protection sociale, tarifs préférentiels d'accès à l'énergie, aides au logement…

Les situations présentées ci-dessous ne sont que des exemples.
Même si aucune d'elles ne correspond à votre situation personnelle, n'hésitez pas à appeler le **<<Téléphone>>** pour fixer un rendez-vous personnalisé avec un conseiller Caf.

"J'ai repris un emploi à temps partiel"

> Vous pouvez bénéficier de la Prime d'activité ou l'Allocation de Solidarité Spécifique (ASS), versées chaque mois.

"Je suis toujours à la recherche d'un emploi et je perçois peu ou pas d'allocations chômage"

> Vous pouvez peut-être bénéficier du revenu de solidarité active (Rsa) ou de l'Allocation de Solidarité Spécifique (ASS). Ces deux prestations vous garantissent un niveau minimum de revenu chaque mois.

"J'ai besoin de me soigner, mais j'hésite car je n'ai pas de complémentaire santé"

> Vous pouvez peut-être bénéficier de la Couverture Maladie Universelle Complémentaire (CMU-C) ou de l'Aide pour une Complémentaire Santé (ACS).

La CMU-C prend en charge une partie de vos dépenses de santé: consultations médicales, médicaments, soins à l'hôpital, lunettes ou prothèses auditives. Avec la CMU-C, vous n'avez pas à payer directement vos soins : ils sont réglés par la CMU-C.

L'ACS vous rembourse une partie de vos dépenses pour la souscription d'une complémentaire santé protégeant tous les membres de votre foyer. Comme avec la CMU-C, vous n'avez pas à payer directement vos soins.

IMPORTANT. Si vous bénéficiez de la CMU-C ou de l'ACS, vous bénéficiez de tarifs avantageux pour l'électricité et le gaz.

"Je viens d'entrer au chômage et j'ai des difficultés à payer mon loyer ou la mensualité de mon emprunt immobilier"

> Même si vous n'y aviez pas droit avant, vous avez peut-être désormais droit à l'allocation de logement familiale (Alf), l'allocation de logement social (Als) ou à l'aide personnalisée au logement (Apl). Ces allocations sont versées chaque mois.

La proposition de rendez-vous personnalisé avec la Caf fait l'objet d'un traitement informatique de données à caractère personnel afin que la Caf dispose de la liste de personnes susceptibles de prendre ce rendez-vous. Conformément à la loi du 6 janvier 1978 modifiée, vous disposez d'un droit d'accès, de rectification et d'opposition pour des motifs légitimes, qui s'exerce par courrier électronique auprès du correspondant informatique et libertés à Pôle Emploi à l'adresse suivante: courriers-cnil-cada.00247@pole-emploi.fr

Figure C7: Information flyer

Connaissez-vous vos droits ?

Voici quelques exemples* de situations qui pourraient être les vôtres. Elles ont été testées sur le site mes-aides.gouv.fr.

*Attention, ces montants sont donnés à titre indicatif. Le montant définitif vous sera communiqué au moment de l'étude de votre dossier. En effet, votre situation familiale, vos ressources ou celles de votre foyer peuvent être différentes par rapport aux éléments pris en compte dans ces exemples.

Sébastien est un auto-entrepreneur de 30 ans

Sébastien, célibataire, est inscrit à Pôle Emploi depuis presque 2 ans, après avoir travaillé au Smic pendant 6 ans en tant que vendeur dans un magasin d'informatique. En 2015, il a démarré une activité d'auto-entrepreneur qui a pour objet l'achat et la revente d'objets sur Internet. Cette activité a généré un chiffre d'affaires de 30 000 € en 2015. Mais en début 2016, il doit faire face à une baisse temporaire des commandes d'environ deux tiers. Pour les trois derniers mois, le chiffre d'affaires a été de 2 400 €. Il habite dans un meublé, sur Paris, en colocation avec des amis. Sa part du loyer est de 400 € par mois charges comprises.

Sébastien a droit à 770 € par mois d'aides, ainsi qu'à une protection complémentaire santé gratuite :

- *Prime d'activité (250 €/mois)*
- *Revenu de solidarité active (Rsa, 520 €/mois)*
- *Couverture Maladie Universelle Complémentaire (CMU-C)*

Marion (25 ans) et son compagnon, Thomas (25 ans)

Ils habitent ensemble, à Lyon, dans un studio meublé qu'ils louent à 500 €/mois. Marion a travaillé 2 ans au Smic à temps complet. Son dernier CDD s'est terminé il y a 4 mois. Elle a perçu des allocations chômage d'un montant mensuel de 650 €. Depuis 1 mois, elle vient de retrouver un emploi à temps partiel (20 h par semaine) au Smic. Depuis 4 mois, Thomas est en stage rémunéré à 800 €/mois.

Marion et Thomas ont droit à 560 € d'aides par mois et un «chèque santé» de 400 €/an :

- *Aides au logement (150 €/mois)*
- *Prime d'activité (410 € / mois)*
- *Aide au paiement d'une complémentaire (ACS, 400 €/an)*

Christophe et Natalie ont 38 et 40 ans, ils vivent avec leurs deux enfants de 10 et 15 ans et ils ne travaillent pas

Christophe est au chômage depuis plus de 2 ans, après la fin d'un CDI payé 2 000 € net/mois, et Natalie a choisi de ne plus travailler après la naissance de ses enfants. Ils sont propriétaires de leur logement sans charges de remboursement.

