

Welfare to What ?

Experimental evaluation of an activation programme for single mothers in poverty in France

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👤 Caisse nationale des allocations familiales

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Motivations: "Ending welfare as we know it" ?

- Welfare-to-work programmes to facilitate (or mandate) labour market participation of welfare recipients
 - Job search assistance and monitoring, trainings and education, short or long term
- Welfare-to-work predominantly affect women ; mostly single parents (Smedslund 2006).
- European Union: "Social investments", along with early childhood policies (Hemerijck and Huguenot-Noël 2022).
- RSA from 2008, in-work benefit reforms in 2016 then 2019, new ongoing reforms (*France travail*).



Figure: B. Clinton speaking on Welfare in Nashville, 1996.

⌚ Boston Globe, 2022/01/23 www.bostonglobe.com

France 2018-2022: The "anti-poverty strategy"

Le Président de la République a décidé, le 17 octobre dernier, d'engager l'élaboration d'une stratégie nationale de lutte contre la pauvreté afin de promouvoir une « nouvelle approche de la lutte contre la pauvreté. Nouvelle, parce que ce qui a été tenté jusqu'ici n'a pas marché. Nouvelle, parce que nous ne devons pas seulement ajuster les méthodes anciennes, mais changer radicalement d'approche ».

Ce changement radical est double. C'est, d'une part, la priorité clairement donnée à l'investissement social par la lutte contre la pauvreté des enfants et des jeunes, qui sont les premières victimes de la pauvreté dans leur quotidien comme pour leur avenir. C'est, d'autre part, l'engagement d'une politique déterminée de sortie de la pauvreté par le travail, qui renforce les engagements des personnes concernées comme des pouvoirs publics, avec un objectif clair: aucun accompagnement social sans perspective d'accès à l'emploi.

Développer la prévention et l'investissement social, c'est rétablir l'égalité républicaine, celle qui concilie les chances et les places. C'est redonner confiance et espoir dans la République à des millions de familles. C'est refuser que les enfants pauvres d'aujourd'hui soient les adultes pauvres de demain.



Welfare reforms & single parents

- Economics of active labour market policies ⇒ narratives for reforms ;
- Reforms enriched economic theory and empirical methods
 - Welfare reforms and the labour market (Chan and Moffitt 2018)
 - Econometrics of programme evaluation (Heckman, Lalonde, and Smith 1999)
- From the 1970's, steady growth of single parenthood, decreasing employment rate, rising poverty (Nieuwenhuis and Maldonado 2018)
- **Single parents** at the heart of all major **welfare reforms** (Knijn, Martin, and Millar 2007).

Antagonistic views on social justice, family dynamics, gender roles and work

→ Gender × education × social inequalities × social stigma and stereotypes

- *Deserving poors* (Moffitt 2015)
- "*Welfare queen*" (Foster 2008)
- Stereotypes in laws and bills (Carcasson 2006; Mangin 2021)
- Welfare reforms as backlash against single mothers (Reese 2005)

Single parent households and poverty

A highly vulnerable population

Single parenthood is predictive of (but not always causing) essentially **every "bad" outcome or risk factor** in life, for both the parents and their children, despite often being a transient period.

- Health & well-being (Mariani, Özcan, and Goisis 2017; Evans and De France 2022)
- Education (Duncan, Zirol-Guest, and Kalil 2010; Brand and Simon Thomas 2014), crime (Kroese et al. 2021),...
- In France, Share of single parents went from 10 to 25% of Households with children from 1970 to 2020.
- 2020: 2 million households, 82 % of which are single mothers (INSEE 2021).
- 2018: 54% of parents receiving RSA are single parents (91% mothers) (DREES 2022, p.120)
- 40% of children in single parent households live in poverty.

This paper: Research questions

Can large "social investments" in intensive and personalised support help single parents get a job and get out of poverty ?

- ① **Intervention:** Intensive welfare-to-work programme in France
- ② **Design:** Randomised block-encouragement design staggered each year from 2018 to 2022
- ③ **Data:** Matched administrative records from National family allowance (ALLSTAT)
- ④ **Main outcomes:** Labour market participation, cash transfers and disposable income

Empirical strategy

- ① Investigate treatment effect dynamics, heterogeneity and distributional effects.
- ② Contrast experimental results with modern difference-in-differences to reveal selection on unobservables

Literature and contribution

① Welfare-to-work : Large international literature, no data in France

- Systematic reviews Gorey (2009) and Smedslund (2006): no data from Europe ; Gibson et al. (2018) few estimates.
⇒ Small or no effect on employment, poverty, evidence of negative effect for the the most deprived families

② ALMP in the European literature: mostly quasi-experiments ;

- Bergemann and Van Den Berg (2008) 39 quasi experiments - mostly positive effects - 4 RCTs ; 2 positives, 2 negatives ;
- Systematic review from Vooren et al. (2019): **No estimates in France vs 372 estimates in Germany.**
- French reviews by Abadia et al. (2017) and Cervera et al. (2017): few RCTs, mostly for NEETs or unemployed

⇒ (Almost) **no RCTs** on welfare recipients, let alone single parents.

- Crepon et al. (2013): RCTs in the former welfare scheme in Seine-Saint-Denis: ⇒ low take-up and no effect on employment.

Literature and contribution

- ① **Other RCTs in France:** Behaghel, Crépon, and Gurgand (2014) Job placement between private or public providers
 - Private providers no better than the employment agency at job placement
 - Issue of close substitute bias
- ② New analysis of the effects of the EITC Kleven (2023) *vs* Whitmore Schanzenbach and Strain (2021)
- ③ Econometrics of programme evaluation
 - Old tradition of comparing experimental results with alternative methods (Heckman, Ichimura, and Todd 1997; Smith and Todd 2005; LaLonde 1986)
 - New DiD literature with staggered adoption and heterogeneous treatment effects (Callaway and Sant'Anna 2021; Wing, Freedman, and Hollingsworth 2024)

The Reliance programme in a nutshell

A targetted and tailored programme

- ⌚ Single parents under 50 Y-O receiving RSA for more than 2 years
- 🔍 3 non-profits, qualified and experienced supervisors, renovated and accessible premises, childcare provision

Intensive bundled intervention

- 👤 1 year support, ≈ 100 people/year, 10-15h/week, both group and individual sessions
- 🕒 Access to rights, career plans, various workshops (self-confidence, parenthood, budget management etc.)
- ⚠️ Third-party assistance: Pôle emploi, CAF agents and access to aid simulators
- € Average cost per participant ≈ 2800 €; **4 times the average spending for social support**

A social investment programme

- 👤 Strong political backing and institutional support
 - Presented as a "Best practice programme" Cour des comptes (2022), Montaignac and Schweitzer (2020, Mars), Pitollat and Klein (2018, Aout), and Damon (2018)
 - 🏆 AFIGÈSE 2021 prize winner in public policy evaluation ↗
 - ↗ L'Est Républicain, © Credit Cédric JACQUOT



Figure: Visit to Reliance's premises by Minister Agnès Buzyn, Secretary of State Christelle Dubos accompanied by the President of the Departmental Council Matthieu Klein, 26 April 2019.

