Tax burden on the poor: Single mothers' optimisation behaviours following an experimental activation programme in France

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Abstract

In this paper, we challenge the idea that the French tax-benefit system ”makes work pay” for single parents and analyse reactions following a randomised intensive welfare-to-work programme rolled-out from 2018 to 2022. The 2019 reform of in-work benefits was adopted in the timeline of this experiment. The intervention targetted single parents on long-term welfare and directly provided individualised and detailed information on rights and benefits, in a year-long activation programme likely to have further reduced psychological barriers and search costs. We use this experiment to measure the reactions of financially disadvantaged single parents to the reformed tax-benefit system. By employing open-source models, we demonstrate that the composition of total social transfers varies across household compositions, altering incentives. Additionally, our simulations reveal that the implicit marginal tax rate for single parents exceeds that of couples across all scenarios we explored, with a range between 50% and 60% of the full-time minimum wage representing the lowest implicit tax rate, irrespective of household structure. It is bounded above by an implicit marginal tax rate roughly twice as high. Beyond full-time minimum wages, single parents with one or two children face an implicit marginal tax rate exceeding 70%. Once single parents accept welfare provision, they unknowingly sign-in for the highest tax burden of the income distribution. We coined the term ”*Assistaxation*” to convey the idea of providing assistance in a way that becomes burdensome, overly taxing – either mentally, physically, emotionally, or financially – and very hard to escape. . Our primary contribution lies in leveraging experimental variations in assignment probabilities to infer the counterfactual distribution of untreated compliers within an instrumental variable framework, using semi-parametric weighted distribution regressions. These estimates of counterfactual densities enable us to measure the bunching mass at kink points and estimate the elasticity of labour income for treated compliers. We find substantial elasticities of labour income, approximately two, which are ten times higher than those of the comparison group. Furthermore, our analysis indicates that job re-entry leads to lower growth of disposable income for treated compliers compared to untreated compliers, resulting in increased in-work poverty. Lastly, our examination of the programme’s effects on family structure reveals significant heterogeneous effects based on the number of children at baseline. Our findings highlight the considerable tax burden faced by impoverished single-parent households, acting as a significant disincentive to employment for some, while encouraging part-time employment more generally. In either scenario, these incentives perpetuate situations in which households lack sufficient income to escape poverty, thereby leading to reliance on high levels of social transfers.

- **JEL Classification Numbers** : I38, J16, J18
- **Keywords**: Welfare-to-Work, single parents, active labour market policy, non-parametric models, distribution regressions
- **Authors’ contribution**: Arthur Heim was responsible for the experimental design, building datasets and econometic analysis. Alexandra Galitzine provided critical feedback and helped shape the research, analysis and manuscript. Both Alexandra Galitzine and Arthur Heim contributed to the final version of the manuscript.

Résumé

Dans cet article, nous remettons en question le narratif selon lequel le système socio-fiscal français ”rend le travail payant” pour les familles monoparentales et analysons les réactions suite à un programme randomisé intensif d’accompagnement social et professionel déployé de 2018 à 2022. Ainsi, la réforme de 2019 de la prime d’activité a été adoptée dans la période d’évaluation. L’intervention ciblait des familles monoparentales au RSA depuis plusieurs années et leur a directement fourni des informations individualisées et détaillées dans le cadre d’un accompagnement global d’une durée d’un an, susceptible d’avoir réduit, davantage encore, les divers freins à l’emploi. Nous utilisons cette expérience pour mesurer les réactions des familles monoparentales financièrement défavorisées au système socio-fiscal nouvellement réformé. En utilisant des modèles *open source* de ce dernier, nous démontrons que la composition des transferts sociaux totaux varie selon les compositions des ménages, modifiant fortement les incitations. De plus, nos simulations révèlent que le taux marginal implicite d’imposition pour les familles monoparentales dépasse celui des couples dans tous les scénarios que nous avons explorés, avec une fourchette entre 50% et 60% du SMIC à temps plein représentant le taux d’imposition implicite le plus bas, quel que soit la structure du ménage. Cependant, celui-ci est borné par un taux marginal d’imposition implicite environ deux fois plus élevé. Au-delà du salaire minimum à temps plein, les familles monoparentales avec un ou deux enfants font face à un taux marginal d’imposition implicite dépassant 70%. Sans le savoir, celles qui demandent le RSA signent en même temps pour le plus haut taux de taxation de toute la distribution de revenu. Nous avons proposé le terme ”*Assistaxation*” pour désigner ce phénomène consistant à taxer massivement les ressources économiques, physiques et mentales des personnes ayant recours à l’aide publique, leur laissant au passage peu de moyen de s’en extraire. Notre principale contribution réside dans l’exploitation des variations expérimentales des probabilités d’affectation pour déduire la distribution contrefactuelle par variable instrumentale, en utilisant des régressions de distribution semi-paramétriques pondérées. Nous utilisons ces estimations des densités contrefactuelles pour estimer la masse de revenus aux coudes des contraintes budgétaires et mesurer l’élasticité du revenu du travail des participantes. Nous obtenons des élasticités élevées d’environ 2, soit 10 fois que celle du groupe de comparaison. De plus, notre analyse indique que la reprise d’emploi augmente moins le revenu des participantes que dans le contrefactuel, entraînant une augmentation de la pauvreté laborieuse. Enfin, notre analyse des effets du programme sur la structure familiale révèle des effets hétérogènes significatifs en fonction du nombre d’enfants au début de l’étude. Nos résultats soulignent le fardeau fiscal considérable auquel sont confrontées les familles monoparentales défavorisées, qui constitue un puissant frein à une reprise d’activité pour certaines, tout en incitant fortement à l’emploi à temps partiel pour celles qui auraient travaillé davantage. Dans tous les cas, ces incitations perpétuent des situations où les ménages ne disposent pas de revenus suffisants pour sortir de la pauvreté, tout en maitenant une forte dépendance aux aides sociales.

- **Codes Journal of economic literature** : I38, J16, J18
- **Mots clés**: Welfare-to-Work, single parents, active labour market policy, non-parametric models, distribution regressions
- **Contribution des auteurs et autrices** : Arthur Heim a été responsable du design expérimental, de la préparation et de l’analyse des données. Alexandra Galitzine a fourni des commentaires critiques et a contribué à façonner la recherche, l’analyse et le manuscrit. Alexandra Galitzine et Arthur Heim ont tous deux contribué à la version finale du manuscrit.

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« *'Faites des enfants c'est fantastique vous vous sentirez plus femmes et accomplies que jamais'*, *mais faites-les dans une société en dégringolade, où le travail salarié est une condition de survie sociale, mais n'est garanti pour personne, et surtout pas pour les femmes. Enfantez dans des villes où le logement est précaire, où l'école démissionne, où les enfants sont soumis aux agressions mentales, les plus vicieuses, via la pub, la télé, internet, les marchands de sodas et confrères. Sans enfant, pas de bonheur féminin, mais élever des gamins dans des conditions décentes sera quasi impossible. Il faut, de toutes façons, que les femmes se sentent en échec* ».

Virginie Despentes (2006), King Kong Theorie, Grasset. Paris.

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I Introduction

The debate surrounding redistribution policies through progressive taxation and generous transfers revolves around the tension between social justice and economic efficiency. On the one hand, redistribution is essential for ensuring the economic well-being of the less fortunate, acknowledging that differences in earnings often stem from factors beyond individuals' control, such as innate ability, social background or sheer (bad) luck (Fleurbaey and Maniquet 2018). On the other hand redistributive measures can hamper incentives to work among both the wealthy and recipients of transfers, potentially undermining overall economic productivity (Ebert 2005; Piketty and Saez 2013). Over the past three decades, traditional Welfare States have been put under the pressure of fiscal consolidation and shifted towards active labour market policies (ALMP) (J. P. Martin 2015; Crépon and van den Berg 2016). These welfare reforms took various forms around the world but dramatically reshaped labour market institutions and social insurance mechanisms with welfare-to-work programmes, in-work benefits, payroll tax exemptions, training initiatives, job search assistance and monitoring and so on. The US Earned Income Tax Credit (EITC) plays a special role in the economics of welfare reforms. It has been extended and inspired many similar policies around the world, although there is an ongoing debate among academics on its effects on employment (See Section II).

France's adoption of the *Revenu de solidarité active* (RSA) in 2008 is one of such examples and subsequent reforms in 2016 and 2019 – separating the in-work benefit into *Prime d'activité* (PA) and increasing the amounts higher up the wage distribution – underscore the ongoing efforts to incentivise work and increase disposable incomes of low wage workers (Gurgand and Margolis 2008; Bargain and Doorley 2011; Bargain and Vicard 2014; Sicsic 2019).

However, these policies rely on several pre-requisites. First, people need to understand how benefits change their incentives to work. When people have limited understanding of the actual tax-benefit schedules they face, they are likely to perceive them in a crude fashion and under-react to the incentives. Recent work shows that households are often unaware or misinformed about the incentives of tax-benefit systems. In particular, they have problems understanding non-linearities in the system and rely instead on some "*mentally linearised*" tax rate (Rees-Jones and Taubinsky 2020; Caldwell, Nelson, and Waldinger 2023). Uncertainty may be the result of more complex features of the tax code, such as the phase-in and phase-out regions for tax-based transfer programmes or rules for married tax filers. Conversely, providing additional information can trigger large reactions at the intensive margins, inducing large distributional effects (Raj Chetty, Friedman, and Saez 2013; Raj Chetty and Saez 2013; Kostøl and Myhre 2021). However, the means by which information are provided seem to be equally important; many information-only and nudge experiments have proven ineffective (Linos et al. 2020; Nyman, Aggeborn, and Ahlskog 2023) or delivered much lower effects than human assistance (Castell et al. 2022; Finkelstein and Notowidigdo 2019; Bergman et al. 2019).

Second, another prerequisite is that there should actually be incentives to work. The latter may seem obvious considering the amount of public money spent in active labour market policies. However, it may not be the case for everyone, and there may be large frictions impeding adjustments. At the macro-economic level, many scholars underlined the paradox that despite massive increases in public spending in active labour market policies, long-term unemployment and poverty have not been reduced (Vandenbroucke and Vleminckx 2011; Jaehrling, Kalina, and Mesaros 2015; Van Winkle and Struffolino 2018). At the micro-level, a large body of evidence shows that active labour market policies have little effects on labour market outcomes of single parents and often detrimental effects on their health and well-being (Cook 2012; Pega et al. 2013; Gibson et al. 2018), sometime extending to their children (Løken, Lommerud, and Holm Reiso 2018).

In this paper, we challenge the idea that the French tax-benefit system "makes work pay" for single parents and analyse reactions following an intensive welfare-to-work programme of single parents on long-term welfare. The intervention called **Reliance** was rolled-out each year from 2018 to 2022 using a randomised encouragement design to recruit each cohort. The sample includes 1666 households from the 4 first cohorts; 826 controls and 840 single parents in the encouragement groups. 95% of the sample are single mothers followed through for at least 30 months after random assignment. Our data come from the National family allowance fund (Cnaf), the administration in charge of welfare payment, and include all social transfers and reported incomes, allowing to measure labour and non-labour incomes by spouse. This high stake programme has drawn 4 times more resources than the usual budget for social support of welfare recipients to offer a year-long programme to 82 participants each year on average (mean take-up $\approx$ 39%). The intervention was designed to provide both individual and group support through thematic workshops, sometimes involving other institutions. The primary focus of the intervention was to create and validate realistic professional projects, aligning expectations with job opportunities and enhancing job search efficiency. Additionally, the intervention provided extensive social support related to parenthood, self-esteem, gender norms, and other relevant topics. Furthermore, the intervention included several workshops on accessing rights and direct support to alleviate the emotional burden associated with administrative procedures. Notably, regular sessions with social workers from the Family allowance fund (Caf) were organized to help participants understand and access their rights, and we also provided simplified plots of the amounts of social benefits over incomes from simulation models of the Family allowance fund.

Our previous evaluation showed no average effect of the programme on employment and disposable income beyond an expected lock-in effect but revealed heterogeneous treatment effects, particularly concerning employment and disposable income by number of children at baseline (Heim 2024). The key message of this paper is that selection effects are important and hard to neutralise without random assignment. Considering the lack of evidence on similar programmes in Europe, this paper focused on clean identification on outcomes typically investigated in labour economics. Drawing on additional insights from the qualitative evaluation, our primary hypothesis is that while preparing participants for future job re-entry, the programme enhanced their knowledge and understanding of the tax-benefit system. With intensive social support and tailored interventions, it could provide them with the means to define and pursue achievable goals with a clearer understanding of the consequences of their choices.

In general, observing the effects of information requires participants to actually learn things, so incentives need to be largely unknown prior to the intervention. They also need to be sufficiently large to induce changes, and adjustments must be fairly quick. Moreover, the causal mechanism must concern micro-level behaviour, as opposed to things like collective wage bargaining, for the identifying assumptions to be valid (Nyman, Aggeborn, and Ahlskog 2023).

In the timeline of this experiment, the 2019 reform of in-work benefits was adopted unexpectedly in the last quarter of the first cohort's training when the second was being recruited. It was followed by a large increase in the number of recipients, largely coming from previously ineligible households higher on the wage distribution, but also from newly registered (Dardier, Doan, and Lhermet 2022). However, current evaluations are unable to disentangle the extensive margin response from the reduction in non take-up[1].

Our setting ends-up being an ideal framework to measure the reactions of poor single parents to the reformed tax-benefit system. While we cannot measure the effect of the reform *per se*, as all cohorts have been exposed, we can measure if the programme had effects that are consistent with the incentives of the tax-benefit system. Our intervention directly provided individualised and detailed information in a year-long social support likely to have further reduced psychological barriers and search costs. If that is the case, our experiment reveals *quasi-frictionless* elasticities of labour for single parents on long-term welfare. In a way, we leverage this randomised experiment to uncover more structural reactions to the French tax-benefit system, which we therefore need to document. Our investigation is thus guided by two research questions:

1) *How does the tax-benefit system adjust to earnings and family composition ?*
2) *How do single parents react to the incentives of the tax-benefit system*?

We build our arguments in three steps. First, we review the literature on welfare reforms and single parents and build a simple theoretical framework grounded in the literature on bunching. The idea is to think of the way the programme may have affected participants introducing imperfect knowledge, psychological barriers, and adjustment costs with models commonly employed in recent similar research (R. Chetty et al. 2011; Henrik J. Kleven and Waseem 2013; Kostøl and Myhre 2021). We come back to the model with empirical estimates of bunching mass and use this simple framework to estimate and compare the observed elasticities across groups. However, our primary argument is a critique constructed from recent empirical findings, perspectives from feminist economics, and advancements in household economics. Second, we use an open-source model of the tax-benefit system to simulate social transfers by family structure and size, estimate implicit marginal tax rates and demonstrate the disparities and divergent incentives faced by single mothers with different numbers of children. Third, we analyse the causal effects of the programme on income distributions and family structures.

[1] Bozio et al. (2023) worked for France stratégie to evaluate the effect of the reform and we were part of the reviewing committee. Their identification strategy based on difference in difference by number of children was rejected after a placebo test on data from the year before. The report is inconclusive but did not remain in a file drawer, which we think is a good thing for science.

As an answer to our first research question, simulations allow us to illustrate three under-reported[2] stylised facts of the French benefit system: First, the composition of total social transfers is very different across household' structures. Specifically, the greater the number of children, the higher the family and housing benefits, while the lower the RSA and PA. Second, PA amounts are smaller and deplete at faster rates after the minimum wage for single mothers than for couples. Third, the implicit marginal tax rate of single parents is higher than couples in all the simulations we ran, but there is a range between 50% and 60% full-time minimum wage with minimal implicit tax rate and stable levels of all social transfers.

An important consequence of the first point is that different social transfers have different schedules and eligibility rules and are more or less sensitive to adjustments of family structure and incomes. For instance, housing benefits are the same amount with or without a partner, but use the same formula with one or two incomes. The complex interactions between various social transfers with differing schedules result in a much higher tax burden for single parents than for couples. That is mostly because RSA and PA are differential incomes and while their baseline amount increases with the number of children, they entirely deduce all family benefits, a lump-sum amount if they receive housing benefits, and all child support single parents may receive. That means that the higher their amounts of non-labour incomes - including payment from non-custodial parents - the lower welfare and in-work benefits they get. The family support allowance (ASF) – which substitutes child support under strict conditions – is also largely deduced from RSA and PA. As Pucci and Périvier (2022) noted before us, the French system saves welfare spending by taxing 100% of child support of single-parents with RSA or PA allowance. For single parents on welfare, receiving child support reduces their incomes if they received ASF, and changes nothing otherwise.

Ultimately, the range between 50% and 60% full-time minimum wage has the lowest implicit tax rate of the income distribution whatever the household structure, and is bounded by an implicit marginal tax rate roughly twice as high above. Beyond full-time minimum wage, single parents with one or two children face an implicit marginal tax rate higher than 70%. That means that the most typical labour contract for workers with little experience or education is the most heavily taxed of the distribution, making-it very hard for single parents to increase their income through work. In practice, a single mother with two children working full-time on minimum wage would only keep € 23 out of a € 100 net wage increase, with a reduction implemented from the following quarter. Couples very rarely have that level of tax rate anywhere in the income distribution. We discuss additional incentives in the paper, in particular regarding cohabitation and number of children.

Overall, the system is overwhelmingly complex. The precise incentives are not known, even to otherwise well-informed individuals[3]. This lack of salience hinders informed decision-making to the point where it wouldn't be far-fetched to talk about *obfuscation*, *i.e.* intentionally making something more complex or unclear to hide the truth, confuse others, or obscure information. While administrative burden already creates large costs for recipients, the complex interactions between different social transfers - with different schedules - inflates the cost of exit. Once single parents accept welfare provision, they unknowingly sign-in for the highest tax burden of the income distribution. We use the term *Assistaxation* to convey the idea of providing assistance in a way that becomes burdensome, overly taxing – either mentally, physically, emotionally, or financially – and very hard to escape.

Moving on to the data from this experiment, we present three main sets of estimations. First, we estimate typical bunching estimators comparing participants with non-participants using polynomial regressions on the density of observations around kink-points. Adjusting for diffuse bunching, round numbers and other kink points, we find that participants bunch at kink points, with few reporting incomes exceeding 75% of the full-time minimum wage. Facing the highest variations and levels of implicit tax rates, the bunching is particularly pronounced among single parents with two children at baseline. Conversely, the distributions of labour incomes among the control group and never-takers exhibit much lower and diffuse bunching mass at the 60% kink point and another mass at the full-time minimum wage. These first estimates show large differences between groups and also reveal that without the programme, single parents seems pretty un-reactive to the large variations in monetary incentives they face. Using our estimates of the implicit marginal tax rates around 60% minimum wage and bunching mass plugged-in an iso-elastic utility function, the observed elasticities are between .20 and .30 for non-participants. These range of estimates are close to those found in the literature. However, the same elasticity around kink-points among participants is closer to 1.

[2] The recent exceptions are the dedicated reports by Périvier (2022a) and Pucci and Périvier (2022), backing research papers (Allègre, Périvier, and Pucci 2021), and the book coordinated by Le Pape and Helfter (2023).

[3] Despite 6 years of PhD studying this system, we still uncover new rules on a regular basis.

Our main contribution leverages the experimental variations of assignment probabilities to infer the counterfactual distribution of untreated compliers in an instrumental variable framework using semi-parametric weighted distribution regressions. Our identification strategy is facilitated by one-sided non-compliance and a strong first stage allowing to identify the entire distribution of potential outcomes of treated and untreated compliers using causal weights (Frölich and Melly 2013). Unlike the κ-weighting representation of the LATE of A. Abadie (2003), for which some estimators can create negative weighting schemes (Słoczyński, Uysal, and Wooldridge 2022), one-sided non-compliance ensures positive weights and an average-treatment-effect-on-the-treated (ATT) interpretation of instrumental variable estimates (Frölich and Melly 2013). The instrument propensity score is given by design with .5 in expectation but we use a Probit to predict the individual propensity score accounting for uneven numbers of individuals in blocks and imbalance in small blocks.

We then estimate the distribution of potential labour income of treated and untreated compliers using causal weights with the semi-parametric distribution regression estimator proposed by Cattaneo, Jansson, and Ma (2021). The latter implements local polynomial regressions with data-driven optimal bandwidth and provide de-biased simultaneous confidence intervals around the estimated distributions. Since our estimates are based on strictly positive values of income, the densities of 0 income is missing and the estimates could neglect these extensive margin differences. However, this estimator also allows to rescale the distributions. We can therefore estimate the potential outcome masses at 0 using TSLS and use them to rescale the potential outcomes' densities. With this feature, we are able to visualise reactions at both the extensive and intensive margins simultaneously.

Our estimates confirm the sharp bunching and show that the distribution of incomes of untreated compliers would have been higher had they not participate. We use these estimates of the counterfactual densities to estimate the bunching mass at kink point and retrieve the elasticity of labour income of treated compliers. The latter is more than twice as high as the one estimated with typical bunching methods. We confirm these results with estimates of quantile intention-to-treat effects and an additional analysis on the differential effect of job re-entry for treated and untreated compliers using an instrumented triple difference. We leverage the variation in the timing of job re-entry and the share of treated compliers at each date to measure the change of disposable income for treated and untreated compliers in an event-study like estimation. These results confirms that job re-entry causes lower growth of disposable incomes for treated compliers relative to job re-entry for untreated compliers, and increases in-work poverty.

Finally, our analysis of the effects of the programme on family structure also reveals large heterogeneous effects by number of children of baseline. In brief, those with one child are more likely to re-partner, those with two children are less likely to get pregnant for the year following the programme while parents of three or more children are more likely to remain with their older children longer. These effects are large and lasting, showing that active labour market programmes affect many important decisions going far beyond labour market participation. Ultimately, our final result confirms the analysis of Heim (2024) showing a precise null effect on disposable income per consumption unit on the entire quantile distribution. While the programme affected many important decisions and induced large changes, these reactions end up leaving their disposable income at the same level it would have been without the programme, but with critical differences in their composition.

This second analysis allows to better understand the results at the extensive margin detailed in Heim (2024). The latter shows that the programme has no average effect but enrolls single mothers most likely to find a job on their own. These new results show that the programme increased their understanding of the tax-benefit system in a consequential way. As a result, compliers with two children work part-time instead of full-time; compliers with one child are less likely to work, more likely to report living with their partner, aligning the households' earned incomes to the kink of the in-work benefit; those with three children understood that their level of transfers was mostly determined by the number of children in their household, delayed the departure of their oldest children and increased their labour force participation such that they don't lose too much. Our findings also indicate that except single parents with one child at baseline, treated compliers are more likely to be the sole earner than untreated compliers, including when they re-partner.

This research contributes to different strands of literature that we largely discuss in Section II. Our paper was mostly inspired by the few examples in the literature on distributional effects of activation policies. In particular, our work is closest to the evaluation of a similar programme in the US by Alberto Abadie, Angrist, and Imbens (2002), as Cattaneo, Jansson, and Ma (2021) also used these data to showcase their new method. In this paper, the authors use the κ weightings of A. Abadie (2003) to develop instrumental quantile treatment effects and showed large heterogeneous effects by gender. For women, Effects generally positive and higher at the bottom of the income distribution while men have no effect but at the upper end of the income distribution. Autor, Houseman, and Kerr (2017) also use this framework on a work-first job placement programme in the US. However, by focusing on the distribution of potential income and not quantile treatment effects, our approach does not need the often implausible rank invariance assumption such models require (Chernozhukov and Hansen 2008; Melly and Wüthrich 2017; Huber and Wüthrich 2019). Our methodological choices also reflect the recent working paper of Garbinti et al. (2023) using variations in bunching mass over time following a wealth tax reform under the Hollande presidency. To the best of our knowledge, this paper is the only other using instrumental variables to estimate bunching masses and elasticities and avoid parametric assumptions on the counterfactual densities.

Our results also relates to the analysis of the effect of the EITC on the poverty of single parents by Raj Chetty, Friedman, and Saez (2013). This paper uses differences in knowledge of the EITC which they approximate by the degree of bunching across counties. Using that in an event-study framework around birth of a child triggering eligibility, they show that the EITC reduces poverty. In another paper, Raj Chetty and Saez (2013) analysed the effect of providing information on the EITC through professional tax filers and also showed that complying tax-filers induced large bunching. However, we should emphasise the key difference in counterfactual. In the US, not knowing about the EITC means forgoing significant transfers. In our setting, not knowing about incentives is taxed, making the *make work pay* narrative fundamentaly wrong for single parents in France. Conversely, the information experiment on the Swedish EITC of Nyman, Aggeborn, and Ahlskog (2023) shows little to no reaction in a setting where the tax credit is automatic and separated for spouses.

If we attribute the primary effect of the programme to an enhanced understanding of the tax-benefit system, our findings underscore the significant tax burden faced by impoverished single-parent households, which serves as a potent disincentive at the extensive margin for some, while prompting a strong economic incentive for part-time employment in general. In either scenario, these incentives perpetuate situations where households lack sufficient income to escape poverty, thereby leading to a reliance on high levels of social transfers. The data suggests that among untreated compliers, increased work participation is not hindered by a lack of ability or opportunity, but rather by the burden of taxation.

Our work emphasises important contradictions in the tax-benefit system and politicians' narratives. For single parents, social transfers are both too low to lower poverty and deplete too fast to *make work pay*. Yet most do not know that. The government has very little incentives to make rules clearer for them, as reactions would most likely induce lower participation and working hours of single mothers, further increasing in-work poverty, gender inequalities in the labour market, and poverty among children. At the other side of the income distribution, Garbinti et al. (2023) analysed the effect of a wealth tax reform in France and also find strong reactions - lower wealth for more informed taxpayers. However, these results show that they most come from actual tax evasion and mis-reporting made simpler by the new tax regime. Conversely, low income families are more heavily controlled through algorithmic targeting, controllers are granted access to recipients' bank accounts and now, the administration receives direct feeds from payroll taxes, making income manipulations almost impossible and highly deterring. It also induces a large loss of freedom and privacy, as well as higher implicit tax rates (Quadrature du net 2023; Défenseur des droits 2017). Facing harder employment conditions and poor quality jobs (Rodrik and Stantcheva 2021), single parents on welfare really seem trapped in a poverty spiral from which it is hard to escape without drastic change in public policies.

Ultimately, there is a fundamental question on preferences for redistribution, social justice and optimal taxation (Blundell et al. 2009; Maniquet and Neumann 2021; Saez and Stantcheva 2016; Stantcheva 2021). Recent work show that social preferences are heavily politically polarised but do change with better knowledge (Kuziemko et al. 2015; Alesina, Stantcheva, and Teso 2018; Hvidberg, Kreiner, and Stantcheva 2023). A key driver of social preferences revolves around inequalities of opportunities and our work demonstrates that the tax-benefit system creates and exaggerates some of them. We hope that calling-out these injustices may help foster reforms that finally allows single parents to gain agency, means and freedom to improve their lives and that of their children.

The remaining of the paper is structure as Followed: Section II reviews the literature on welfare reforms and single parents, including our theoretical framework and a discussion on gender and household economics. The French tax-benefit system and the intervention are in Section III followed by the description of the data and a first set of descriptive results in Section IV. We discuss our identification strategy in Section V, present our main results in Section VI, and additional estimates in Section VII. Section VIII summarises and discusses our findings.

II Single parents and welfare reforms

In this section, we conduct a critical review of the literature in three parts. We begin by examining the economics of welfare reforms and recent academic debates surrounding the effects of the Earned Income Tax Credit (EITC). We explore the confounding effects of additional activation measures and the importance of factors such as stigma, information frictions, and costs. In section II.2, we incorporate these constraints by adapting the typical model of labour supply with non-linear budget constraints. We then argue that current models and policies often overlook the impact of gender norms, which leads to an incomplete picture for a population like single mothers. Therefore, in part II.3, we focus on understanding the specific constraints and incentives that women, mothers, and single parents may face when making decisions about labour market participation.

II.1 Welfare reforms and the labour market

The literature on this topic originated in the 1960s and early 1970s, when the static model of labour supply was applied to the work incentives of a negative income tax[4]. In their review, Chan and Moffitt (2018) underline how the international literature has been closely tied to policy developments and to welfare reforms in different countries. Many economic theories were built on reforms and new reforms were inspired by the diffusion of economic ideas[5]. While models usually build on genderless agents, It is worth noting that a significant share of the literature was concerned about the effects of welfare on fertility and single-parenthood (Robert A. Moffitt 1998 ; Ellwood 2000).

Typical welfare reforms with an activation orientation introduce in-work benefits[6], increase them up to a point in the wage distribution, add work requirements, sometimes in systems with variations in the marginal tax rates. Figure 1 reproduces the illustrations of such reforms from Chan and Moffitt (2018). In this typology, the 2019 reform of in-work benefit in France corresponds to the Panel b, moving-up monetary incentives with a necessary flatter slope after the kink C′. These four panels conveniently represent the typical behavioural reactions that such welfare reforms potentially introduce. The point being, with heterogeneous agents, reactions can go in many directions and the overall effects can hide sizeable reactions at the intensive margin. The first generation of static models were already very clear about that (See e.g. Burtless and Hausman 1978; Robins 1985). Theoretical models predict that the extensive margin decision depends on taxes and transfers at the desired level of earnings, *i.e.* the intensive and extensive margin decisions are inter-dependent (Eissa, Kleven, and Kreiner 2006).

The effect of the EITC: A controversy The Earned income tax credit plays a special role in the academic literature and political discourses on *making work pay*, inspiring many similar schemes in the world - including France (See Section III.1). A large literature documents a sharp rise in the employment of single mothers, especially single mothers with two or more children, following the 1993 EITC expansion for these family types, and concludes that reactions at the extensive margin strongly dominate those at the intensive margin. (Meyer and Rosenbaum 2001; Grogger 2003; Eissa and Hoynes 2006; Herbst 2011; Hoynes, Miller, and Simon 2015).

