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B. CRÉPON[1] – M. GURGAND[2]
T. KAMIONKA[3] – L. LEQUIEN[4]

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1 CREST (INSEE) and JPAL.
2 Paris School of Economics, CREST and JPAL.
3 CNRS and CREST.
4 DARES and CREST (INSEE).

Is Counseling Welfare Recipients Cost-Effective? Lessons from a Random Experiment

Bruno Crépon[*], Marc Gurgand[†]
Thierry Kamionka[‡], Laurent Lequien[§]

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Abstract

Job-search counseling is a potentially desirable labor market policy because it reduces market frictions, but it is strongly work-intensive as it requires repeated individual contact between job-seeker and case worker. Although it has become widely used, little is known about its cost-efficiency. This paper uses an experiment where individuals who have been on welfare for more than two years in a French district were randomly allocated to a counseling firm. We show that the policy causal impact is to increase employment and decrease the amounts of welfare transfers paid to the beneficiaries. However, the effects are small relative to the cost charged by the providing firm. Therefore, the net public cost, accounting for gains in welfare transfer payment, remains larger than reasonable social values that can be attached to having a former welfare recipient on a job. Although this is true for the policy as a whole, as implemented in this experiment, there is significant heterogeneity. In particular, it is more efficient and more cost-effective on a population of limited seniority on welfare.

[*]Crest-Insee and JPAL
[†]Paris School of Economics, Crest and JPAL
[‡]CNRS and Crest
[§]Dares and Crest-Insee

1 Introduction

Active labor market policies have received increasing attention over the last decades, both from policy makers and researchers. Training programs have been extensively studied, and so have incentive policies. More recently, policy interest has shifted to monitoring and counseling job-search, because it tends to directly decrease market frictions (rather than indirectly through financial incentives), an approach that has some socially desirable properties: Boone et al. (2007) argue that decreasing unemployment benefits is a second best policy when effort is not observable, but is dominated by monitoring and counseling if job-search can be observed and influenced *at reasonable cost*. The extent to which employment services are able to modify people's search effort or efficiency is an empirical issue. As indicated in the review by Card et al. (2010), evaluation of such policies is generally positive, and compares well with other types of labor market interventions. For instance, positive effects are found in Great Britain by Dolton and O'Neill (1996, 2002) and Blundell et al. (2004), in Denmark by Graversen and van Ours (2008), in the Netherlands by Gorter and Kalb (1996) (but not by van den Berg and van der Klaauw (2006) in that same country), or in Portugal by Centeno et al. (2009). In France, Crépon et al. (2005), Behaghel et al. (2009), Fougère et al. (2010) find that counseling policies increase job finding. However, those policies can be extremely costly, because they usually require hours of *individual* contact with a counselor or a case worker, and one may wonder if they are sustainable, in spite of their apparent efficacy. As mentioned by Card et al. (2010) and Kluve (2010), very few studies in active labor market policy evaluation provide information on costs. An important exception is Jespersen et al. (2008), but they do not cover job-search policies. As a result, to the best of our knowledge, cost-effectiveness of this potentially very costly class of intervention has never been formally evaluated in the literature, which therefore lacks a metric to judge whether measured effects should be considered high or low.

This paper considers a job-search counseling program targeted at individuals that have been on welfare for at least two years, in a French district. Since January 2004, French districts ("*Départements*") are in charge of welfare recipients and can propose innovative policies. In 2005, this district selected a private provider to run the counseling program. Access to the labor market is particularly difficult for the target population: it is generally very poorly educated, often suffers from healthcare problems, has long unemployment spells, has lost contact with the labor market, etc. Whether intensive counseling can be helpful to this particular population is an open question. We evaluate the impact of this policy on job access, using a random allocation of welfare recipients to the program. We also evaluate its cost-effectiveness. An argument for granting the program to a private provider, rather than rely on existing public social services, was that its increased effectiveness would compensate the additional cost, thanks to the induced reduction in welfare payment. We thus compare the financial net flows in the treated and control populations. Furthermore, by assigning reasonable social values to the employment status of former social recipients, we estimate a social net benefit of the policy.

We find some impact of this policy on employment and welfare transfers, and stronger ones among the population that has the shortest seniority on welfare. However, the cost of the policy is so high that it is never compensated by welfare payment reduction. To give orders of magnitude, welfare recipients receive on average about 1,000 € per trimester, whereas the provider receives a lump-sum payment of 2,200 € per enrolment into the program, plus additional payment per trimester when the person remains in employment. Even in the absence of additional payment, break-even would require a causal impact of more than 2 trimesters of employment, which is far from what we estimate. Overall, we typically find the net cost to be above 2,000 €. Even when a social value of employment is included, the policy fails to have a positive return. The policy is more efficient on individuals with "only" 2 to 4 years of seniority in welfare, but the net cost remains high at about 1,700 €.

In the French context, this finding remains hard to interpret, because the counseling market has only recently been opened to the private sector: it is possible that private providers price over their marginal cost in a market in infantry where competition is limited. On another hand, it is also possible that private providers are not particularly efficient: based on another random experiment, Behaghel et al. (2009) have shown that the public service is both more efficient and less costly at providing counseling to the unemployed. Generally, this result emphasizes that counseling and monitoring are very intensive policies, for which cost-effectiveness is an important issue.

On the methodological side, we discuss identification of a treatment effect when entry into the treatment can occur some time after randomization has taken place. In our design, a list of individuals is assigned into either control or treatment group. The treatment list is handed to the provider of counseling, but it takes time to get in touch with welfare recipients. Furthermore, some individuals from the control group may ask for treatment, and cannot be refused. Compliance to the random assignment is not perfect, and we must use some instrumental variable estimation. It is well known that, when there is essential heterogeneity[1], the estimated parameter is a Local Average Treatment Effect (Angrist and Imbens 1994), and must be interpreted as the

[1] i.e. when treatment impact is heterogenous and correlated with the propensity to enter treatment.

causal impact on a population of compliers. However, we show that, when there is dynamic entry into treatment, the LATE interpretation becomes more complicated. The reason is that a person from the treatment group can be a never taker at some date (as long as she did not enter treatment) and a complier later on (once she has entered treatment). Similarly, a person from the control group can be a complier, then an always taker. Therefore, the estimated impact parameter can be different at every point in time, because it is identified on a different population of compliers.

As a consequence, a model where the structural policy impact is changing with time since entry into treatment cannot be identified under essential heterogeneity. The reason is that both essential heterogeneity and such time-dependance of the impact imply parameter changes over time. One has to choose to assume either essential heterogeneity or time-varying impact. Depending on the assumption, parameter interpretation and estimation methods both differ. This is a different issue from the ones analyzed by Abbring and van den Berg (2005).

We first present the policy, the experimental design and the data. We then discuss identification and estimation issues along the lines just summarized. Results on employment status and cost-effectiveness are then presented. The final section concludes.

2 Policy and experimental design

2.1 A counseling scheme

Since 2004, French districts ("*Départements*") manage the welfare system that consists mainly in the payment of a guaranteed minimum income ("*Revenu Minimum d'Insertion*") to individuals with no or very low income, and in providing counsel and support to welfare recipients, traditionally either through in-house services or with the support of NGOs. As soon as 2005, an urban district decided to spend substantial resources on employment policy for welfare recipients living in that district. Its goal was to provide help and counsel to those likely to have the most severe difficulties on the job market and, it believed, had few chances to obtain a job without intensive assistance. The policy was thus targeted at those with at least 2 years of seniority into the welfare system. These recipients could be registered at the National Employment Agency and actively looking for a job, but had no obligation to do so.