The Reliance programme: Timeline

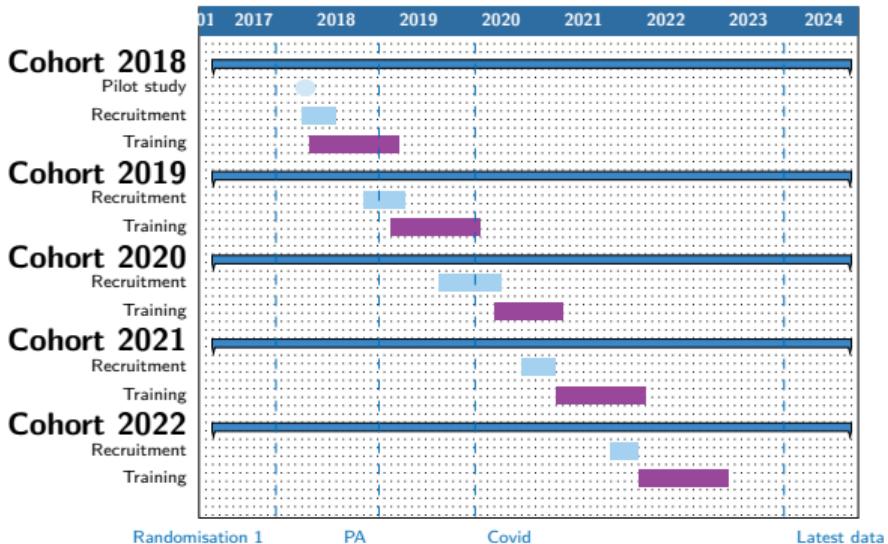
Staggered design in troubled times

- Random draws with varying recruitment timing, duration and process
- Heterogeneous economic and labour market conditions across cohorts

Global economic outlook

- August 2017-2018: End of subsidized employment (*contrats aidés*)
- January 2019: Reform of in work benefit (*Prime d'activité*) following the yellow vest movement
- March 2020-June 2021: Covid-19 pandemic
- Many reforms of the labour market

Figure: Timeline of the experiment



Sampling and randomisation protocol

Sampling and screening

Pre-registered design, outcomes and main evaluation ↗ socialscienceregistry.org

- ① Random sample ($\approx 500/\text{year}$) among eligible population from CD-54 database
- ② Screening of each file: $\approx 1/5$ excluded (already working, enrolled in similar programme, etc.)
- ③ Experimental sample for those with 'favourable' or 'reserved' assessments

Block-randomised encouragement design

① Blocking variables

- Registration to the employment agency (*Pôle emploi*) (True/False)
- Number of dependent children (1,2, 3+)
- Years receiving RSA (2 to 5 years, 5 to 10 years, 10+)

② Encouragement group invited to information sessions and signing of integration contract

③ Control group under ordinary law

Data

Matched administrative data

- Initial datasets matched with ALLSTAT: Monthly data from Cnaf from 01-2017 to 12-2023
- Outcomes:
 - ① Labour market participation (positive labour incomes)
 - ② Per capita income (total income by consumption units (in constant €2015))
 - ③ Total cash transfers (in constant €2015)
- Other variables:
 - Covariates measured the month from random assignment, either centred or dummmified.
 - French citizenship, quartiles of age, having children under 2, having children between 3 and 5 and having children older than 16, education group, dummy for taxable incomes 2 years before higher than the median, quartiles of disposable income per consumption unit at baseline, and dummies for housing benefits, public alimony (ASF) and receiving child support

[▶ Figure Attrition](#)[▶ Balance table](#)

Empirical strategy: Main models

① First stage effects

- Average First stage: OLS regression of D on Z and block \times cohort FE
- Heterogeneous participation: Add \dot{X} and $Z \cdot \dot{X}$ on the right hand side

② Intention to treat (ITTs)

- Main model: Stacked regressions [▶ Model ITT](#)
- Robustness checks: Covariates and Adaptation of Callaway and Sant'Anna (2021) [▶ Model CS 2021](#)

③ Average treatment effect on the treated (ATTs)

- Main model: Stacked TSLS regressions with recentered instrument (Borusyak, Hull, and Jaravel 2022)
- One sided non-compliance \Rightarrow Monotonicity trivially holds & LATE = ATT (Frölich and Melly 2013)
- Exclusion restriction: No effect of encouragement on never-takers.

④ Inference

- Cluster robust standard errors at the block \times cohort level
- Simultaneous inference and multiple test hypothesis using Holm-Bonferroni correction.
- Wild cluster bootstrap for models with Callaway and Sant'Anna (2021) estimators.

Descriptive statistics

Sample's main features

1 Low and balanced attrition rates

[Figure attrition](#)

- Baseline experimental sample: 1666 with 4 cohorts
- 826 controls, 840 encouraged among which 327 treated.
- 1488 complete cases 36 months from random assignment : 746 controls, 742 encouraged, among which 298 treated.

2 Balanced observables at baseline

[Balance table](#)

3 Overall, the sample include highly vulnerable families, 97% in poverty at baseline.

- 95% Women, 36 y-o on average, 80% French citizens ;
- 34% registered unemployed, 40% 1 child, 30% two and three or more children
- 1/4 with less than middle school diploma, 1/3 with vocation degree, 1/5 with higher education
- 1/3 have children under 3, 1/3 have children older than 16,
- Only 20% receive child support and 65% receive family support allowance (ASF)

Descriptive statistics

Heterogeneous employment trajectories in the counterfactual

Design variables predict outcomes as expected

- Higher job finding rates for those closer to the labour market
- Registered unemployed were more likely to work in the past
- Social workers' assessment strongly predict employment

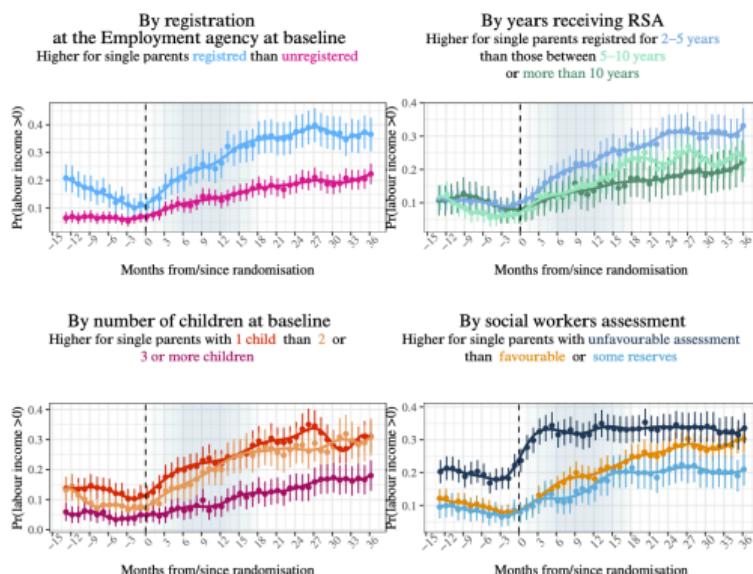


Figure: Average employment in relative time from random assignment

First stage

Average first stage effect: 39%

Different "encouragement" methods and reactions

- 2018 : Positive letters + text message and phone call reminders
- 2019 : Threatening letter ("droit et devoir") + warmer meetings in Reliance building
- 2020 : + feedbacks from former participants
- 2021 : Individual meetings + feedbacks
- 2022 : Reminders not fully implemented
- Heterogeneity: Higher take-up for those closer to the labour market, but most usual predictors have no effects.