Christophe et Natalie ont droit à 1 160 € d'aides par mois ainsi qu'à une protection complémentaire santé gratuite :

- *Allocations familiales (130 €/mois)*
- *Allocation de Solidarité Spécifique (ASS, 490 €/mois)*
- *Revenu de solidarité active (Rsa, 480 €/mois)*
- *Bourse de collège (360 €/an)*
- *Bourse de lycée (360 €/an)*
- *Couverture Maladie Universelle Complémentaire (CMU-C)*

Sandrine, 39 ans, vient de se séparer de son conjoint et a trouvé un emploi à temps partiel

Sandrine élève seule sa fille de 12 ans, à Quimper. Elle n'a pas travaillé pendant les 10 dernières années, et elle s'est inscrite à Pôle Emploi. La semaine dernière, elle a trouvé une activité à temps partiel de 15 h par semaine au Smic. Elle loue un trois pièces 500 €/mois.

Sandrine a droit à 1 130 € d'aides par mois ainsi qu'à une protection complémentaire santé gratuite :

- *Aides au logement (350 €/mois)*
- *Allocation de soutien familial (Asf, 100 €/mois)*
- *Allocation de Solidarité Spécifique (ASS, 490 €/mois)*
- *Revenu de solidarité active (Rsa, 190 €/mois)*
- *Couverture Maladie Universelle Complémentaire (CMU-C)*

Et vous, à quoi avez-vous droit ?
Pour le savoir, allez sur le site mes-aides.gouv.fr et répondez à l'invitation de votre Caf.

Figure C8: Stigma reduction flyer

Les aides sociales sont un droit, elles sont là pour vous aider dans les moments difficiles

D'anciens bénéficiaires témoignent

Muriel,

Agent administratif, 52 ans

À la naissance de mon fils, j'ai choisi de mettre entre parenthèses ma vie professionnelle pour m'occuper de lui. Durant un temps, le père de celui-ci subvenait à nos besoins. Cependant, il y a quelques années, il n'a plus été en mesure de nous verser de l'argent. Je me suis retrouvée brutalement avec mon fils sans ressources et sans logement. J'ai donc entrepris plusieurs démarches dont une demande de RSA auprès de la Caf. Cette prestation m'a permis de faire face à une partie de mes difficultés financières et d'envisager plus sereinement la recherche d'un logement et d'un emploi. **Aujourd'hui, je suis en contrat dans l'administration et j'ai retrouvé un appartement.**

Olivier,

Ouvrier agricole, 45 ans

Lorsque ma compagne est décédée il y a 2 ans, je me suis retrouvé un peu perdu face aux démarches administratives. Certaines de mes aides ont été suspendues. J'ai eu rendez-vous avec une assistante sociale de la Caf. Celle-ci m'a accompagné pour résoudre les problèmes liés à mon dossier. Son intervention m'a aidé pour solder ma dette locative et j'ai pu bénéficier de l'allocation logement et de l'allocation de soutien familial auxquelles j'avais droit. **Ces aides m'ont aidé à surmonter certaines des difficultés financières que j'ai pu rencontrer à cette période de ma vie.**

Ils ont rencontré des difficultés financières

Martine,

Hôtesse administrative, 34 ans

Suite à une séparation et à des charges de loyer élevées, J'ai commencé à contracter une dette locative qui s'est aggravée pendant l'arrêt maladie que l'on m'a prescrit durant ma grossesse. J'ai cependant pu reprendre le travail après la naissance de mon fils grâce à la Caf qui m'a aidé à trouver un mode de garde pour lequel je pouvais bénéficier d'aides financières. J'ai aussi fini par trouver un appartement au loyer moins élevé et j'ai pu bénéficier d'un prêt par l'intermédiaire de la Caf pour le meubler. **Aujourd'hui, j'ai donc un emploi et un logement où je vis avec mon fils de 4 ans.**

Les aides sociales leur ont permis de rebondir

Des aides existent pour la santé, le logement, l'éducation de vos enfants…

Figure C9: Neutral invitation to face-to-face or phone meeting

pôle emploi

ALLOCATIONS FAMILIALES

Madame, Monsieur,

Vous étiez inscrit à Pôle Emploi au JJ MM AAAA. Vous avez peut-être droit à des prestations sociales et familiales dont vous ne bénéficiez pas encore.

Nous vous invitons à prendre contact, dès aujourd'hui, avec votre Caf en appelant le

<<Téléphone>>

Pour quoi faire ?

Bénéficier d'un rendez-vous personnalisé avec un professionnel de votre Caf pour étudier vos droits de manière détaillée.
Vous informer sur les prestations sociales et familiales auxquelles vous pouvez prétendre, mais dont vous ne bénéficiez peut-être pas encore.

Comment faire ?