However, there is an academic debate on the validity of these estimates[7]. In a first working paper, H. Kleven (2019) reconsiders the data and the many reforms of the EITC. He estimates hundreds of difference-in-differences considering different reform episodes, different samples, different comparison groups, different extensive margin measures, and different control variables. Allowing for all possible permutations of specification choices, the estimates are symmetrically distributed around zero, except those on the 1993 reform. For the latter, the distribution of estimates is shifted to the right and has a mean elasticity of 0.63. Leaving aside the 1993 reform, the main message from this paper is that the EITC has no effect on labour *per se*. A conclusion challenged by Whitmore Schanzenbach

[4] Burtless and Hausman (1978); Diamond (1998). See R. Moffitt (1992) for a review of this early literature.

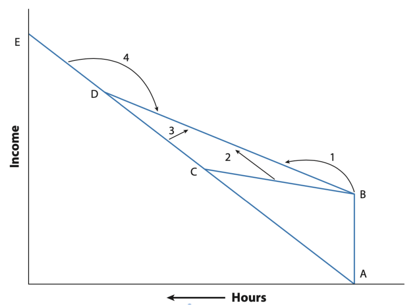
[5] For instance, many reforms were directly influenced by the negative-income tax model of Friedman, and many new models came after some new policies were implemented. See Robert A. Moffitt (2003) for a specific discussion of the role of Friedman's model in shaping welfare reforms around the world.

[6] In-work benefits can be tax credits or cash transfers.

[7] Identification and methodological challenges are also important. In particular, they mostly use difference-in-differences where treatment status is based on fertility, a very strong assumption considering the impact of children on labour market outcomes *per se* (H. Kleven, Landais, and Leite-Mariante 2024). Regressions aggregating different groups treated at different times may be biased by negative weightings in two-way fixed-effects regressions (Goodman-Bacon 2021).

Figure 1: A typology of welfare reforms from Chan and Moffitt (2018)

(a) Negative income tax



(b) Increased in-work benefits

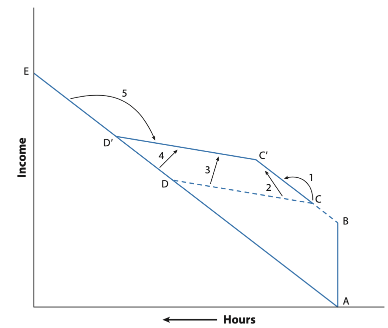


Figure 6

Effects of increasing the deduction of a welfare program. The budget constraint before increasing the deduction is ABCDD′E. The budget constraint after increasing the deduction is ABCC′D′E. The numbered arrows represent the labor supply effects of increasing the deduction.

(c) Work requirements

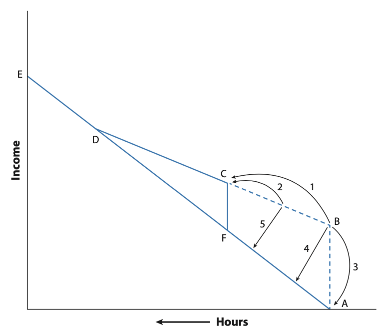


Figure 5

Effects of imposing a work requirement on a welfare program. The budget constraint without the work requirement is ABCDE. The budget constraint with the work requirement is AFCDE. The numbered arrows represent the labor supply effects of the work requirement.

(d) Work requirements with a kink

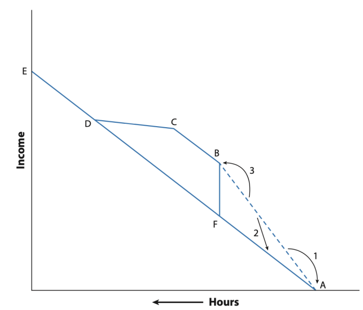


Figure 4

Effects of imposing a work requirement on an earnings subsidy. The budget constraint without the work requirement is ABCDE. The budget constraint with the work requirement is AFBCDE. The numbered arrows represent the labor supply effects of the work requirement.

and Strain (2021), who propose a similar re-analysis of the many waves of extension of the EITC, with different samples and including controls for the business cycle, a larger observation window and scope of the analysis with non-federal schemes. While Kleven uses labour participation the previous week (the official ILO measure), they choose labour market participation over the year, arguing that the EITC depends on yearly labour income. They also argue that the three-way fixed effect interaction between state x time x children of Kleven absorbs the effect. In a revised version, Henrik Jacobsen Kleven (2023) argue that the specifications of Whitmore Schanzenbach and Strain (2021) are "*strong outliers in the distribution of estimates across a wide range of specifications*".

There are four main arguments explaining why estimates for the 1993 expansion of the EITC are different from others. First, it is strongly confounded by welfare reforms occurring from 1992 to 1996, including the Temporary Assistance for Needy Family (TANF) programme and the many welfare-to-work programmes of the 1990's. Second, it was bigger and more advertised, which matters in a world with optimisation friction. Third, the economy was booming, which may have had heterogeneous effects on single women with and without children, especially at a time where welfare reform were putting strong pressure on single mothers to find employment. Fourth, significant changes regarding social norms towards welfare assistance and work were documented in the 1990s (Peterson 1997; Ellwood 2000; Robert A. Moffitt 2015).

Welfare-to-work programmes and other activation measures Monetary incentives are generally conditioned upon participation in other active labour market activities, including welfare-to-work. The economic motivations for activation concern 1) human capital, 2) frictions in the labour market, and 3) the moral hazard effects of unemployment insurance and welfare provision (Crépon and van den Berg 2016). Researchers and policymakers have implemented various *carrots vs. sticks* schemes to foster participation in ALMP, insisting on a "threat" or "pull" effect, with varying results (Arni, Lalive, and Van Ours 2013; Hohmeyer and Wolff 2018; Avram, Brewer, and Salvatori 2018; Morescalchi and Paruolo 2020). They have been widely implemented with many variations in policy instruments, target groups and context. As a result, the econometrics literature has dramatically improved in quality (Heckman, Lalonde, and Smith 1999; Rothstein and von Wachter 2017, Mars) and has been extensively reviewed in several systematic reviews (Card, Kluve, and Weber 2010 ; Filges et al. 2015; Card, Kluve, and Weber 2018; Vooren et al. 2019). They generally exhibit positive effects, but with considerable variations in effect sizes. Results between systematic reviews also vary depending on inclusion criteria, especially regarding the quality of the research design and target group.

However, the literature on the effects of welfare-to-work on single parents is very contrasted, and usually more negative (Smedslund 2006; Gorey 2009; Campbell et al. 2016; Gibson et al. 2018). There is a strong discrepancy between the numerous experimental research in North America and their scarcity in Europe. As a consequence, most European evaluations rely on stronger hypotheses and may be far less reliable (Heim 2024). For instance, the review from Bergemann and Van Den Berg (2008) on ALMPs for women in Europe concludes that they are very effective but only 4 out of 39 included research use a randomised experiment, two of which show negative results. There is also a large heterogeneity on the type of intervention and treatment effects. For instance, Alberto Abadie, Angrist, and Imbens (2002) use instrumental quantile regression on a randomised welfare-to-work programme in the US and find strong positive effect concentrated at the bottom of the distribution for women. Conversely, Mogstad and Pronzato (2012) analyse the Welfare reform in Norway which introduced in-work benefits and workfare requirements and show average positive effects on single mothers on the one hand, and large negative effects for those at the bottom of the distribution. This heterogeneity may also hide exacerbated psychological costs in that the mandatory aspect of programmes is frequently experienced by participants as a loss of autonomy, especially when said programmes are deemed inadequate for those they're supposed to help. The systematic review of Shahidi et al. (2019) concludes that social assistance programmes in high-income countries are insufficient in preserving the health of socio-economically disadvantaged populations, indicating that the scope and generosity of existing programmes fall short in compensating for the negative health consequences associated with poverty.

In France, around 1/4 of all minimum aid recipients (of which 54% are women) cite health issues as their main obstacle to employment (Cour des comptes 2022). Policymakers therefore face an asymmetric information problem: they do not know which part of the population cannot work and which part can. The definition of *being able to work* is a social construct and active labour market policies typically changed these definitions[8]. When imposing workfare obligations, policies can "*miss*" their targets in two ways: i) by futilely mobilising or penalising people who cannot work; ii) by forcing people who do not need them to take part in these programmes. The efficacy of mandatory welfare-to-work programme therefore depends on the size of each group and raises important ethical questions. For Molander and Torsvik (2015), mandatory workfare cannot be justified if such groups exist in the eligible population, and should only exist when they target specific populations.

These considerations were formalised by Kreiner and Tranaes (2005) in a model incorporating people who are in work, unemployed or not looking for work, and where it is not possible (or costly) to observe job-seeking efforts. One consequence of this configuration is that the risk of involuntary unemployment is under-insured. By incorporating obligations and constraints - even unproductive ones - the model predicts better coverage of the risk of unemployment for workers. Mandatory welfare-to-work then acts only as a screening mechanism where the constraint reveals the latent types of individuals. If these policies are sufficiently effective to produce the expected incentives, then they allow better targeting of minimum social benefits' recipients. Pavoni and Violante (2007) propose an *optimal welfare-to-work* model considering together unemployment insurance, workfare and welfare. The optimal design they propose includes decreasing replacement rate of the unemployment insurance, in addition to job-search monitoring and support, training and career orientation plans upon exhaustion, and a basic income schemes for those who *cannot* work. The latter point is crucial in acknowledging that some individuals may be unable to hold a job and should not be imposed costly activation measures.

[8] Imposing workfare obligation to claimants of the disability allowance for instance. See for instance the evaluation of the programme *pathway to work* in the UK from Adam et al. (2008).

Non take-up of social programme At this point, it is useful to add that in-work benefits and tax credits are not directly provided and require people to submit their incomes and accept the conditions under which such benefits are offered. The application processes may be highly taxing *per se*. Welfare reforms and activation are strongly linked to a logic of reducing and rationalising public spending, driven both by public debt concerns and the rise of the New Public Management paradigm, which first gained prominence in English-speaking and Scandinavian countries (White 2019). This led to a strengthening and institutionalisation of the logic of evaluating policy implementation and outcomes (Lacouette Fougère and Lascoumes 2013; Bozio 2014; Fougère and Heim 2019; Bono et al. 2021), but also to the multi-faceted complexification of administrative processes. This directly connects to the notion of *"administrative burden"*, and how seemingly parametric measures may have deep economic, political and social impacts. Administrative burden are composed of three components: Learning costs, compliance costs and psychological costs[9]. According to Herd et al. (2023) in the US case, burdens are "*policymaking by other means*" that have have large effects on access to rights and public services, facilitate social control and reinforce inequality. Moreover, their effects accumulate over time and people with fewer resources are less equipped to manage burdens.

For policymakers, more rigorous screening process may improve targeting efficiency, but the associated complexity is costly to applicants. Henrik Jacobsen Kleven and Kopczuk (2011) rationalise this in a model characterising optimal programmes when policy makers choose benefit level, eligibility and screening intensity. Consistent with many real-world programmes, optimal choice for policymakers feature high complexity, incomplete take-up, classification errors of both over-rejection and excess-award.

In the field, reducing low take-up has proven to be very hard to achieve, generally leaving the most vulnerable behind. Bhargava and Manoli (2015) analyse the role of "psychological frictions" in the incomplete take-up of EITC in a field experiment on 35,050 tax filers who failed to claim $26 million despite an initial notice. The information of the unclaimed benefit led to substantial additional claiming but attempts to reduce perceived costs of stigma, application, and audits did not. Linos et al. (2020) ran six pre-registered, large-scale field experiments to test whether "nudges" could increase EITC take-up (N=1 million). Despite varying the content, design, messenger, and mode of their messages, they find no evidence that they affected households' likelihood of filing a tax return or claiming the credit. Similarly, Nyman, Aggeborn, and Ahlskog (2023) ran an information experiment randomising 37 000 leaflets providing information on the Swedish EITC in a pre-registered randomised experiment and also find precise null effect both at the extensive and intensive margin. Similar results were found in France by Chareyron, Gray, and L'Horty (2018) in a mail experiment on 4000 new welfare claimants to foster registration at the employment agency for welfare recipients. The variations in the mails have no effect on registration. Finkelstein and Notowidigdo (2019) run a field experiment over 30 000 elderly likely eligible for the Supplemental Nutrition Assistance Program (SNAP), either provided with information that they are likely eligible, provided with this information and offered assistance in applying, or are in a "status quo" control group. 6% of the control group enrolled over the next 9 months, 11% in the information group and 18% in the information with assistance. Evidence suggest optimisation frictions greater for needier individuals as compliers tend to have higher net income and are less sick than the average enrollee. In France, Castell et al. (2022) ran two field experiments to foster access to all social benefits. In the first experiment, they show that receiving assistance increase take-up of new benefits by 30% while the second only use an online simulator through an information experiment. Using a marginal treatment effect analysis, they show that benefits are mostly concentrated among the least likely to attend and suggest that transaction costs deter eligible people from applying to benefits and from accessing government's assistance to help them apply.

However, both Finkelstein and Notowidigdo (2019) and Castell et al. (2022) show that human assistance and personalised information do reduce non-take-up of social benefits. Similarly, Raj Chetty and Saez (2013) also ran an experiment in which tax pre-payers provided simple but personified information on the EITC to half of their clients, reaching a total sample size of 43000 households. They show that about half of the tax-prepayers induced their clients to chose earning closer to the kink and 10% less likely to have very low income compared with control. In a two phase randomised control trial of the *moving to opportunity* programme in the US, Bergman et al. (2019) show that providing high-intensity, customised support *i.e.* clear and tailored information about high-opportunity areas, short-term financial assistance, customised assistance during the housing search process, and connections to

[9] "Learning costs arise from engaging in search processes to collect information about public services, and assessing how they are relevant to the individual. Psychological costs include the stigma of applying for or participating in a programme with negative perceptions, a sense of loss of power or autonomy in interactions with the State, or the stresses of dealing with administrative processes. Compliance costs are the burdens of following administrative rules and requirements." (Moynihan, Herd, and Harvey 2015)

landlords - increased the moving rate from 15% in the control group to 53% in the treated group. The second phase of the experiment tests each component of the high intensity support to disentangle which mattered most. Results show that information yields treatment effects 5 times lower than the combined intensive treatment and reduced support with information 3 times lower. It is the combination of high intensity support with clear and tailored information that works.

Welfare stigma and compliance costs In the early literature, R. Moffitt (1983) introduced welfare stigma as a cost in the utility function, generating non-take-up at equilibirum. More recently, Kline and Tartari (2016) built a model with both welfare stigma and compliance cost affecting utility and participation in the programme. This model incorporates the "hassle" associated with welfare work requirements and also allows agents to manipulate their reported incomes at a cost. Using this model, they are able to make restrictions on preferences to offer informative bounds on the labour supply response of single mothers in a randomised experiment of a welfare-to-work programme in the US. This programme strengthened work requirements and increased sanctions for welfare recipients who fail to seek work. Moreover, it changed the manner in which welfare benefits phase out by disregarding earnings up to an eligibility threshold (or "notch") above which benefits abruptly drop to zero. These incentives push at least 20% of control group women whose earnings are above the notch to reduce their earnings below the notch (but remain working) and receive welfare under the experiment.

The stigma associated with social benefits is very pervasive. In a behavioural economics experiment manipulating the social image of access to a benefit, Friedrichsen, König, and Schmacker (2018) show that the stigma associated with both "living off others" and receiving aid "for the least qualified" causally reduces the intention to use this aid. Celhay, Meyer, and Mittag (2022) compare participation in social programmes reported in declarative surveys with information on participants' networks and document strong under-reporting and manage to exclude mechanisms other than stigma to explain it.

In France, while we can observe a steady increase in the number of welfare recipients since the 2008 reform[10] (Hannafi et al. 2022), estimates of non-take-up range from 28% to 35% of eligible people (Domingo and Pucci 2013). In a qualitative analysis of non-take-up, Chareyron (2018) mentions that 14% of eligible non-recipients cite not knowing about the aid as the main reason why they don't resort to it, and 71% that they do not know how the amount is calculated. To those 85% ascribable to learning costs are added 23% associated to stigma, or psychological costs, although research has yet to identify exactly how dissuasive they are. Interestingly, since having a child opens rights to benefits and therefore facilitates contact with the relevant institutions, single parents are proportionately more numerous within beneficiaries (7%) than non-takers (under 3%). On the control of welfare recipients, practices of the administration in charge (*CNAF*) towards single parents – mostly women – have been subject to heated controversies in the past years: in his analysis of the institutional evolutions of social benefits, Dubois (2021) presents the institutionalisation and hardening of control policies and practices since the 1990s as part of a "symbolical and moral economy" where "assistantship" (*assistanat*) and fraud are the necessary deviances to the promotion of the norm, i.e. *la valeur travail*. Dubois' conclusions that these evolutions mostly penalise vulnerable populations, in particular the disabled, migrants and single mothers, were further supported by new evidence regarding CNAF's practices in scoring recipients' likelihood of fraud (Quadrature du net 2023). Like many administration around the world, they use predictive models to flag files with higher risk of *errors or fraud*, without distinction. The model is a simple Logit where variable have been previously selected using Lasso. The thorough analysis of the source code by Quadrature du net (2023) shows that the most impoverished beneficiaries consistently have a higher suspicion score. Elements such as separations or single-income households increase the score in a way that it is hard not to conclude to the targetting of single parents, leading many to argue that CNAF's control policy disproportionately sanctions vulnerable people going through a particularly tough time. Beneficiaries are also required to notify any change in relationship status, and not doing so is akin to fraud. In spite of criticisms from for instance the *Défenseur des droits* (Défenseur des droits 2017), the concept of "marital situation" is vague enough that non-financially enmeshed people can still see their rights revoked. For single mothers, such intrusiveness considerably (needlessly?) raise the compliance and psychological costs.

[10] See Subsection III.1 for a brief description of the French welfare system and Appendix A for the history of welfare reforms in France and a short review of the literature on the effects of monetary incentives in France.

II.2 Tax salience, information and optimisation frictions

In this section, we lay down the utility maximisation model used in public economic literature on bunching that rationalises intensive margin reactions around variations of tax rates[11]. As our presentation goes, we discuss how the literature introduces the frictions we discussed earlier.

The bunching model Consider individuals indexed by i, whose heterogeneity can be summarised by scalar variables a_i and ψ_i, representing abilities and adjustment costs, respectively. Typically, ψ_i can be interpreted as monetised stigma or costs associated with changing situations (Kline and Tartari 2016). Later, we will explore how these parameters might be endogenous to the tax-benefit system or active labour market policies. For now, we abstract from gender and family structure, following a simplified model of optimisation friction.

Individuals maximise their utility U by choosing disposable income Y through optimising pre-tax earnings X, subject to a budget constraint that includes mean-tested social transfers, leading to kinks in the implicit tax rate.

We consider the typical iso-elastic utility function:

$$U(Y,X) = Y - \frac{a_i}{(1+\frac{1}{e})}\left(\frac{X}{a_i}\right)^{1+\frac{1}{\varepsilon}} - \psi_i \tag{1}$$

where $Y = X - T(X)$ is the net disposable income and ε is the elasticity parameter, constant across individuals.

This model disregards factors like children or spouses, which do not pass a basic reality check. Both are endogenous, affect tax rates, and spouse incomes are part of the household income Y but may not enter a parent's utility function as such. We come back to this point in subsection II.3. Despite these important simplifications, this is the structural model commonly used in estimating labour elasticity of single parents, particularly around kink points of programmes like the EITC (Saez 2010; Raj Chetty, Friedman, and Saez 2013).

To formalise the optimisation problem, we express the budget constraint in linear form: $X = (1-\tau')X + R$, where $\tau' \equiv T'(X)$ is the (perceived) marginal tax rate and $R \equiv T'(X) \cdot X - T(X)$ is virtual income[12]. In a tax-benefit system without kinks, the first-order condition yields the optimal pre-tax income[13]:

$$X_i^* = a_i(1-\tau)^{\varepsilon}$$

Taking this expression in logs, we get $log(X_i^*) = \varepsilon \log(1-\tau) + \log(a_i)$, which makes clear that the parameter ε measures the elasticity of pre-tax incomes to the net-of-tax rate.

In the presence of a kink K with marginal tax rates τ_a below X^* and τ_b above X^*, workers who would chose a level of pre-tax income above the kink in its absence now chose $X = K$ because they face a higher marginal tax rate. Among the bunchers, the one with the highest ability a_i is the one who would chose exactly $X = K$ if the higher tax rate τ_b prevailed below the kink point. This individual is the *marginal buncher* and his location is given by

$$\Delta X^* = K(1-\tau_b)^{\varepsilon}$$

All individuals with ability between $\frac{K}{(1-\tau_a)^{\varepsilon}}$ and $\frac{K}{(1-\tau_b)^{\varepsilon}}$ bunch at K.

The resulting distribution has a mass-point at K but is otherwise continuous, as presented in Figure 2. An important result from Saez (2010) is that for small tax rate changes, we can relate the elasticity to the earnings change ΔX^* for the individual with the highest *ex ante* earnings who bunches *ex post*:

$$\varepsilon = \frac{\Delta x^*/x^*}{\Delta\tau/(1-\tau_a)}, \tag{2}$$

[11] See for instance Blundell et al. (2009);Saez, Slemrod, and Giertz (2012);Bierbrauer, Boyer, and Peichl (2021);Henrik Jacobsen Kleven (2016);Kostøl and Myhre (2021)

[12] The intercept of a linear budget set passing through the point $(x, T(X))$. See Piketty and Saez (2013).

[13] To see why, note that the marginal benefit of a variation in earnings is given by 1-τ', while the marginal cost is $(\frac{x}{a_i})^{\frac{1}{\varepsilon}}$). At equilibrium, both equate and yield the given solution.

where $\Delta\tau = \tau_b - \tau_a$ and ε is the elasticity of pre-tax income to variation in the marginal tax rate. The higher the elasticity and the change in taxes at the kink, the larger is the range ΔX^* of bunchers.

Figure 2: Bunching at kink point (Kleven 2016)

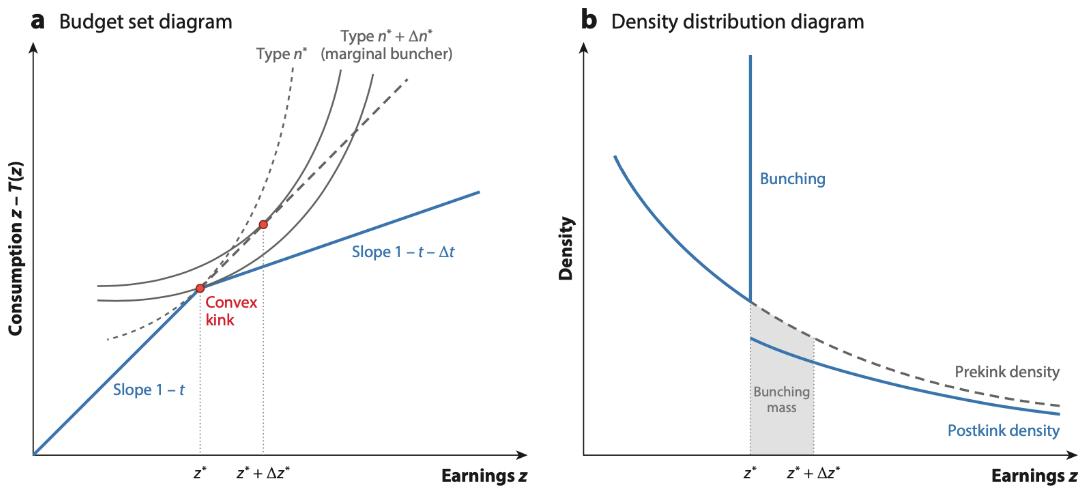


Figure 1

Kink analysis, showing the effects of a convex kink—a discrete increase in the marginal tax rate from t to $t + \Delta t$ at the earnings threshold z^*—in a (*a*) budget set diagram and (*b*) density diagram. In panel *a*, the individual with ability $n^* + \Delta n^*$ is the marginal bunching individual. This individual chooses $z^* + \Delta z^*$ before the kink is introduced and z^* after the kink is introduced. All workers initially located on the interval $(z^*, z^* + \Delta z^*)$ bunch at the kink, whereas all those initially located above $z^* + \Delta z^*$ reduce earnings within the interior of the upper bracket. As shown in panel *b*, the implications of these responses for the earnings distribution are sharp bunching at z^* (the size of which is equal to the *gray shaded area* just above z^*) and a left shift of the distribution in the upper bracket.

Identification with bunching design The general idea behind this model is that the size of the bunching mass may be informative of the underlying elasticity, but requires several additional information to be identified. The estimation methods proposed by R. Chetty et al. (2011) or Raj Chetty and Saez (2013) rely on an affine approximation of the counterfactual density in a chosen interval defining the bunching region[14].

The bunching mass is defined by:

$$B^* = \int_{X^*}^{X^*+\Delta X^*} f_{Y0}(u)du \approx f_{Y0}(K) \cdot K \left(\left(\frac{1-\tau_a}{1-\tau_b} \right)^{\varepsilon} - 1 \right)$$

where the approximation is that proposed by R. Chetty et al. (2011).

The problem is that this estimator is sensitive to the width of the bunching window and the quality of the parametric fit outside the bunching region. With kinks, the interval length is unknown and could be large because it depend on the elasticity we want to estimate. In Fact, Blomquist and Newey (2017) show that there is no way of inferring the elasticity parameter from bunching mass and continuity alone. This is particularly true for kinks where there is no way to know the position of the marginal buncher without assumptions on the other parameters.

The polynomial fit typically uses data outside both side of the kink to fit the implicit ability distribution a_i. In a few cases, researchers use a control group to infer the missing distribution, but not in the context of welfare policies[15]. Another approach consists in modelling or manipulating information to generate reactions by reducing frictions.

[14] Saez (2010) uses a trapezoidal approximation and implicitly assumes an affine function over the bunching interval. See Bertanha et al. (2023) for a very pedagogical discussion.

[15] See for instance Coles et al. (2022) on corporate tax, Gelber, Jones, and Sacks (2020) on age retirement and Garbinti et al. (2023) on wealth tax.

Optimisation frictions So far, the model assumes no friction and immediate reaction to the tax rate. A first type of optimisation friction arises for individuals around kinks and notches for whom adjusting to incentives may be too costly. In such case, the bunching mass is diffuse and identification of the bunching region is even more sensitive. In the presence of optimisation frictions, the observed elasticity does not correspond to the structural elasticity and must be modelled explicitly.

For instance, Kostøl and Myhre (2021) introduce a "*knowledge*" parameter in the household budget constraint such that part of the population does not know about the change in tax rate at the kink and optimises as if the tax rate was linear at the kink. Formally,

$$T(x) = \tau_a(X_i) + \theta_i \tau_b (X_i - X) \cdot \mathbb{1}(X > X)$$

The parameter θ_i measures the degree to which agent i wrongly perceives the tax rate for incomes over k. Like before, the utility maximisation yields the optimal pre-tax income with frictions X_{Fi} which now takes the following form:

$$X_{F,i} = \begin{cases} a_i\,(1-\tau_a-\theta_i\tau_b\cdot\mathbb{1}(X>k))^{\varepsilon} & \text{if } U_i\,(Y,X_F)-U_i\,(Y,X^*)\geq\psi_i \\ a_i\,(1-\tau_a)^{\varepsilon} & \text{if } U_i\,(K,X_F)-U_i\,(K,X^*)<\psi_i. \end{cases} \tag{3}$$

This equation shows that both adjustment costs ψ_i and information frictions θ_i reduce the impact of monetary incentives. Only agents with costs lower than the benefits of adjusting, and individuals who are aware of the kink, respond. It also emphasises the interdependence between these two types of frictions. Increasing the proportion of individuals with the correct understanding of the tax also changes the perceived benefits of re-optimisation. We come back to this when we present our empirical strategy in section V.2.

Kostøl and Myhre (2021) use this model to analyse a tax reform in Norway together with an information experiment providing details on the reform and the existence of notches. Consistent with this model, their results show large adjustments at the intensive margin, twice higher in the group that received information than in the control group. Raj Chetty and Saez (2013) use a similar approach in the information experiment discussed earlier and also find much larger elasticities in the group who received information and support. Caldwell, Nelson, and Waldinger (2023) analyse the effect of uncertainty around the amount of EITC and show that they distort individuals' consumption-savings choices enough to cause welfare losses among EITC filers on the order of 10 percent of the value of the EITC. In Denmark, welfare recipients are required to work 225h in the past 12 months and face a reduction in their monthly payments if they fail to comply with the work requirement. In a field experiment, Cairo and Mahlstedt (2023) randomised i) access to a personalised online tool that offers continually updated personalised information including the number of working hours they have accumulated and their personal deadline for compliance with the work requirement; ii) notification messages that are almost identical to those available in personalised tool, but workers assigned to the message treatment do not gain access to the online tool iii) a control group neither receiving notification messages nor have access to the online tool. Their results show heterogeneous reaction depending on individual initial situation and treatment received. Personalised information increases employment for those who initially fail to meet a work requirement at the start of the intervention while notifications decrease the labour supply of workers with limited incentives to work extra hours in the immediate future.

Raj Chetty, Friedman, and Saez (2013) have a rather unique approach in this literature combining a natural experiment with bunching to recover the effects of the earned income tax credit on labour market participation of single parents and poverty. Like Saez (2010), they observe sharp bunching at the first kink of the earned income tax credit, but with significant variation across counties, which they interpret as a lack of *knowledge* of the EITC. They use an interesting identification strategy, essentially using bunching as a first stage of the effect of EITC on the distribution of incomes of single mothers around birth. Assuming that differences in bunching by self-employed individuals around the first kink of the EITC around child birth across counties only reflect knowledge of EITC, they leverage differences in i) the share of bunchers ii) by relative months from birth iii) between high and low knowledge county, where the shifting share of bunchers after birth in high knowledge counties rescales the change in disposable incomes around birth. Their findings suggest substantial intensive margin earnings responses to EITC incentives, conditional on knowledge and showed that the EITC increased incomes at the bottom of the distribution.

Theses results have important implications. The very existence of this natural experiment shows that salience matters and that a large share of the population does not know about programmes intended for them. Interestingly, while the informational and psychological frictions associated with non-take up of social programmes are widely acknowledged in the literature (van Oorschot 1991; Portela et al. 2022; Hannafi et al. 2022; Ko and Moffitt 2022), they have been used mostly as an explanation for not observing any intensive margin responses. However, theoretical models of intensive and extensive margin responses (see e.g. Eissa, Kleven, and Kreiner 2006) imply that such frictions are equally important for the extensive margin.