▶ First stage heterogeneity

	sample					
	Full sample	Cohort 2018	Cohort 2019	Cohort 2020	Cohort 2021	Cohort 2022
Encouragement	0.386*** (0.018)	0.279*** (0.027)	0.365*** (0.028)	0.421*** (0.039)	0.472*** (0.045)	0.374*** (0.036)
Num.Obs.	2065	395	395	383	493	399
R2 Adj.	0.250	0.159	0.211	0.271	0.330	0.230
Std.Errors	by: strataXc					
FE: strataXc	X	X	X	X	X	X

* p < 0.1, ** p < 0.05, *** p < 0.01

Sample: Cohorts 2018 to 2022 at the time of randomisation.

OLS regressions of participation on encouragement and blocks x cohort FE and inverse encouragement propensity score weighting.
Cluster robust standard errors adjusted by blocks x cohorts in parenthesis.

Figure: 39% average Take-up rising from 28 to 48% for the 1st and 4th cohort

Treatment effect on employment

Dynamics across cohorts

- 2018: promising trends, then stable employment rate
- 2019: PA reform during recruitment, pandemic at the exit
- 2020: Adapted intervention
- 2021: Better economic outlooks
- Overall, no more than 40% employed

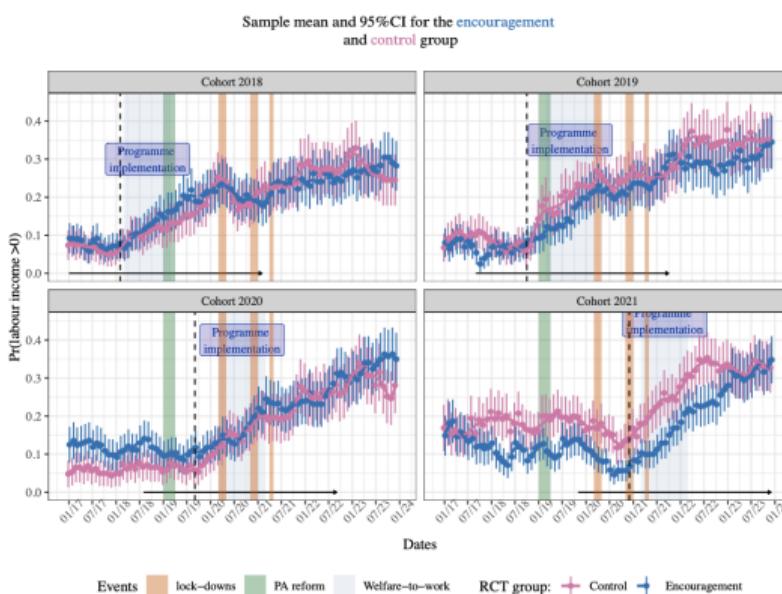


Figure: Employment levels by cohort and encouragement status

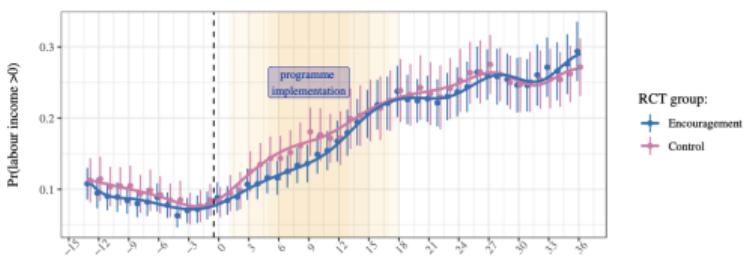
Treatment effect on employment

Reliance does not increase labour market participation

- 10% baseline employment before random assignment
 - Lock-in effects up to 9 months and fast catching-up
 - No post-training effect on employment
 - Average employment around 30% in both groups.
 - No improvement for the three first cohorts up to 51 months
- [▶ Figure Long term](#)

[▶ Table](#)

Average labour market participation by encouragement group and intention-to-treat estimates
Change in employment



intention-to-treat: Event study

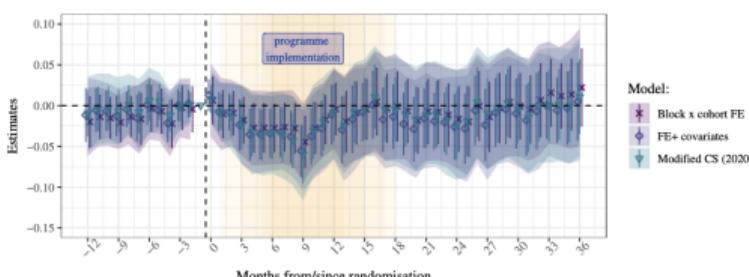


Figure: Aggregated average employment and intention to treat

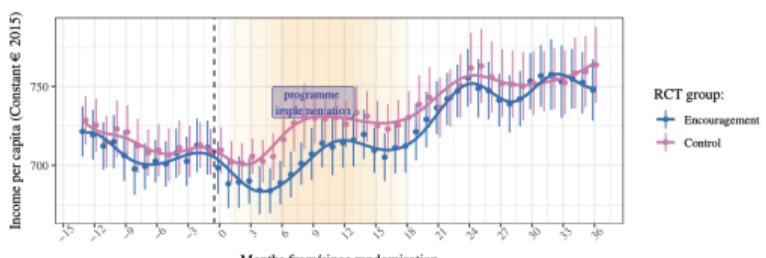
Treatment effects on other outcomes

Treatment effects on per-capita income

- \approx €700 monthly income at baseline
- Lock-in effect on employment significantly affects incomes
- No average effect of the programme on per-capita income

▶ Table

Average disposable income per capita by encouragement group and intention-to-treat estimates
Change in disposable incomes per capita



intention-to-treat: Event study

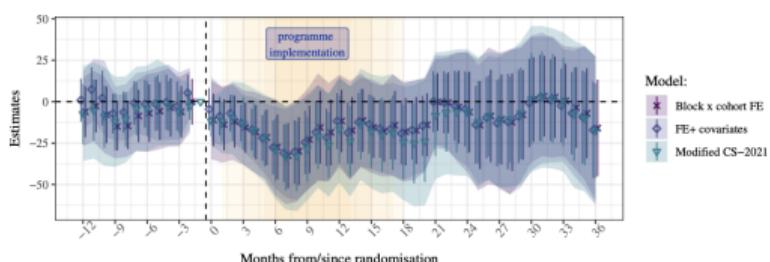


Figure: Aggregated average income per capita and intention to treat

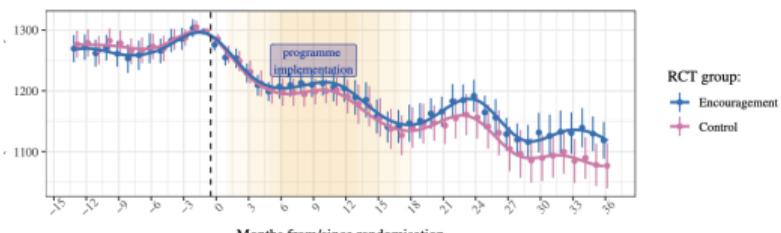
Treatment effects on other outcomes

Effects of the programme on cash transfers

- Decreasing cash transfers in both groups
- Slower decrease post-programme for the encouragement group
- Continuing trend for the three first cohorts on the long run

▶ Figure Long term

Average total cash transfers by encouragement group and intention-to-treat estimates
Change in total transfers from Cnaf



intention-to-treat: Event study

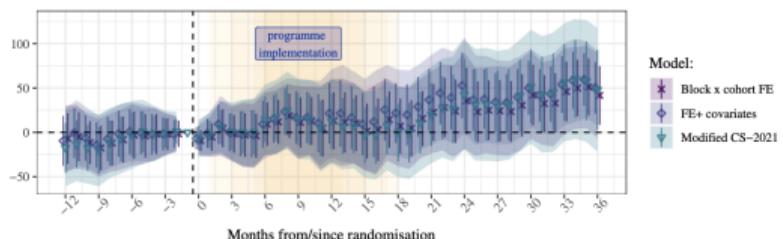


Figure: Aggregated average cash transfers and intention to treat

Treatment effects on other outcomes

Results

In brief

- ① Lock-in effect on employment, disposable income then no significant effect.
- ② Increasing average treatment effects on total cash transfers ⇒ **income substitution**.
- ③ No effect on the probability of receiving RSA or In-work benefits (not shown here).