As a novel feature at the time, the district elected assembly decided to select a private operator to implement this intensive counseling. The private market for job-search counseling was just starting to develop in France, particularly under the initiative of the Unemployment Benefit Agency (*Unédic*). Few operators had yet invested the area, and few were in a position to answer the tender.

The operator was supposed to contact a list of eligible individuals (on welfare since at least two years), and counsel them until they had found a job. With a letter or a phone call, the company explained that it contacted them on behalf of the district authority to help them find a job. It also invited them to a collective meeting where they would have more information on the counseling scheme and could enlist into the programme. Enrollment was voluntary, and welfare recipients could refuse to attend this meeting or enter treatment. When individuals were enrolled, the provider would allocate them a unique counselor that would meet him or her at least once a week in the company's offices. Once the person was employed, the counselor would follow up on the job.

Payment to the operator included a lump-sum for each individual counseled, and increasing bonuses if she worked during 3, 6 or 9 consecutive months between her enlistment and the end of the experiment in October 2007. The lump-sum was 2,200 €; additional payment was 1,600 € after 3 months employment, plus an additional 500 € after 6 months and 500 € more after 9 months.

2.2 Experimental design and take-up

From the outset, the district considered that this policy was experimental. The decision to continue or not such a policy was conditioned by the results obtained during the experimental period. Clearly, the district was unsure about the effectiveness of the policy. In order to provide rigorous evaluation, we proposed to assign individuals randomly into the list that would be transmitted to the operator. Randomization took place between March and October 2006. All welfare recipients in the district with at least two years of seniority into the welfare system at the moment of randomization were eligible. Each eligible individual had a 75% chance of being affected to the treatment group, and 25% chance to the control group.

The first random draw occurred in March 2006, and concerned the 14,980 welfare recipients fulfilling the seniority requirement between December 2005 and February 2006. Among them, 11,222 individuals were randomly assigned to the treatment group, and the rest (3,758) to the control group. This constituted the first wave of entry into the experiment.

The second wave included welfare recipients who became eligible between March and May 2006: 1,176 individuals were randomly affected to the treatment group, and 395 to the control group. Likewise, a third random draw concerned those becoming eligible between the second wave and October 2006: 838 persons were added to the treatment group, and 277 to the control group.

From November 2006 on, entry into the experiment was not possible anymore.

These three waves of entry cumulate a total of 17,666 individuals taking part into the experiment. The treatment group includes 13,236 individuals, and 4,430 belong to the control group. As expected, this repartition is extremely close to the theoretical proportions induced by a 75% / 25% random draw.

The experiment ended in October 2007. Therefore wave 1 was present in the experiment during 6 trimesters, whereas waves 2 and 3 lasted only 5 and 4 trimesters respectively.

All our data is from administrative files on welfare recipients. They include a limited set of individual characteristics, but an accurate series of transfer payments, and an employment status variable. This information is complemented with the treatment/control variable that we generated, and it is merged with the list of individuals that entered treatment at every point in time. This list is established by the provider for billing purposes, therefore it is very reliable.

Table 1 presents descriptive statistics on the two randomized groups: proportion of women, age, number of children, seniority into the welfare system, employment rates in the last two months before entry into the experiment, and the amount of welfare transfer received during the four trimesters before entry into the experiment. As expected, these two groups are statistically similar along all dimensions. One noticeable fact is that the first wave constitutes 85% of the total sample.

Figure 1 plots the cumulative distribution of seniority in wave 1, measured at entry into the experiment in March 2006. Half of this subsample has between 2 and 4 years of seniority in the welfare system. 35% have more than 6 years of seniority, and some individuals are present in the welfare system since its creation. As will be observed later, this is a source of significant heterogeneity in treatment impact.

The private operator was to offer counseling to all individuals in the treatment group. In reality, enrollment was on a voluntary basis, and a large majority of the treatment group did not sign up. Indeed the operator had difficulties first to contact eligible welfare recipients, and then to convince them to enter the counseling program. As a result, only 17% of the treatment group in waves 1 and 2 were actually treated at some point (Figures 2 and 3). This participation rate is slightly lower in wave 3, certainly because the experiment went to an end sooner for that wave (Figure 4).

The control group was supposed to be untreated: no advertising on the counseling scheme was targeted towards them, and the private operator was not supposed to contact them. However, some individuals did hear about the policy, and specifically asked to be included into the program. In that case, the private operator was allowed to enroll these individuals, even if they were part of the control group. This happened for about 6% of the control group in waves 1 and 2, and a similar pattern exists for wave 3.

This low take-up, and the limited differential between individuals assigned to treatment and control group, is a source of low statistical precision in spite of the large sample size. Moreover, the fact that take-up is varying very strongly over time, as apparent from the figures, has important consequences for the identification strategy. The next section presents the outcomes and net cost parameters of interest and discusses identification in such a context.

3 A cost-benefit analysis

In the above design, there is one treatment that can have impact on three outcomes: the employment status of the beneficiary, e, the welfare transfer received by the beneficiary, paid on the public budget, T, and the price paid, also on the public budget, to the private provider of the policy, C. Both e and T can be defined on a monthly or quarterly basis, with calendar time indexed by t. For each period, we can define two counterfactuals for transfer and employment outcomes:

$$\left.\begin{array}{c} e_t^0 \\ T_t^0 \end{array}\right\} \text{ if the person has not entered treatment by time } t$$
$$\left.\begin{array}{c} e_t^1 \\ T_t^1 \end{array}\right\} \text{ if the person has entered treatment by time } t$$

It is clear that the counterfactual transfers received depend themselves on the chances of employment with and without treatment, but we don't need to make this structural relation explicit for the present purpose. The counterfactual price paid to the provider has two components: a lump-sum when the beneficiary enters treatment (A), and a premium when she gets a job during treatment window and keeps it a sufficient number of months (B). Call D the treatment window, that is the period during which the provider is in charge of the beneficiary and assume that a person entered treatment at date s. Whether the provider receives the premium B is a specific function of employment status over this period: call this function $I\left(\{e_t^1\}_{s\leq t\leq s+D}\right)$. Then, we can define the counterfactual payment to the provider:

$C_s^0 = 0$ if the person has not entered treatment by time s

$C_s^1 {=} A{+}B{\times}I\left(\{e_t^1\}_{s\leq t\leq s+D}\right)$ if the person has entered treatment at time s

If we restrict ourselves to the treatment window, we can compute the net public cost of sending someone into treatment, that is the price paid to the provider minus the possible cumulative gains in welfare payments:

$$C_s^1 - \sum_{s \le t \le s+D} \left(T_t^0 - T_t^1\right)$$

If the the policy is meant to be self-financed, then it should be continued only if this net public cost is zero or negative. This can be judged by testing:

$$E\left(C_s^1\right) \le \sum_{s \le t \le s+D} \left[E\left(T_t^0\right) - E\left(T_t^1\right)\right]$$

where the means can be conditioned on some relevant population. This is to say that the reduction in welfare transfers resulting from better access to jobs under the policy must fully compensate for the direct cost of it. Notice that it is not necessary to estimate separately the terms A and B in C^1 in order to compute the expected cost: it is observed directly in the treated group.