The elasticity of women labour supply is an important question for which there does not seem to be a definitive answer despite important sophistications in the model used. For instance, Attanasio et al. (2018) shows that there is substantial heterogeneity in women's labour supply elasticities at the micro level and build a life-cycle model to aggregate elasticities. In France, Briard (2020) reviews the literature and shows that most estimates use structural models with discrete choices but lack consistency with regards to their spouse's income.

It is also worth noting the choice of vocabulary used in models of labour participation of single parents on welfare. Most of the work on the EITC consider single parents and all model their decision as "*tax evasion*". Agents maximise consumption, leisure time and minimise tax. The models trying to account for stigma models single mothers as *genderless tax-avoiders*, which is not without irony. This brings us to what we think is the most important critic of this literature: the lack of consideration for the specifics of the targeted populations and the context of decisions.

II.3 Welfare reforms and the economics of gender, family and human capital

***Gender trouble* in economics** Feminist economics criticised the rationality framework used in mainstream economics focusing on the individual characteristics of lone mothers, which is, at best, conceptualised as human capital (education, training, and experience), individual resources (e.g. income), and constraints (e.g. number and age of children). However, what is economically rational is different in different welfare state regimes which in turn determines their origins in collective political and ideological views of society, particularly concerning the relation between individuals, families, states, and markets (S. Duncan and Edwards 1997; Pollmann-Schult 2018). The source of economic rationality, therefore, at least in this case, partially lies outside the market and in the domain of collective and highly gendered understandings about proper social behaviour and the internalisation of such norms in personal values (Stavrova and Fetchenhauer 2015; Hakovirta, Kallio, and Salin 2021; Gong, Stinebrickner, and Stinebrickner 2022; Reich-Stiebert, Froehlich, and Voltmer 2023). For instance, in England, Cavapozzi, Francesconi, and Nicoletti (2021) find that having peers who embrace egalitarian gender norms leads mothers to be more likely to engage in paid employment and have a greater share of total paid hours within their households. These effects are particularly pronounced among less educated women. They estimate that about half of the impact on labour force participation is due to women conforming to their peers' gender role attitudes, while the other half is attributed to the peer behaviour spillover effect on the labour market. A similar result was demonstrated in France by Maurin and Moschion (2009) using an instrumental variable strategy. Starting from the premise that the gender of the first two children affects the likelihood of having a third child and has a negative reduced-form effect on employment and income (J. Angrist and Evans 1998), Maurin and Moschion (2009) show that the presence of more or fewer families with two children of the same gender in the neighbourhood (and therefore, mothers working less due to this) reduces other women's labour force participation. When there is participation, it remains constrained by gendered household organisation : for instance in France, Le Barbanchon, Rathelot, and Roulet (2020) shows that when considering a job, women trade-off commuting distance and wage rates.

In order to understand such rational, economists must take into account the wide range of such trade-offs that mothers have to deal with, in spite of their generally implicit nature. Not least among them is the fact that time spent with children, particularly in early childhood, remains the least replaceable and one of the two most valuable investments in human capital that parents can provide, together with income (Elango et al. 2016; Doyle 2020; G. Duncan et al. 2023).

Moreover, and although definitive evidence is hard to obtain, many economists agree that maternal and paternal times are more complements than substitutes (as is the case, for instance, with the Cobb-Douglas form adopted by Del Boca, Flinn, and Wiswall (2014) or Goussé, Jacquemet, and Robin (2017)). Education, fertility, marriage, investment in children education and employment choices are increasingly analysed together ; and men and women play different roles in the models (Pierre-André Chiappori, Radchenko, and Salanié 2018; Goussé, Jacquemet, and Robin 2017). Bargaining in the household is not a negligible aspect. For instance, the analysis of the effects of unilateral divorce laws in the US showed that it did not increase divorce beyond a very short-lived bump (Wolfers 2006). However, Stevenson and Wolfers (2006) showed that they reduced female suicide by 8 –16 percent, domestic violence by roughly 30 percent for both men and women, and feminicides by 10 percent.

On the economics of households More sophisticated theories have been developed, incorporating, for example, strategic, cooperative, or non-cooperative interactions among household members, preferences, and transferable utility that may depend on (gendered) reference norms, which are more compatible with the data. Within mainstream economics, new perspectives on gender have thus emerged (Bertrand 2011). Yet, labour economics remained unaffected by these evolutions in its cousin field of household economics. In the bunching literature, when researchers define utility of the households, they usually use the unitary household with separable utility[16] although this model has been consistently rejected in the data (Lundberg and Pollak 1996; Pierre-Andre Chiappori and Mazzocco 2017).

In our presentation, we defined X as pre-tax income, living the possibility that the latter comes from single mothers's own income or that of a potential spouse, and depend on the family structure. If we introduce children and spouse as choice variables, the tax rates also vary across conditions and depend on joint spouse incomes. This would require a much more sophisticated model to represent the household's decision making. Such formalisation is out of the scope of this research but emphasises the complexity and endogeneity of many choices, all dynamic with lasting effects. Among them, the many choices and shocks that led them to be poor single parents.

Bertrand et al. (2021) develop a model with nested generations in which women decide whether or not to acquire skills and then, based on the partner they obtain, decide whether or not to marry and, in a non-cooperative manner, decide how to allocate their time between work, leisure, and the production of a "public good", *i.e.* children's education. The marriage gap between qualified and non-qualified women emerges endogenously due to higher wages resulting from their degrees and the different time allocation decisions these higher wages generate. The key force behind the emergence of this marriage gap lies in gender norms that create marital disagreements about the level of education to dedicate to children. The model predicts a negative relationship between the mother's education and the probability of marriage until the return on education is such that it becomes attractive for men to be with them or when gender norms are more egalitarian. The significant increase in women's education levels worldwide since the 1960s, along with access to technologies reducing the time required for domestic tasks or enabling fertility control and the emergence of care outsourcing services, drastically altered the balance within families (Goldin 2006-May). However, despite these major changes, gender specialisation in domestic or educational tasks has only partially evolved, while structurally modifying gender relations in society (Juhn and McCue 2017).

There is also a rather large literature showing that gender norms and frictions within households are sometimes the very cause of single parenthood to the point that researchers used manifestations of such frictions as instrument of single parenthood. For instance, Bedard and Deschênes (2005) use the gender of the first child (assigned female at birth) as an instrumental variable to predict a higher risk of separation[17]. They measure its effects and find that women separated because of having a girl as their first child have significantly higher average incomes than if they had not been separated. This effect is driven by increased labour force participation and higher working hours. Ananat and Michaels (2008) use the same instrument to analyse the effects of separation on income distribution quantiles. Their results indicate that separation increases the share of women in the lower and upper tails of the income distribution, meaning higher shares of impoverished and affluent women, resulting in negative income impacts. Simultaneously, divorce increases the share of women in the upper echelons of the distribution and boosts their income levels. Frictions on incomes are also a major cause of separation and many research find a sharp discontinuity in the density of couples where mothers earn more than their partners, also associated with a larger happiness gap and less leisure for women (Krueger 2008; Stevenson and Wolfers 2009; Bertrand, Kamenica, and Pan 2015; Bodson and Kuépié 2012; Lippmann, Georgieff, and Senik 2020; Flèche, Lepinteur, and Powdthavee 2020; Blanchflower and Clark 2021).

[16] See for instance the work of Nyman, Aggeborn, and Ahlskog (2023) on information on the Swedish EITC

[17] see also Dahl and Moretti (2008)

Promissing young women There are several mechanisms explaining why single mothers are so at risk of poverty and why the gender penalty is so strong between single fathers and mothers. To be short and review evidence closer from our empirical setting, we only include research using French data. First, single parenthood is often a consequence of cumulative vulnerabilities. In particular, there is a considerable education gap: 45% of single mothers did not complete high school, 33% hold a university degree, while 50% of mothers in *traditional* households hold a university diploma and 29% less than a high school degree (Le Pape and Helfter 2023, 36–39).

Second, the child penalty is more severe for mothers with low education levels and those at the bottom of the income distribution (Meurs and Pora 2019; Bazen, Xavier, and Périvier 2022). Fast return to work after birth strongly depends on previous labour experience, firm characteristics and the business cycle (Rodrigues and Vergnat 2019). Childrearing also triggers marital specialisation, generating a more inegalitarian gender division of house and care chores, especially among parents with lower education (Bianchi et al. 2014; Solera and Mencarini 2018; Reich-Stiebert, Froehlich, and Voltmer 2023; Briselli and Gonzalez 2023). French women have access to extensive State-provided childcare, rather generous parental leave, and home care allowances that support parental care for two or more young children. Family policies are promoted as giving women a "free choice" and while they are associated with relatively high employment levels for mothers, as a whole, these policies have encouraged women's caregiving rather than promoted men's equal role in care (Misra, Moller, and Budig 2007). Despite being better educated, women generally earn less than men, and continue to shoulder the majority of domestic work even when holding employment (Champagne, Pailhé, and Solaz 2015; Kandil, Périvier, and Richou 2021). Solera and Mencarini (2018) show that around the first child birth, the traditional division of household tasks, in which women perform more than 75% of all household tasks, increases from 21% to 29%.

Third, by opting for a more family-oriented career, mothers contribute less to the household's income and generally have lower ability to accumulate wealth and savings of their own. Legal statuses of couples (cohabitation, civil union, and marriage) and property regimes (community and separate property) create different levels of protection and compensating mechanisms (alimonies) associated with the gender gap in wealth and saving within couples. As Frémeaux and Leturcq (2022) show, married couples with separate property regimes accumulate more wealth and also have the highest gender wealth gap. Therefore, the single-parenthood penalty is higher among initially richer households while social transfers only partly buffer the shock for low-income households (Bonnet, Montaignac, and Solaz 2024).

Fourth, women's living standards generally decrease more than men's following divorce, due in large part to pre-divorce within-couple earnings inequality resulting from marital specialisation (Bonnet, Garbinti, and Solaz 2021). In addition, mothers are more likely to move than fathers, and shared custody arrangements result in greater mobility for mothers compared to fathers (Ferrari, Bonnet, and Solaz 2019).

Fifth, single-parenthood is not a permanent state and 50% of them are no longer single-parents after 3 years (Costemalle 2017). However, the duration spells vary significantly, with separated or widowed parents leaving single parenthood more quickly than those who had a child outside of a relationship. Most men find new relationships fast while a large share of single mothers remain in this situation.

This "flip side of marital specialisation" leads to a significant re-entry of formerly inactive mothers into the labour market (Bonnet, Garbinti, and Solaz 2021). Shared custodial arrangement have large positive effects on women labour market participation, earning and standard of living (Bonnet, Garbinti, and Solaz 2022). However, Gobillon, Meurs, and Roux (2015) explore gender heterogeneity in access to jobs in France and showed that women have significantly lower access to high-paid jobs than to low-paid jobs. Those who re-partner quickly experience a much lower loss of living standard (Abbas and Garbinti 2019). Bonnet, Montaignac, and Solaz (2024) show that the decline in living standards disappears when the custodial parent re-partners, but this effect only applies to 30% of children six years after separation. On average, children's living standards are 20% lower six years after separation compared to a counterfactual scenario derived from a matched event-study design. Separation also reduces children's educational attainments, more severely for boys than girls and when the separation occurs when the child was young (Le Forner 2020).

III Activating single parents on welfare: Policy environment and intervention

There are 2.25 million single parent households in France in 2018, 4 single mothers for every single fathers. Break-ups represent 78% of entry flows, 16% come from children born among non-cohabiting parents, either by choice or through unplanned pregnancy, and 6% from one parent's death[18]. These families are 2.6 times more likely to live below the poverty thresholds than *traditional* couples[19], with an average poverty rate of 40% ; 22.7% when the custodial parent works and 77.7% among single parents with no job (Le Pape and Helfter 2023, 36–39). Lone parents and single-parent families are particularly vulnerable to situations of insecurity and poverty, and they tend to have a pessimistic view of their current situation, their future, and society as a whole (Pirus 2021). They are also strongly in favour of more generous support for families. A large share live in poverty, relying heavily on various social transfers.

In this section, we first start by presenting what social transfers they may receive and take the example of a single parent family with two children to illustrate their interactions. Then, subsection III.2 presents the **Reliance** programme, a randomised intensive welfare-to-work programme in place from 2018 to 2022 in the North-East of France. We discuss the results of the first evaluations focusing on the first causal evaluation of Heim (2024) and the qualitative study of FORS (2020). From there, we motivates our analysis with a discussion on the objective of the programme, some key components of the intervention and suggestive evidence from the qualitative evaluation. Finally, we discuss the incentives of the tax-benefit system in subsection III.3 and the possible behaviours that participants may exhibit when these incentives are made salient.

III.1 Social transfers for low income families

The French tax-benefit system involves many different policy instruments targeting different populations. In Appendix A, we review the history and the evaluation of policies introducing monetary incentives in France. In short, low income families have access to different aids which serve different policy objectives. The main ones are:

1) **Minimum income scheme** RSA (*Revenu de soliarité active*) and **in-work benefits** PA (*Prime d'activité*). They are mean-tested monthly payments based on household's quarterly income and family structure;
2) **Housing benefits** AL (*Allocations logement*) depend on rent and number of dependent children, but not on cohabitation. They depend on household's income from the past 12 months as a moving average updated quarterly[20];
3) **Family benefits** AF (*Allocation familiales*) for parents of two, with mean-tested **supplements** CF (*Complément familale*) for families of three or more children. There is also a mean-tested **back-to-school** allowance paid in summer;
4) **Early childhood benefits** PAJE (*Prestation d'accueil du jeune enfant*) which includes a mean-tested basic allowance: AB (*Allocation de base*) and **childcare allowance** CMG (*complément mode de garde*) for active parents resorting to childminders, and **shared parental leave** PrePare (*Prestation partagée d'éducation de l'enfant*);
5) **Disability benefits** AAH (*Allocation adulte handicapé*) for parents, AEEH (*Allocation d'éducation d'enfant handicapé*) for children, and AJPP (*Allocation journalière de présence parentale*) when parents take care of a sick child.

Parental leave provides between 50 and 2/3 full-time minimum wage but is barely used by single parents because this level does not compensate for the time off work, whereas mothers in couples have the resources of a spouse (Périvier 2022a). Those transfers are not specific to single parents but take their situation into account either through

[18] Costemalle (2017) based on INSEE data from the Family and Housing survey 2011.

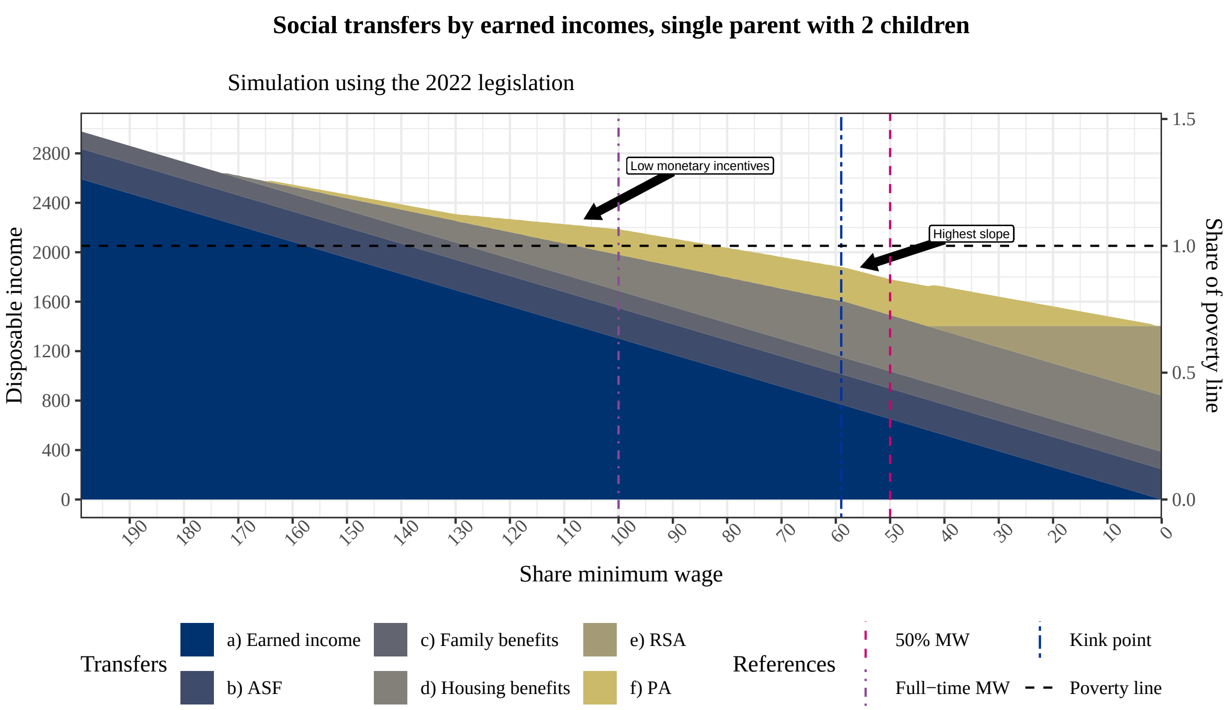
[19] The poverty rate among *traditional* couple is 15.6%, 4.7% in dual-earner couples, 30% among single-earners and 71% when no parent works.

[20] There are actually three types of housing benefits: APL (*aide personalisée au logement*) for anyone that rents a house, which is only contingent upon income and rent prices. ALF (*Allocation de logement familial*) is open to households with family allowance or disabled child allowance but also married couples for the first 5 years after marriage. ALS (*Allocation de logement social*) for other cases.

increased amounts or eligibility cut-offs. The only specific aid is the **family support allowance** ASF (*Allocation de soutien familial*), a lump-sum quarterly transfer for custodial single parents who receive no or too little child support from the child(ren)'s other parent. Its amount depends on the number of children and is first offered for four months. To keep receiving ASF, single parents must prove they started the administrative or judiciary process to set child support. If the other parent cannot pay child support, the administration verifies and can condition ASF to new judiciary processes. If child support has been defined but is not (entirely) paid, ASF is an advance and the administration will try to recover the sum from the other parent. Any child support paid is deduced from ASF, and ASF is mostly deduced from RSA and PA. Last, if the single parent starts living with a partner again, they are no longer eligible for ASF. We come back to these points in the subsequent paragraphs. In November 2022, its amount has been increased by 50%, going from € 122 to € 184 top per child.

Except for pregnancies which are reported directly by physicians to the health system, any other change must be reported by households as it defines many eligibility rules and amounts of cash transfers. Failure to report changes can have severe consequences for households. In case of *undue payments*, they may be required to repay the amount for up to 2 years back.

Figure 3: The static model of the French tax-benefit system for a single parent of two.



Sources: DREES, EDIFIS.
Case study for single parent with two children reiving RSA, no temporary supplement.
Arrows and labels indicate pieces where the implicit marginal tax rate is locally highest or lowest.
a) Earned income as a share of the full−time minimum wage (MW, 1302.67 euros in 2022).
b) ASF is the public child support paid quarterly when the other parent doesn't. Lump−sum payment by number of children.
c) Family benefits are open for parents with 2 or more children, depend on taxable incomes (Year − 2).
d) Housing benefits, baseline amount depends on number of dependent children, rent price and their due payment.
Decreases with earned incomes over the past 11 months, from the previous month.
e) RSA is the French minimum income scheme, f) PA is an in−work benefit, both are mean−tested
and depend on family composition and household's income.
Poverty line is 1140 euros per Consumption unit (CU) times 1.8 CU (1 for the parent,
.3 per child between 3 and 14 and .2 as a single parent).

To fix ideas, consider a single parent family with two children between 10 and 14 years old and no income in 2022. This parent receives neither temporary supplement-RSA, nor children allowance, but receives ASF for the two children. We use the open-source simulation model of the Statistics department of the Ministry of solidarity[21] to represent all these social transfers and their interaction with labour incomes in Figure 3.

[21] Accessible from https://drees.shinyapps.io/Drees_Maquette_Edifis/

RSA and PA are differential incomes. They are computed as the difference between a statutory level based on household size and composition from which the relevant *reference income* is deduced, which defines the final amount received. We provide details on the formula and an important reform that took place during our time-frame in Appendix A. There are three important features. First, households must report all their incomes from the previous quarter by household member and type of income. Second, they depend on three measures of income: joint labour earnings over the quarter, individual labour earnings and joint pre-tax incomes. Third, any reported income is deduced (almost) entirely from RSA while PA considers all labour incomes together, with an individual bonus starting at 50% minimum wage[22].

Without labour incomes, a 2-children single parent household receives up to €1492 if they access all their rights. Accounting for family size, this amount to €933 per consumption unit[23] which is 18% lower than the poverty line[24]. Note that the equivalent scale of consumption units tend to overestimate the standard of living of single parents and parents without full custody (H. Martin and Périvier 2018). For single parents in this situation, 38% of their incomes are mean-tested and change every quarter (RSA and PA); 30% are housing benefits and they are now changing monthly using a moving average from income 12 month earlier to two month before the month of payment[25]. The remaining 26% are not mean tested.

Last, this static model shows that the households' budget increases rather smoothly with labour income while the amount of social transfers fades out until 1.8 times the full-time minimum wage (MW hereafter). On average, the implicit marginal tax rate is about 38% but there are three important non-linear pieces. The first notch and following convex kink are due to i) a €15 minimal payment threshold at the RSA exit-point, ii) the starting point of individual[26] PA supplement from 50% MW, and iii) maximum amount of housing benefits up to 60% MW. The second convex kink starts right at the MW level as the amount of PA rapidly decreases up to 1.3 MW, where last piecewise non-linearities create non-convex kinks. The alignment with the full-time minimum wage is a consequence of the 2019 reform of in-work benefits - a point we discuss and illustrate In Appendix A.III.

Single parent households on welfare receive no money from child support. Family benefits are not a significant portion of the budget of a single parent of two on welfare. Nevertheless, ASF is a crucial element of the overall budget and has strict eligibility criteria, as previously mentioned. It is important to note that a single parent without ASF would not lose a proportionate amount. Actually, most[27] of the ASF and 100% of family benefits have already been deducted from the RSA. Conversely, when single parents receive child support from the other parent, this amount is 100% deduced from RSA and PA. Their income does not change when they receive it or not. For these families, child support neither benefit the child nor the parent; rather, it reduces public spending[28].

This feature has important consequences for poor families. First, as noted by Pucci and Périvier (2022), it creates strong inequalities between high and low income single parents, where the former keep ASF or child support in full while the poorest lose all child support or keep only a portion of ASF. Périvier (2022a) shows that because of their inclusion in baseline ressource of RSA and PA, better recovery of unpaid child support can reduce the standard of living of lone parents, particularly lone mothers. This reduces incentives to make the other parent pay since the custodial parents receive nothing or even be poorer. Moreover, child support and ASF also reduce share of mean-tested benefits in all transfers. Consequently, their exit points in the earning distribution also gets lower with any non-labour income.

[22] There is a subtlety in the precise computation: PA is based on each month's labour income over the quarter while RSA takes all incomes in the quarter and average them. One month with more than € 1500 gives PA for one month but remove RSA for one quarter or reduced it for two, depending on which month was worked in the quarter.

[23] The CAF uses the following weighting: 1 consumption unit for the first adult, 0.5 for each adult or child aged 14 or older, 0.3 for children under 14, 0.2 for single-parent families. For a discussion on the limits of equivalent scales, see H. Martin and Périvier (2018).

[24] € 1140 in 2019.

[25] This new computation formula has been in place since January 1st, 2021. Before, the reference income was the monthly average of the taxable income, therefore based on two-years old incomes.

[26] The PA formula incorporates an *individual bonus* that commences at a minimum of 50% of the wage, which serves to diminish the marginal tax rate while simultaneously taking into account the number of individuals in households with earnings exceeding the designated threshold. However, it should be noted that these individual bonuses are not awarded separately to each contributing member; rather, they contribute to the overall amount. See Appendix A.III for details.

[27] Before the 2021 reform, 80% of ASF was deduced from RSA and PA. The increased has not been changed in RSA and the share deduced is now 53% (DREES 2022).

[28] For a discussion and proposition of new policies, see Pucci and Périvier (2022).

social transfers are tightly linked together but use different notions of income. The smoothness of the budget constraint largely depends on the stack of social transfers parents are entitled to, but these transfers have different eligibility criteria. They use different measures of income, adjusting more or less rapidly to changes and accounting for family structure differently. For instance, RSA and PA are adjusted based on household's quarterly earned income and family structure, while housing benefits use a moving average of earned income in the past 12 months and depend on the number of children, rent, and household income. They are the same amount with or without a partner but use the same formula with one or two incomes. The other transfers are either not mean-tested (ASF) or based on tax-income from 2 years before. As a result, social transfers do not adjust to changes in labour income or family situation in the same way. In Appendix A.II, we demonstrate how challenging it can be to track and understand changes in social transfers in a simple scenario of a single-parent taking a job.

In December 2018, 76% of RSA recipients had been receiving it for more than a year, with an average registration time of 5.9 years, and 59% received it continuously between 2008 and 2018 (DREES 2022). **Total cash transfers are lower than the poverty line and RSA recipients often remains in this situation for many years**. The French minimum income (RSA) is conditioned upon participating in mandatory social support and job search. However, their implementation have been strongly criticised in official reports (Pitollat and Klein 2018, Aout ; Damon 2018 ; Cour des comptes 2022). In a 2022 survey, Athari (2023) reports that 45 % or RSA recipients received no support in the past 12 months, 87 % are involved in a social programme but they start late: only 59 % of those registered for less than 6 months are.

III.2 Reliance: A randomised intensive welfare-to-work programme

From 2018 to 2022, we ran a randomised control trial of an intensive support programme in the North East of France. This high stake programme has been supported by the National family allowance fund, the local administration in charge of workfare obligations, the Employment agency and a public investment fund (*caisse des dépôts et consignation*) and cited as a benchmark for future welfare-to-work programmes in the aforementioned reports. The assumptions regarding the effects of the programme are based on both a *capacity-building* approach – where participants benefit by developing or maintaining skills, building a network, etc. – and an *emancipatory* approach that seeks to alleviate the specific burdens and obstacles faced by each family. Both reflect the '*social investment*' perspective of this programme, notably inspired by the "*capability"* approach of social justice from Amartya Sen (Martignani 2016; Hemerijck 2018). The average cost per participant is estimated to be around €2800, approximately four times the average expenditure for regular support (Mahdi 2021). However, the recruiting process was more embedded in the *workfare* approach of active labour market policies.

A staggered block-randomised encouragement design This programme is based on a staggered block-randomised encouragement design. Each of the five years of implementation, a random sample of 500 eligible households has been drawn from administrative records. Social workers from the Departmental council assessed and classified every file and those already in another programme, in employment, or deemed inapt, were excluded from the experimental sample (1/5 of the initial sample on average). The remaining households in each cohort were then randomly assigned encouragement within blocks based on the cross product of number of children (1, 2, 3+), registration at the Employment agency (True/False) and number of years on welfare (2-5, 5-10, more than 10 years). The product set of these variables defines single parents' *type* between which we expect different reaction to the encouragement, different outcomes and possibly heterogeneous treatment effects. Registered unemployed are expected to be closer to the labour market, the number of children increases constraints due to parental obligations while it has been shown that the longer people receive RSA, the less likely they are to find a job[29].

To ensure high levels of compliance, we adapted our recruitment process over the years, employing both "threats" and "pull" strategies in our recruitment process. Initially, we sent a formal letter to parents inviting them to a public meeting in the welfare-to-work department of the Departmental council. The following year, we added an ambiguous yet threatening sentence about "rights and duties" in the invitation letter, which served as a "threat effect" to encourage participation. Additionally, we made our recruitment sessions more welcoming and personalised by moving from collective information sessions in the welfare-to-work division of the Departmental council to

[29] See Heim (2024) for more details and descriptive statistics across groups

individual face-to-face interviews with project managers in newly renovated and well-equipped premises. We also involved former participants in the recruitment sessions to answer questions and provide positive testimonies as peers. Table B.4 in the Appendix summarises the average effects of encouragement on participation and separates estimations by cohort[30]. On average, we achieved a take-up of 38.9 in the four first cohorts, increasing by 19.25 pp between the first and fourth.

Year-long social support, childcare and job-search assistance The initial stages of the programme focused on tackling major issues such as over-indebtedness, housing, healthcare, and children's education. The programme was designed to be conveniently scheduled in relation to school and daycare timetables, and participants were required to commit approximately 15-20 hours per week. Childcare duties were shared among participants in a dedicated, colourful space that was equipped with baby-care supplies, toys, games, and books. The programme's target population required a lot of social support, and the *Reliance* programme combined "classic" individual support with group support through thematic workshops. Activities primarily focused on creating and validating realistic professional projects, addressing steps like education, internships, and improving job search efficiency, aligning expectations with job opportunities. Some workshops addressed daily life and organisation, offering strategies to cope with upcoming changes and find appropriate solutions. Others explored self-awareness and relationships with others, including issues related to parenthood, relationships, and gender norms and roles. The programme aimed to humanise administrative procedures and alleviate the emotional burden associated with them. For instance, social workers offered assistance with applications for social housing, affordable school lunch or suitable childcare options. Lastly, the programme organised regular sessions with social workers from the Family allowance fund. These workshops focused on access to benefit rights and parenthood to help participants understand and know how to gain access to their rights. We will return to this important point later.

Policy and macro-economic changes in the timeframe During this period, the economic environment was significantly impacted by a major and sudden reform of PA in 2019 and the COVID-19 crisis from March 2020.

First, the pandemic disrupted the implementation of the programme for the 2020 cohort, and the economy was almost entirely shut down for a while before slowly recovering.[31] As a result, we decided to continue with the programme and secured funding for two additional cohorts. To increase precision, we enlarged the sample for the 2021 cohort. In 2022, the pool of eligible families that had not been sampled was too small, especially for long-term recipients. To build the 2022 cohort, we sampled the remaining eligible population and random samples from the control groups of the previous cohorts. From November 2021, the composition of the control groups of the four previous cohorts changed. Ultimately, the intervention was rolled out from 2018 to 2022 in a staggered design, as illustrated in Figure B.20 in the Appendix.