Further investigations

① Marginal distributions of potential outcomes

- [Technical details](#) [Distributions of potential disposable income](#)

② Endogenous selection

- Modern difference-in-differences between participants and alternative control groups

③ Conditional ATTs

- Sub-group analysis of heterogeneous treatment effects [Plots by social workers' assessment](#)

Misleading pre-trend and positive differences post treatment

- Doubly robust DiD (Callaway and Sant'Anna 2021)
 - Convincing pre-trend due to sampling (at least 2 years RSA)
 - Lock-in then positive "effects"
 - Better comparison groups and covariate adjustment reduce estimates
- Covariates halve the bias but still exclude the experimental estimates from the 95% CI

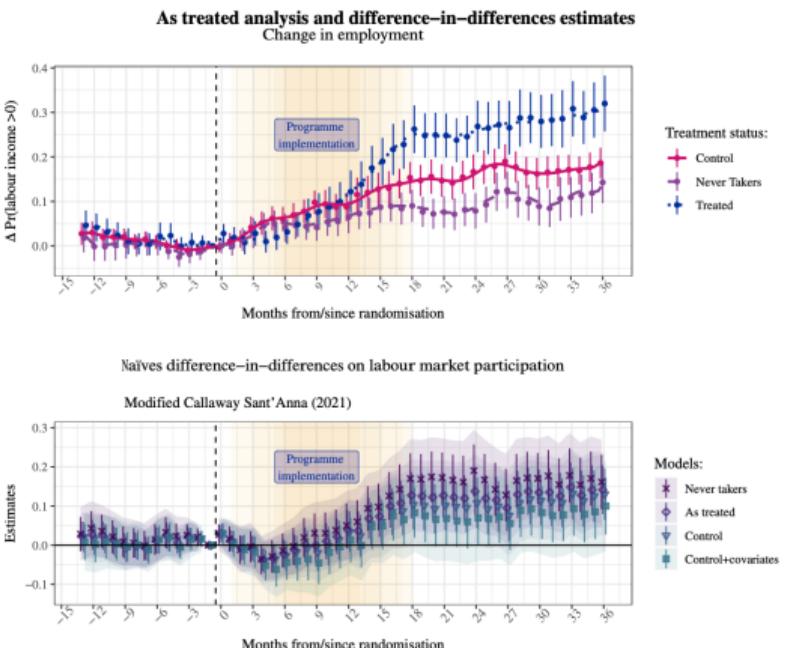


Figure: Aggregated average outcome and intention to treat

Main take-away

- ① Intensive welfare-to-work for single parents **did not help them get a job and get out of poverty**
 - **short term** (<9 months): Lock-in effect on employment ⇒ Measurable opportunity costs
 - **middle term** (18 - 36 months): No average treatment effect on employment and per-capita income but increasing treatment effects on total cash transfers
- ② **Without random assignment, would conclude that the programme increased employment**
 - Modern, doubly-robust Difference-in-differences could not retrieve the experimental estimates
 - Evidence of time varying unobserved differences between compliers and never-takers
 - Programme attracted those closer to the labour market but slowed their job finding rates.
- ③ Evidence of larger opportunity costs for those closer to the labour market.
- ④ Activation of those who don't need it, while still leaving most of them out of work.

Validity

- ① Internal validity: well conducted randomisation, small and balanced attrition, robust estimates
- ② External validity: Random sampling but screening from social-workers
 - The experimental sample includes single parents further away from the labour market
 - Social-workers screening predicts outcome but has puzzling heterogeneous treatment effects
- ③ Global context: Many reforms + Covid-19 Pandemics
 - No sign of heterogeneous treatment effects beyond lock-in effects
 - Reliance no better than the "usual" for job placement
- ④ Main mechanism: Endogenous selection (high potential employment, closer to the labour market, fear of sanction ?)

Policy implications

- ① Intensive welfare-to-work programmes don't help single parents, don't save public money. Instead, they may increase public spendings !
- ② Lack of high quality evidence: have welfare reforms been decided on wrong estimates ?
- ③ Fear of sanction seems to foster enrolment, but may not select those who would benefit from it
- ④ Other paper Galitzine and Heim (2024): Learning the tax-benefit schedule ? ► Intensive margin reactions
- ⑤ 2024: RSA and "France travail": Most likely to worsen single parents' situation

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Attrition[▶ Back to descriptive statistics](#)

Attrition between the experimental groups and excluded families

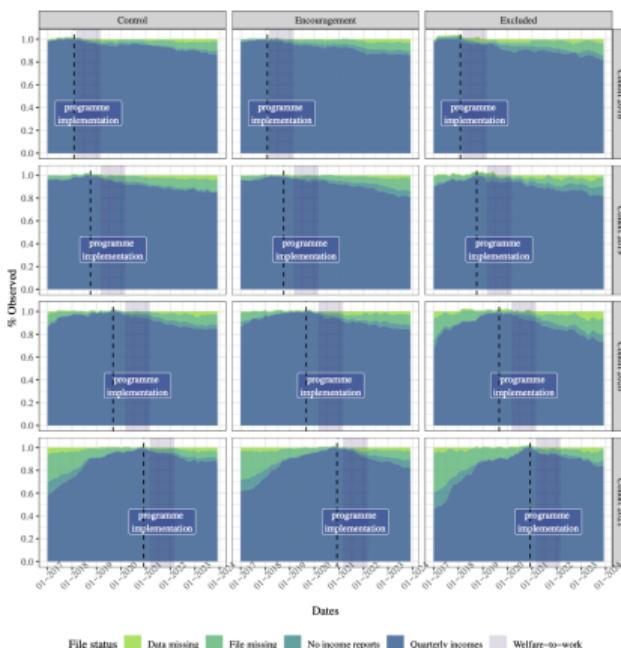


Figure: Attrition by cohort and experimental arm

Attrition

▶ Back to Descriptive statistics

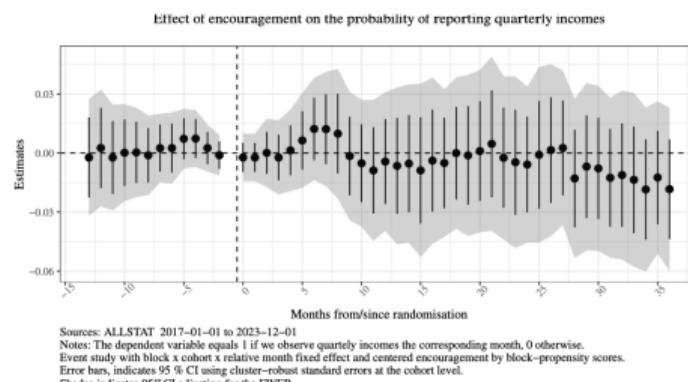


Figure: Average effect of encouragement on attrition

Identification strategy

Main model for intention to treat analysis

Appendices

I estimate the following stacked reduced form equation using OLS:

$$Y_{ibm} = \underbrace{\sum_s \mathbf{B}' \alpha_{bs}}_{\text{block } \times \text{ period FE}} + \underbrace{\sum_s \tilde{\mathbf{Z}}_{ib} \tau_s}_{\text{Covariates } \times \text{ period}} + \underbrace{\sum_s \mathbf{X}' \rho_s}_{\text{Covariates } \times \text{ period}} + \varepsilon_{ibm} \quad (1)$$