Otherwise, if the public decision maker is ready to pay in order to get welfare beneficiaries enter employment, then the counterpart to this cost is the change in employment status generated by the policy:

$$\sum_{s \le t \le s+D} \left(e_t^1 - e_t^0\right)$$

This is the policy causal impact on employment. Assume that the decision maker gives a monthly value W to the fact that a welfare recipient is employed rather than non-employed. Then, the policy is acceptable as long as:

$$W \cdot \left\{ \sum_{s \le t \le s+D} E\left(e_t^1\right) - E\left(e_t^0\right) \right\} \ge E\left(C_s^1\right) - \sum_{s \le t \le s+D} \left[E\left(T_t^0\right) - E\left(T_t^1\right)\right] \quad (1)$$

Based on this, and provided that we can identify the counterfactual means contained in this expression, we can follow two routes: one is to compute the value W that can justify the policy, given the other parameters. We would however have a very imprecise estimation of W. Another is to fix W using alternative spending on employment policies or the market value of a job, or any other amount that can be considered a social value to having someone on the job, and test that the causal impact on employment, $\left(e_t^1 - e_t^0\right)$, is sufficiently large to justify the policy.

Notice that we will have little information on employment and welfare transfers effects outside the treatment window D, because our data has limited length. If one believes that treatment has lasting effects, then we may understate the returns to the policy. However, such lasting effects are rarely found in the literature (e.g. Card and Hyslop 2005). Moreover, our causal impacts will be small, so that this will not be a serious issue in practice.

4 Identification and estimation

Evaluating the general return to the policy along the above lines requires identification of the counterfactuals, e^0, e^1, T^0, T^1, C^1, at least for some population of interest. This is possible here in a very transparent way because the data is based on a random experiment. However, the date the treated enter treatment is not controlled: this dynamic structure of entry into treatment does restrict the model that can be identified. We can identify the model if there is either essential heterogeneity or if treatment effect is time-varying, but not both. This is a dichotomy that is present elsewhere in the evaluation literature based on duration models (Abbring and van den Berg 2003). We now discuss identification under several hypothesis.

Experiment starts when randomization has taken place and a given cohort of individuals may enter treatment any time. We use t for time elapsed since the start of the experiment. We note Z the random variable that takes value 1 if the individual lies on the list of persons that should be contacted in priority; $Z = 0$ otherwise. Z has been randomly determined and is constant over time. As we have previously shown, a significant number of persons did not comply with either treatment intention: few people from the list actually entered treatment during our period of observation and some people not on the list did enter treatment. Thus compliance appears to be dynamic in this framework: the same person, whatever its randomized status in the trial, may be untreated in t but treated in $t+1$, therefore a complier or a non-complier depending on time.

Define x_t an indicator variable that takes value 1 if an individual has entered treatment by time t since the start of the experiment (and zero otherwise). We thus have $E(x_t) = p(t)$, where $p(t)$ is the cumulative function of entry into treatment.

In this section, we use y as a generic outcome variable, representing either e, Tor C^1. Counterfactual outcomes with or without treatment respectively are $y^1_{t,s}$ and y^0_t, where t is time elapsed since randomization and s is the date at which individual actual entry into treatment took place. We will consider two cases. In this section and section 4.1, we first assume that counterfactual output $y^1_{t,s}$ (thus also impact) does *not* depend on s, ie length of exposure to treatment. In sections 4.2 and 4.3 we then assume that it does depend on s.

First assume, therefore, $y^1_{t,s} = y^1_t$. By definition of the counterfactuals, we observe:

$$y_t = x_t y^1_t + (1 - x_t) y^0_t$$

We can always assume that Z is independent from $\{y^1_t, y^0_t, x_t(Z{=}0), x_t(Z{=}1)\}$.

We have:

$$E(y_t|Z) = E(y^0_t|Z) + E\left(x_t(y^1_t - y^0_t)|Z\right)$$

If we assume that the distribution of treatment effect, $\{y^1_t - y^0_t\}$ is independent from the distribution of entry into treatment $\{x_t\}$,[2] this simplifies to:

$$\begin{aligned} E(y_t|Z) &= E(y_t^0) + E(y_t^1 - y_t^0)E(x_t|Z) \\ &= E(y_t^0) + E(y_t^1 - y_t^0)p(t|Z) \end{aligned}$$

Therefore, we have the following result:

Result 1

If counterfactual y_t^1 is not indexed on the date of entry into treatment s, and the date of entry into treatment is not correlated with the program impact $y_t^1 - y_t^0$, then the Wald estimator measures the average program impact at date t:

$$\frac{E(y_t|Z=1) - E(y_t|Z=0)}{p(t|Z=1) - p(t|Z=0)} = E(y_t^1 - y_t^0)$$

This estimator depends on t only through the time dependance of y_t^0 and y_t^1.

4.1 Essential heterogeneity

The modern evaluation literature is careful to the fact that entry into treatment may not be unrelated to the benefit from treatment. As a result, $\{y_t^1 - y_t^0\}$ may not be independent from $\{x_t\}$, a situation labeled "essential heterogeneity". In that case, we can identify a Local Average Treatment Effect in the sense of Angrist and Imbens (1994) if we assume a monotonicity assumption. Call $x_t(Z)$ the counterfactual treatment situation of any individual at time t depending on the intention to treat variable Z. We assume:

$$x_t(Z=1) \geq x_t(Z=0) \qquad \forall t$$

[2]Notice that the fact that y_t^1 is not indexed on s does not imply that treatment effect could not be *correlated* with date of entry into treatment in the population, in the sense that those with high potential treatment impact tend to enter sooner (or the reverse).

This means that an individual that was not on the priority list who happens to be treated however, would also be treated if he were on the priority list. In that case, using independence of counterfactuals with respect to Z, we have:

$$
\begin{aligned}
& E(y_t|Z=1) - E(y_t|Z=0) \\
&= E(x_t(y_t^1 - y_t^0)|Z=1) - E(x_t(y_t^1 - y_t^0)|Z=0) \\
&= E(x_t(Z=1)(y_t^1 - y_t^0)) - E(x_t(Z=0)(y_t^1 - y_t^0)) \\
&= E([x_t(Z=1) - x_t(Z=0)]\,(y_t^1 - y_t^0)) \\
&= E((y_t^1 - y_t^0)|\,[x_t(Z=1) - x_t(Z=0)] = 1)\ P([x_t(Z=1) - x_t(Z=0)] = 1) \\
&\quad + 0 \times P([x_t(Z=1) - x_t(Z=0)] = 0) \\
&\quad + E(-(y_t^1 - y_t^0)|\,[x_t(Z=1) - x_t(Z=0)] = -1)\ P([x_t(Z=1) - x_t(Z=0)] = -1)
\end{aligned}
$$

Using monotonicity we have the following result:

Result 2

If counterfactual y_t^1 is not indexed on the date of entry into treatment s; if the date of entry into treatment can be correlated with the program impact $y_t^1 - y_t^0$; and under the above monotonicity assumption, the Wald estimator measures the average program impact at date t on the complier population at that date:

$$
\frac{E(y_t|Z=1) - E(y_t|Z=0)}{p(t|Z=1) - p(t|Z=0)} \;=\; E\left[(y_t^1 - y_t^0)|x_t(Z=1) = 1, x_t(Z=0) = 0\right]
$$

This estimator depends on t through the time dependance of y_t^0 and y_t^1 and changes in the complier population.