Vous avez le choix entre deux solutions

1- Vous vous déplacez sur le lieu de votre rendez-vous:
En appelant le **<<Téléphone>>**, vous convenez d'un rendez-vous et obtenez immédiatement toutes les informations pour vous y rendre.

2- Vous préférez un rendez-vous téléphonique:
Vous appelez le **<<Téléphone>>** pour convenir d'un rendez-vous téléphonique. Un professionnel de la Caf vous contactera et réalisera le rendez-vous par téléphone.

Pour vous permettre d'accéder à l'ensemble de vos droits aux prestations familiales et sociales, appelez dès maintenant le **<<Téléphone>>** !

Nous sommes à votre service pour vous présenter les prestations sociales et familiales qui correspondent à votre situation. Vous êtes susceptible d'être contacté par un agent de la Caf à partir du numéro **<<Téléphone>>** pour vous donner toutes les informations utiles au déroulement du rendez-vous proposé.

Dans l'attente de votre appel, recevez, Madame, Monsieur, nos salutations respectueuses.

<<Prénom Nom>>
<<Fonction>>
<<Signature de Pôle Emploi>>

La proposition de rendez-vous personnalisé avec la Caf fait l'objet d'un traitement informatique de données à caractère personnel afin que la Caf dispose de la liste de personnes susceptibles de prendre ce rendez-vous. Conformément à la loi du 6 janvier 1978 modifiée, vous disposez d'un droit d'accès, de rectification et d'opposition pour des motifs légitimes, qui s'exerce par courrier électronique auprès du correspondant informatique et libertés à Pôle Emploi à l'adresse suivante: courriers-cnil-cada.00247@pole-emploi.fr

D Targeting thresholds for unemployment benefits

In the targeting phase, we only keep individuals whose benefit amount is less than a threshold determined by the individual's marital status, number of children, and, where available, partner's earnings. There are two sets of thresholds. The first is used for all individuals. The second is used for individuals for whom we have information on the partner's earnings.

Table D21: Thresholds for all individuals

Marital status	**Number of children**	**Threshold (Euros per month)**
Not married	0	1220
Not married	1	1900
Not married	2	2260
Not married	3+	2760
Married	0	1500
Married	1	1900
Married	2	2260
Married	3+	2760

Table D22: Thresholds for individuals for whom data on partner's earnings is available

Marital status	**Number of children**	**Partner income (Euros per month)**	**Threshold (Euros per month)**
Married	0	$0 < 571 \leq$	890
Married	0	$571 < 685 \leq$	770
Married	0	$685 < 799 \leq$	650
Married	0	$799 < 913 \leq$	530
Married	0	$913 < 1027 \leq$	410
Married	0	$1027 < 1142 \leq$	290
Married	0	$1142 < 1256 \leq$	170
Married	0	$1256 < 1370 \leq$	50
Married	0	> 1370	0
Married	1	$0 < 571 \leq$	1300
Married	1	$571 < 685 \leq$	1180
Married	1	$685 < 799 \leq$	1060
Married	1	$799 < 913 \leq$	940
Married	1	$913 < 1027 \leq$	820
Married	1	$1027 < 1142 \leq$	700
Married	1	$1142 < 1256 \leq$	580
Married	1	$1256 < 1370 \leq$	460
Married	1	$1370 < 1484 \leq$	340
Married	1	$1484 < 1598 \leq$	220
Married	1	$1598 < 1712 \leq$	100
Married	1	> 1712	0
Married	2	$0 < 571 \leq$	1660
Married	2	$571 < 685 \leq$	1540
Married	2	$685 < 799 \leq$	1420
Married	2	$799 < 913 \leq$	1300
Married	2	$913 < 1027 \leq$	1180
Married	2	$1027 < 1142 \leq$	1060
Married	2	$1142 < 1256 \leq$	940
Married	2	$1256 < 1370 \leq$	820
Married	2	$1370 < 1484 \leq$	700
Married	2	$1484 < 1598 \leq$	580
Married	2	$1598 < 1712 \leq$	460
Married	2	$1712 < 1827 \leq$	340
Married	2	$1827 < 1941 \leq$	220
Married	2	$1941 < 2055 \leq$	100
Married	2	> 2055	0
Married	3+	$0 < 571 \leq$	2160
Married	3+	$571 < 685 \leq$	2050
Married	3+	$685 < 799 \leq$	1930
Married	3+	$799 < 913 \leq$	1810
Married	3+	$913 < 1027 \leq$	1590
Married	3+	$1027 < 1142 \leq$	1570
Married	3+	$1142 < 1256 \leq$	1450
Married	3+	$1256 < 1370 \leq$	1330
Married	3+	$1370 < 1484 \leq$	1210
Married	3+	$1484 < 1598 \leq$	1090
Married	3+	$1598 < 1712 \leq$	970
Married	3+	$1712 < 1827 \leq$	850
Married	3+	$1827 < 1941 \leq$	730
Married	3+	$1941 < 2055 \leq$	610
Married	3+	$2055 < 2169 \leq$	490
Married	3+	$2169 < 2283 \leq$	370
Married	3+	$2283 < 2397 \leq$	250
Married	3+	$2397 < 2512 \leq$	130
Married	3+	> 2512	0