- Individual i in block b observed m months from random assignment.
- $s \in S$ index subsets that partition the panel into sets of months and, optionally, additional covariates.
- $\tilde{\mathbf{Z}}_{ib}$, the centred encouragement variable $(Z_{ib} - \hat{q}_b)$, with \hat{q}_b derived from a probit model of Z on block fixed effects \mathbf{B} (Borusyak, Hull, and Jaravel 2022)
- Treatment effects are the set of τ_s
- Full set of interaction of period and block fixed effects close to Sun and Abraham (2021)
- Cluster robust standard errors at the block level.

[▶ Back to empirical strategy](#)

Identification strategy

Treatment effects on the treated

Appendices

- To estimate ATT(s), I also use stacking of the TSLS systems of equations (2) for each subset s . Formally:

$$\begin{cases} Y_{ibm} = \sum_s \mathbf{B}' \beta_{bs} + \sum_s \mathbf{D}'_{ibs} \delta_s + \mu_{ibm} \\ \sum_s \mathbf{D}_{ibs} = \sum_s \mathbf{B}' \lambda_{bs} + \sum_s \tilde{\mathbf{Z}}'_b \pi_s + \epsilon_{ibm} \end{cases} \quad (2)$$

- Block dummies \mathbf{B} instrument themselves and period-treatment indicators \mathbf{D}_{ibs} are instrumented by period-specific centred encouragement variables.
- The δ_s coefficients estimate period-specific average treatment effects on the treated.

▶ Back to empirical strategy

Identification strategy

Adaptation of Callaway and Sant'Anna (2021)

Appendices

- Estimates every cohort-time treatment effects $TT_c(t)$,
- Event-study parameter $\theta_{es}(m)$ as weighted average of the $TT_c(m = t - c)$, with weights proportional to cohorts' size.
- Cohort-time effects estimate the sample analogue of the following equation:

$$TT(c, t) = \mathbb{E} \left[\left(\frac{T_{ic}}{\mathbb{E}[T_{ic}]} - \frac{\frac{p_c(\mathbf{X})(1-T_{ic})}{1-p_c(\mathbf{X})}}{\mathbb{E}[\frac{p_c(\mathbf{X})(1-T_{ic})}{1-p_c(\mathbf{X})}]} \right) (Y_t - Y_{c-1} - m_c(\mathbf{X}, t, c)) \right] \quad (3)$$

- T can be encouragement or treatment ; Propensity scores $p_c(\mathbf{X})$ depend on covariates
- $m_c(\mathbf{X}, t, c) = \mathbb{E}[Y_t - Y_{c-1} | \mathbf{X}, T_i = 0]$: outcome regression - comparison group in cohort c at time t .
- Weights for event study parameters:

$$\theta_{es}(m) = \sum_c \mathbf{1}\{t = c + m\} P(C = c \mid t = c + m) TT(c, c + m) \quad (4)$$

▶ Back to empirical strategy

Identification strategy

Estimating potential outcomes' average and distribution

Appendices

- Imbens and Rubin (1997): With valid IV, the entire marginal distribution of potential outcomes is identified.
- Abadie (2003) and Frölich and Melly (2013) generalize for any function of potential outcomes and covariates
- In practice (See Angrist, Hull, and Walters (2023)):
 - ① To estimate $\mathbb{E}[Y(0)|D(1) > D(0)]$: TSLS of $Y \times (1 - D)$ on $(1 - D)$ instrumented by \tilde{Z}
 - ② To estimate $\mathbb{E}[Y(1)|D(1) > D(0)]$: TSLS of $Y \times D$ on D instrumented by \tilde{Z}
 - ③ To estimate $\mathbb{E}[Y(d) < y|D(1) > D(0)]$ i.e **CDF**: TSLS of $\mathbf{1}(Y < y) \times \mathbf{1}(D = d)$ on $\mathbf{1}(D = d)$ instrumented by \tilde{Z}
 - ④ To estimate the **PDF**, one can define the outcome as $g(Y) = \frac{1}{h} K(\frac{Y_i - y}{h})$ with $K(\cdot)$ a kernel function and h a bandwidth shrinking to 0

Identification strategy

Estimating potential outcomes' average and distribution

Appendices

To estimate the distribution of potential outcomes, I follow Abdulkadiroğlu, Pathak, and Walters (2018)

- I use a symmetric kernel function $g(X, Y(d)) = \frac{1}{h}K(\frac{Y_i - y}{h})$ as the outcome of the TSLS equation
- $K(\cdot)$ gives the kernel density of the outcome for compliers around y .
- Bandwidth defined by the Silverman (1986) rule of thumb

$$h = 1.06 \times N^{-\frac{1}{5}} \sigma_d$$

- σ_d consistently estimated in the data.
- In practice: Run a 100 TSLS by bins of width h over the support of the outcome.

▶ Back to empirical strategy

Additional estimates

Variable	Overall N = 1,666 ¹	Control N = 826 ¹	Encouragement N = 840 ¹	p-value ²	q-value ³
Cohort 2019	395 [24%]	201 [24%]	194 [23%]		
Cohort 2020	383 [23%]	188 [23%]	195 [23%]		
Cohort 2021	493 [30%]	243 [29%]	250 [30%]		
Senior				>0.99	>0.99
10+ years	367 [22%]	182 [22%]	185 [22%]		
2-5 years	714 [43%]	353 [43%]	361 [43%]		
5-10 years	585 [35%]	291 [35%]	294 [35%]		
French	1,374 [82%]	668 [81%]	706 [84%]	0.088	0.93
Women	1,579 [95%]	788 [95%]	791 [94%]	0.26	>0.99
Age	36 (8)	36 (8)	36 (8)	0.77	>0.99
Education				0.86	>0.99
Higher education	313 [19%]	150 [18%]	163 [19%]		
Low education	405 [24%]	202 [24%]	203 [24%]		
Unknown	395 [24%]	202 [24%]	193 [23%]		
Vocational degree	553 [33%]	272 [33%]	281 [33%]		
Has child(ren) under 2	499 [30%]	257 [31%]	242 [29%]	0.30	>0.99
Has child(ren) 3 to 5	544 [33%]	276 [33%]	268 [32%]	0.51	>0.99
Has child(ren) over 16	512 [31%]	264 [32%]	248 [30%]	0.27	>0.99
(Missing)	2	2	0		
Receive family allowance	938 [56%]	467 [57%]	471 [56%]	0.85	>0.99
Receive family supplement	288 [17%]	142 [17%]	146 [17%]	0.92	>0.99
Receive housing benefit	1,476 [89%]	735 [89%]	741 [88%]	0.62	>0.99
Receive family support allowance	1,081 [65%]	540 [65%]	541 [64%]	0.68	>0.99
Receive child support	343 [21%]	170 [21%]	173 [21%]	>0.99	>0.99
Pr[Labour income>0]	133 [8.1%]	69 [8.5%]	64 [7.7%]	0.56	>0.99
(Missing)	22	13	9		
RSA	1,632 [98%]	808 [98%]	824 [98%]	0.69	>0.99
Per-capita income	710 (153)	709 (151)	711 (155)	0.80	>0.99
(Missing)	3	2	1		
Total cash transfers	1,295 (496)	1,297 (498)	1,292 (493)	0.72	>0.99