At any time t, treatment effect is identified over the population of compliers, as is necessarily the case under essential heterogeneity. Notice, however, that “complier” is a changing status. The same person can be a complier at t (if $x_t(Z=1)=1$, $x_t(Z=0)=0$), an always taker at $t+1$ (if $x_{t+1}(Z=1)=1$, $x_{t+1}(Z=0)=1$) and a never taker at $t-1$ (if $x_{t-1}(Z=1)=0$, $x_{t-1}(Z=0)=0$). The parameter is thus not identified over a stable population, and it can be time-varying for this reason.

4.2 Variable effect and no essential heterogeneity

In the above model, the policy impact, $y_t^1-y_t^0$, is not indexed on the time elapsed *since entry into treatment*. We now consider this important possibility. We allow counterfactual $y_{t,s}^1$ to actually depend on s and we assume that if the person entered treatment at time s, then treatment effect at time $t \geq s$ is $y_{t,s}^1-y_t^0=\delta_{t-s}$. We thus assume that the effect of program participation now depends on the length of exposure to treatment $t-s$, instead of depending on t as in the previous section. In that case:

$$y_t = y_t^0 + \sum_{s=1}^{t} \delta_{t-s} \left[x_s - x_{s-1}\right]$$

(with $x_0 \equiv 0$).

If we assume that there is no essential heterogeneity, δ_{t-s} is independent from $\{x_t\}$ and we can write:

$$\begin{aligned} E(y_t|Z) &= E(y_t^0) + \sum_{s=1}^{t} E(\delta_{t-s}) E(\left[x_s - x_{s-1}\right]|Z) \\ &= E(y_t^0) + \sum_{s=1}^{t} E(\delta_{t-s}) \left[p(s|Z) - p(s-1|Z)\right] \\ &= E(y_t^0) + \sum_{s=1}^{t} E(\delta_{t-s}) f(s|Z) \end{aligned}$$

where $f(s|Z)=[p(s|Z)-p(s-1|Z)]$ is the discrete-time equivalent to a density of entry into treatment. The whole sequence of $E(\delta_{t-s})$ is thus identified.

For instance:

$$E(y_1|Z=1) - E(y_1|Z=0) \; = \; E(\delta_0) \times [f(1|Z=1) - f(1|Z=0)]$$

identifies $E(\delta_0)$. Then:

$$\begin{aligned} E(y_2|Z=1) - E(y_2|Z=0) \; &= \; E(\delta_0) \times [f(2|Z=1) - f(2|Z=0)] \\ &\quad + E(\delta_1) \times [f(1|Z=1) - f(1|Z=0)] \end{aligned}$$

identifies $E(\delta_1)$, and so on. Therefore, we have:

Result 3

If counterfactual $y^1_{t,s}$ is indexed on the date of entry into treatment s such that impact is $y^1_{t,s} - y^0_t = \delta_{t-s}$; and if the date of entry into treatment is independent from the program impact, then it is possible to identify the set of parameters $E(\delta_\tau)$ for all observable values of τ. The estimator is an iterative version of the Wald estimator.

This estimator depends only on exposure to treatment $t - s$.

4.3 Variable effect and essential heterogeneity

If essential heterogeneity is present in addition to variable treatment effect, we can no longer identify a meaningful parameter, even under a monotonicity assumption. Using the notation $\Delta x_t = [x_s - x_{s-1}]$, we have:

$$\begin{aligned} & E(y_t|Z=1) - E(y_t|Z=0) \\ & \qquad = \; \sum_{s=1}^{t} E(\delta_{t-s}\,[x_s - x_{s-1}]\,|Z=1) - E(\delta_{t-s}\,[x_s - x_{s-1}]\,|Z=0) \\ & \qquad = \; \sum_{s=1}^{t} E(\Delta x_s(Z=1)\delta_{t-s}) - E(\Delta x_s(Z=0)\delta_{t-s}) \\ & \qquad = \; \sum_{s=1}^{t} E(\delta_{t-s}\,[\Delta x_s(Z=1) - \Delta x_s(Z=0)]) \end{aligned}$$

Monotonicity is not sufficient because the two following situations are compatible with monotonicity:

1. $\Delta x_s(Z=1)=1$ and $\Delta x_s(Z=0)=0$, which is true for people who are never takers in $s-1$ and compliers in s;

2. $\Delta x_s(Z=1)=0$ and $\Delta x_s(Z=0)=1$, which is true for people who are compliers in $s-1$ and always takers in s.

Therefore:

Result 4

If essential heterogeneity is present in addition to variable treatment effect, we can no longer identify a meaningful parameter, even under a monotonicity assumption.

The general intuition for this negative result is that there are now two reasons why the estimated impact could vary with time: the population over which it is identified (the compliers) vary over time *and* the structural parameter varies with time. These two sources cannot be disentangled.

Abbring and van den Berg (2005) analyze some identification issues in social experiments with instrumental variable and duration outcomes. Their analysis of the dynamic take-up issue refers to a different setup than this one. They emphasize in particular that knowledge of their assignment status is likely to influence employment behavior of individuals even before they actually enter treatment. This generates a direct effect of the instrument on the outcome, that limits substantially identification. This does not seem relevant in our context, because people are unaware of their assignment status and not even aware that there is something as an assignment status. The restrictions to identification that we discuss here come from a different source, not examined by Abbring and van den Berg (2005), namely essential heterogeneity.

In practice, we will provide the two estimations, and they will prove to be hardly different, so that the distinction is one of principle in this application. In theory, though, it might be better to think in terms of a model with time-varying effects. There are two reasons for this. First, counseling is viewed by providers as a process that requires time to gain efficiency. In contrast, we cannot judge the extent to which self-selection into treatment could be based on actual treatment impact (especially given that the treatment is new and individuals have little way to make an appropriate judgment on its efficiency). Second, evaluation of equation (1) requires that treatment impact is measured over the same population at consecutive dates. This is not possible under essential heterogeneity.

4.4 Estimations

Estimations are run on a quarterly basis, and we have information on 4, 5 or 6 successive trimesters after entry into the experiment depending on the individual (see section 2.2). We set employment status e_t at trimester t equal to the number of months one has been employed during that trimester. So e_t takes values in $\{0, 1, 2, 3\}$. T_t is the amount of welfare transfer received during trimester t by the individual, and x_t is a dummy set to 1 if the individual has entered treatment by date t.

All the estimators presented in the previous section use combinations of reduced form (or "intention to treat") parameters, as does the Wald estimator. We thus estimate a set of reduced form parameters that are then combined according to the above formulas, in order to form the structural parameters of interest that will be shown in the tables.

Estimations are thus run in two successive steps. We first simultaneously estimate the following OLS regressions :

$$y = \alpha \ + \beta \ Z + \epsilon \tag{2}$$

where y can either be employment status e_t, welfare transfer T_t, cost C_t^1 or treatment status x_t at date t ($1 \leq t \leq 6$). $\hat{\beta}$ gives an estimation of $[E(y|Z{=}1) - E(y|Z{=}0)]$ for each of these 24 dependant variables (4 different outcomes and 6 time periods), along with a variance-covariance matrix robust to heteroscedasticity.