Second, the 2019 reform of in-work benefits was adopted unexpectedly as an answer to massive demonstrations from the Yellow Vest movement. In the timeline of this experiment, it occurred at the last quarter of the first cohort and right when the second was being recruited. At that time, we had meetings with project managers to discuss how to explain the reform to participants and further emphasise monetary incentives in the programme. We provided simple plots[32] of the amounts of social benefits over incomes in percentage of the minimum wage from simulation models of the Family allowance fund (A very similar plot as Figure 3). We also provided figures illustrating the effects of the reform, borrowing results from colleagues who simulated the reform to estimate the cost of the reform ex-ante[33]. Unbeknownst to us for years, social workers used these tools a lot and we only discovered that when they asked us for an updated version during a meeting.

[30] Simply regressing participation on encouragement and block fixed effects.

[31] For more details on the adaptation and consequences of the pandemic, see Appendix B.

[32] Figure 3 is one example although this one uses the simulation model from DREES while we used the one from Cnaf at that time. These models are almost identical by design.

[33] Results based on these estimations have now been published by Dardier, Doan, and Lhermet (2022), and the figure we mentioned is reported in Figure A.18 in the Appendix.

No effect on employment and disposable incomes after training In a companion paper, Heim (2024) offers a first examination of the programme's effects and provides further details on implementation and participant characteristics. The findings indicate that the programme slows the job finding rate during its initial phase, leading to a pronounced lock-in effect on poverty rates, disposable income, and employment. However, these anticipated negative effects wane by the end of the programme, and there are no average effect for the year after. The central message of this paper is that without random assignment, one would incorrectly conclude that the programme enhances employment. In reality, the programme attracts individuals with the highest potential employment levels but does not boost labour market participation. The selection bias is so strong that estimates using modern doubly-robust matched difference-in-differences fail to include the experimental results within the simultaneous 95% confidence interval. This research also showed that participants were more likely to be among the poorest and least educated but also more likely to be registered at the Employment agency, a group notably more likely to find a job both among participants and in the control group.

An independent team of consultants also conducted a qualitative evaluation from 2019 to 2020 (FORS 2020), adding the views of participants to the analysis of the implementation. They note that the initial three months of the programme creates a rapid shift in momentum, boosting confidence and self-esteem relatively quickly. The group dynamic and "lever effect" of collective activities helped individuals overcome isolation. However, isolated individuals with limited work experience and reduced autonomy, and those with active social networks but not actively seeking employment due to care-giving responsibilities, health issues, etc. faced a longer timeline for employment reintegration, as their focus was not on immediate employment but rather on formulating a professional plan.

The initial findings indicate that this programme had a strong *screening effect*, drawing in individuals who were most likely to secure employment. These results align with the qualitative evaluation's findings. However, the programme did not prove effective in expanding employment opportunities at the extensive margin. After 30 months, 89% of participants remained in poverty and 66 % have no job.
The analysis of the heterogeneity of treatment effects in Heim (2024) revealed puzzling patterns, particularly by median income and number of children at baseline. The effects on disposable income and labour market participation exhibit divergent patterns, implying changes in the composition of income, which is corroborated by the rise in cash transfers at the end of the period. This can only mean that the programme affected family composition and source of income differently for parents with more or less children at baseline.

The problem with this analysis is that we have very little guidance for interpreting the results. However, welfare and policy implications are not the same if the programme affects household size through fertility, the merging of two single-parent families, or if older children remain or leave the household. In Heim (2024), the research was guided by the policymakers' objectives and a typical evaluation problem. This helps to define clear normative criteria and decision rules. However, it cannot explain *why* the programme did not increase employment beyond selection effects. To provide more informative results on the mechanisms, we briefly reopen the black box of the programme with insights from the qualitative evaluation.

As Hemerijck and Huguenot-Noël (2022) write, "*The normative heart of the social investment paradigm, beyond fair distribution, underlines the importance of secure capabilities enabling citizens to flourish over the life course*". In line with this view, favourable outcomes may not be a matter of resources or preference satisfaction but, rather, of what a person is able to do and be. As such, poverty could be understood as agency-based capability deprivation (Mani et al. 2013; Shah et al. 2018; Banerjee, Duflo, and Sharma decembre 2021; Henderson and Follett 2022).

In this setting, participants spent a large amount of time in this programme and what we know from the qualitative evaluation is that many participants really felt that the programme helped; at least those interviewed. The qualitative evaluation is filled with verbatims of "empowered single mothers" with higher sense of agency and expanding horizons.

Optimisation as functionnings The programme's interaction with participants through regular visits from social workers provided valuable opportunities for knowledge-building and empowerment. These interventions ensured that participants understood their rights and the complexities of eligibility criteria and benefit calculations. By using visual aids such as Figure 3 of the tax-benefit we provided, social workers illustrated to participants that pursuing employment would not result in a complete loss of allowances. This hands-on approach was instrumental in equipping participants with a deeper understanding of their rights and future prospects.

As highlighted in the qualitative evaluation by FORS (2020), simulations of returning to work organised with the CAF were particularly enlightening for participants, enabling them to envision their future financial resources. Testimonials express newfound awareness of their entitlements and a shift in mindset towards employment. For instance, one participant reflected, "*I know I have rights, but at the moment I can't necessarily open them (alimony, housing benefits, etc.), I asked at the CAF when they came. That's what I learnt at Reliance, that I could activate that. We found out about CAF-related stuff, etc., and that's something*" (F, 41 years old, 3 children, separated for 2 years).

Another participant shared, "*When I first came here, I remember a discussion with [Project manager] where we talked about income: I didn't want to work more than half-time either, so as not to lose income, and she told me I was wrong. And in fact she wasn't wrong: my attitude did in fact change. That day there was a social worker from the CAF with us, and I was stubborn and narrow-minded*" (F, 32, 2 children).

These anecdotal evidences suggest that some participants benefited from the programme by learning about French tax-benefits, rights they were previously unaware of, or eligibility criteria. However, it also highlights that some participants' understanding of their rights remained limited despite years in the system. Nevertheless, the programme's use of visual aids, simulations, and personalised advice may have contributed to the development of enhanced agency and capability.

If these results go beyond anecdotal evidence, we expect participants to have more *rational* reactions to salient incentives. If these incentives are strong, they can generate reactions large enough for us to measure.

III.3 '*Assistaxation*': economic incentives and family structure

We want to know what the monetary incentives are for single parents on long-term welfare and understand how it differs for single/coupled parents with different numbers of children. Our approach consists simply in visually inspecting plots of social transfers when we vary the number of children and cohabitation with labour incomes of a single earner. We use the EDIFIS model to simulate these situations and report the results in Figure 4.

Three facts on welfare benefits for single parents in France: This graphics shows three under-reported stylised facts of the French benefit system:

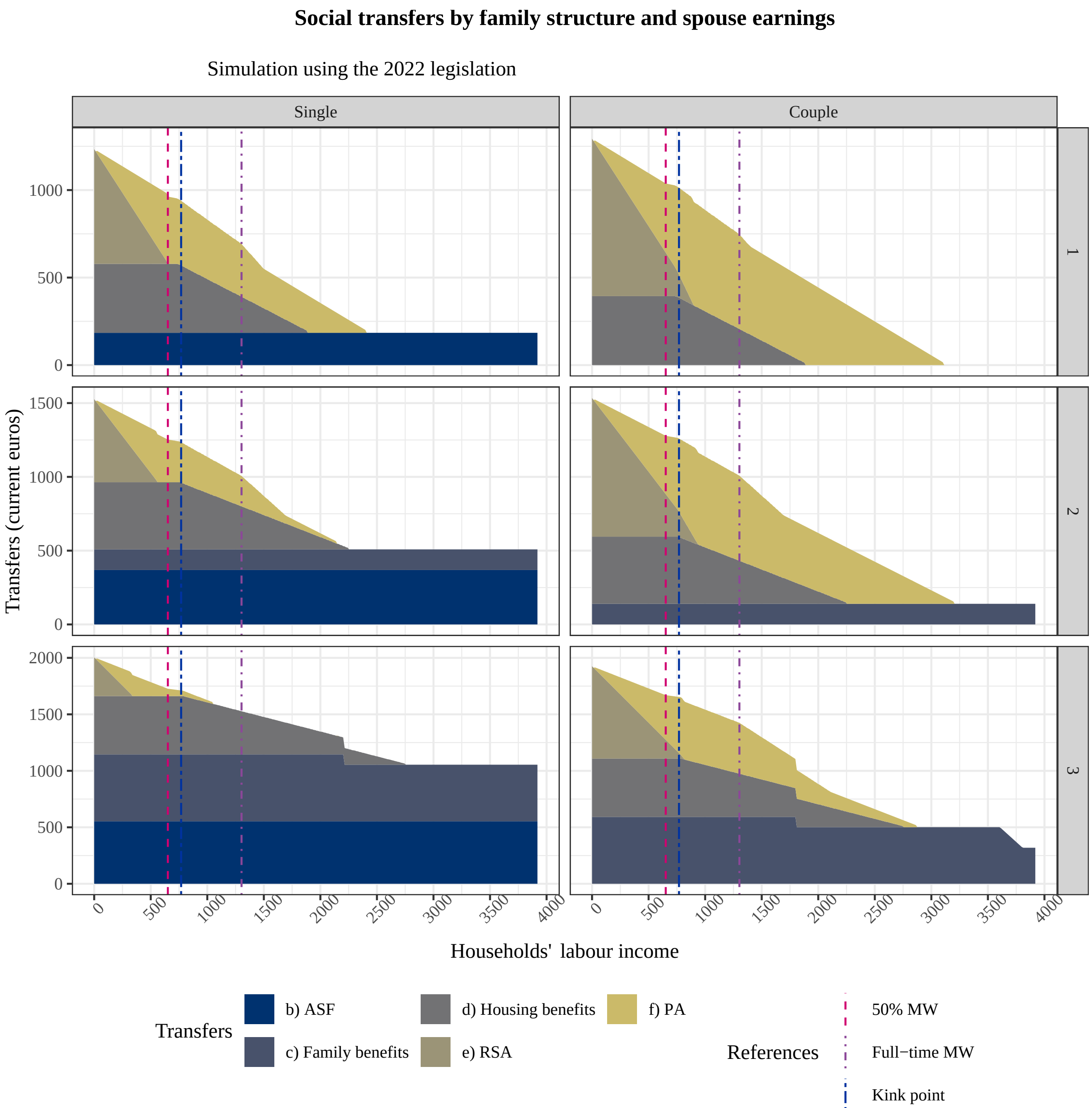
Fact 1. *The composition of total social transfers is very different across household' structures.*

Fact 2. *In-work benefits decrease at faster rates after the minimum wage for single mothers than for couples. Its amount are much smaller and exhaust more rapidly than for couples.*

Fact 3. *There is a range between .5 and .6 full-time minimum wage with minimal implicit tax rate and stable levels of all social transfers.*

All three have important consequences and incentives. With Fact 1, we see that almost all incomes for single parents with one child are mean-tested, but housing benefits do not depend on cohabitation and adjust more slowly. For single parents with two children, family benefits and ASF represent an important part of the budget and are largely deduced from RSA and PA. Therefore, in-work benefits and RSA are lower and exhaust faster. Note that if the parent was receiving child support instead of ASF, there would be an even larger share deduced from RSA and PA. Because of this feature, parents of three receive so little RSA and PA that they do not receive in-work benefits with earnings over 3/4 of the full-time minimum wage. Receiving child support makes it even worse. For single parents of three, their level of transfers depends mostly on their number of dependent children for both housing and family benefits.

Figure 4: Monetary incentives for households with 1 to 3 children, with or without a partner (in which case, x axis is household income)



Sources: DREES, EDIFIS.
Case study for single parent or couple with only one varying labour income.
b) ASF is the public child support paid quarterly when the other parent doesn't. Lump−sum payment by number of children.
c) Family benefits are open for parents with 2 or more children, depend on taxable incomes (Year − 2).
d) Housing benefits, baseline amount depends on number of dependent children, rent price and their due payment.
Decreases with earned incomes over the past 11 months, from the previous month.
e) RSA is the French minimum income scheme, f) PA is an in−work benefit, both are mean−tested
and depend on family composition and household's income.

Fact 2 has strong political and fairness implications. The overall interactions of all allowances and complex eligibility rules make them very hard to understand and can create large variations on the implicit marginal tax rates, as Figure A.19 in the Appendix illustrates. Using again the simulations from the EDIFIS model, we report implicit marginal tax rates in variation with earned incomes by percent of minimum incomes. These interactions create different plateaus with highly different tax rates. Consistent with these figures, the highest marginal tax rate is located at the minimum wage level for single parents with one and two children. Between 100 and 110% of the minimum wage, the IMTR is 72% for single parents of one child and 69% between 100% and 120% of the minimum wage for single parents of two children. These variations of implicit marginal tax rate motivate our analysis of the reaction at the intensive margin using bunching around kink points to retrieve observable elasticities. In Section VI.1, table 1 reports the average implicit marginal tax rates, giving a clearer picture of the sharp inequalities and incentives.

The most typical labour contract for low-educated workers is the most heavily taxed for single parents. It is also the lowest level of payroll taxes (Bozio, Breda, and Guillot 2023). This makes it much harder for single parents to increase their disposable income by working longer hours. This is particularly the case for single parents with two children, for whom any improvement from the full-time minimum wage results in a rapid and sharp reduction of in work benefit: a €100 monthly increase at the minimum wage level induces a reduction of in-work benefits of €75 per month in the next quarter. Having higher constraints than those with one child (Briard 2020), **this group has the largest disincentive to work-full-time**.

The situations is very different with a partner, where a single wage opens large amounts of in-work benefits, even with one part-time job. For single parents with one child, finding a partner with a child would compensate the loss of ASF through family benefits. For parents of two, however, cohabitation means losing €246 of ASF.

We are not the first to identify the unfairness and sharp inequalities between single parents and more traditional families. Périvier (2012) offers additional arguments, and notably that mandatory support and job search for RSA recipients is lifted when households earn more than € 500. Couples in the traditional bread-earner models are not *bothered* by workfare obligations while a single mothers receiving RSA is, which the author sums-up in the article's title: "*work, or get married*". Paradoxically, while single parents are most heavily taxed on full-time jobs, statistical data from the Labour Force survey indicates that 40% of those working part-time express a willingness to work more hours if given the opportunity. Interestingly, partnered women in part-time positions are only 22% as likely to express this desire (Périvier 2022a).

Hidden tax on single parents: the assistaxation trap. In the qualitative study of FORS (2020), several individuals and group interviews revealed that, in most cases, ex-partners are disengaged or even absent in the upbringing of their child(ren), and their financial contribution to material expenses is often minimal or non-existent. At Baseline, only 20.6% of the sample receive child support and therefore, 65 perceive ASF instead[34]. However, some mothers express reluctance to apply for the ASF, fearing the repercussions from their ex-partner or potentially harming the quality of the relationship that the ex-partner maintains with their shared child(ren). "*A while ago, I applied for ASF; the dad and I didn't get along. We went to court, and he was supposed to pay child support, but he never did. But now that we get along a bit better, and when the girls need something, he still buys it for them. We prefer not to reapply. Otherwise, I'm afraid we won't get along anymore.*" (A., 40 years old, 2 children, on RSA for over 10 years). Pucci and Périvier (2022) recommend to remove ASF from the reference incomes for RSA, PA and housing benefit and to neutralise at least part of child support so that single parents who receive it actually see their income increase.

As for parents of three children (or more), the inclusion of family benefits in the baseline income for RSA and PA drastically reduce their amount so much that the exit point of PA is below 80% minimum wage. That is because having 3 children or more increases family benefits and (may) open an additional allowance[35] that are both immediately and entirely deduced from RSA and PA base level. For single parents of 3 or more children, RSA and PA do not play the role of buffer and monetary incentives, these are done via family and housing benefits. Paradoxically, it also means that their main source of income is 100% taxed until they leave welfare and have a lower and flatter implicit tax rate. We also note that family and housing benefits are not taxable incomes. They are only fully taxed for those receiving social assistance.

According to Fact 3, individuals in all three groups find a *sweet spot* in the labour earnings distribution between 50% and 60% of the full-time minimum wage level. This holds true even for those who repartner. For parents of

[34] The remaining share receives neither.
[35] *Complément familal*

two children, recognising the taxing nature of the full-time minimum wage system should be highly discouraging. Part-time jobs also serve as a strong reference point (Tversky and Kahneman 1974). They represent an attainable goal that may have been encouraged by social workers. For households with a strong aversion to loss, part-time jobs do not impact housing benefits, only RSA and PA adjust. However, such jobs usually do not generate income high enough to get out of poverty and often have limited opportunity to build more human capital (Blundell et al. 2016).

The complex interactions between various social transfers with differing schedules result in an increased tax burden on one of society's most vulnerable groups. We propose that "*Assistaxation*" is an appropriate term to convey the concept of providing assistance that becomes burdensome, overly taxing, or difficult mentally, physically, emotionally, or financially. It is not just about the administrative burden and stigma discussed in the literature; it also involves heavier and implicit taxation of labour income and a 100% tax rate on child support and family benefits of the poorest.

At this point, if the programme allowed participants to make better inferences about their expected income, few reactions to *assistaxation* could follow:

1. Part-time instead of full-time work, with a focus on the *sweet spot*.
2. Single parents with one child have little to lose by repartnering and may even receive higher social support if their partner also has children.
3. Those with two children are strongly discouraged from working full-time at the minimum wage and above, with no clear incentives regarding cohabitation.
4. The income of those with three or more children mostly depends on family benefits, and monetary incentives are limited.

We now present the data we use to investigate such behavioural changes following this experimental welfare-to-work programme.

IV Data and descriptive statistics

The design of this experiment relies on multiple random samples of eligible households extracted from the administrative records of the Departmental council. These samples are organised into cohorts that have been randomly assigned treatment following the protocol outlined in the previous section. The initial datasets have been enhanced with these design variables and aligned with the monthly administrative records from the National family allowance fund (CNAF).

IV.1 Source and definition of the main variables

The ALLSTAT files provide information on the situation of every beneficiary for a specific month. They include details on the "household heads" and their possible spouse, such as gender, year of birth, marital status, activity status, nationality, and more. Additionally, they provide information on dependent children, including their year of birth, alternate residence status for family benefits, absence of a parent, the legal benefits they receive, and various measures of household income. The complete database spans from January, 2017 to June, 2023. Cohorts have been randomised at different time and we define the month before random assignment as the main reference to measure the average dynamics of treatment effects across cohorts. To avoid bias, we restrict our window of observation over the 30 months after random assignment over which all four cohorts are observed. We provide details on the pre-treatment of our database and balance check in Appendix C.

In this research, our main variable of interest are incomes and family composition for which we have several variables.

Measures of income We use self-declared incomes of the quarterly report for RSA and PA to define a set of measures for individual and household incomes[36]. In particular, we define four measures of pre-tax/benefit incomes:

- **Individual's labour incomes** are the labour incomes earned by the participant of the programme reported to the administration ;
- **Individual's pre-tax incomes** are labour incomes and other sources of income such as child support or other non-labour incomes.
- **Household's labour incomes** are the sum of all labour incomes reported. It includes potential spouse labour earnings.
- **Household's pre-tax incomes** are the total household income before tax and benefits are adjusted.

The database also contains **total cash transfers** and with it, a measure of **household disposable income** which is the post-tax-benefit income of the household. Then, the Family allowance fund defines the **number of consumption units** and computes the **disposable income per capita** accounting for family size. The number of consumption units is 1 for the first adult, 0.5 for each adult or child aged 14 or older, 0.3 for children under 14, 0.2 for single-parent families. This latter variable can serve as a composite index of family structure over which parents have agency.

All monetary values are converted in constant € in 2015 values using the consumer price index of the bottom 20% of the income distributions. We also use the full-time minimum wage as a reference point and use variables divided by the value of the actualised minimum wage that month.

Couple formation To measure if the programme affects family composition, we consider **cohabitation** with a partner as a first outcome. For that, we use the binary variable from the ALLSTAT files that codes 1 if the person lives with another adult with whom she is in a romantic relationship[37]. This variable is very important and conditions most payments. It is either modified after the parent reports her change or a control from CAF. It captures living together officially rather than romantic relationships *per se*.

Cohabitation is a very confuse notion. For the Family allowance fund, cohabitation is made of two people living together, considered as a couple by their social circle, who share financial responsibilities and household duties. However, living separately from one's partner can still be considered cohabitation if the two share financial responsibilities. Conversely, a room-mate is not a cohabitation unless they are known to be romantically involved.

This variable captures official cohabitation which may be affected by the programme through increased share of relationships - possibly with a former partner - and higher reporting among participants with better knowledge of the definition and stakes. Cohabitation can be more or less incentivising for parents, depending on their situation and that of their partner. For the Family allowance fund, it means entirely pooling their income and possibly including other children.

Children number and characteristics There are several measurements for the number of children in the ALLSTAT database, because they are used for different aids and some have different eligibility rules. Households receive family benefits until the child is 20, but housing benefits and additional family benefits can be extended until the child is 21. To be consistent with other outcomes, we use the variable updated quarterly with income report for RSA and PA and use the others as robustness checks. We construct variables of the oldest child in a given month and use it to define quartiles of households with children close to autonomy at the time of random assignment. The top quartile of that variable corresponds to 16.6 years old. This means that parents in this quartile have children old enough to move-out by the time the programme ends and soon after.

We measure fertility with the probability that the mother is pregnant a given month. The variable is a dummy coming from social security flows after a doctor declared the pregnancy. This very reliable measurement allows to capture fertility effects about 6 months before birth, which may be important considering the censored window of observations.

[36] For administrative clarity, these monthly gross incomes are separately identified for "*Monsieur*" and "*Madame*" (as per the terminology still used in the CNAF administrative files 12 years after the same-sex marriage law was passed). The constructed variables are defined accordingly.
[37] And compute the eligibility and amounts of all transfers accordingly.

Timing of events We want to investigate how taking a job affects our final outcomes and possibly other dimensions. For that, we want to compare changes around this event. We define a pair of variables for the **month-of-event**, and **time-to-event**, which measures the number of months relative to the month of event. Month-of-event defines groups of single parents for whom the first job re-entry occurred the same month. We explain how we use these variables in section V.

IV.2 Descriptive evidence of important heterogenous reactions

Following our hypothesis, we first look at the distribution of reported earned incomes across treatment groups. We first start with household's labour income which is the main income used to compute PA.

Bunching at the *sweet spot* Using random assignment as a reference point, cohorts started the programme from the 2^{nd} to the 6^{th} month after and the programme ended between 14 and 18 months after random assignment. We look at the 12 months from the 18^{th} to the 30^{th} since random assignment and only look at observations with positive labour income. Figure 5 presents the kernel-density estimates of households' labour income, separated by actual participation and number of children at baseline. The in-work benefits are computed from this variable. In line with the optimisation friction and bunching literature, we also added the theoretical amount of in-work benefits (PA) using the EDIFIS simulation model, and the *sweet spot* where the implicit marginal tax rate is lowest and housing benefits do not decrease.

The distribution of labour incomes among treated households contrasts sharply with the other groups, who overlap for the most part. Participants with one or two children report a mass density of household's labour incomes within the range of €600_{2015} to €800_{2015} net per month; the *sweet spot* previously identified. Conversely, control and never-takers' distributions have thicker tails than participants. There is a thicker distribution of part-time jobs among never-takers with two children than in the control group. The income distribution for single parents of three or more is higher for part-time jobs, but similar across groups, with thicker and longer tails. Figure D.27 in the Appendix displays the same kernel densities over individual labour incomes and shows a very similar pattern.

Without any assumption, these plots show that participants (or their partners) are more likely to work part-time than other comparison groups. However, this observation is not enough to make a compelling case on participants capabilities. It may very well stem from advises from social workers and the definition of part-time job as an objective. However, PA is computed over the joint-distribution of labour income for all members of the household. If participants re-partner and optimise, this should also affect the repartition of incomes between spouses. This is precisely what we observe in Figure 6.

We present the bivariate-density of individual incomes and household incomes, conditioning on the latter being positive[38]. The x-axis shows participants' labour incomes and the y-axis the households' incomes, each panel presenting the actual treatment status for different family sizes at baseline.

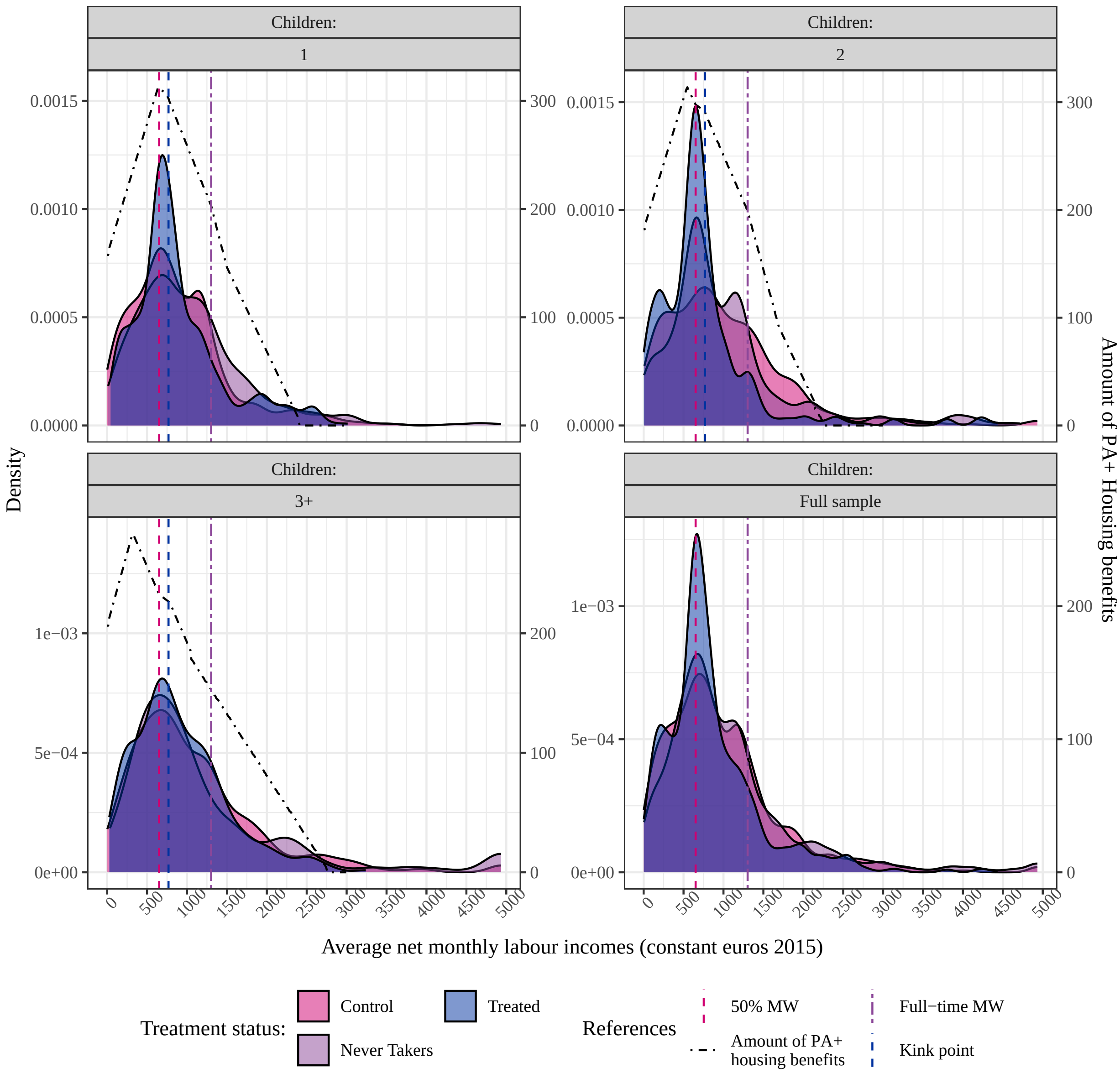
Figure 6 shows that among participants, the labour incomes essentially come from their own labour incomes as the 2D-density aligns with the 45° line, contrasting with other comparison groups. Control and never-takers have thicker densities for *bread-earner* households type: positive household incomes with 0 labour incomes from mothers' employment. Those figures show that participants are more likely to be the sole-earner of the household, bunching at the *sweet spot* with far less variability than in the comparison groups. In particular, the figures for parents of 2 show that incomes in the control group go much higher in both dimensions than either the never-takers or treated ones. However, these differences could also partly be due to changes in the share of parents who re-partnered.

[38] In other words, we use the same sample as in Figure 5.