¹n [%]; Mean (SD)²Pearson's Chi-squared test; Wilcoxon rank sum test³False discovery rate correction for multiple testing

Figure: Balance check the month before random assignment

Additional estimates

Few observables predict higher participations

- More recent cohorts
- Close to the labour market (Unemployment registration + Favourable initial assessment)
- Low education, mid age range, mid income range.
- Most usual predictors have no effects

▶ Back to first stage

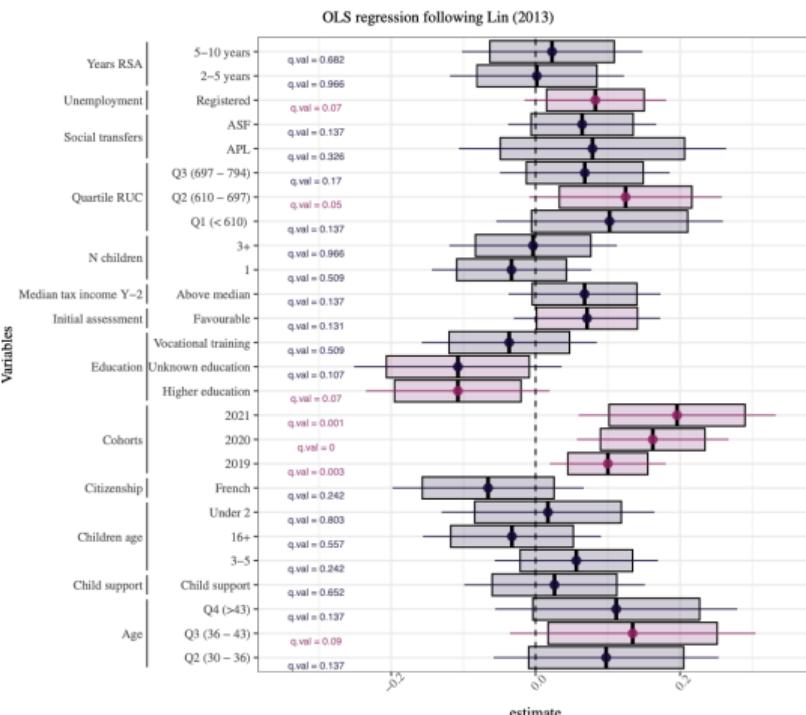


Figure: Heterogeneous encouragement effects on participation

Additional estimates

**Faster transition
to employment in
the counterfactual**

- Lock-in effect
half the

▶ Back to ITT plot

Table I.5: Aggregated treatment effects on the treated on labour market participation

	Mean control	Compliers' Y(0)	OLS		TSLS	
			No covariates	Covariates	No covariates	Covariates
			adj.p.val.	adj.p.val.	adj.p.val.	adj.p.val.
{-7 ; -1 f	0.09*** (0.01) [0.06, 0.11]	0.09*** (0.02) [0.03, 0.16]	-0.01 (0.01) [-0.04, 0.02]	-0.01 (0.01) [-0.03, 0.02]	-0.02 (0.03) [-0.11, 0.06]	-0.02 (0.02) [-0.08, 0.04]
{0 ; 6 f	0.11*** (0.01) [0.08, 0.14]	0.13*** (0.03) [0.06, 0.20]	-0.01 (0.01) [-0.04, 0.02]	-0.02 (0.01) [-0.04, 0.01]	-0.04 (0.03) [-0.11, 0.04]	-0.04 (0.03) [-0.11, 0.03]
{6 ; 12 f	0.16*** (0.02) [0.13, 0.20]	0.23*** (0.04) [0.13, 0.33]	-0.03** (0.01) [-0.06, 0.01]	-0.04*** (0.01) [-0.07, -0.00]	-0.07** (0.03) [-0.16, 0.01]	-0.10*** (0.03) [-0.19, -0.01]
{12 ; 18 f	0.20*** (0.02) [0.16, 0.25]	0.28*** (0.05) [0.17, 0.40]	-0.01 (0.02) [-0.05, 0.04]	-0.01 (0.02) [-0.06, 0.03]	-0.02 (0.04) [-0.12, 0.09]	-0.03 (0.04) [-0.14, 0.07]
{18 ; 24 f	0.24*** (0.02) [0.18, 0.29]	0.38*** (0.04) [0.27, 0.49]	-0.01 (0.02) [-0.06, 0.04]	-0.02 (0.02) [-0.06, 0.02]	-0.02 (0.05) [-0.14, 0.10]	-0.05 (0.04) [-0.16, 0.06]
{24 ; 30 f	0.26*** (0.02) [0.21, 0.31]	0.39*** (0.05) [0.28, 0.51]	-0.01 (0.02) [-0.06, 0.04]	-0.02 (0.02) [-0.07, 0.03]	-0.02 (0.05) [-0.15, 0.11]	-0.04 (0.05) [-0.17, 0.08]
{30 ; 36 f	0.25*** (0.02) [0.21, 0.30]	0.38*** (0.04) [0.28, 0.49]	0.01 (0.02) [-0.04, 0.06]	-0.01 (0.02) [-0.06, 0.04]	0.02 (0.05) [-0.11, 0.14]	-0.02 (0.05) [-0.15, 0.11]
	adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.956	adj.p.val. = 0.956	adj.p.val. = 0.803	adj.p.val. = 0.956
Num.Obs.	67901	65742	65742	65742	65742	65742
R2	0.187	0.259	0.109	0.266	0.108	0.261
R2 Adj.	0.150	0.223	0.067	0.229	0.066	0.224
Covariates				X		X
Mean F-stat					51	74