Sections 4.1 and 4.2 showed that the parameters of interest under essential heterogeneity and variable effect are functions of these 24 coefficients. So we use them to compute estimates of the causal impacts and their standard errors (using the delta method) for the 6 trimesters under both frameworks.

In order to improve efficiency, we also estimate the system of equations (2) with a set of additional covariates : age, sex, number of children, city of residence, amount of welfare transfer received in each of the 4 trimesters before entry into the experiment, and seniority into the welfare benefit system.

Eventually, we implement the test described in (1) to check whether the policy is profitable for the public budget. We compute

$$R = W \cdot \left\{ \sum_{s \leq t \leq s+D} E\left(e_t^1\right) - E\left(e_t^0\right) \right\} - E\left(C_s^1\right) + \sum_{s \leq t \leq s+D} \left[E\left(T_t^0\right) - E\left(T_t^1\right)\right]$$

for given values of W. The policy is considered to be profitable if R is statistically greater than 0. The variance of R is computed using the variance-covariance matrix of the simultaneously estimated causal effects on employment, transfers and costs at all relevant trimesters, obtained using equation (2).

In our context, three possibilities naturally arise when determining the value of W that measures the value that society or the administration gives to having someone on the job rather than not. The first one is the wage for the kind of job that this population is likely to enter, equal to the minimum wage (around 1,000 € per month). An alternative is the public cost of subsidized jobs targeted towards welfare recipients (*CIRMA* or *CAV*). This is a measure of the amount of money that is spent on alternative active employment policies to have low educated people on a job. Depending on the type of contract, the amount spent on public funds varies from around 450 € per month to 90% of the minimum wage. Eventually, we can focus exclusively on the district budget instead of society welfare, and therefore set W to 0 €. In total, we compute R for $W = 0$ € , $W = 450$ € and $W = 1,000$ €.

5 Results

Table 2 shows the results of the estimations on the whole sample. The coefficient at the intersection of row 'Trimester n' and column 'essential heterogeneity' is the LATE estimated on compliers of the n^{th} trimester under essential heterogeneity. The interpretation is different under a variable effect framework (the last two columns): the coefficient is the causal impact during the n^{th} trimester after entry into treatment.

The employment variable is based on a reporting card that each welfare beneficiary must send every trimester at some anniversary date. No adjustment is made for part-time employment (on which we have no reliable information): as our employment social value W is based on full-time work, we could overstate the value we give to employment if treatment induces more full-time jobs; but also understate it if it induces more part-time jobs. This does not affect the purely financial net cost evaluation, however (when $W = 0$). When individuals get out of welfare, we no longer have a reporting card: we assume that they are employed and we know they no longer receive transfers.

The amount of transfers is directly dependant on work income. Individuals with no income receive the full amount of transfer, that varies with family composition. As they earn more income, their amount of transfer is reduced (in most cases at a 50% implicit tax rate, but the details are more complex) up to a point where they are out of the program (for a single person with no child, this is about when she earns the full time minimum wage). It is important to note that transfers paid a given trimester are based on income reported a trimester earlier. This generates a lag between our employment variable and our transfer variable.

A similar pattern emerges in Table 2 for both essential heterogeneity and variable effects frameworks, whether or not covariates are included: the counseling program often has a positive impact on employment, but this effect is not significant at any date. A coefficient of 0.17 on trimester 2, for instance, means that an additional 0.17 month is worked over the trimester as a result of treatment. This seems a low figure, but the control group worked only 0.65 month during the first trimester[3], so the impact is relatively large in proportion. Unfortunately, because of low take-up, this is very imprecisely estimated.

Treatment effect on welfare transfers is always negative, but significantly different from 0 only on the third trimester. It is very unlikely that the counseling scheme should affect transfers otherwise than through employment (the only other determinant of transfer is household composition). Therefore,

[3]As apparent from Figures 2-4, only a very small share of the control group entered treatment during the first trimester of the experiment, so that this value is close to the counterfactual employment rate under no treatment for the whole population.

impacts on those two outcomes should be linked. Most of the employment impact seems to happen during the second and third trimesters, as if it took the first trimester for the counselor to prepare some action and set up a search program for the person. This same timing is apparent on the transfer variable. Remember that there is a trimester lag between employment and transfer, and that treatment can start at any date between two successive income reporting: as such, trimester 1 coefficients for transfer are only partly affected by the program. Indeed, the point estimates, although negative, are much smaller for this first trimester than later on. Thereafter, the shape of transfer impacts are related with the shape of employment impacts (with a lag). At trimester 3, the significant transfer impact is rather large at around 200€, representing about 20% of an average baseline transfer.

The average amount of money received per treated by the provider is close to 3,150€ in all specifications (see section 2.1 for a description of the payment scheme). The cost benefit analysis reveals a clear deficit for the public budget. This was expected, given that the average welfare payment is about 1,000 € per trimester and the program only has limited impact on employment and resulting transfers. Interestingly enough, the net public cost can be estimated quite precisely in spite of the large standard errors on most of the transfer and employment impacts. This is because the cost component is high enough to cover many likely values of the other parameters.

However, this last result must be interpreted with caution in Table 2, because the costs and benefits are not estimated on a stable population for all trimesters. As explained in section 2.2, we lack information on wave 2 for trimester 6 and on wave 3 for trimesters 5 and 6. Therefore estimations in Table 2 are based on a stable population only for the first four trimesters. A solution to this problem is to run the estimations only on individuals present during the 6 trimesters. Table 3 shows the results of such estimations.

Based on 85% of the total sample (wave 1 individuals), results in Table 3 are similar to those on the whole population. Although the impact on employment is positive and greater than previously during trimesters 1 and 2, it is not significant at the 10% confidence level. The negative and significant impact on welfare transfers in the third trimester is still present, and is even stronger than in Table 2; effects at trimesters 2 and 4 are also stronger and more significant, in line with the higher employment point estimates at trimesters 1 and 3. The cost benefit analysis is now meaningful, since all individuals are present over the whole experiment. Over this observation window, payment to the counseling firm has been considerably higher than gains in welfare transfers, by more than 2,200 € . Thus the counseling scheme is far from cost-effective for the public budget. Because employment intensity is limited, this is not counterbalanced by a monthly social value of $W = 450$ € for putting a welfare recipient to work. Remember this is the subsidy paid by the State on a class of jobs targeted towards welfare recipients. With this social value to employment, net cost remains strongly negative and significantly so. When the value of work is set to about the minimum wage $W = 1,000$ €, the effect is still negative but not significant anymore, certainly because this specification gives more weight to the - imprecisely estimated - impact on employment. Also, this value for W assumes full time work versus no work, which is probably not the general case because of the incidence of part-time work (remember we do not observe working time). Generally, therefore, although the employment impacts of the program are imprecisely estimated, due to the low take-up rate into the program, its cost-effectiveness is very clearly established by the data and this results from the high price paid to the counseling firm. The fact that more than half of the average cost is lump-sum payment, received for every person entering the program, certainly plays a strong role.