Figure 5: Participants bunch at the sweet spot of household labour incomes

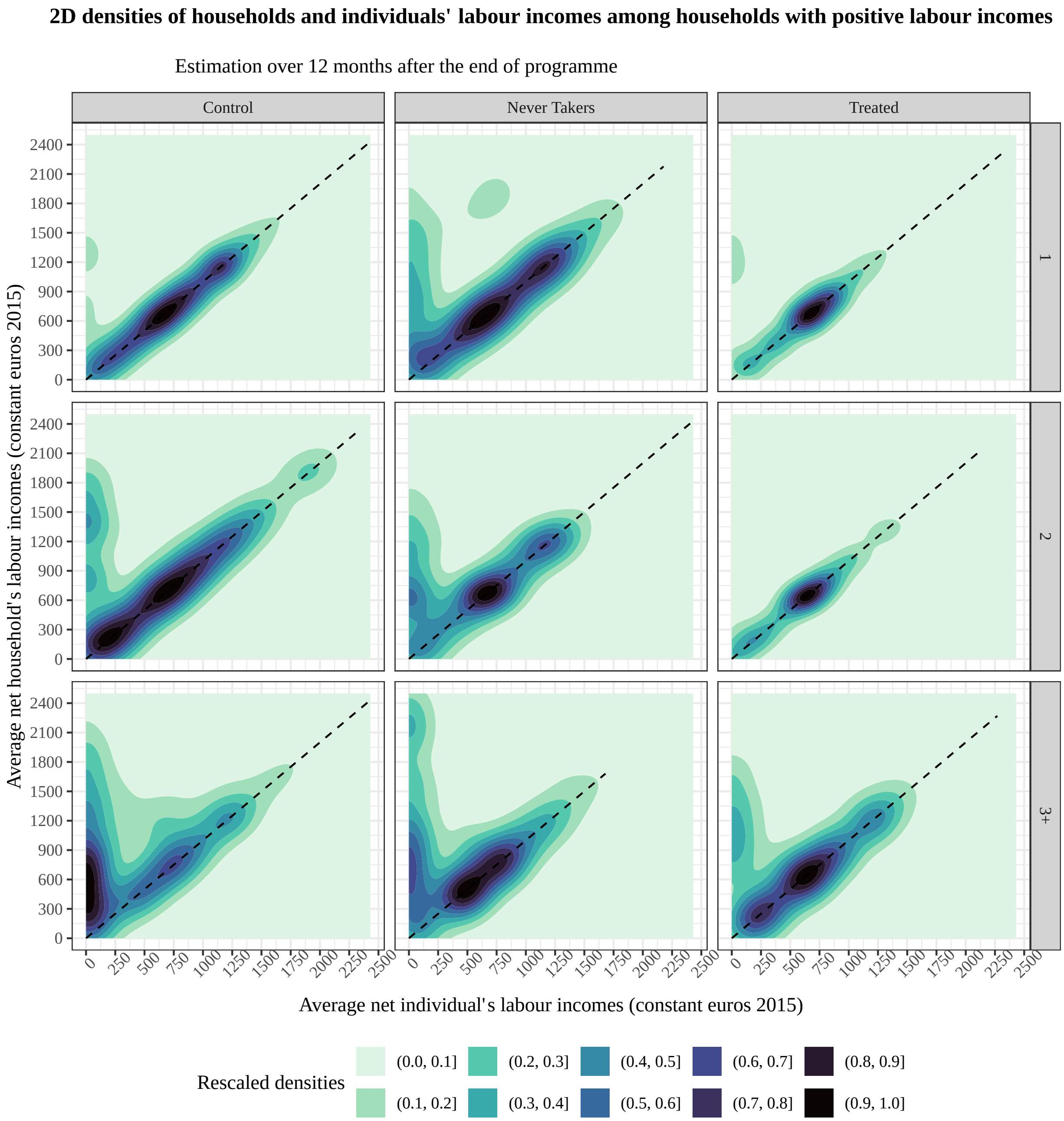
Distribution of households' labour incomes among those who work and theoretical amount of PA

Estimation over 12 months after the end of training



Sources: ALLSTAT, restricted sample over 12 months after the end of the programme among those who report positive labour incomes and smaller than 5 000 euros for clarity.
Notes: Kernel density of labour income for those with positive labour incomes. The PA reference line indicates the theoretical amount of in−work benefits and housing benefits received for single parents by number of children and net labour income based on EDIFIS using the 2022 legislation.
Kink points indicate the level of income that minimises the implicit marginal tax rate.

Figure 6: 2D density plot of households and individual labour incomes



Sources: ALLSTAT, restricted sample over 12 months after the end of the programme among those who report positive labour incomes
Notes: 2D−Kernel densities rescaled so that the highest level equals 1.
The x−axis indicates mothers' labour incomes and the y−axis indicates households' income. Dark colors around the y−axis indicates 'bread−earner' couples.
Densities around the 45° line indicates mothers as sole earners (single or in a couple).

Small families expands, mothers of two are less pregnants, large families hold their own We now look at the evolution of the family structure variables defined in subsection IV and compare means from 12 months before random assignment to 30 after for the first 4 cohorts. Figure 7 estimates the averages using OLS without constant and a dummy for each treatment arm $\times$ relative month, weighting observations with the inverse propensity score of encouragement[39]. We present results for cohabitation, number of dependent children, number of children under 2 and pregnancies, by number of children at baseline.

These figures show at least one important difference in each group and in particular:

- higher cohabitation for parents with one child,
- lower pregnancies for parents of two, and
- higher number of children for those with three or more.

For single parents with three children, the only difference between the encouraged group and the control group is that the average number of children decreases at a slower pace in the former than in the latter.

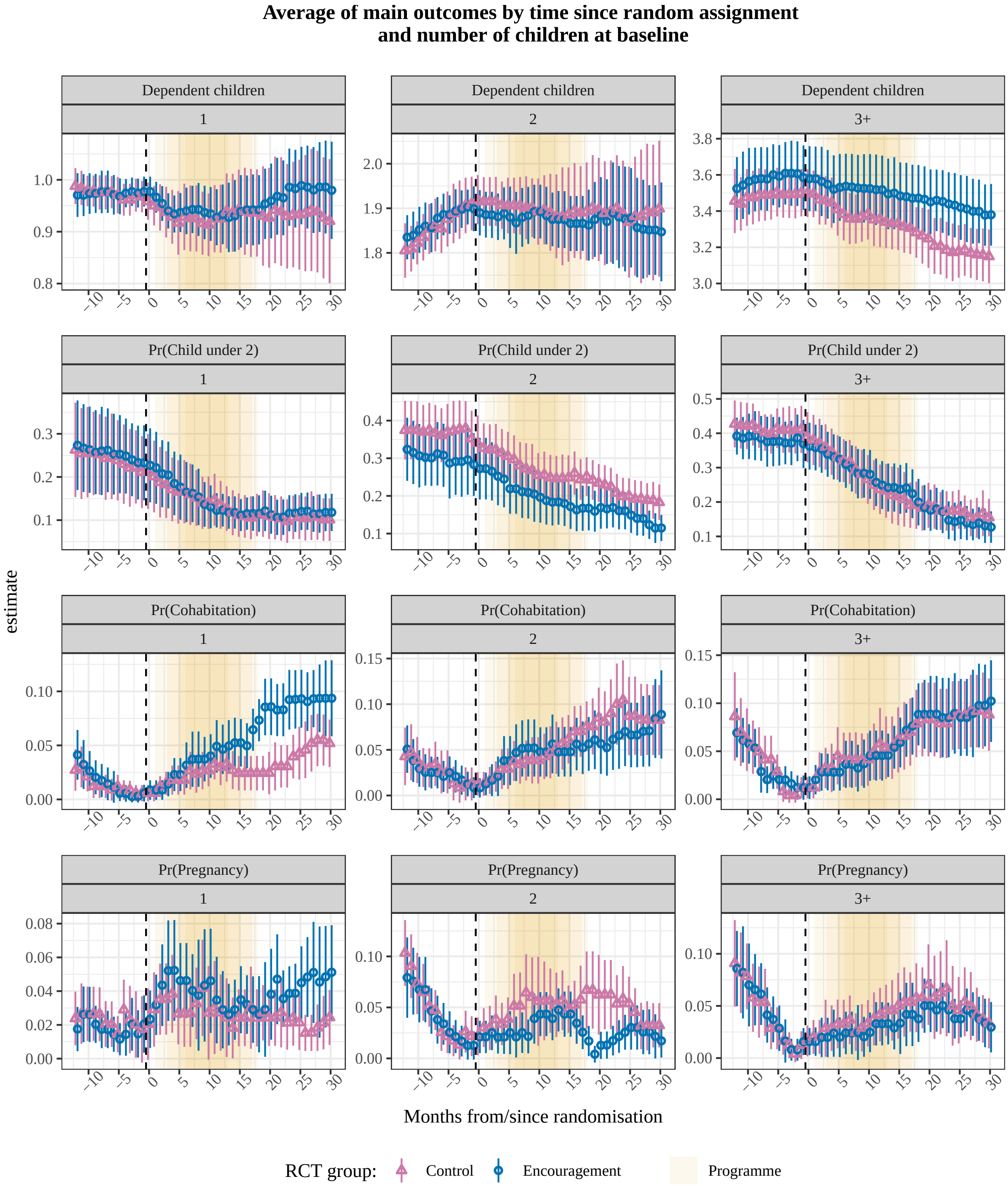
Single parents with one child in the encouragement group are significantly more likely to re-partner, and this occurs right at the end of the training programme. The control group for this type of family remains significantly more single than other family structures. There is no difference between the encouraged and control groups for the number of children under two, but there is an imprecise five percentage point difference in the total number of children. With a difference of about 7 percentage points in cohabitation, this suggests the merging of single-parent families. Later, there is a 2 percentage point difference in pregnancy rates at the end of the timeframe.

Single parents with two children in the encouragement group were less likely to be pregnant during the training period, and the number of pregnant women in this group fell to 0 right after the end of the programme. This period also coincides with a slightly lower share of coupled households, but this difference swiftly fades out. The effect on pregnancy is only slightly starting to show up in the number of children under two at the end of the timeframe.

These descriptive statistics inform on the heterogeneity in the average intention-to-treat effects of the programme on family structure, by number of children at baseline. Importantly, these changes affect the eligibility and amount of transfers received. Figure D.28 in the Appendix also presents the average long difference of the share of families with different sources of incomes, over relative time since random assignment and by number of children. It shows that on average, there are few differences by encouragement group, with two exceptions. First, mothers of three or more children in the encouragement group are more likely to receive family allowances than those in the control group. Second, there is a short lived effect of the programme on the probability of receiving child support, and a symmetric effect on family support allowance for parents of two children.

[39] We estimated a Probit of encouragement on block fixed effect and predicted the assignment probabilities to construct inverse weights. See Heim (2024) for more details.

Figure 7: Faster cohabitation for mothers of 1 child, slower reduction of the number of children for mothers of 3 or more



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021 from −12 to 30 months from random assignment.
Means and point−wise 95% confidence intervals estimated by OLS regressions on month x group x encouragement dummies without intercept, using cluster−robust standard errors adjusted at the block x cohort level and inverse instrument propensity score weighting.
Rows correspond to different outcomes from separate regressions. Columns display results by number of children at baseline.

V Empirical stategy

We want to estimate how the programme affected single parents' capabilities by observing changes in their income distributions and relate these changes to the economic incentives we identified. In particular, we want to estimate the difference in densities of participants around kink points of the tax rate benefit system with the relevant counterfactual densities. Second, we want to estimate the average treatment effect of the programme on family structure by sub-group of number of children at baseline. We access rich panel data from administrative records of randomly-sampled cohorts assigned using an experimental design. We first start by introducing notations and our main assumptions steaming from this research design. Then, we present our strategy to estimate counterfactual densities, bunching estimator and elasticities in subsection V.2. The estimations of dynamic treatment effects on family structure is presented in subsection V.3.

V.1 Notations and definitions

We observe a random sample of individuals $i \in I$ over calendar months $t \in \mathcal{T}$ and we set $t = 1 \equiv$ January, 2017 and $max(\mathcal{T})$ =June, 2023. I is composed of cohorts $c \in \mathcal{T}$ defined by the month before random assignment. We are interested in the effects of the programme aggregating cohorts in relative time and define $m = t - c$, such that $m = 1$ is the month of random assignment. Individuals i in cohort c are defined by a set of attributes including number of children (1,2,3+), registration at the Employment agency (True/False) and number of years receiving RSA (2-5, 5-10, more than 10 years). The cross-product of these characteristics defines blocks $b_{jc} \in B_c \subset B$. B_c is the set of blocks j in cohort c and B contains the $j \times c$ blocks of all cohorts. We denote f_B and F_B the distribution and cumulative distribution of blocks, integrating over b. For estimations, we also denote $b_{ijc} = \mathbb{1}(i \in b_{jc})$ the dummy for block b_{jc} and $\mathbf{B}$ the matrix of block dummies. Number of children at baseline is contained in $\mathbf{B}$ and we denote $\mathbf{B_f} \subset \mathbf{B}$ the set of blocks (and individuals) with families of size f at baseline.

Let $Z = \mathbb{1}(Encouragement)$ and $D = \mathbb{1}(Participant)$ be the random variable for encouragement and participation, and let $\mathbf{Y}$ denote the matrix of outcomes Y^k where k index the outcome of interest. Some outcomes Y^k are *decision* outcomes $i.e.$ they lie in the causal path between D and a final outcome $Y^{k'}$. For instance, taking a job is an outcome possibly affected by the treatment and affecting disposable incomes. In such cases, we let $W \equiv Y^k$ denote this intermediary *decision/outcome*.

Let $f_{Z|B}$ and $F_{Z|B}$ denote the density and cumulative distribution of encouragement across blocks, characterising the instrument propensity score. We denote q_b the encouragement probability in block b with $\mathbb{E}[q_b] = .5$ by design. Due to uneven and small block sizes, there are some with slights variations. To account for that, we run a Probit of Z on $\mathbf{B}$ and use the predicted probability $\hat{q}_b$ as instrument propensity score. Like Heim (2024), we define $\tilde{Z}_{ib} = Z_{ib} - \hat{q}_b$ the centred instrument propensity score. In the framework of Borusyak, Hull, and Jaravel (2022), it follows the idea the distribution of *shocks* $\mathbf{q}$ given $\mathbf{B}$,$\mathbf{q}$, denoted $G(\mathbf{q}|\mathbf{B})$ is known. It then follows from their main results that using $\tilde{Z}_{ib}$ as an instrument for D_i ensures conditional independence and identification of causal effects[40].

Finally, for some random variable R, we define the τ-quantile, r_τ, of R as

$$r_\tau = F_R^{-1}(\tau) := \inf\{r : F_R(r) \geq \tau\},$$

where F_R denotes the cumulative distribution of W.

Our database is made of four random samples of cohorts randomised at time $t = c$ observed $\mathcal{T} - c$ periods after random assignment each. We make the following assumption regarding the properties of our sample:

Hypothesis 0.1 (Random sampling). $\forall c$, the sample $\{Y_{ic}^k, Y_{ic+1}^k, \dots, Y_{i\mathcal{T}-c}^k, \mathbf{B_c}, Z_{ic}, D_{ic}\}_{i=1}^{I_c}$ is independent and identically distributed (iid)

[40] Borusyak, Berkeley, and Hull (2023) propose a more intuitive discussion of the results of Borusyak, Hull, and Jaravel (2022) with numerous illustrations including the link with block random assignment with imperfect compliance.

In words, we assume that the sequence of individual observations in each cohort over the time-frame is randomly sampled from a larger population[41]. Assumption 0.1 allows us to view all potential outcomes as random and imposes no restriction between potential outcomes and treatment allocation, nor does it restrict the time series dependence of the observed random variables. This assumption matters in the justification of standard errors with asymptotics based on the convergence to a population parameter[42]. Together with our identification hypothesis 0.2, this allows to use conventional (cluster) robust standard errors with finite sample corrections to approximate the variance of our estimator.

This assumption may feel at odd with the experimental setting for which design-based estimators of the variance-covariance could improve precision (Alberto Abadie et al. 2020 ; Alberto Abadie et al. 2022). Such estimands use uncertainty in the assignment mechanism and the question is about internal validity: do estimates from the sample reflect causal parameters in this sample? External validity bears on the question of whether causal parameters from the sample correspond to the population parameter. Consistent with our approach of using this experiment as a natural experiment to uncover capabilities and reactions to structural parameters of the tax-benefit system, we think this approach is more relevant and easier to interpret with minimal assumptions on the data generating process.

Potential outcomes and identification For now, we abstract the panel nature of our data and consider identification in the cross-sectional setting at some point after random assignment or over a pre-defined subset of periods. We omit the time subscript to alleviate notation and reintroduce them when it matters. We follow the usual potential outcome notations of Joshua D. Angrist, Imbens, and Rubin (1996) and let $D_{ijc}(z)$, $Y^k(d) = Y^k(D(z)) \equiv Y^k(d,z)$ denote the potential participation and outcomes as function of potential participation and encouragement. Participants constitute the usual "compliers" and their proportion estimates the first stage effect. Because no member of the control group enrolled, there are no "always-takers" of the programme and monotonicity trivially holds. Under SUTVA[43], the observed outcomes reveal potential outcomes and we assume no spillover across groups. Formally, $Y^k(1,1)$ is the revealed potential outcome for treated compliers and $Y^k(0,0)$ the revealed potential outcomes for the control group.

By design, Z and $Y^k(0)$ are independent conditional on attributes summarised in the matrix of blocks. Hypothesis 0.2 summarises our main assumptions:

Hypothesis 0.2 (Identification hypotheses)**.**

1. **One-sided non-compliance:** $Pr(D(0)|Z=0)=1$
2. **Meaningful first stage:** $Pr(D(1)|Z=1)>0$
3. **Conditional independence and exclusion:** $Y^k(0) \perp Z|\mathbf{B}=b_{jc} \quad \forall\, b_{jc}$ **where** $Pr(D=1|\mathbf{B}=b_{jc})>0$
4. **Common support:** $f_{Z|B}(Z=0|\mathbf{B}=b_{jc})>0 \quad \forall\, b_{jc}$ **where** $Pr(D=1|\mathbf{B}=b_{jc})>0$

Note that in the usual instrumental variable setting, the independence assumption includes potential participation and potential outcome for the treated. However, Frölich and Melly (2013) showed that this is the only conditional

[41] This assumption reflects closely the actual sampling process although it neglects social workers' assessment and exclusion of about 1/5 ineligible households from the initial random samples. We assume that exclusion is consistent across years and that the remaining experimental sample remains representative of the larger population of eligible single parents.

[42] For instance, consider the estimand δ of the difference in mean between the encouragement sample of size N_1 and control of size N_0. The total variance in the sample is given by the Neymann Variance for randomised experiment

$$V^{\text{total}}\left(N_1, N_0, n_1, n_0\right) = \operatorname{var}\left(\hat{\delta} \mid N_1, N_0\right) = \frac{S_1^2}{N_1} + \frac{S_0^2}{N_0} - \frac{S_\delta^2}{n_0+n_1}$$

with n_0 and n_1 the unknown size of the population and $S_.$ denotes the population variance of the outcome in each group $(0,1)$ and the variance of the treatment effect δ. For fixed N_0 and N_1, if $n_0, n_1 \to \infty$, the total variance and the sampling variance are equal:

$$\lim_{n_0,n_1,\infty} V^{\text{total}}\left(N_1, N_0, n_1, n_0\right) = \lim_{n_0,n_1,\infty} V^{\text{sampling}}\left(N_1, N_0, n_1, n_0\right) = \frac{S_1^2}{N_1} + \frac{S_0^2}{N_0}.$$

See Negi and Wooldridge (2021) and Alberto Abadie et al. (2020) for details.

[43] Stable unit treatment value assumption

independence required with one sided non-compliance. More precisely, under Hypothesis 0.2, the average treatment effect on the treated is identified by the ratio of the intention-to-treat on the first stage (Frölich and Melly 2013):

$$\begin{aligned} ATT = \mathbb{E}[Y^k(1) - Y^k(0)|D=1] &= \frac{\mathbb{E}[Y^k|Z=1,\mathbf{B}] - \mathbb{E}[Y^k|Z=0,\mathbf{B}]}{\mathbb{E}[D|Z=1,\mathbf{B}] - \mathbb{E}[D|Z=0,\mathbf{B}]} \\ &= \frac{1}{Pr(D=1)} \int \mathbb{E}[Y^k(1) - Y^k(0)|\mathbf{B}, D=1] Pr(D=1|\mathbf{B}) dF_B \end{aligned} \tag{4}$$

In words, the effect of the programme on the participants is, in expectation, the ratio of the weighted within-block difference in outcomes between encouragement and control over the within-block weighted difference in participation. The weights correspond to the distribution of blocks, but will typically depend on the estimation method and integration over periods.

Identification of the distribution of missing potential outcomes Under the same set of hypotheses, the average missing potential outcome for the treated is identified and, in fact, so is any measurable function $g(\cdot)$ of that potential outcome, as long as $g(\cdot)$ has finite first moment (Frölich and Melly 2013). Formally:

$$\mathbb{E}[g(Y^k(0))|D=1] = \frac{\int \mathbb{E}[g(Y^k(0))|\mathbf{B}=b, Z=0] dF_B(b) - \mathbb{E}[g(Y^k(0)) \times (1-D)]}{Pr(D=1)} \tag{5}$$

This result is particularly interesting in our setting. It means that any function of the *missing* potential outcome for treated compliers is identified. In particular, this equation allows identification of the entire marginal distribution of potential outcomes of treated compliers, had they not been treated.

First, note that under SUTVA and exclusion, the marginal distribution of potential outcomes for treated compliers is directly observed. Indeed, Since $Z = D$, under exclusion, Z is ignorable for compliers given $\mathbf{B}$ and $F_{Y_i|Z=1,D=1,\mathbf{B}} = F_{Y_i(1)|D=1,\mathbf{B}}$. What's missing is the counterfactual distribution $F_{Y_i(0)|D=1,\mathbf{B}}$, which is therefore also identified with this instrumental variable strategy. To alleviate notations, we denote $f_{Y^1|D^1}$ the marginal density of **treated compliers**, $f_{Y^0|D^1}$ the counterfactual density for **untreated compliers**. The marginal density of never-takers is $f_{Y^0|D^0}$.

By setting $g(Y_i(d)) = \mathbb{1}(d \times Y_i \leq y)$ for a constant y and each value of d, we obtain the complier cumulative distribution functions of $Y_i(1)$ and $Y_i(0)$ evaluated at y[44]. For instance, if we want to compute the probability that compliers had no labour income, had they not participated, we can define $g(\cdot) = (1-D_i)\mathbb{1}(Y_i \leq \varepsilon)$ with $\varepsilon \to 0$ and run a TSLS on $(1-D_i)$ instrumented by Z_i, with block fixed effects.

Since we want to look at the bunching around the *sweet spot* of labour incomes, we are more interested in the derivative of g $i.e.$ the density of compliers potential labour income. For that, we can set $g\left(Y_i(d)\right) = \frac{1}{h} K(\frac{Y_i - y}{h})$, where $K(\cdot)$ is some kernel function with bandwidth *h* that shrinks to zero asymptotically.

Abbadie's Kappa and weighted distribution There is a close connexion between this result and that of A. Abadie (2003) in the case with two-sided non-compliance. In this paper, Abadie shows that there are weighting representations of the LATE based on quantities he calls κ. Using these weights, one can identify the potential outcome distribution and derivatives for treated and untreated compliers. Alberto Abadie, Angrist, and Imbens (2002) further use these weights to define instrumental quantile treatment effects and implement this method on data from the JTPA, a randomised welfare-to-work programme in the US. We use the following Theorem as our main identification framework:

[44] A very intuitive presentation of this result can be found in the chapter on method for school effectiveness by J. Angrist, Hull, and Walters (2023). Heim (2024) uses this method to estimate the distribution of disposable income over the year after the programme ended and found no differences.

Theorem 0.1 (Weighted representation of expected functions of potential outcomes)**.** *Under Hypothesis 0.2, Frölich and Melly (2013) show that with one-sided non compliance:*

$$\begin{aligned}\mathbb{E}[g((Y(0)|D=1)] = & \frac{1}{Pr(D=1)}\mathbb{E}\Big[g(Y)\times(1-D)\frac{Pr(Z=1|\mathbf{B})-Z}{Pr(Z=1|\mathbf{B})\,(1-Pr(Z=1|\mathbf{B}))}\Big] \\ = & \frac{1}{Pr(D=1)}\mathbb{E}\Big[g(Y)\times(1-D)\frac{-\tilde{Z}}{q_b\,(1-q_b)}\Big]\end{aligned} \tag{6}$$

$$\begin{aligned}\mathbb{E}[g((Y(1)|D=1)] = & \frac{1}{Pr(D=1)}\mathbb{E}\Big[g(Y)\times(D)\frac{Z-Pr(Z=1|\mathbf{B})}{Pr(Z=1|\mathbf{B})\,(1-Pr(Z=1|\mathbf{B}))}\Big] \\ = & \frac{1}{Pr(D=1)}\mathbb{E}\Big[g(Y)\times(D)\frac{\tilde{Z}}{q_b\,(1-q_b)}\Big]\end{aligned} \tag{7}$$

The proof of Theorem 0.1 is given in Frölich and Melly (2013) and we simply adapt notations to our setting. The second lines of each equation simply replace $P(Z=1|\mathbf{B})$ by the instrument propensity score q_b previously defined. We use this theorem to estimate counterfactual densities of participants. Other recent work revisit the use of κ weights to estimate the LATE (Słoczyński, Uysal, and Wooldridge 2022). In this paper, attention is drawn to the different estimands for the weights which may be negative and not sum to unity. Then again, one-sided non-compliance have better properties than two-sided non-compliance and in particular, non-compliance ensures positive weights (Proposition 3.3 of Słoczyński, Uysal, and Wooldridge (2022)). In our application, we simply *plug* estimations of the instrument propensity score q_b to compute these weights (See Subsection V.2 below).

Comments: what two-stage least square estimates Consider the following simple TSLS system at any period after random assignment:

$$\begin{cases} Y_{ib} = & \mathbf{B}'\beta_b + \delta D_{ib} + \mu_{ib} \\ D_{ib} = & \mathbf{B}'\alpha_b + \pi\tilde{Z}_{ib} + \epsilon_{ib}\end{cases} \tag{8}$$

In this system, block x cohort instrument themselves in the second equation while participation is instrumented by the demeaned instrument. Because blocks are discrete and **B** contains indicators for each possible realisation, the specification is saturated in B. This is the typical TSLS of J. D. Angrist and Imbens (1995) but with centred instrument. As the recent work of Blandhol et al. (2022) shows, TSLS retrieves a LATE interpretation only in saturated specifications, such that the projection matrix of the first stage fits the conditional expectation. Note that the original result on TSLS and the LATE use a fully saturated model in the first stage $i.e.$ interacting the instrument with each set of dummy covariates. This amounts to integrating the covariate-specific LATE over the distribution of covariates with treatment-variance weighting. This "saturate and weight" specification is like nonparametric conditioning but uses only a single treatment variable (Blandhol et al. 2022; J. Angrist and Kolesár 2022). The problem with this specification is that it has many excluded instruments and is more sensitive to both small sample and many-instruments bias. However, Borusyak, Hull, and Jaravel (2022) show that centring on the propensity score recovers the same parameter, while also making explicit the modelling assumption of the first stage to estimate the propensity score q_b. They show that equation (8) identifies weighted averages of conditional-on-block IV coefficients[45]:

$$\begin{aligned}\delta = & \mathbb{E}\Big[\frac{\sigma_Z^2(\mathbf{B})\pi_b}{\mathbb{E}[\sigma_Z^2(\mathbf{B})\pi_b]}\delta_b\Big] \\ = & \int_b \frac{q_b(1-q_b)\pi_b}{\int_b q_b(1-q_b)\pi_b dF_B}\times\delta_b\, dF_B\end{aligned}$$

[45] See the chapter by J. Angrist, Hull, and Walters (2023) with a very clear presentation and application to school choice and the recent review by Borusyak, Berkeley, and Hull (2023) for more intuition on the theoretical results and review of other applications.

An important issue is the weighting of this TSLS that is proportional to the conditional variance of the instrument and the conditional first stage π_b. When propensity scores are constant (which is asymptotically our case), $\sigma^2_Z(\mathbf{B}) = \mathbb{E}[\sigma^2_Z(\mathbf{B})]$ and the β_b are weighted only by the conditional complier shares, yielding the unconditional LATE[46]. Note that from an identification perspective, block fixed effects are then unnecessary. We include them to improve precision in the second stage.

V.2 Estimating distributional effects

To assess the optimisation behaviours of treated compliers, we want to estimate their distribution of potential incomes had they not been treated and compare the bunching around the kinks of the French tax benefit. For that, we define a window of observation with balanced number of months across cohorts and consider the density of incomes over that period. To gain intuition and link our approach to the model presented in Subsection II.2 of our theoretical framework, we first present and estimate typical bunching estimators *à la* Saez, Slemrod, and Giertz (2012).

The usual bunching estimator Recall that the objective is to estimate the excess and missing mass of observations around the kink points of the budget set to measure the sensitivity of pre-tax income to variation of tax rate, with varying knowledge of the tax-benefit system between participants and non-participants.

In the bunching literature, one usually does not have comparison groups, and the counterfactual density needs to be estimated from the same observations (or comparing densities around a reform), usually using a parametric polynomial regression of the density.

We follow standard practices[47] and define bins H indexed over j with fixed width h set as 5% minimum wage around the bunching point X^*. The latter is at 60% minimum wage but we also run models at other kinks. We then estimate the following model:

$$C_j = \sum_{\upsilon=0}^{p} \beta_\upsilon \left(H_j\right)^\upsilon + \sum_{x=H_L}^{H_U} \gamma_x \mathbb{1}\left[H_j = x\right] + \sum_{r\in R} \rho_r \mathbb{1}\left[\frac{H_j}{r} \in \mathbb{N}\right] + \sum_{k\in K} \theta_k \mathbb{1}\left[H_j \in K \wedge H_j \notin [H_L, H_U]\right] + \mu_j \tag{9}$$

C_j is the observation count in bin j, p is the order of polynomial used to fit the counts, and H_L and H_U stand for the lower and upper bins that define the bunching region. We define these parameters by visually inspecting the bunching region to account for imprecise bunching. In practice, we use a degree-4 polynomial.

This specification corrects for round numbers R (defined by $r \in \mathbb{N}$), and for other fixed effects in a set K that feature a bunching mass in the estimation bandwidth outside the bunching range $z \in [H_L, H_U]$ but that are not associated with X^* (through the ρ and θ coefficient vectors respectively). In particular, weekdays are susceptible to create bunching mass every 20% of the minimum wage and there may be other bunching mass due to other kinks/notch or reference point. Not controlling for round number bunching can significantly bias the bunching estimate upwards since $X^* = 60\%$ is also a round number. This is because some of the observed bunching will be driven by factors unrelated to the change in incentives driven by the discrete change in the constraint that we want to attribute the bunching to. Similarly, not controlling for other bunching masses can exert a downward bias in the bunching estimate at X^* by biasing the counterfactual estimate upwards.

Given this estimation strategy, the predicted counterfactual density in the absence of the kink is given by:

$$\hat{B}_0 = \sum_{j=H_L}^{H_U} \left(C_j - \hat{C}_j\right)$$

[46] A very clear note on this transformation can be found on the web page of Peter Hull: https://about.peterhull.net/metrix

[47] We use the `Bunchit` package that directly implements the estimates *à là* R. Chetty et al. (2011) and Henrik J. Kleven and Waseem (2013).