Figure: Aggregated treatment effects on labour market participation

Additional estimates

	Mean control	Compliers' Y(0)	OLS		TSLS	
			No covariates	Covariates	No covariates	Covariates
{-7 ; -1 f}	710*** (7) [691, 729]	701*** (14) [664, 738]	-5 (7) [-23, 13]	-0 (6) [-15, 15]	-13 (18) [-60, 34]	-0 (15) [-39, 38]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.724	adj.p.val. = 0.975	adj.p.val. = 0.720	adj.p.val. = 0.975	
{ 0 ; 6 f}	705*** (6) [690, 719]	712*** (10) [685, 739]	-15* (7) [-33, 2]	-12*** (4) [-24, -0]	-39** (18) [-86, 7]	-31*** (12) [-61, -0]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.109	adj.p.val. = 0.041	adj.p.val. = 0.114	adj.p.val. = 0.042	
{ 6 ; 12 f}	727*** (8) [708, 747]	758*** (19) [709, 807]	-24** (9) [-48, -1]	-26*** (8) [-46, -5]	-63*** (24) [-123, -2]	-66*** (20) [-118, -13]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.043	adj.p.val. = 0.007	adj.p.val. = 0.040	adj.p.val. = 0.006	
{ 12 ; 18 f}	727*** (7) [710, 745]	751*** (18) [703, 798]	-15* (8) [-37, 7]	-15* (8) [-35, 5]	-38* (21) [-93, 17]	-38** (19) [-88, 11]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.264	adj.p.val. = 0.182	adj.p.val. = 0.249	adj.p.val. = 0.164	
{ 18 ; 24 f}	740*** (8) [719, 762]	770*** (20) [717, 823]	-9 (8) [-30, 13]	-9 (7) [-29, 11]	-23 (21) [-77, 31]	-23 (19) [-72, 26]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.617	adj.p.val. = 0.558	adj.p.val. = 0.608	adj.p.val. = 0.544	
{ 24 ; 30 f}	756*** (10) [729, 782]	802*** (27) [732, 871]	-10 (12) [-42, 22]	-10 (11) [-40, 19]	-26 (31) [-105, 54]	-26 (28) [-100, 48]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.724	adj.p.val. = 0.665	adj.p.val. = 0.720	adj.p.val. = 0.659	
{ 30 ; 36 f}	755*** (9) [731, 779]	796*** (24) [732, 860]	-0 (11) [-29, 28]	-3 (11) [-32, 26]	-1 (28) [-73, 70]	-7 (28) [-80, 66]
adj.p.val. = 0.000	adj.p.val. = 0.000	adj.p.val. = 0.968	adj.p.val. = 0.961	adj.p.val. = 0.968	adj.p.val. = 0.960	
Num.Obs.	68567	67181	67181	67181	67181	67181
R2	0.863	0.750	0.084	0.203	0.081	0.200
R2 Adj.	0.857	0.738	0.042	0.164	0.039	0.161
Covariates				X		X
Mean F-stat				51		77

Figure: Aggregated treatment effects on disposable income

Additional estimates

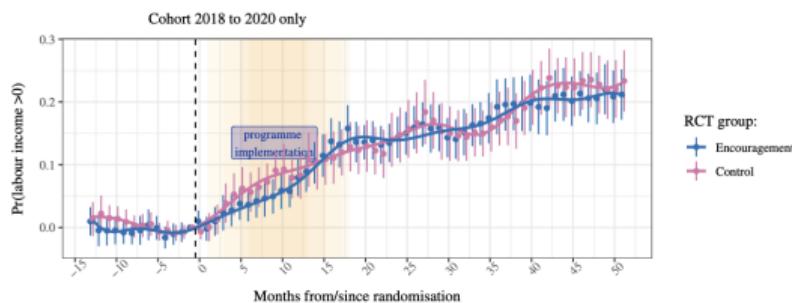
Table I.7: Aggregated effects of the programme on poverty

	Mean control	Compliers' Y(θ)	OLS		TSLS	
			No covariates	Covariates	No covariates	Covariates
{-7 ; -1}	0.97*** (0.01) [0.96, 0.99]	0.97*** (0.02) [0.93, 1.01]	0.00 (0.01) [-0.01, 0.02]	0.00 (0.01) [-0.02, 0.02]	0.01 (0.02) [-0.03, 0.05]	0.00 (0.02) [-0.04, 0.05]
{0 ; 6}	adj.p.val. = 0.000 0.97*** (0.00) [0.96, 0.98]	adj.p.val. = 0.000 0.96*** (0.01) [0.92, 1.00]	adj.p.val. = 0.539 0.01* (0.01)	adj.p.val. = 0.786 0.01* (0.01)	adj.p.val. = 0.538 0.03* (0.01)	adj.p.val. = 0.786 0.03* (0.01)
{6 ; 12}	adj.p.val. = 0.000 0.94*** (0.01) [0.92, 0.96]	adj.p.val. = 0.000 0.90*** (0.02) [0.85, 0.96]	adj.p.val. = 0.252 0.03*** (0.01)	adj.p.val. = 0.227 0.03*** (0.01)	adj.p.val. = 0.233 0.07*** (0.02)	adj.p.val. = 0.215 0.07*** (0.02)
{12 ; 18}	adj.p.val. = 0.000 0.93*** (0.01) [0.91, 0.95]	adj.p.val. = 0.000 0.89*** (0.02) [0.83, 0.96]	adj.p.val. = 0.018 0.02** (0.01)	adj.p.val. = 0.008 0.03*** (0.01)	adj.p.val. = 0.015 0.06** (0.02)	adj.p.val. = 0.008 0.07*** (0.03)
{18 ; 24}	adj.p.val. = 0.000 0.92*** (0.01) [0.90, 0.95]	adj.p.val. = 0.000 0.89*** (0.03) [0.82, 0.96]	adj.p.val. = 0.089 0.02 (0.01)	adj.p.val. = 0.026 0.02* (0.01)	adj.p.val. = 0.077 0.04 (0.03)	adj.p.val. = 0.021 0.05* (0.03)
{24 ; 30}	adj.p.val. = 0.000 0.89*** (0.01) [0.86, 0.93]	adj.p.val. = 0.000 0.84*** (0.03) [0.76, 0.93]	adj.p.val. = 0.312 0.02 (0.01)	adj.p.val. = 0.227 0.02 (0.01)	adj.p.val. = 0.291 0.04 (0.03)	adj.p.val. = 0.215 0.05 (0.03)
{30 ; 36}	adj.p.val. = 0.000 0.88*** (0.01) [0.85, 0.92]	adj.p.val. = 0.000 0.82*** (0.03) [0.73, 0.90]	adj.p.val. = 0.368 0.02 (0.01)	adj.p.val. = 0.234 0.03* (0.01)	adj.p.val. = 0.355 0.05 (0.03)	adj.p.val. = 0.220 0.06** (0.03)
	adj.p.val. = 0.000 Num.Obs. R2 R2 Adj.	adj.p.val. = 0.000 68567 0.882 0.877	adj.p.val. = 0.312 67181 0.770 0.074	adj.p.val. = 0.199 67181 0.105 0.071	adj.p.val. = 0.291 67181 0.061 0.029	adj.p.val. = 0.180 67181 0.099 0.056
	Covariates			X		X

Figure: Average effect of encouragement on poverty

Additional estimates

Average labour market participation by encouragement group and intention-to-treat estimates
Change in labour income participation



▶ Back to ITT employment

intention-to-treat: Event study

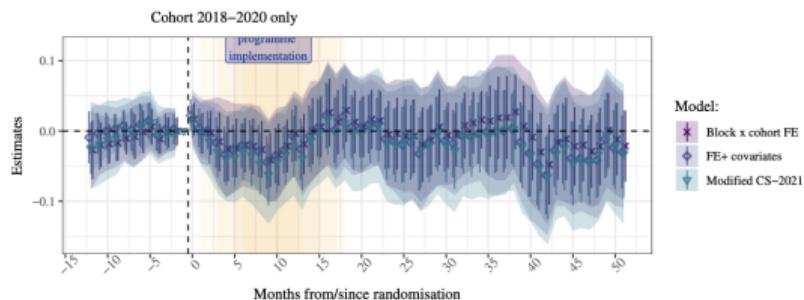
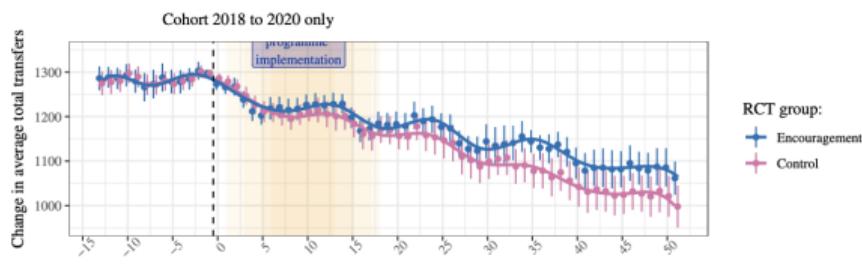