Tables 4 to 7 are variants of the previous estimations on wave 1, where we focus on specific subpopulations based on age, tenure into the welfare system, and children. The general picture is not affected by age or the presence of children in the household (Tables 4, 5 and 7), although the cost-benefit analysis is less negative in the presence of children. This is because welfare payments are much higher, so that decreases in transfer, when they occur, are also substantially stronger.

Table 6 illustrates the presence of a strong impact heterogeneity. Obviously, a counseling policy is much more efficient on a population that does not have too much tenure on welfare. When we consider only individuals with 2 to 4 years of seniority (as opposed to more than 4), we find stronger and longer lasting employment and transfer impacts. In particular, transfer impacts are stable and statistically significant all over the period. It could imply that this population is closer to the labor market and more likely to find a job and remain employed with some outside help. The cost/benefit analysis still reveals a deficit for the public budget, but it is now estimated at 1,700 €, thanks to the stronger reductions in welfare transfer.[4] When employment is given a social value, a zero net cost becomes more likely. This implies that such policies may be efficient and cost effective for some populations and not for others, something that can only be judged by systematic evaluation. It questions the policy decision to target this type of policy to individuals with long tenure in welfare, even if this population is of strong need of help.

[4]This refers to estimates with control variable, that are not significantly different from the other estimates, but are more precise.

6 Conclusion

This paper studies a job-search counseling program targeted at individuals that have been on welfare for at least two years. This population has severe difficulties to access the labor market, and was offered the possibility to benefit from intensive counseling. This program was provided by a private operator, and was costly to the public budget.

We evaluate the impact of this policy on job access and its cost-effectiveness, using a random allocation of welfare recipients to the program. Although the low take-up of the program implies that some parameters are imprecisely estimated, we can reach strong conclusions of interest for public policy. First, there seems to be some effects of the program on employment and resulting transfers. Second, those effects are, in general, far too small to ensure that the program is cost-effective, even if we give employment situations a social value on top of the transfer gains for the public budget. Third, there is significant heterogeneity, and we can isolate a population of relatively recent welfare status, for which the program is more efficient, still not cost-effective from a strictly financial point of view, but for which the net budget cost is closer to the order of magnitude of the social value of employment.

The policy as implemented receives a poor evaluation. But one may wonder how general is this finding. Obviously, there is scope for more targeted intervention, and more experimentation would be needed to decide on which kind of population precisely. In particular, we cannot decide here if individuals with less than 2 years tenure in welfare, or more or less educated people, should benefit from job search counseling. Also, the pricing level and mechanism of the private counseling firm is the source of high costs. We don't know if more competition on that market could induce lower costs to the public budget or if the policy is intrinsically too costly. Eventually, it is possible that the important lump-sum component of the cost delivers a poor incentive to the firm that may be a reason for its limited efficiency, especially in the long run. On that side too, more experimentation is needed.

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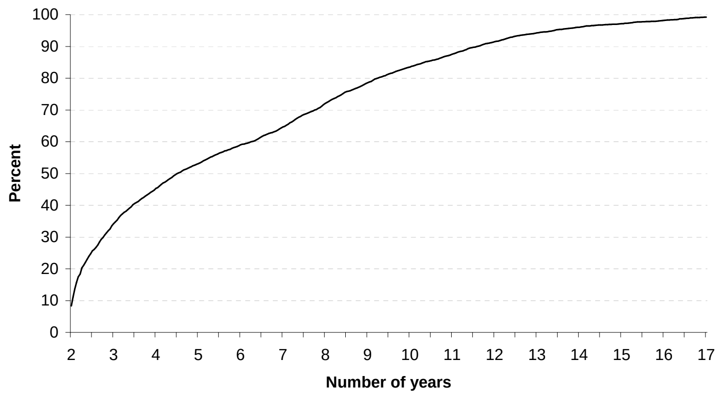
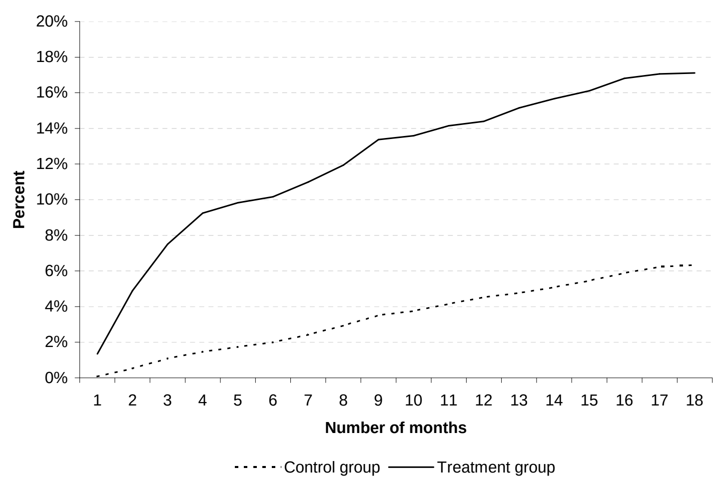
Figure 1: Cumulative distribution of seniority in wave 1



Note: Cumulative distribution of seniority into the welfare system, for the 14,980 individuals belonging to the first wave.

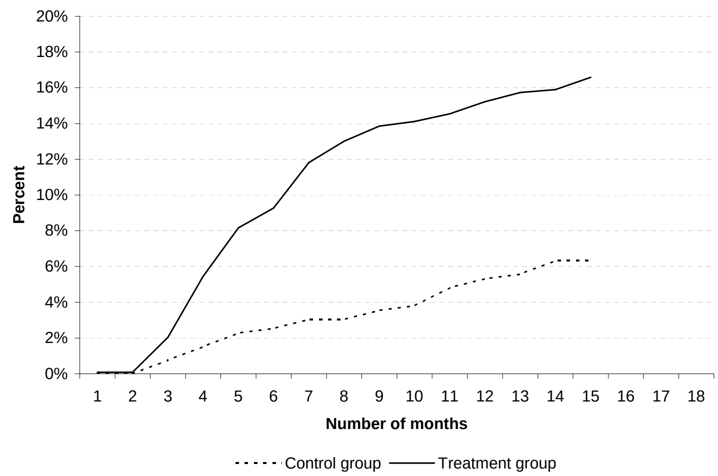
Lecture: Around 30% of individuals in wave 1 have less than 3 years of seniority into the welfare system.

Figure 2: Participation rate, wave 1



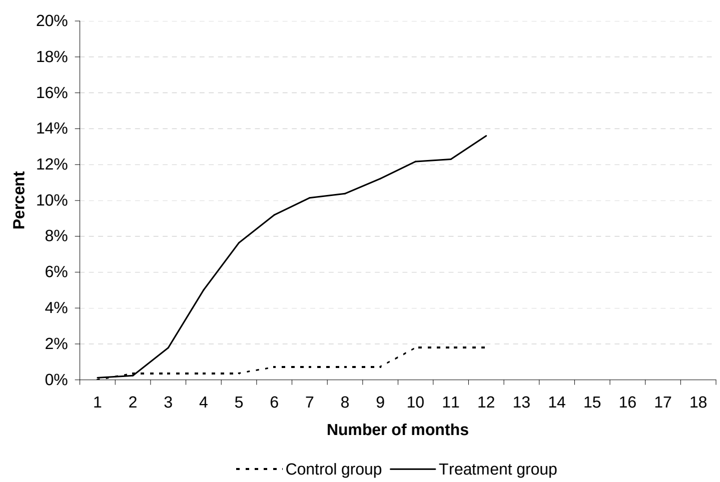
Note: Proportion of the 14,980 individuals in wave 1 that entered the couseling program, depending on whether they belonged to the treatment group (plain line) or the control group (dotted line).