To be able to compare bunching masses across different kinks or groups featuring varying heights of counterfactuals, we use a normalisation where we divide the total excess mass with the height of the counterfactual at X^*. The normalised mass is:

$$\hat{B} = \frac{\hat{B}_0}{\hat{C}_0}$$

In the model presented in Section II.2, this parameter corresponds to the following approximation:

$$B = \int_{X^*}^{X^*+\Delta X^*} h_0(z)dz \approx C_0(X^*)\Delta X^*$$

From there, we can estimate the elasticities using the value of the tax rate around the kink and pluging the density estimates in equation (2) presented in the theoretical framework. This approximation is only valid for small changes in the marginal tax rate so instead, we assume iso-elastic utility functions and retrieve the parametric observed elasticity with this simple expression[48]:

$$e = -\frac{\ln\left(1+\frac{\hat{B}}{C_0(X^*)X^*}\right)}{\ln\left(\frac{1-\tau_0}{1-\tau_1}\right)} \tag{10}$$

We estimate such models separately for the treated and untreated participants and by number of children over the post-treatment period and see descriptively how these distributions diverge and what the implied elasticities are. We use bootstrap to compute standard errors for the bunching mass and elasticities.

These models use parametric assumption and data from the same group to estimate the counterfactual density. Among participants, the bunching mass corresponds to optimising families with ability affected by the programme, higher knowledge and possibly lower adjustment costs. Conversely, non-participants contain twice the share of never-takers population and the untreated compliers. Using the identification results of Theorem 0.1, we can instead use the counterfactual density of untreated compliers to estimate these elasticites. We now define our preferred strategy to retrieve the counterfactual densities.

Local regression distribution Our main results are based on the so called *"local regression distribution estimator"* proposed by Cattaneo, Jansson, and Ma (2021) and implemented with the R package `lpdensity`. A very interesting feature of this estimator is that it is semi-parametric and data driven. It uses local polynomial regressions with MSE-optimal bandwidth. The authors also define point-wise and uniform confidence intervals using bootstrap the package implements. Another important advantage of this estimator is that it does not require preliminary smoothing of the data and hence avoids preliminary tuning parameter choices.

An important condition for estimation is that $F(\cdot)$ is suitably smooth near y. The function solves for parameters $\theta(\mathbf{y})$ using the local regression estimator:

$$\hat{\theta}(\mathbf{y}) = \underset{\theta}{\operatorname{argmin}} \sum_{i=1}^{n} W_i \left(\hat{F}_i - R_i'\theta\right)^2, \tag{11}$$

where $W_i = \frac{1}{h}K\left(\frac{y_i - y}{h}\right)$ for some kernel $K(\cdot)$ and some bandwidth h, $R_i = R(y_i - y)$, and

$$\hat{F}_i = \frac{1}{n}\sum_{j=1}^{n} \mathbb{1}(y_j \leq y_i)$$

[48] To get to this result, denote the ability level of the marginal buncher by a^*, his optimal earnings are $x^* + \Delta x^* = n^*(1-t_0)^e$ under the linear schedule and $x^* = a^*(1-t_1)^e$ under the kinked schedule. Combining these gives:

$$e = -\frac{\ln\left(1+\frac{\Delta X^*}{X^*}\right)}{\ln\left(\frac{1-t_0}{1-t_1}\right)}$$

is the empirical distribution function evaluated at y_i, with $\mathbb{1}(\cdot)$ denoting the indicator function. The generic formulation (11) is particularly interesting when $F(\cdot)$ is sufficiently smooth, in which case, the Taylor expansion gives:

$$F(y) \approx F(x) + f(x)(y-x) + \cdots + f^{(p-1)}(x)\frac{1}{p!}(x-x)^p \quad \text{ for } y \approx x, \tag{12}$$

and $f^{(s)}(x) = \frac{\mathrm{d}^s f(y)}{\mathrm{d}y^s}\Big|_{y=x}$ are higher-order density derivatives. The approximation (12) is of the form (11) with $R(u) = (1, u, \ldots, u^p/p!)'$, and hence $\theta(x) = \big(F(x), f(x), \ldots, f^{(p-1)}(x)\big)'$ (Cattaneo, Jansson, and Ma 2021). More importantly, their estimator also applies to the class of generic weighted distribution parameters

$$H(\mathbf{x}) = \mathbb{E}[w_i \mathbb{1}(x_i \leq \mathbf{x})]$$

where weights w_i can be used to retrieve the distribution of treated and untreated compliers using the results of Theorem 0.1. The weighted cumulative distribution function is defined similarly:

$$\hat{F}_{wi}(y) = \frac{1}{n}\sum_{j=1}^{n} \mathbb{1}(w_i y_j \leq y)$$

To estimate the distribution of potential income for treated compliers $i.e.$ $f_{Y^1|D^1}(y)$, or their unobservable potential outcomes $f_{Y(0)|D_1}(y)$, the distribution of compliers had they not been treated (what we call *untreated-compliers*), Theorem 0.1 shows that choosing $g(\cdot) = \hat{F}_{wi}(y)$ and using the κ-weighting schemes retrieve the expected density or cumulative CDF of potential outcomes. With a known design, we can simply plug the value of the instrument propensity scores $\hat{q}$ or centred instrument $\tilde{Z}_i$ in the formula. In their empirical application, Cattaneo, Jansson, and Ma (2021) do exactly that with data of the JTPA, a very similar setting. Formally:

$$\begin{cases} \kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)} \\ \kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)} \end{cases}$$

Joint visualisation of intensive and extensive margin reaction Finally, this method also has the advantage of allowing to rescale the density over a subset of the support of the outcome. This is interesting because we need the distribution to be bounded away from 0 to be estimated with this estimator, and we can estimate the mass point at 0 for the counterfactual distribution. For that, we need estimates of $\mathbb{E}[Y(0) = 0|D = 1]$ and $\mathbb{E}[Y(1) = 0|D = 1]$. The latter can be immediately computed while for the former, we can also use Theorem 0.1 and set $g(\cdot) = (1 - D_i)Y_i$. In practice we use a simple TSLS regression of $(1 - D_i)Y_i$ on $(1 - D_i)$ instrumented by $\tilde{Z}_i$ and block fixed effects. By scaling the weighted density over the support $1 - \mathbb{E}[Y(\cdot) = 0|D = 1]$ for each density, we can jointly observe the excess and missing densities over the rescaled support.

Remarks Our data and experimental design allow us to estimate potential outcomes densities corresponding to our treatment: the Reliance programme. It allows to retrieve the distribution of compliers had they not been treated and therefore, provides new measurements of the selection bias strongly emphasised in Heim (2024). For that, we can compare the density of never-takers $f_{Y^0|D^0}(y)$ directly observed in the encouraged group with the counterfactual density of untreated compliers $f_{Y(0)|D_1}(y)$. We can therefore see if without the treatment, compliers' income distribution differs from never-takers.

By comparing the density mass around the kink point for treated and untreated compliers and never-takers, we can measure how the programme affected optimisation behaviours and use these estimates as bunching estimators. The general idea is that without strong simplification such as those imposed in the model of Subsection II.2 of our theoretical framework, the observed bunching may confound the financial incentive effect with the reference point of part-time minimum jobs as well as optimisation friction and knowledge costs. If we are willing to make additional assumptions as to which parameter was affected by the programme and which remains constant in comparison of never-takers and untreated compliers, we could recover more structural parameters.

Finally, the comparison of the distribution of potential outcomes for individual and household income also shed light on the evolution of within-household income sources and the role of partners' income. We now briefly describe our estimation strategy for the outcomes on family structure.

V.3 Treatment effects on the treated

To assess the dynamic treatment effect of the programme on cohabitation and the number of children, we estimate the $ATTs$ defined in equation (4) for some periods $S(m)$. For that, our approach simply consists in stacking the TSLS systems of equations (8) for each period $S_s(m)$, with $s \in S$ by estimating the model over $S_s(m) \quad s.t.$ $m \in \{-12, \dots \mathcal{T} - max(c)\}$ interacting all right hand side elements with a set of S dummies for $S(m)$. Formally, we estimate:

$$\begin{cases} Y_{ibm} = & \sum_s \beta_{bs} \mathbf{B}' S_s(m) + \sum_s \delta_s D_{ib} S_s(m) + \mu_{ibm} \\ \sum_s D_{ib} S_s(m) = & \sum_s \alpha_{bs} \mathbf{B}' S_s(m) + \sum_s \pi_s \tilde{Z}_{ib} S_s(m) + \epsilon_{ibm} \end{cases} \tag{13}$$

In words, there are S first stages, one for each period s where participation D is instrumented by the corresponding interaction between the centred instrument and a period dummy. Fixed effects interacted with period dummies instrument themselves in the second stage and allow for block $\times$ period specific differences *between* blocks. As noted by Blandhol et al. (2022) "*nonparametric TSLS specifications that restrict attention to the population with $X = x$ and use a first stage that is saturated in Z will be both rich (trivially) and monotonicity-correct. Each value of x produces a different estimand $\beta_{TSLS}(x)$, each of which is weakly causal. Any positively weighted sum of $\beta_{TSLS}(x)$ across $x \in X$ will be weakly causal*".

The collection of $\hat{\delta}_s$ estimate the $ATTs$. If $m < 1$, the estimations are $placebo$ estimates and should be close to zero. We restrict such test above -12 as we can only observe the first cohort that many leads before random assignment. Similarly, we only use observations up to the maximum leads for the last cohort to have a balanced composition of groups over the periods.

The equivalent reduced form equation is:

$$Y_{ibm} = \sum_s \lambda_{bs} \mathbf{B}' S_s(m) + \sum_s \rho_s Z_{ib} S_s(m) + \upsilon_{ibm} \tag{14}$$

In which case, the collection of estimated parameters $\hat{\rho}_s$ are intention-to-treat effects at period p.

We use cluster-robust standard errors adjusted at the block level to account for possible heterogeneity within block following C. de Chaisemartin and Ramirez-Cuellar (2022). We also account for simultaneous inference by constructing a uniform 95% confidence interval for all $\hat{\delta}_s$ (resp. $\hat{\rho}_s$) using the Holm-Bonferroni correction (Hothorn, Bretz, and Westfall 2008).

Instrumented difference-in-differences The previous model is fully saturated and computes one coefficient per period $S_s(m)$. Alternatively, we could omit the coefficient corresponding to the period before random assignment and estimate an instrumented $event - study$ design. This model is just identified, while the interaction of block fixed effects with period dummies ensures clean comparisons. It assumes parallel trend in both the equivalent reduced form (ensured by random assignment) and the first stage. In our setting, once participants are assigned treatment, there is no switch-back, ensuring a constant share of treated units at every period. Under such settings, C. D. Chaisemartin and D'Haultfœuille (2017) show that the TSLS correspond to the LATE.

Heterogeneous treatment effects We often want to identify the effect of the programme on participants with different baseline characteristics; number of children in particular.

Because this variable is part of those defining blocks, the conditional treatment effects on participants is simply identified over the subset of blocks whose participants have the characteristic of interest.

To simultaneously estimate the models by groups of family size $\times$ period, we estimate the following equations by TSLS:

$$\begin{cases} Y_{ibm} = & \sum_s \beta_{bs} S_s(m) \mathbf{B}' + \sum_s \sum_f \delta_{sf} \mathbf{B_f}' D_{ib} S_s(m) + \mu_{ibm} \\ \mathbf{B_f}' D_{ib} S_s(m) = & \sum_s \alpha_{bs} \mathbf{B}' S_s(m) + \sum_s \sum_f \pi_{sb} \mathbf{B_f}' \tilde{Z}_{ib} S_s(m) + \epsilon_{ibm} \end{cases} \tag{15}$$

On the right hand sides, we still have block fixed effect interacted with each period. Number of children are part of these blocks. Then, we have a double interaction between treatment, number of children in the matrix $\mathbf{B^f}$ and either treatment dummies in the second stage, or the demeaned instrument in the first. This estimates one average treatment effect on the treated per family structure at period $S_s(m)$. To correct for simultaneous inference, we consider the uniform 95% confidence interval over periods, but within group of family size.

VI Optimisation of income sources and number of shares

As announced in the previous section, we first start by presenting bunching estimates comparing the distribution of individual labour incomes reported over the 12 months from 18 to 30 months after random assignment. Then, we present our main results comparing counterfactual densities using weighted regression distributions. While we always report results for the entire sample, most of the analyses focus on heterogeneous responses by number of children at baseline.

VI.1 Bunching estimates

In order to estimate observable elasticities and understand the magnitude of the change in implicit tax rate single parents face, we need tax-rate parameters. For that, we use the EDIFIS model and use rough average tax rate for calibration. Elasticities using bunching estimates rely on strong assumptions and we do not interpret them as structural parameters. However, they offer a simple metric for comparisons across groups. Table 1 presents the average marginal tax rate over (large) bins of household labour incomes by household size, relationship status and additionally, we include simulation when the partner earns either nothing (RSA) or a full-time minimum wage. The continuous implicit marginal tax rate is presented in Figure A.19 in the Appendix.

Aggregate implicit tax rates This table shows the sharp variations around the identified kink points (60 and 100% full-time minimum wage) and differences across family structures for different combinations of incomes. In particular, single parents with one or two children are more heavily taxed on almost every bins of the (low) earning distribution. Furthermore, their implicit tax rate is very high except the *sweet spot* hole. This comes from the fact that the PA has individual bonuses starting at .5 minimum wage, sharply reducing the implicit tax rate for dual-earners households from 50% to 100% MW. Above minimum wage, the IMTR increases for everyone, but is much higher for single parents or single-earner couples.
Single parents with three children have a rather decreasing marginal tax rate except the notch at 60% minimum wage and above where they receive no more PA. It is also striking to see that on average, the implicit marginal tax rate at the *sweet* spot decreases with number of children but is the same with or without a partner if he does not work. However, because of the individual bonus, it is even less taxing for the household to remain at the sweet spot with a partner with a full-time minimum wage.

Table 1: Average implicit marginal tax rate by family structure and earnings

N children	Relationship	Partner's income	Earned income bin							
			[0;40[	[40;49[	[50;60[	[60;80[	[80;100[	[100;110[	[110;120[	[120;150[
1	Single	No partner	39	66	24	46	46	74	55	39
	Couple	RSA	39	39	24	62	46	64	39	39
		SMIC	45	39	13	17	22	39	39	39
2	Single	No partner	39	71	19	42	43	70	67	49
	Couple	RSA	39	39	19	61	43	70	67	49
		SMIC	61	39	13	13	13	39	39	56
3	Single	No partner	49	39	13	38	31	26	24	26
	Couple	RSA	39	39	13	61	38	66	63	88
		SMIC	64	63	40	51	13	39	39	39
	Single	Average	42	59	19	42	40	57	49	38

Source: EDIFIS, 2022 legislation. Simulation based on children between 3 and 14, single parent receiving ASF and no other income. Mean implicit marginal tax rates in 2022 by family structure. All rates have been rounded to the unit. The last row averages rates for single mothers of 1, 2 or three children.

Parametric bunching estimates We present our estimates of the bunching densities of household pre-tax incomes around the 60% kink point for participants and non-participants in Figure 8. We also report the estimate of bunching at the full-time minimum wage that we comment after. All models use bins of 5% minimum wage and a 4-degree polynomial fit with dummies for round numbers every 20% minimum wage. The predicted counterfactuals are displayed in red. For the bunching around 60% minimum wage, we enlarge the bunching region to 2 bins on the left and 1 on the right, allowing imprecise bunching. These choices were made observing the plots, and means that we compute marginal bunchers from the region between 50 and 65 % minimum wage.

In the top panel, we observe the number of participants reporting incomes in each bin over the 12 months period, with massive bunching of observation in the *sweet spot*. Compared with the polynomial fit, there is a missing density before, another excessive density around 70-75% minimum wage then a rapidly fading mass of reported labour incomes. Conversely, the density of participants exhibits much lower and diffuse bunching mass around the *sweet spot*, but another bunching mass at full-time minimum wage. This is why we also present a model for the bunching mass among non-participants around the full-time minimum wage. For this model, we add another fixed effect at 60% minimum wage to capture the first density mass, 1 bins left and 2 bins on the right.

These estimates are similar to the kernel density presented in Figure 5 in Section IV.2, but use a parametric model to predict the counterfactual from the density of observations used in each model, excluding the bunching region.

Interpretation and important caveats It is striking how much labour incomes are missing above 75% minimum wage among participants. Compared with non-participants, most incomes above that threshold have been shifted to the bunching region. For non-participants however, the very different distribution needs to be discussed.

Before going into more economic interpretations, we should recall important caveats regarding these estimations and their underlying assumptions.

An important aspect of bunching estimate is that it requires structure on preference, ability or other parameters to be informative about the underlying structural parameters. This important result has been showed by Blomquist and Newey (2017). Without *a priori* knowledge of the elasticity, the size of the interval and the quality of the approximation are both unknown. The elasticity is sensitive to the shape of the underlying distribution and tuning the polynomial order and bunching window can lead to substantial variations in the estimations (Bertanha et al. 2023).

There are therefore three possible explanations for the shape of the distribution of income among non-participants. 1) higher friction costs ψ_i, 2) different knowledge and perception of the tax schedule θ_i and 3) differences in ability a_i.

Regarding point 1), we showed that the full-time minimum wage is the most heavily taxed for single parents which should create bunching below. However, the minimum wage sets the lowest wage rate and earnings can only be lower through a part-time job. This may create sparser sets of choices, over number of worked days for instance[49]. In the literature, most empirical estimates use discrete choice models both as a facilitating assumption for estimation but also because it better reflects the institutional setting in the labour market (Briard 2020).

Regarding point 2), part-time and full-time minimum wages are important reference points and the bunching mass may also reflect the institutional setting of the labour market, and how people and society value mothers at work (Maurin and Moschion 2009; Cassar and Meier 2018; Fleche, Lepinteur, and Powdthavee 2018; Cavapozzi, Francesconi, and Nicoletti 2021; Schmidt et al. 2023). However, it may also be that single mothers do not know about tax rate variations and make their decision as if the tax rate was linear. Liebman and Zeckhauser (2004) use the term "*schmedule*" and the action of "*schmeduling*" to designate economic agents' inaccurate perception of tax-schedule, causing two typical schmeduling behaviours: "*ironing*", i.e. making decisions based on the average price at the point where they consume, or "*spotlighting*", when agents identify and respond to immediate or local prices, and ignore the full schedule, even though future prices will be affected by current consumption. For instance, Rees-Jones and Taubinsky (2020) analyse these perceptions of tax schedule in the US and find that 43% of the population *irons*. Conversely, participants had training sessions and simulations and may be less likely to *schmedule* and be closer to frictionless optimisation. We discuss point 3) in the next paragraph.

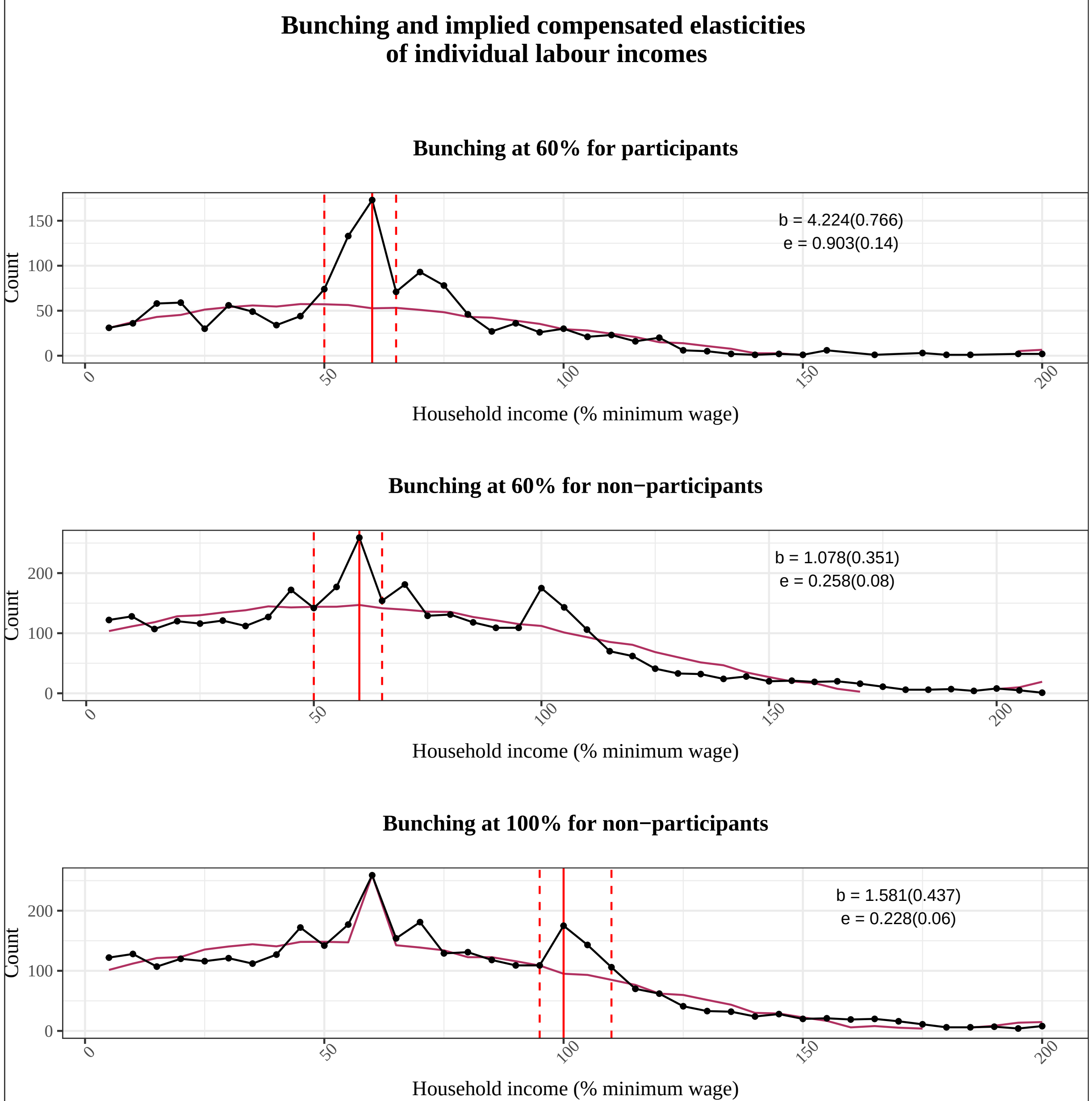
Implied Elasticities of labour incomes An important difference between participants and non-participants is the composition of their groups in terms of latent compliance type. The distribution of non-participants includes untreated compliers and twice the group of never-takers. These two populations may differ in terms of potential outcomes *i.e.* $f_{Y^0|D^1} \neq f_{Y^0|D^0}$. The distribution we observe and the counterfactual are a mixture of both groups, and is therefore not an appropriate counterfactual for treated compliers. However, this mixture of types may explain the second bunching at the minimum wage if we think never-takers and untreated compliers differ in term of *ability* a_i, as defined in the theoretical model. In this case, the prediction of this model is that individuals with higher ability chose a level of labour income higher than the kink region and are therefore insensitive to it. However, for those who would chose an optimal level of labour income slightly above the minimum wage in the absence of a kink, these can be the bunchers we observe[50].

If we are willing to make this different ability assumption and also that their utility is iso-elastic, then this bunching mass identifies the elasticity of individual income to the implicit tax rate for this particular group. We use the tax rates presented in the last row of Table 1 to compute the observed elasticities in both groups around the kink. For non-participants around full-time minimum wage, the observed elasticity is 0.23, which is rather low, and consistent with other estimates in the literature (Briard 2020; Bargain et al. 2014). Interestingly, we observe roughly the same elasticity when we use bunching at the 60% minimum wage kink (0.26) for this group.

[49] This is why we use dummies for round numbers every 20% of the minimum wage in the estimation of counterfactual densities

[50] This would be consistent with the result in Heim (2024) showing that compliers are more likely to among the lowest education group.

Figure 8: Large changes in the distribution of individual labour incomes



Sources: ALLSTAT 2017−01−01 to 2023−06−01 . Observations from 18 to 30 months after random assignment.
The database is collapsed into frequencies of individual labour incomes falling into bins of 5% minimum wage.
The statistical units are individual x months with positive labour incomes.
The top panel is from sub−sample of participants, the bottom panels are from the control group and never−takers.
Counterfactual densities estimated using polynomial of degree 4 , with round number dummies for every 20% SMIC corresponding to round number of days worked.
The bunching regions are delimited by the dashed red lines and we use the average IMTR for single parents with 1,2,3 chidren presented in the table at the begining of the section.
Elasticities use parametric restriction and standard errors are computed by bootstrap using 499 replications.

The elasticity that we compute with the same structural hypotheses for participants is however much higher. For the latter, the marginal buncher is located at 81% of the minimum wage, and the implied elasticity is 0.9, 3.5 times higher than the elasticity for non-participants at this kink point. The marginal buncher for the latter is located at 65% of the minimum wage.

Figure E.30 in the Appendix compares the bunching estimates by encouragement status and an estimate for never-takers, which reveal interesting results. Never-takers exhibit a rather sharp bunching at the kink point on an otherwise very rather uniform distribution until 110% minimum wage, where the density sharply decreases. Compared with the density in the control group – in witch they represent 61% of households –, these two figures suggest that untreated compliers would have been the one to bunch on the full-time minimum wage. The weight of never-takers in the control group explain most of the bunching mass at the 60% kink point observed in the control group. However, never-takers do not bunch at the full-time minimum wage.

We complete this picture by estimating the model separately by number of children at baseline and report the results in Figure E.31 in the Appendix. There are three important results we should note.

1) The entire excess mass among non-participants around full-time minimum wage comes from single parents who had one child at the time of random assignment.
2) Non participants with two children bunch sharply at part-time minimum wage.
3) Among participants, the bunching concerns every groups although there are far fewer parents of three with positive labour incomes (in both groups).

These first results confirm the sharp differences observed in the descriptive statistics and provide first estimates of the labour elasticities between participants and non-participants. Without the programme, single parents on welfare seem pretty un-reactive to incentives of the tax-benefit system. However, the counterfactual densities are estimated using parametric assumptions within these groups so their interpretations rely on strong hypotheses. We now present our semi-parametric approach to estimate the counterfactual densities of untreated compliers.

VI.2 The distribution of potential outcomes of treated and untreated compliers

To estimate the densities for treated and untreated compliers, we use the new method of weighted and rescaled regression distribution of Cattaneo, Jansson, and Ma (2021) presented in section V.

To start, we measure the bunching mass at 0 income for treated and untreated compliers by TSLS and report these estimates in Table 2. Each column is a separate outcome and we estimate jointly the potential outcomes by number of children using stacked regressions. First, the more children they have, the less likely they are to report labour incomes of their own. Second, as already shown by Heim (2024), the programme has a negative effect at the extensive margin for single parents of one child: 76% of participants reported no individual labour income over the year, but they would have been 61% had they not participated. These significant differences are also seen on household labour and individual incomes. However, we see no significant difference in potential null income for parents with more children.

On average, about half of single parents with one or two child(ren) reported some individual incomes over the year, and between 2/3 and 3/4 did not report any labour income. We use these estimates to rescale the densities estimated over positive incomes and use the weighted local distribution regression using the estimator of Cattaneo, Jansson, and Ma (2021) presented in Section V. We report estimates for individual labour incomes in the body of the paper and provide estimations for other outcomes in Appendix F.

Table 2: Average potential probability of 0 income over 30 months

	Labour income		**Pre-tax income**	
	Individual	Household	Individual	Household
1(Y(0)=0):Children=1	0.614	0.591	0.446	0.449
	[0.526, 0.702]	[0.496, 0.685]	[0.330, 0.561]	[0.320, 0.577]
1(Y(0)=0):Children=2	0.739	0.711	0.511	0.447
	[0.622, 0.856]	[0.592, 0.829]	[0.261, 0.760]	[0.223, 0.671]
1(Y(0)=0):Children=3+	0.896	0.728	0.701	0.556
	[0.775, 1.017]	[0.604, 0.852]	[0.529, 0.874]	[0.400, 0.713]
R2	0.497	0.407	0.293	0.253
R2 Adj.	0.496	0.407	0.292	0.252
1(Y(1)=0):Children=1	0.759	0.714	0.569	0.537
	[0.706, 0.812]	[0.661, 0.767]	[0.486, 0.652]	[0.454, 0.620]
1(Y(1)=0):Children=2	0.721	0.676	0.488	0.450
	[0.661, 0.780]	[0.610, 0.742]	[0.422, 0.554]	[0.387, 0.513]
1(Y(1)=0):Children=3+	0.844	0.777	0.606	0.537
	[0.799, 0.889]	[0.723, 0.830]	[0.539, 0.674]	[0.468, 0.607]
R2	0.742	0.686	0.521	0.477
R2 Adj.	0.742	0.685	0.520	0.476
Num.Obs.	48473	48453	48471	48470

Notes: TSLS regressions of the probability of 0 income over 30 months for compliers, with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re-centred instrument and block fixed effect, with T = D for Y(1) and T=(1-D) for Y(0). Models estimated jointly by number of children.
We use cluter robust standard error adjusted by block to construct point-wise 95% confidence intervals.

Densities of compliers' individual labour incomes We start by the estimation of the potential individual labour incomes for treated and untreated and compliers. We use data over the same post-treatment period and use κ weights to recover the counterfactual densities. Results are presented in Figure 9. In the three panels by number of children; we also added the shape of the in-work benefits stacked over the housing benefits. These have been rescaled to fit the density plots and have no vertical units. They only illustrate the variations of social transfers and marginal tax rate.

Consistent with our previous observations, the estimated densities of treated compliers (blue) show important bunching masses at the *sweet spot*. However, these estimates show that the counterfactual distribution (pink) presents no bunching at all, for any group. Instead, the counterfactual densities are mostly uniform up to the full-time minimum wage, from where they rapidly drop.

The fourth panel shows the aggregate estimates. These results can be compared with the previous bunching estimations with, this time, a more robust causal interpretation. First, there is no difference in density before 50% minimum wage. Then, we have roughly twice as much observations in the sweet spot than in the counterfactual and a missing density from 75% MW until the end.