Figure: Average effect of encouragement on labour market participation, longer window

Additional estimates**Average total cash transfers by encouragement group and intention-to-treat estimates**
Change in total transfers from Cnaf[▶ Back to ITT Cash transfers](#)

intention-to-treat: Event study

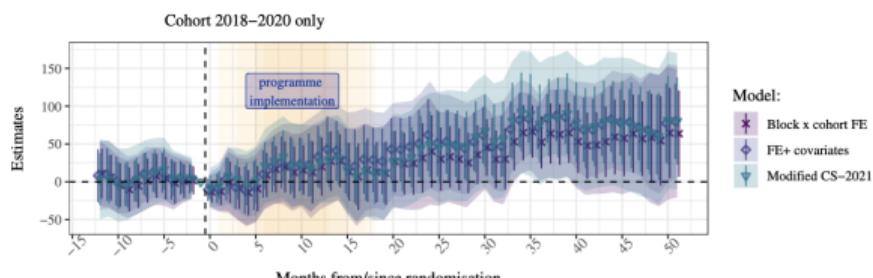


Figure: Average effect of encouragement on total cash transfers, longer window

Additional estimates

Estimating marginal distributions of potential per-capita income

- Per capita-income from 18 to 36 months since random assignment
- 100 TSLS regressions of a symmetric kernel of income over the support
- Similar distributions for treated and untreated compliers (K.S. test p.value= .2)

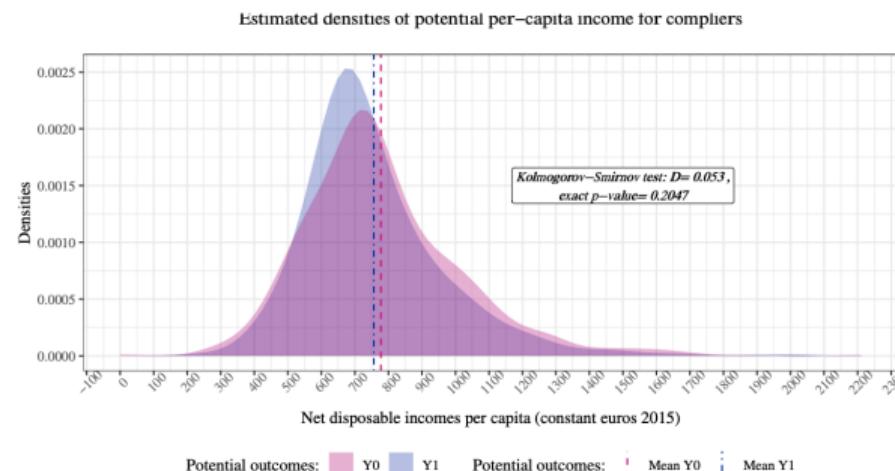
[▶ Technical details](#)[▶ Back to results' summary](#)

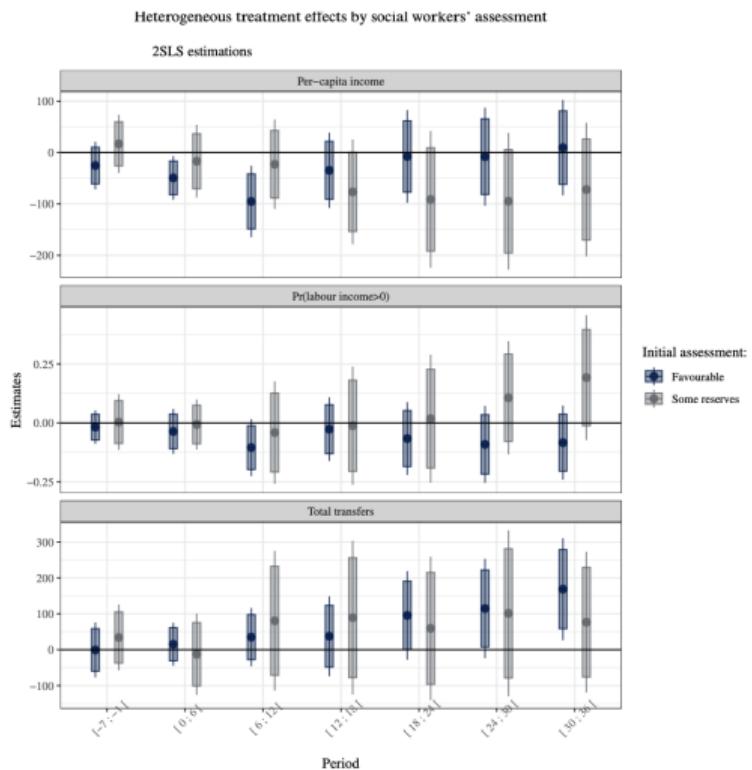
Figure: Instrumental distribution regressions of per-capita income

Additional estimates

Heterogeneous treatment effects

- Beyond Lock-in, little evidence of heterogeneous treatment effects
- Lock-in stronger for those closer to the labour market and recent cohorts
- Effects on cash transfers higher for older cohorts and parents with several children
- Interesting heterogeneous treatment effects by caseworkers' initial assessment

[Back to results' summary](#)



Additional estimates

Treated compliers' optimisation behaviours

Instrumental local regression distribution (Cattaneo, Jansson, and Ma 2024)

- ① **child:** Lower employment, bunching at kink-point
- ② **children:** No more full time-jobs
- ③ **or more children:** Higher densities for low wages

▶ Back to discussion

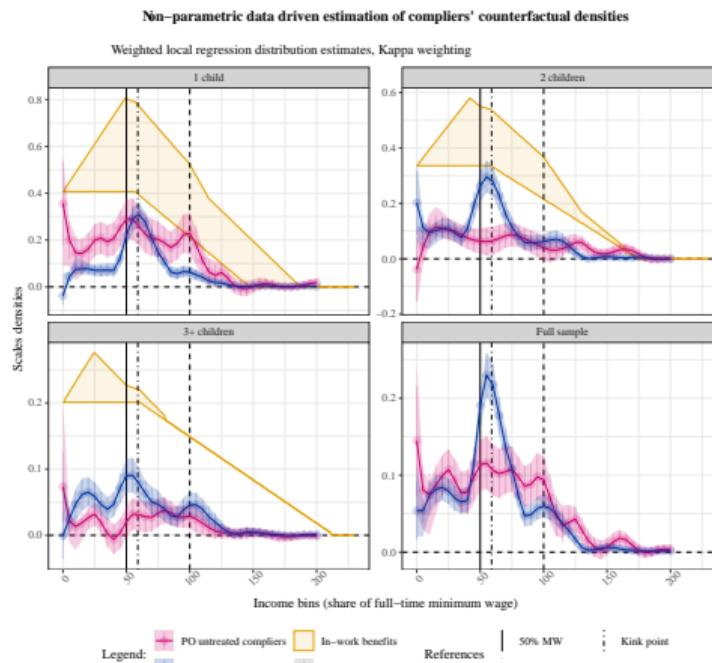


Figure: Compliers potential individual labour income over 18 months after randomisation