Figure 3: Participation rate, wave 2



Note: Proportion of the 1,571 individuals in wave 2 that entered the couseling program, depending on whether they belonged to the treatment group (plain line) or the control group (dotted line).

Figure 4: Participation rate, wave 3



Note: Proportion of the 1,115 individuals in wave 3 that entered the couseling program, depending on whether they belonged to the treatment group (plain line) or the control group (dotted line).

Table 1: Descriptive statistics: Control and treatment groups

	Control group	Treatment group	Difference
Observations	4430	13236	
Proportion in wave 1	0.85	0.85	-0.00047
	(0.36)	(0.36)	(0.0062)
Proportion in wave 2	0.089	0.089	-0.00032
	(0.29)	(0.28)	(0.0049)
Proportion in wave 3	0.063	0.063	0.00078
	(0.24)	(0.24)	(0.0042)
Woman	0.46	0.46	-0.0028
	(0.5)	(0.5)	(0.0087)
$age \leq 30$	0.15	0.15	-0.0071
	(0.36)	(0.35)	(0.0062)
$30 < age \leq 40$	0.35	0.35	-0.0006
	(0.48)	(0.48)	(0.0083)
$40 < age \leq 50$	0.27	0.28	0.0093
	(0.44)	(0.45)	(0.0077)
$2 \leq seniority \leq 4$	0.53	0.5	-0.035*
	(1)	(0.99)	(0.018)
$4 < seniority \leq 6$	0.56	0.55	-0.012
	(0.5)	(0.5)	(0.0086)
Number of children	0.13	0.14	0.0089
	(0.34)	(0.35)	(0.0059)
Transfer 1 trimester	1117	1118	1.4
before entry	(428)	(439)	(7.5)
Transfer 2 trimesters	1090	1091	1.5
before entry	(442)	(458)	(7.7)
Transfer 3 trimesters	1074	1072	-1.6
before entry	(457)	(459)	(7.9)
Transfer 4 trimesters	740	735	-4.6
before entry	(342)	(345)	(5.9)
Employment rate 1 month	0.22	0.22	0.0019
before entry	(0.41)	(0.41)	(0.0071)
Employment rate 2 months	0.21	0.21	-0.00097
before entry	(0.41)	(0.41)	(0.007)

Scope: Individuals participating into the experiment.
* Significant coefficient at the 10% confidence level.

Table 2: Causal impact of counseling, whole population

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	0.037	0.04	0.037	0.04
	(0.38)	(0.37)	(0.38)	(0.37)
Trimester 2	0.13	0.13	0.17	0.17
	(0.27)	(0.26)	(0.25)	(0.24)
Trimester 3	0.084	0.08	0.06	0.055
	(0.22)	(0.22)	(0.23)	(0.22)
Trimester 4	-0.0055	-0.0067	-0.094	-0.094
	(0.22)	(0.22)	(0.26)	(0.26)
Trimester 5	0.02	0.014	0.051	0.042
	(0.21)	(0.2)	(0.24)	(0.24)
Trimester 6	0.0087	0.017	0.0054	0.026
	(0.19)	(0.19)	(0.22)	(0.22)
Welfare transfer				
Trimester 1	-75	-73	-75	-73
	(139)	(91)	(139)	(91)
Trimester 2	-136	-131	-162	-156
	(110)	(87)	(113)	(100)
Trimester 3	-195**	-192**	-249**	-249**
	(99)	(83)	(111)	(105)
Trimester 4	-104	-106	-23	-27
	(105)	(92)	(127)	(114)
Trimester 5	-105	-95	-95	-75
	(99)	(88)	(118)	(111)
Trimester 6	-108	-107	-135	-141
	(96)	(86)	(113)	(107)
Cost	3145***	3152***	3147***	3155***
	(43)	(44)	(48)	(49)
Cost - Benefit				
W= 0 €	-2422***	-2448***	-2407***	-2435***
	(556)	(425)	(535)	(432)
W= 450 €	-2297**	-2324***	-2301**	-2327***
	(949)	(806)	(892)	(773)
W=1,000 €	-2144	-2172	-2172	-2194
	(1612)	(1460)	(1495)	(1364)
Covariates	no	yes	no	yes

Scope: 17,666 individuals participating into the experiment. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.

Table 3: Causal impact of counseling, wave 1

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	0.12	0.12	0.12	0.12
	(0.35)	(0.34)	(0.35)	(0.34)
Trimester 2	0.26	0.26	0.3	0.3
	(0.28)	(0.28)	(0.28)	(0.27)
Trimester 3	0.13	0.12	0.079	0.067
	(0.24)	(0.23)	(0.24)	(0.24)
Trimester 4	0.017	0.015	-0.075	-0.076
	(0.24)	(0.24)	(0.27)	(0.26)
Trimester 5	-0.083	-0.083	-0.15	-0.15
	(0.23)	(0.22)	(0.25)	(0.25)
Trimester 6	0.0087	0.017	0.037	0.051
	(0.23)	(0.22)	(0.25)	(0.24)
Welfare transfer				
Trimester 1	-47	-27	-47	-27
	(132)	(77)	(132)	(77)
Trimester 2	-181	-169*	-218*	-208**
	(118)	(89)	(122)	(99)
Trimester 3	-282***	-274***	-362***	-360***
	(108)	(89)	(117)	(107)
Trimester 4	-203*	-200**	-156	-155
	(115)	(100)	(131)	(117)
Trimester 5	-128	-124	-67	-64
	(111)	(98)	(126)	(118)
Trimester 6	-108	-107	-94	-94
	(113)	(101)	(127)	(119)
Cost	3145***	3152***	3146***	3153***
	(51)	(51)	(55)	(55)
Cost - Benefit				
W= 0 €	-2197***	-2251***	-2203***	-2246***
	(608)	(457)	(604)	(481)
W= 450 €	-1996*	-2050**	-2065**	-2105**
	(1026)	(865)	(1002)	(863)
W=1,000 €	-1750	-1803	-1896	-1933
	(1734)	(1567)	(1676)	(1527)
Covariates	no	yes	no	yes

Scope: 14,980 individuals participating into the first wave of the experiment. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.