These estimates show that the programme had strong effects at the intensive margin The most important effect is driven by single parents of two children at baseline. For them, the counterfactual distribution shows no sign of optimisation and is essentially linearly decreasing over the support of the distribution. The estimates for single parents of one child suggest that the programme moved those who would have worked full-time to work part-time, and lowered participation among those who would have worked fewer hours. As for parents of three or more children, we observe higher densities of labour income until the exit point of in-work benefits then a rather linear slope following incentives of the housing benefits.

The illustration of the shape of the in-work benefits over that of housing benefits may help interpreting why working part-time seems to attract participants so much: it also corresponds to the point before they start losing housing benefits. The latter play an important role in these households' budget and loss-aversion may drive part of these behaviours (Fehr and Goette 2007).

Implied elasticities Like in the previous model, we can use the difference in density mass between treated and untreated compliers to estimate the elasticity of labour income that the treatment induced.

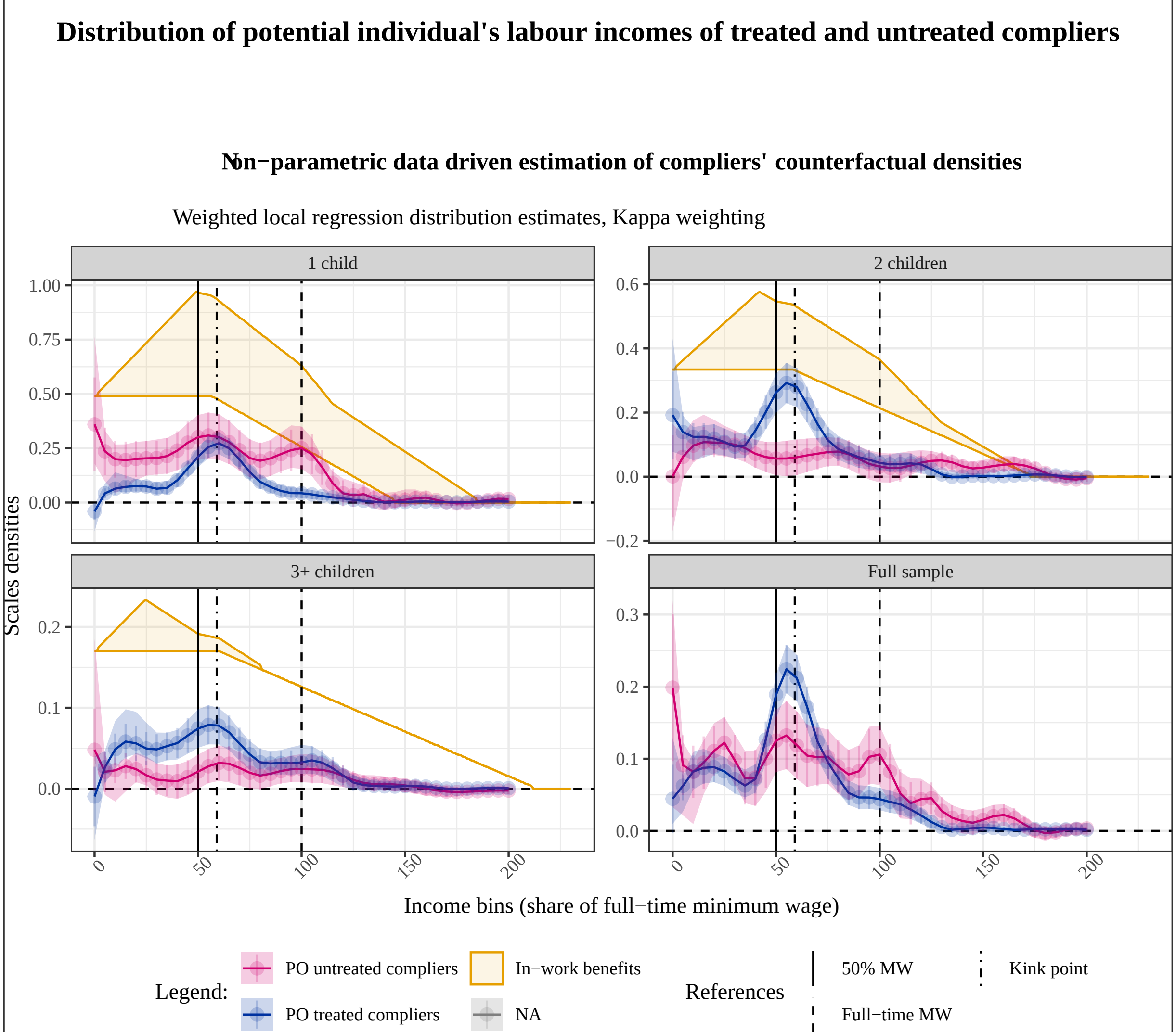
We define the bunching zone as the bins where the joint upper confidence interval of $f_{Y^0|D^1}$ exclude the point estimate of $f_{Y^1|D^1}$ and integrates the distribution over this part of the distribution to recover $\hat{B} = \int_h^{h+\Delta} \frac{f_{Y^1|D^1}(h) - f_{Y^0|D^1}(h)}{f_{Y^0|D^1}(h)} dh \approx 2.63$, with Y^* the 60% minimum wage kink point. With the average change in tax rate of $dt = 23$ pp, the elasticity can then be approximated as:

$$\hat{e} \approx \log\left(1 + \frac{\hat{B}}{Y^*}\right) / \log\left(1 - \left(\frac{dt}{1 - \tau_a}\right)\right)$$

Which gives $\hat{e} \approx 2.05$, an elasticity 2.3 times higher than the one found earlier with parametric regressions. This new estimate still relies on strong functional form assumptions. However, they use experimental variations and compare the distribution of potential outcomes in the absence of the programme. The difference in bunching mass has a clear causal interpretation.

It is important to understand that the underlying model assumes that the distribution of income is shaped by individual ability a_i, knowledge θ_i and monetised psychological costs ψ_i. In the frictionless model, if the elasticity ε is zero, then the distribution of earnings coincides with the distribution of ability. As ε rises, or if θ_i is shifted to 1 representing *perfect* knowledge, individuals become more sensitive to the tax rate and react by lowering earnings (Bertanha et al. 2023). As a result, the distribution of log earnings becomes a left-shifted version of the distribution of ability. The larger shifts are a consequence of both high tax rate variation and higher awareness of such changes.

Figure 9: Counterfactual densities of compliers' own labour incomes



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
The dependent variable is the individual's labour income as share of full−time minimum wage.
The shaded area above the estimates is the scaled amount of in−work benefits with housing benefits adjusting.
These data are simulated using EDIFIS and rescaled for each panel using the width of the density support.
Notes: Estimations of the potential density functions for compliers following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 3, simultaneous 95% confidence intervals use polynomial of order 4, 1 degree higher for biais correction.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re−centred instrument and block fixed effect, with T = D for Y(1) and T=(1−D) for Y(0).

Weights are computed for each sub−group separately: $\kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)}$ $\kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)}$

The rapid learning and bunching To better understand what changed for treated compliers, we look at the evolution of this density over time, estimating the same model over periods of 6 months. This gives us more plausible proofs that the programme created the bunching mass through reduced optimisation friction and learning of the tax-benefit system We start with the 6 months before random assignment and from 0 to 30 months after and plot the results in Figure 10.

This figure shows that the optimisation really stems from the last part of the programme. Before random assignment, distributions overlap and reach 0 before the full-time minimum wage. The recruiting period lasted up to 6 months and during that time, compliers *miss* full-time jobs and we see the lock-in effect starting to show-up on wages of untreated compliers. Heim (2024) showed that the lock-in effect maxed at about 9 months from random assignment. In the window from 6 to 12 months, the counterfactual density gets bigger and flatter while the treated compliers are still barely reporting any income.

In the last 6 months of the programme, almost every new added mass among those with positive labour income bunch at kink point. This bunching mass remains for the next three panels, while the counterfactual density is almost uniform up to the full-time minimum wage. This suggests that some untreated compliers may start part-time jobs then move to full-time, a pattern we do not observe among treated compliers; at least up to this point.

Comparison of untreated compliers with never-takers Since compliers and never-takers may differ in terms of potential outcomes, it is interesting to compare the counterfactual density of untreated compliers $f_{Y^0|D^1}$ with that of never-takers $f_{Y^0|D^0}$. The latter can be directly estimated from observations in the encouragement groups. We estimate these models over the 6 periods to see how these distributions evolve over time and present these results in Figure F.36 in the Appendix.

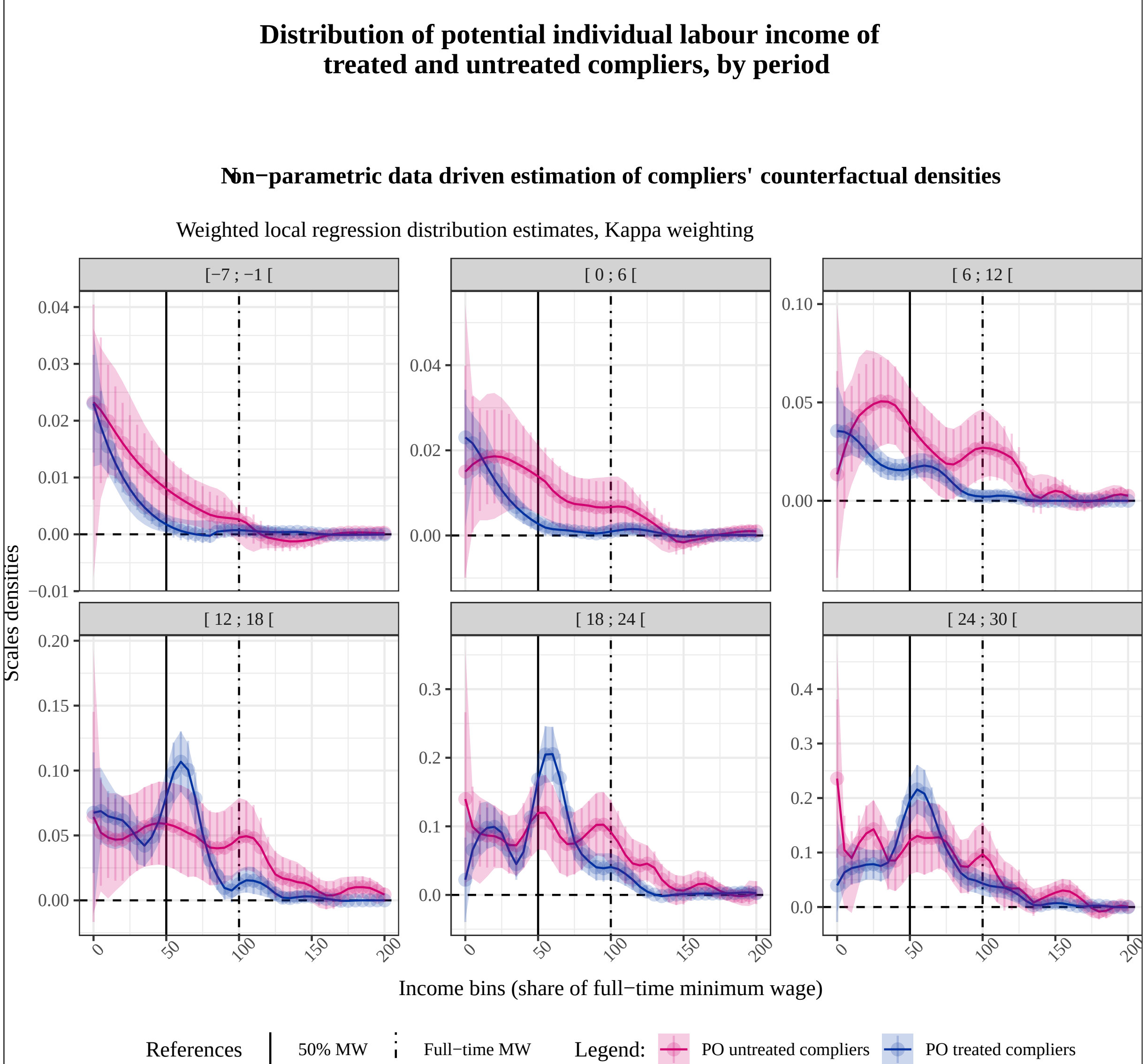
The results show that the potential distributions are very similar in the first two periods, then the lower job finding rates of never-takers leave the distribution lower than that of untreated compliers. However, they do not exhibit much bunching either. While these averages over number of children may still hide differences, they support the idea that in the absence of the programme, households do not optimise much.

Densities of household's labour income for treated and untreated compliers We run similar estimates for household incomes and report them in the Appendix. Figure F.34 uses household's labour income over the same period. Since these incomes include those of spouses, we do not present the shape of in-work benefits for clarity. However, this is the main driver of the amount of in-work benefits.

Unsurprisingly, these estimates are very close to the individual labour incomes, as most remain single parents. The most important difference concerns parents of three or more children. Indeed, the two distributions of treated and untreated compliers largely overlap, while individual incomes were higher among treated compliers in Figure 9. For households with three or more children, the programme shifted the source of labour incomes from fathers to mothers. The reported incomes are however significantly lower on the higher part of the distribution. It is also worth noting the shape of the counterfactual density around the kink for parents of two, which exhibits a hole. This further confirms the observation made earlier that untreated compliers would have mostly chosen full-time jobs and not part-time jobs.

We reproduce this analysis over 6 months period to show the evolution of potential household's labour like individual income. The results are presented in Figure F.35 in the Appendix. Results are very similar with one main difference: in the last period, we see the bunching mass dissipate and slightly thicker densities up-to 1.2 full-time minimum income suggesting spouse incomes.

Figure 10: Evolution of potential individual's labour income by 6 month periods



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
The dependent variable is the monthly individual's labour income as share of full−time minimum wage.
Notes: Estimations of the potential density functions for compliers following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 3 , simultaneous 95% confidence intervals use polynomial of order 4 , 1 degree higher for biais correction.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re−centred instrument and block fixed effect, with T = D for Y(1) and T=(1−D) for Y(0).

Weights are computed for each sub−group separately: $\kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)}$ $\kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)}$

Additional estimates In Figure F.32 in the Appendix, we present the estimates on individual pre-tax income using the same method. This outcome includes labour incomes and other non-labour incomes parents report; typically child support or unemployment insurance. For single parents with one child, the programme shifted the entire distribution to the left and participants are also less likely to report small non-labour incomes contrary to the counterfactual. The opposite is true for single parents of three children or more who are more likely to report low levels of income compared with the counterfactual. The estimations for single parents of two essentially overlaps except for a small difference at the sweet spot.

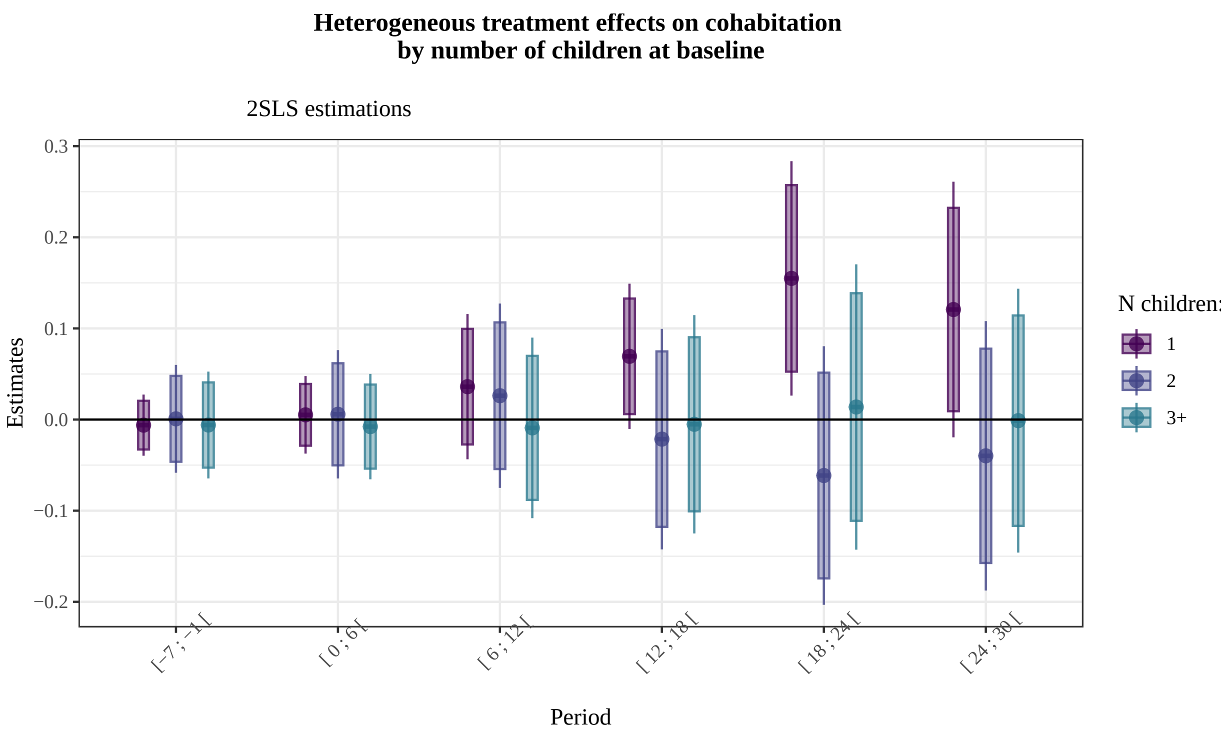
This suggest that untreated-compliers have different source of income, often non labour ones - that treated compliers do not have. The programme dramatically changed the distribution of incomes and shifted them to part-time jobs around the end of the programme. We now look at the effects of the programme on family structure.

VI.3 The effects of the programme on family structure

In this subsection, we estimate the dynamic average treatment effects on the treated by number of children at baseline on the two main outcomes for family size: cohabitation and number of children.

Fewer single mothers with one child at baseline Figure 11 reports the estimate of the instrumental variable estimations of the effect of the programme on the probability of cohabitation with a partner by number of children at baseline following the methodology described in V. These results confirm our hypotheses: the programme dramatically increased the probability of cohabitation for single mothers with one child. In order of magnitude, the TSLS coefficient over the year after the end of the programme represents twice the average of the control group. The other groups of parents are not affected by the programme. Note that the fading effects in the end is not due to more break-ups but a catching-up in the control group, as we noticed in the results of Figure 7.

Figure 11: Average effect of the programme on cohabitation by number of children at baseline

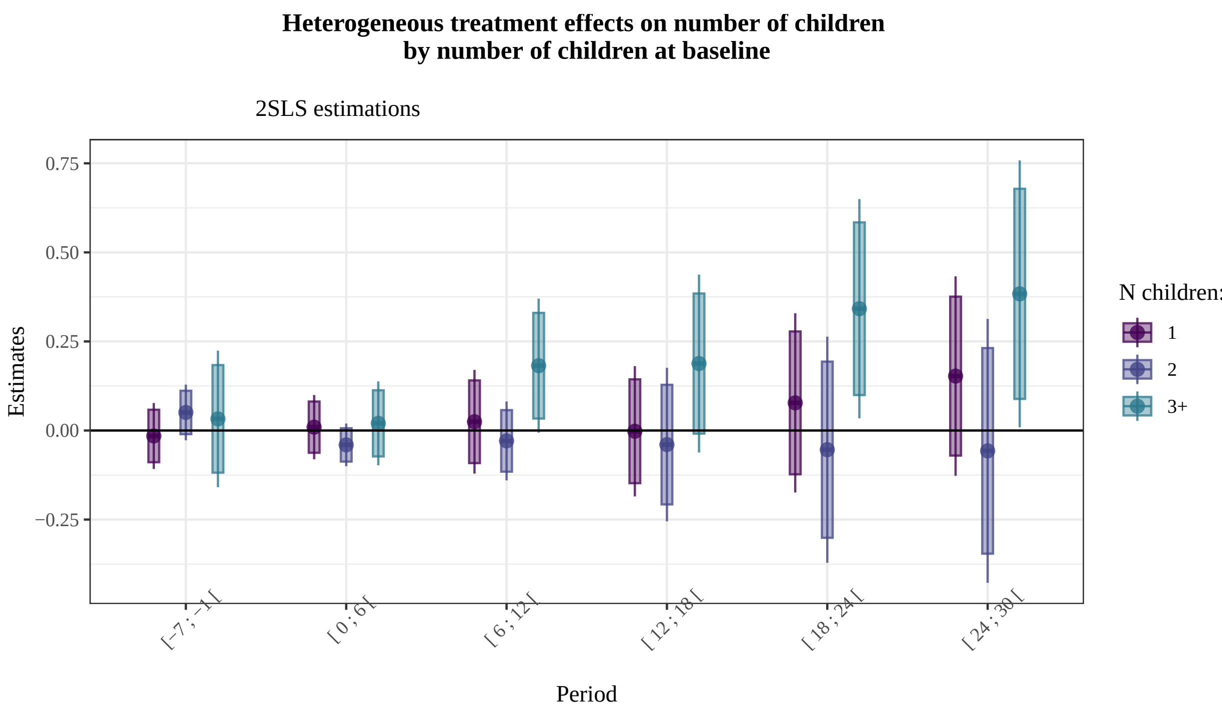


Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021.
Notes: The dependent variable is the number of dependent children.
Cluster−robust standard errors at the block x cohort level.
− Error bars indicate 95 % pointwise confidence intervals and extended lines account for FWER by subgroup.
− All models include blocks x cohort x relative months fixed effects and instrument propensity score weighting.

These results have to be interpreted with a few caveats in mind. First, the Family allowance fund uses a definition of cohabitation no other public institution formally use. It is different from what the civil code defines and what the National statistic agency uses for households. As discussed in Section III.1, recipients must report any change as soon as it occurs, in spite of a very broad definition. We cannot rule out the possibility that the programme increased compliers' reporting of cohabitation - either because of strategic incentives or reinforced fears of punishment - and not *de facto* cohabitation. However, these results are consistent with the incentives we discussed and either show better optimisation or more institutionalised romantic relationships.

Changes in the number of dependent children Similarly, we estimate the effect of the programme on the number of dependent children using the same method and present the results in Figure 12. In this plot, we use the definition of dependent children that reflect the RSA and PA updates and present the same estimates with the two alternative measurements in Figure G.38.

Figure 12: Relative increase in the number of children for parents of three or more



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021.
Notes: The dependent variable is the long difference between the number of children under responsibility at month m and the number of children the month before random assignment.
Cluster−robust standard errors at the block x cohort level.
− Error bars indicate 95 % pointwise confidence intervals and extended lines account for FWER by subgroup.
− All models include blocks x cohort x relative months fixed effects and inverse instrument−propensity score weighting.

These estimates show no effect of the programme on the number of children for parents of one and two children but a very large increase among those who had 3 or more children at baseline. The magnitude of these estimates implies that among this group, almost half of treated compliers have one more dependent children than untreated compliers. While the results with the measure using the definition for family benefits is very close to this one, there is a striking difference with measurement difference based on the definition used in housing benefits (Figure G.38). The number of children in the house sharply increases for those with three children or more from the middle of the programme and continues to increase after.

However, these results do not come from new births but from fewer older children leaving the household. Figure G.37 in the Appendix estimates the same models but instead of splitting the samples by number of children, we estimate separate models by quartile of the age of the oldest children at baseline. The 4rth quartile roughly corresponds to children aged 16 or more at the time of random assignment. The coefficients for this quartile roughly equals the estimates for parents of 3 children, showing that this is the underlying mechanism. For the others, there is no sign of any change in the number of dependent children.

Beyond the presentation of these results and the discussion of their effects on social transfers, it is hard to further interpret what they imply. For sure, they are part of the constrained optimisation problem and may be a way to limit benefit loss. At the same time, de-cohabitation of older children requires some level of autonomy and delayed departure could also reflect changes in higher education or other important life decisions. These data are simply unfit to make further inference.

VII Aggregate effects and additional mechanisms

We run a series of additional estimations to further understand the optimisation we observe and its consequences. First, we analyse the treatment effects of the programme on cumulative labour incomes. To do that, we look at the quantile treatment effect on the annual labour income. Then, we leverage additional variations from the timing of job re-entry to measure the effect of first job re-entry for treated and untreated compliers.

VII.1 Quantile intention-to-treat effects

Our estimation strategy uses **yearly** individual and household labour incomes as dependent variables. As a first step, we estimate the individual sum of incomes over the 12 months after the end of training[51] and define Y_{ib}^{a} where a is either *individual labour incomes* or *household labour incomes*.

Let $F_{Y^a|Z=1}$ and $F_{Y^a|Z=0}$ denote the distributions of Y^a conditional on being in the encouragement or group, respectively. Then the quantile intention-to-treat effect $(\text{QITT})3$ is defined as

$$\text{QITT}(\tau) = F^{-1}_{Y^a|Z=1}(\tau) - F^{-1}_{Y^a|Z=0}(\tau)$$

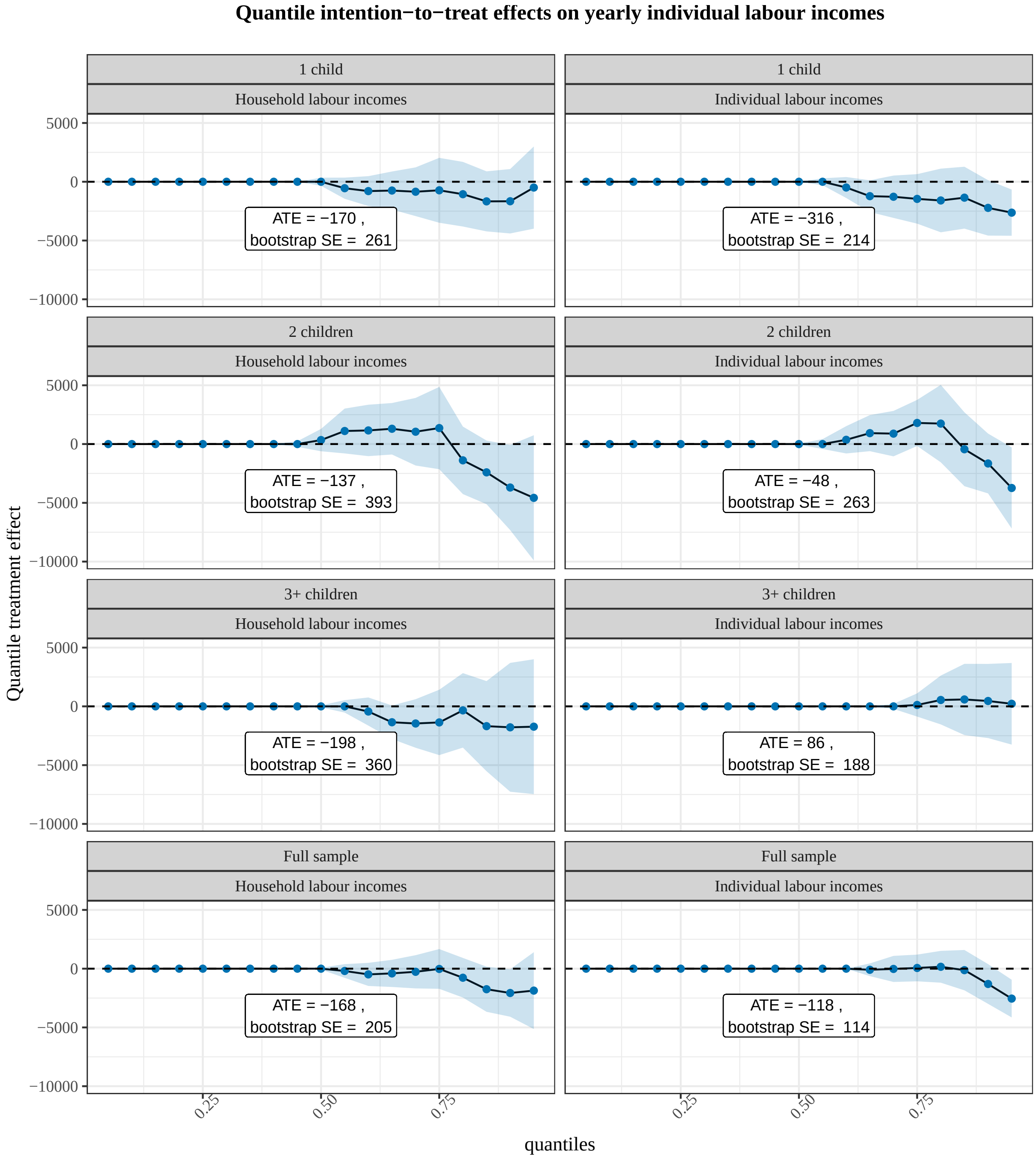
We use the estimator proposed by Firpo (2007) and implemented in the R package `rqte`. This method uses inverse propensity weighting of the distributions and bootstrap standard errors.

Figure 13 presents the estimates on the yearly labour income, separately by number of children and for individual an household incomes. Both sets of estimates confirm the previous results, namely that households and individual labour incomes were reduced for single mothers at the top of the income distribution by choosing part-time minimum wage jobs. However, these results bring new insights on the heterogeneity of the responses. Single parents with one child at the top of the income distribution have much lower total individual labour incomes, but the result is less pronounced for their households' incomes, their partners' wages compensating their lower individual incomes. Single parents of two children have pretty much the same quantile treatment effects on their households and individual labour incomes, showing that they are more likely to be the only income in the household. Single parents of three or more children have no significant quantile treatment effects.

We also estimate the same model on individual and household income, including non-labour incomes and report the results in Figure G.40 in the Appendix. They confirm the negative effects at the top of the distribution for parents of 2 and three children, and individual income of those with one child at baseline. However, there is no difference in household income for that group, showing the mitigating effect of increased incomes through cohabitation.

[51] We use $m \in [18:30]$

Figure 13: Quantile treatment effects on cumulative individual labour incomes over the year after the end of the programme



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
Notes: Estimations of the quantile intention−to−treat effect controling for blocks x cohort by inverse−propensity score weightingfollowing Firpo (2007).95% Confidence intervals estimated using bootstrap.

VII.2 The differential effect of job re-entry for treated and untreated compliers

Throughout the paper, we presented participants as better optimiser, strongly reacting to the incentives they learned. The underlying model assumes that they optimise disposable income after tax. The next question is: how does this optimisation affect disposable income per capita ? For that, we then again use the features of our experimental setting to uncover more important causal parameters under minimal additional assumptions. More precisely, we want to estimate how *different* is the effect of job re-entry on disposable income per capita for compliers, had they not participated.