Table 4: Causal impact of counseling, wave 1, less than 40 years old

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	0.42	0.57	0.42	0.57
	(0.57)	(0.56)	(0.57)	(0.56)
Trimester 2	0.47	0.56	0.49	0.56
	(0.43)	(0.42)	(0.41)	(0.4)
Trimester 3	0.26	0.29	0.12	0.087
	(0.35)	(0.34)	(0.36)	(0.35)
Trimester 4	0.17	0.2	0.072	0.11
	(0.35)	(0.34)	(0.41)	(0.4)
Trimester 5	0.089	0.13	0.045	0.095
	(0.34)	(0.33)	(0.38)	(0.38)
Trimester 6	0.092	0.15	0.073	0.14
	(0.34)	(0.33)	(0.38)	(0.37)
Welfare transfer				
Trimester 1	145	-41	145	-41
	(195)	(126)	(195)	(126)
Trimester 2	-73	-177	-153	-228
	(169)	(137)	(178)	(158)
Trimester 3	-207	-275**	-350**	-377**
	(149)	(129)	(167)	(161)
Trimester 4	-176	-240*	-125	-195
	(158)	(142)	(188)	(173)
Trimester 5	-121	-179	-67	-116
	(155)	(141)	(185)	(176)
Trimester 6	-90	-147	-75	-130
	(159)	(147)	(184)	(175)
Cost	3278***	3276***	3282***	3280***
	(76)	(77)	(84)	(85)
Cost - Benefit				
W= 0 €	-2756***	-2217***	-2656***	-2194***
	(849)	(680)	(840)	(707)
W= 450 €	-2084	-1365	-2110	-1493
	(1510)	(1311)	(1460)	(1293)
W=1,000 €	-1262	-324	-1444	-636
	(2594)	(2370)	(2477)	(2283)
Covariates	no	yes	no	yes

Scope: 7,124 individuals participating into the first wave of the experiment, less than 40 years old. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.

Table 5: Causal impact of counseling, wave 1, more than 40 years old

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	-0.06	-0.23	-0.06	-0.23
	(0.44)	(0.43)	(0.44)	(0.43)
Trimester 2	0.14	-0.0087	0.17	0.035
	(0.37)	(0.36)	(0.37)	(0.36)
Trimester 3	0.059	-0.074	0.06	-0.063
	(0.32)	(0.32)	(0.33)	(0.32)
Trimester 4	-0.067	-0.2	-0.14	-0.27
	(0.33)	(0.32)	(0.35)	(0.35)
Trimester 5	-0.19	-0.29	-0.26	-0.35
	(0.31)	(0.31)	(0.34)	(0.34)
Trimester 6	-0.0047	-0.094	0.05	-0.019
	(0.3)	(0.3)	(0.32)	(0.32)
Welfare transfer				
Trimester 1	-226	19	-226	19
	(179)	(94)	(179)	(94)
Trimester 2	-314*	-130	-331**	-159
	(165)	(112)	(167)	(121)
Trimester 3	-391**	-246**	-435***	-318**
	(155)	(120)	(164)	(141)
Trimester 4	-269	-137	-224	-94
	(167)	(139)	(183)	(159)
Trimester 5	-173	-59	-105	-9.7
	(156)	(136)	(174)	(160)
Trimester 6	-166	-50	-144	-42
	(157)	(140)	(175)	(163)
Cost	3024***	3032***	3028***	3038***
	(67)	(68)	(71)	(72)
Cost - Benefit				
W= 0 €	-1485*	-2430***	-1563*	-2435***
	(862)	(610)	(859)	(646)
W= 450 €	-1541	-2835**	-1642	-2839**
	(1375)	(1137)	(1351)	(1145)
W=1,000 €	-1610	-3331	-1740	-3334
	(2291)	(2068)	(2232)	(2037)
Covariates	no	yes	no	yes

Scope: 7,856 individuals participating into the first wave of the experiment, more than 40 years old. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.

Table 6: Causal impact of counseling, wave 1, between 2 and 4 years of seniority

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	0.6	0.54	0.6	0.54
	(0.51)	(0.5)	(0.51)	(0.5)
Trimester 2	0.55	0.48	0.53	0.47
	(0.38)	(0.38)	(0.37)	(0.37)
Trimester 3	0.32	0.24	0.16	0.066
	(0.32)	(0.32)	(0.33)	(0.33)
Trimester 4	0.14	0.074	0.012	-0.047
	(0.32)	(0.32)	(0.37)	(0.37)
Trimester 5	0.093	0.032	0.039	-0.015
	(0.31)	(0.3)	(0.34)	(0.34)
Trimester 6	0.12	0.066	0.12	0.074
	(0.3)	(0.3)	(0.34)	(0.34)
Welfare transfer				
Trimester 1	-246	-123	-246	-123
	(179)	(110)	(179)	(110)
Trimester 2	-333**	-239*	-362**	-277**
	(156)	(124)	(163)	(141)
Trimester 3	-354**	-278**	-385**	-326**
	(140)	(121)	(155)	(149)
Trimester 4	-329**	-258*	-301*	-229
	(150)	(134)	(175)	(160)
Trimester 5	-349**	-279**	-367**	-300*
	(145)	(131)	(165)	(156)
Trimester 6	-358**	-298**	-374**	-324**
	(147)	(136)	(166)	(158)
Cost	3280***	3288***	3280***	3287***
	(74)	(75)	(80)	(82)
Cost - Benefit				
W= 0 €	-1311*	-1813***	-1246	-1708***
	(791)	(623)	(783)	(650)
W= 450 €	-491	-1171	-587	-1222
	(1376)	(1206)	(1332)	(1192)
W=1,000 €	511	-385	219	-627
	(2339)	(2175)	(2241)	(2104)
Covariates	no	yes	no	yes

Scope: 7,302 individuals participating into the first wave of the experiment, between 2 and 4 years of seniority into the welfare system. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.

Table 7: Causal impact of counseling, wave 1, with at least one child

	Model hypothesis:			
	Essential heterogeneity		Variable effect	
	(1)	(2)	(3)	(4)
Employment				
Trimester 1	0.097	0.0092	0.097	0.0092
	(0.6)	(0.57)	(0.6)	(0.57)
Trimester 2	0.39	0.29	0.46	0.36
	(0.49)	(0.47)	(0.49)	(0.47)
Trimester 3	0.36	0.27	0.42	0.33
	(0.4)	(0.4)	(0.42)	(0.41)
Trimester 4	0.086	0.019	-0.11	-0.15
	(0.41)	(0.4)	(0.46)	(0.45)
Trimester 5	0.03	-0.04	-0.071	-0.13
	(0.41)	(0.4)	(0.47)	(0.46)
Trimester 6	-0.036	-0.1	-0.037	-0.1
	(0.41)	(0.41)	(0.46)	(0.46)
Welfare transfer				
Trimester 1	-47	21	-47	21
	(280)	(122)	(280)	(122)
Trimester 2	-280	-221	-335	-276
	(247)	(156)	(250)	(176)
Trimester 3	-490**	-439***	-665***	-611***
	(219)	(168)	(239)	(216)
Trimester 4	-447*	-407**	-439	-405*
	(237)	(197)	(273)	(241)
Trimester 5	-367	-335	-274	-256
	(237)	(205)	(275)	(246)
Trimester 6	-333	-318	-305	-301
	(247)	(220)	(275)	(250)
Cost	3289***	3311***	3304***	3326***
	(110)	(114)	(115)	(118)
Cost - Benefit				
W= 0 €	-1325	-1613*	-1239	-1498
	(1271)	(849)	(1269)	(916)
W= 450 €	-910	-1409	-899	-1359
	(1938)	(1520)	(1916)	(1558)
W=1,000 €	-401	-1161	-484	-1189
	(3097)	(2696)	(3037)	(2690)
Covariates	no	yes	no	yes

Scope: 4,105 individuals participating into the first wave of the experiment, with at least one child. *, **, ***: Significant coefficient at the 10%, 5% and 1% confidence level. Employment measurement unit is month of employment; other parameters are measured in €. See sections 4.1 and 4.2 for estimation formula under each model hypothesis. W is a monthly social value for employment.