Intuitions So far, we used the variations from individual shocks $\tilde{Z}$ to measure the effect of the programme D on outcomes under 0.1 and 0.2. These two minimal hypotheses ensured by the experimental design could retrieve the full distribution of potential outcomes for compliers aggregating over cohorts. In particular, we can identify $\mathbb{E}[W_{im}(1)|D(1), m]$ and $\mathbb{E}[W_{im}(0)|D(1), m]$, where $W = \mathbb{1}(\text{labour incomes} > 0)$, and the difference between the two is the causal effect of the programme on employment (Heim 2024).

We can introduce a new variable G to denote the month at which household i experience the event "first job re-entry" at month m:

$$G_i = \mathbb{1}(W_{im} = 1, \sum_{m<g} W_{im} = 0) \cdot m$$

Then, $G = g$ indicates that an individual gets their first job re-entry at month $m = g$. Let $F_G(g) = Pr(G_i < g)$ be the cumulative distribution function (CDF) of the timing of first job re-entry. Similarly, let $e = m - g$ denote the timing-of-event.

The main issue is that G is endogenous and censored as many individualq never switch and early switchers may be different from late switchers. For instance, first job re-entry may select different single parents at the early dates than at the end because of unobservable individual endowment, efforts, assortative matching on the labour market and so on. For instance, Figure D.26 in the Appendix presents the distributions of groups G, in number and proportion. We see that the distribution of first job re-entry is downward sloping with a kink around 15 months, $i.e$ roughly the end of the programme. The training period is associated with an increase of first job re-entry among participants which fade out afterward. This creates variations in the *share* of treated compliers among switchers between groups over time.

However, all these problems may actually be solved using the shifts in the share of compliers across groups G, and using the recent results of (Borusyak, Jaravel, and Spiess 2022) on re-centred instruments for *shocks* affecting groups G: the *level* variables. For that, we need an additional exclusion restriction that is: $G(d, z) = G(d)$, the month of first job re-entry does not depend on the instrument beyond participation and can be excluded from potential outcomes. This hypothesis allows us to consider the timing-of-event as an effect of the programme that we can instrument. Since G is a time variable, this additional exclusion implies a parallel-trend assumption, which we can informally assess with estimates on leads of first job re-entry.

With this additional hypothesis, we can recover the potential incomes of treated and untreated compliers around first job re-entry by using i) variations over time of ii) variations of the mass of job re-entry instrumented by iii) variations over time of iv) the mass of compliers.

To gain intuition, first note that under SUTVA and one-sided non compliance, $Y_i = Y_i(1) \quad \forall D_i = 1$ and $\mathbb{E}[Y_i(1)|W_i, e_i = m - g_i, D_i = 1]$ is observed and can be estimated. For instance, we can regress Y on event-time dummies and no constant on the sub-sample of participant and a balanced window of observation around the event. This regression integrates over individuals and groups g observed at month $m = g + e$. This is also true when $e = -1$ in which case $W_i = 0$ and the difference between any month e and $e = -1$. Denote $\Delta_e Y_w(1)$ this long difference:

$$\Delta_e Y_w(1) = \mathbb{E}[Y_i(1)|W_i = 1, e = m - g_i, D_i = 1] - \mathbb{E}[Y_i(1)|W_i = 0, e = g_i - 1, D_i = 1]$$

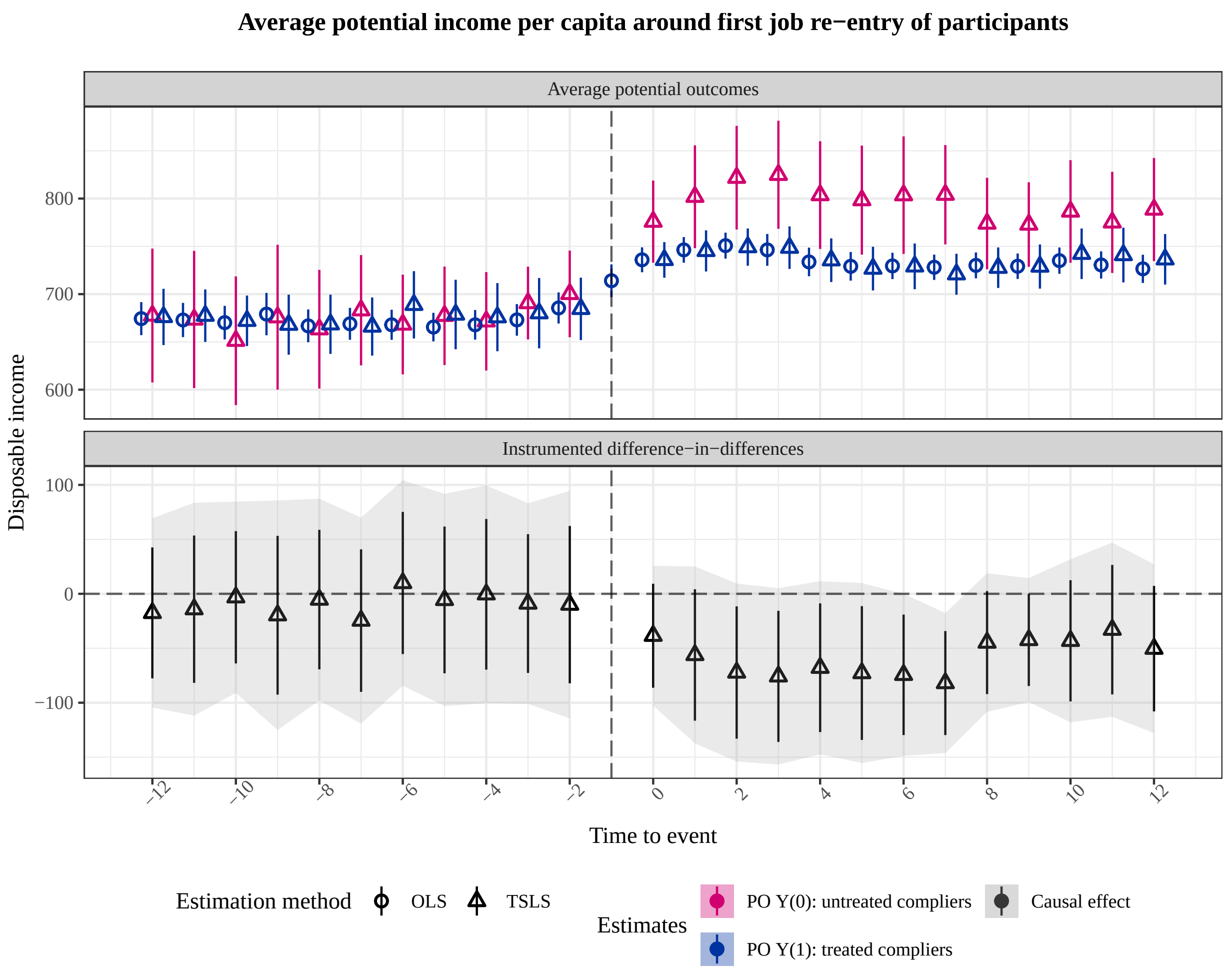
Again, these quantities could be estimated in an event study. However, we can also retrieve the means or their difference using our identification results from Theorem 0.1 and estimate a TSLS event study of $D_i Y_{im}$ on relative-event dummies interacted with D_i and block x month fixed effects, and a first stage of each event-dummy-treatment variables on event-dummy-centred instruments. By replacing D_i by $(1 - D_i)$ in the same equation, we can also retrieve the average potential outcome or average long difference for untreated compliers. Formally:

$$\begin{cases} g(T \cdot Y_{ibm}) = \beta_{bm} \mathbf{B}' S_m(m) + \sum_{e \neq -1} \delta_e (T_i \cdot \mathbb{1}(m = e) + \varepsilon_{ibm} \\ \sum_{e \neq -1} T_i \cdot \mathbb{1}(m = e) = \alpha_{bm} \mathbf{B}' S_m(m) + \sum_{e \neq -1} \tilde{Z}_i \cdot \mathbb{1}(m = e) + \upsilon_{ibm} \end{cases} \tag{16}$$

With $T_i = D_i$ to recover potential outcomes of treated compliers, $T_i = (1 - D_i)$ for the untreated compliers.

Each first stage projects the probability of being e months from first re-employment for those with T=1 conditional on the variation in the probability of being e months due to Z conditional on block and month since random assignment. This is where the re-centred instrument makes this TSLS in the framework of Borusyak, Jaravel, and Spiess (2022) and is central for our results to hold. This model uses the variations in the timing of job re-entry caused by the programme - through the lock-in in particular. We present these estimates in the top panel of Figure 14. We also display the result of the OLS regression as a validation test. As expected, the two methods yield almost identical estimates. The only difference is precision as the TSLS uses only part of the variation while OLS uses all variations on a restricted sample.

Figure 14: Lower potential incomes for treated compliers after job re-entry



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021 from 0 to 30 months from random assignment.
Notes: The event W is the first month with positive labour incomes.
IV models for potential outcomes use DY (resp. (1−D)Y) on D (resp. (1−D)) interacted with event−time dummies.
The latter are instrumented by event−time dummies interacted with centred encouragement, with block x month fixed effects instrumenting themselves in the second stage.
The OLS model regress Y on event−time dummies without constant among the sub−sample of participants.
Event−time dummies omit the first month of the window and the month before the event.
95% pointwise Confidence intervals based on cluster robust standard errors adjusted at the block level

From there, we are therefore able to identify:

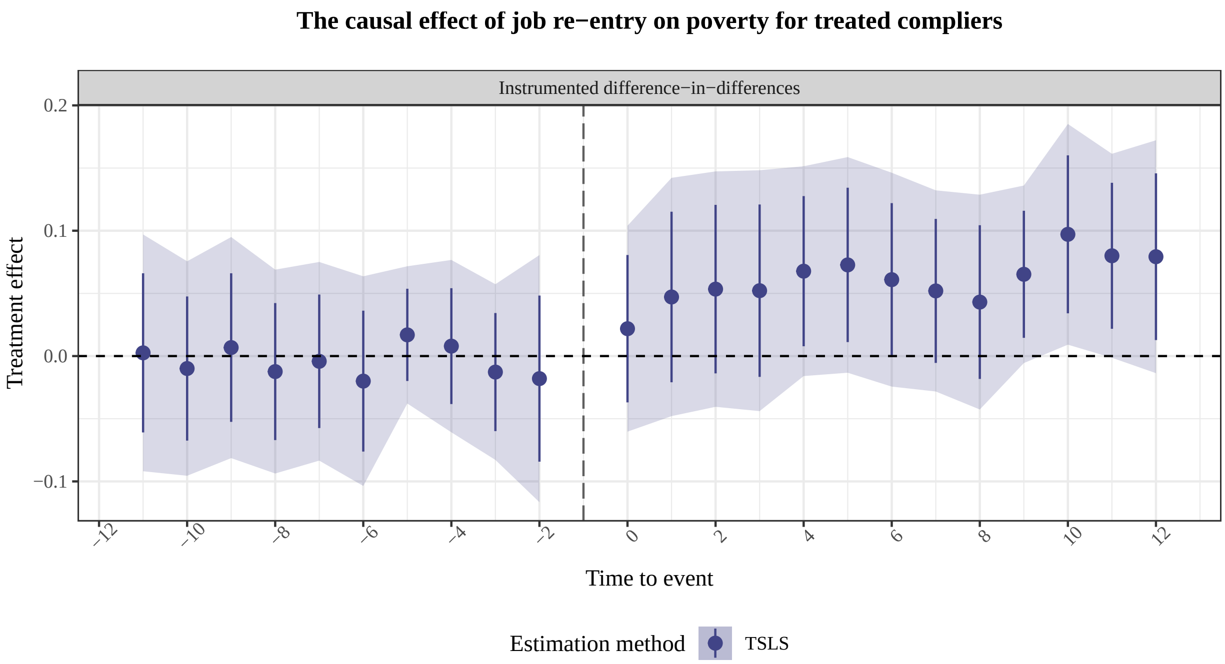
$$\Delta_e Y_w(0) = \mathbb{E}[Y_i(0)|W_i = 1, e = m - g_i, D_i = 1] - \mathbb{E}[Y_i(0)|W_i = 0, e = g_i - 1, D_i = 1]$$

And it is then easy to estimate the triple difference:

$$\Delta\Delta_w(Y) = \Delta_e Y_w(1) - \Delta_e Y_w(0) \tag{17}$$

i.e. the difference of disposable income around first job re-entry for treated compliers compared with the effect of job re-entry for untreated compliers. In other words, the differential effect of job re-entry on disposable income due to the programme. These estimates are presented in the bottom panel of 14. We build a simultaneous 95% confidence interval using the Holm Bonferroni correction. These results have a causal interpretation under the modified exclusion restriction that the effect of the instrument on the timing of employment only goes through participation.

Figure 15: Job re-entry of participants causes higher poverty



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021 from 0 to 30 months from random assignment.
Notes: The event W is the first month with positive labour incomes.
The model instrument event−time dummies interacted with participation by event−time dummies interacted with centred encouragement, with block x month fixed effects instrumenting themselves in the second stage.
Event−time dummies omit the first month of the window and the month before the event.
95% pointwise Confidence intervals based on cluster robust standard errors adjusted at the block level.
Simultaneous 95% CI using Holm Bonferroni correction.

Interpretations Figure 14 shows that before the first job re-entry, treated and untreated compliers had €676 of income per consumption unit, same for treated and untreated compliers. After the first job re-entry, treated compliers have about € 735 of monthly income per consumption unit, with little variation from the initial shock.

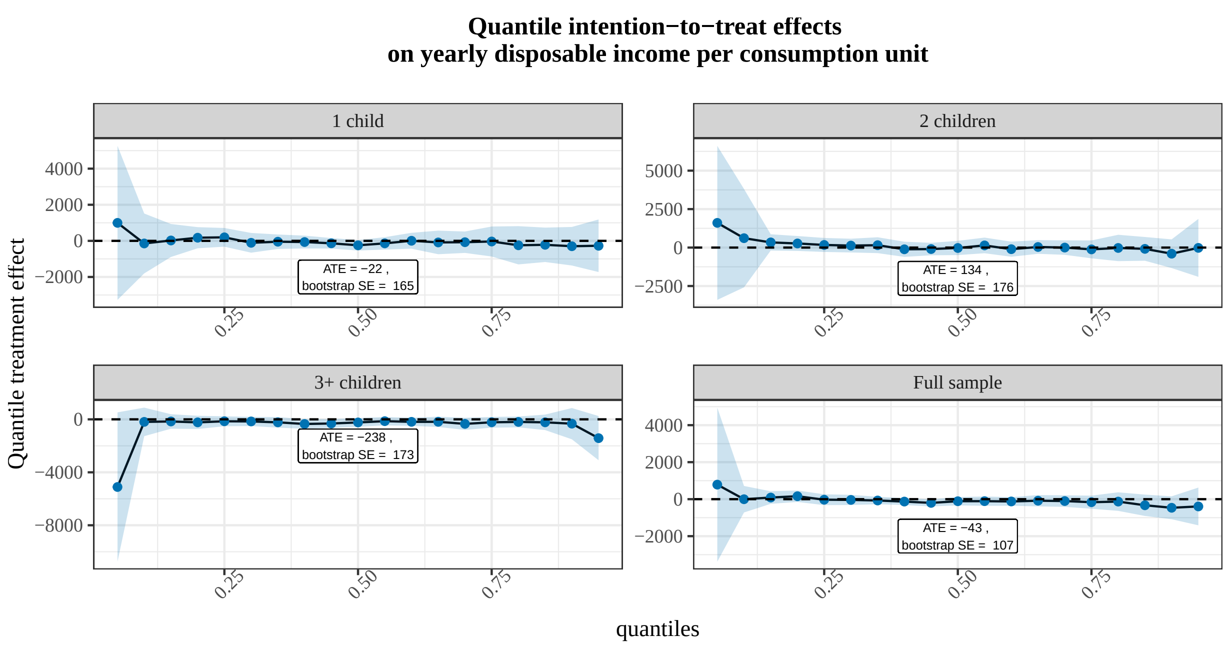
Consistent with our previous results, we see that disposable incomes of untreated compliers are higher after job re-entry than treated compliers, reaching € 796 for the year after. Untreated compliers took jobs with higher wages and treated compliers have lower disposable income when they take a job than they would have had had they not participated. This difference is estimated in the bottom panel and further reinforces the causal interpretation of these results, with absolutely no difference on the lead events. Job re-entry reduces disposable income per capita of treated compliers by € -60 per month on average. Each point estimate excludes 0 from the 95% confidence interval but the joint confidence band are wider and only exclude 0 seven months after job re-entry.

Note that we are not measuring the causal effect of employment as some may switch on and off after. We only measure the difference between treated and untreated compliers stemming from the first job re-entry, and net of other causal paths. Optimisation of labour income at part-time minimum wage reduces the income available for them and their children, but gives them more time out of the labour market than in the counterfactual. However, this lower income also means that for treated compliers, job-re entry increases their probability of living in poverty, as shown in Figure 15. This model is estimated exactly like the previous one, only using poverty as outcome.

Overall, the programme reduced participants' labour market participation either through fewer worked hours hours worked (for the most) and at the extensive margin for those with one child. The effect of taking a job on income is lower and increases the risk of living in poverty. However, these are the effects of the programme that go through employment and we showed that there were large effects on family size. Before moving to the conclusion, we want to discuss the fact that while this paper showed large reactions at the intensive and extensive margins, heterogeneous by number of children at baseline, the net effects of all those changes on disposable incomes is precisely 0, on the entire distribution of all groups.

Absolutely no change in the distribution of cumulated disposable incomes In Figure 16, we estimate the quantile intention-to-treat effect on the disposable income per consumption unit separately by number of children at baseline. These estimates are strikingly flat: a precise 0 effect on the entire distribution. For these single mothers, optimisation allowed to maintain the same level of disposable income they would have had had they not chosen the programme. The number of children at baseline creates very heterogeneous incentives and except single mothers with one child, they are more likely to be the sole earner. Last, when they manage to receive more money from non-custodial fathers, this amount is 100% deduced from RSA and parents lose family support allowance (See section III.3). Recall that at Baseline, only 20.6% of the sample receive child support and 65% perceive ASF instead. Figure G.39 in the Appendix documents the strong effect of the programme on the probability of receiving child support for parents of two and no effect on other parents. However, as soon as the programme ended, the effect dissipates. In the bottom panel, estimates on family support allowance are symmetric. Like those in the control group, they are even less likely to receive the Family support allowance after the programme ended, and as likely to receive child support. Part of this loss of ASF can be due to non-take up or the effect of re-partnering. Indeed, ASF is cancelled when a single parent re-partners, even if the new partner has no link with the children and the father does not pay.

Figure 16: Quantile intention-to-treat effects on disposable income per consumption unit



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
Notes: Estimations of the quantile intention−to−treat effect controling for blocks x cohort by inverse−propensity score weightingfollowing Firpo (2007). 95% Confidence intervals estimated using bootstrap.

VIII Discussion and concluding remarks

This paper analyses the effects of an intensive welfare-to-work programme for single parents implemented in France from 2018 to 2021. Taking advantage of the randomised encouragement design, we study how the programme impacted the distribution and composition of household incomes, as well as family size and structure.

We structured our analysis into three phases. First, we conducted a literature review on welfare reforms and single parents, constructing a theoretical framework grounded in the literature on bunching with imperfect knowledge, psychological barriers, and adjustment costs commonly employed in existing research. However, we critique this approach for its failure to account adequately for the unique circumstances of single parents and question the validity of labour elasticity interpretations that overlook gender norms, children, and household bargaining dynamics. Second, we simulate social transfers by family structure and size using open-source models of the tax-benefit system to estimate implicit marginal tax rates and demonstrate the disparities and divergent incentives. In the third phase, we analysed data from the experiment to quantify the causal effects of the programme on income distribution and family structure. Leveraging the theoretical framework presented earlier, we estimated observed elasticities using parametric models akin to those used by R. Chetty et al. (2011), Henrik J. Kleven and Waseem (2013) or Kostøl and Myhre (2021). However, our main contribution lies in utilising the experimental variations of assignment probabilities to infer the counterfactual distribution of untreated compliers within an instrumental variable framework and measuring effects on family structure. In summary, our main findings are as follows:

1) **Participants bunch at kink points** - between 50% and 60% minimum wage - where the implicit marginal tax rate is minimal, with few reporting incomes exceeding 75% of the full-time minimum wage. This trend is particularly pronounced among single parents with two children at baseline.
2) The distributions of labour incomes among the control group and never-takers exhibit **much lower bunching mass** in the 50-60% range but display another bunching mass at the full-time minimum wage. However, disparities between never-takers and the control group suggest a higher likelihood of part-time work among never-takers.
3) The **implicit marginal tax rate essentially doubles around 60% of the minimum wage**, with variations across households with different numbers of children. At the full-time minimum wage, the implicit marginal tax rate exceeds 70% for single parents with one or two children.
4) The observed elasticities for untreated households range between 0.2 and 0.3, aligning closely with estimates from existing literature. However, the observed elasticity derived from parametric estimations around kink points among participants is closer to 1.
5) Estimations of counterfactual densities indicate that compliers would have reported significantly higher incomes above 75% of the minimum wage had they not participated in the programme.
6) The programme **markedly shifted earning distributions**, reflecting substantial intensive margin reactions, and also affected the extensive margin for single parents with one child at baseline.
7) The elasticity obtained using the counterfactual density of untreated compliers to estimate the bunching mass is approximately 2.05, representing 2.3 times the bunching elasticity found using parametric models among participants.
8) Additional estimations reveal that the effect of the first job re-entry for participants led to a reduction in earned incomes compared to the effect of job re-entry for untreated compliers. Consequently, **treated compliers who secured employment are economically worse-off than untreated compliers who experienced similar job re-entries**.
9) The programme also increased cohabitation among single parents with one child, reduced fertility among single parents with two children, and delayed the departure of older children, primarily affecting parents of three or more children.
10) Ultimately, the programme had no effect on disposable income per consumption unit. The varied heterogeneous reactions and adjustments in labour market participation and family structure resulted in precisely zero effect across all quantiles of the income distribution for all groups.

Internal Validity Our findings are rooted in a well-conducted randomised experiment with administrative data, yielding minimal and balanced attrition. The sample size and rather large first stage with many observations per household contribute to the precision of our estimates. However, it's important to note that our results hinge on the assumption of no direct effect of encouragement on the outcomes of never-takers. While it's plausible that never-takers may have experienced temporary scrutiny and heightened pressure, we posit that any such effect were short-lived and didn't have lasting impacts. Our quantile intention-to-treat analysis, which doesn't rely on this assumption, consistently corroborates our instrumental variable results. We do not use covariates beyond design and time fixed effects and all our models ensure either an average LATE interpretation, or a counterfactual potential outcome distributions or some of their moments.

Employing modern estimation techniques, we derived counterfactual densities from experimental variations, estimated the bunching mass around kink points in the tax-benefit system, and integrated them into a small structural model to compute and interpret elasticities. This methodological approach represents a significant innovation and has revealed notable disparities compared to estimates obtained using conventional bunching estimators. All results point toward the same consistent story: the programme has generated significant changes in participants' choices, extending beyond labour market participation and hours worked to encompass various aspects of household structure and behaviour.

External validity: the effect of the tax-benefit system on poor single parents Acknowledging the inherent limitations of the bunching model employed, it nonetheless provides a framework for understanding the mechanisms through which the programme may have induced substantial behavioural changes. We identified three key parameters potentially affected by the programme: ability, knowledge, and psychological barriers. Did the programme affect abilities ? If we think of abilities as capabilities, then yes. The programme made participants change their behaviours in ways that are consistent with higher agency. However, it is unlikely that the programme affected ability in a way that would shift the distribution of potential outcomes downward without strong elasticities and important frictions the programme alleviated. We argue that lower psychological barriers and better knowledge of the tax-benefit system facilitated by the high-quality support provided are more likely to explain the results we observe. Under these assumptions, the estimated elasticities can be interpreted as closer to structural parameters, suggesting that single parents exhibit high elasticities but face barriers related to knowledge and psychological factors.

Even without these structural assumptions of the bunching estimates, our empirical findings indicate a significant increase in part-time employment among participants compared to what would have occurred had they not participated in the programme. Moreover, our results reveal other substantial behavioural responses that align with the incentives of the tax-benefit system. Cohabitation induces lower loss of social transfers for parents with one child compared to those with more children, resulting in higher rates of cohabitation among this group. Parents of three or more children with low or no labour income rely heavily on housing and family benefits, incentivising them to stay with their children longer, a phenomenon we observe in our data. Despite these adjustments, our analysis demonstrates precisely no effect on disposable income per consumption unit.

If we think that most of the effect of the programme comes from the understanding of the tax-benefit system, our results show that the very high tax burden poor single parent household face creates strong disincentive at the extensive margin for some and a strong economic incentives for part-time jobs in general. Either way, incentives foster situations without enough income to exit poverty and further induce high level of social transfers. In the absence of salient incentives, untreated compliers work more and the problem is not a lack of ability or opportunity, but of tax burden. In the general population, there is already a massive issue of hindered opportunities and 40% single parents working part-time jobs are so involuntarily (Périvier 2022b).

The inadequacy of gender-neutral labour-supply models For a long time, economists neglected the origin of gender differences in the effectiveness of labour market policies. For instance, Card and Hyslop (2005) analyse another randomised welfare-to-work initiative in the USA with 95% women in the sample, which is only mentioned once in the introduction, once commenting table 2 and twice in illustrative examples. The paper develops a theoretical model to explain the low impacts of the programme without considering once the specifics of the sample. Ashworth et al. (2004) produced a meta-analysis of randomised welfare-to-work programmes and also had a sample made of 95 % women, but still generalised their conclusion as if it was balanced between gender. This erases the many constraints women in general, and single mothers in particular, have to deal with while considering their labour market participation. Disregarding childcare, domestic labour and their systemic relegation to women, as well as ignoring the system that pushes them into dependency on either men or the State, can not lead to appropriate or effective programmes - if the goal is to combat poverty through employment.

The literature on taxation also has a very specific language: they explicitly model genderless agents as tax avoiders maximising consumption. We find it ironic that most of the recent literature on bunching was developed using optimisation behaviours of single mothers, acknowledged as heavily stigmatised, while modelling them using such morally loaded terms (Saez 2010; Raj Chetty, Friedman, and Saez 2013). Wording matters. They shape narratives, discourses and political decisions. Eliaz and Spiegler (2020) define a narrative as a "*causal model that maps actions into consequences, weaving a selection of other random variables into the story*". Narratives incorporate normative components in the sense that they make strong yet often implicit assumptions on agents' behaviours. Sometimes, a new terminology is better than a stigmatising short-cut. For single mothers, these representations are both stigmatising and inaccurate, for there is an important missing equation: the domestic production function and more specifically, children human capital [Gelber and Mitchell (2012);Blundell et al. (2016);AttanasioEtAl2018]. These parents chose the level of employment that minimises the time spent in the labour force while minimising the loss of disposable income from taxation, possibly spending more time with their children.

For Maniquet and Neumann (2021), this reaction may very well be an improvement. Focusing on income disregards the labour time it takes agents to earn it. However, labour time is also a determinant of well-being - at least if one defines well-being in a consistent way with preference satisfaction - and an input in the human capital production function of children; a role single mothers in this sample play on their own. When increasing income from below to above the poverty line goes together with an increase in the labour time, anti-poverty policies may decrease the well-being of the income-poor, namely if the latter actually prefers to work less to use their time more efficiently. A large literature comparing time use across different household structures shows that single mothers invest as much - if not more - time in their children's education as coupled mothers[52] (Kendig and Bianchi 2008; Craig and Mullan 2011; Bianchi et al. 2014; Lavoie and Saint-Jacques 2020; Prickett and Augustine 2021).

Tax burden on the poor and coercion: the poor laws of the Twenty-first century ? Our investigation into the French tax-benefit system has unveiled significant disparities and disincentives for labour participation, particularly among single parents. In particular, single parents on welfare receive no child support. The latter is 100% taxed through lower welfare payment, reducing by the same amount any monetary incentive for employment unless the parent earns enough to be ineligible for in-work benefit. Then only would they receive child support. All other family benefits are also fully taxed for welfare recipients while they are not part of the taxable income for parents out of welfare. Moreover, single parents, especially those with multiple children, face disproportionately heavy tax burdens, particularly at income thresholds near minimum wage levels. This situation creates significant disincentives for increasing labour participation and hampers efforts to improve disposable incomes. The intricate interactions between various social transfers create a web of complexities that further exacerbates the tax burden on vulnerable groups. This complexity contributes to a lack of transparency and understanding among beneficiaries, hindering their ability to make informed decisions about employment and financial planning.

We coined the word *"Assistaxation"* to convey the idea of providing assistance in a way that is burdensome, overly taxing, or difficult mentally, physically, emotionally, or financially. It is not just about the administrative burden and stigma discussed in the literature; it also involves heavier and implicit taxation of labour income and a 100% tax rate on child support and family benefits of the poorest. *Assistaxation* also better reflects what type of taxation is being avoided here: sophisticated scheme and coercive measures to force labour market participation while taxing

[52] A notable fact from Dotti Sani and Treas (2016) is that French parents are the only ones in their international comparisons who spend less and less time with their children.

them most unless they leave assistance or repartner. If we were to make an optimal taxation argument considering the high elasticities we found, we would typically go in the direction of a gender-based taxation accounting for these structural differences in elasticities, as studied by Alesina, Ichino, and Karabarbounis (2011). For the current system sets monetary incentives such that the optimal level leaves working single parents and their children in poverty, while still relying heavily on social transfers.

The practices of the CAF have faced criticism and condemnation from the "*Défenseur des droits*" (Défenseur des droits 2017) in France, particularly in cases of perceived harassment towards economically vulnerable households. Recipients often report a lack of notification, discovering deductions on their CAF personal accounts or bank statements without understanding the rationale or procedures. These deductions can be substantial, reaching up to 50% or even 100% of the recipients' resources, raising concerns about their legality and fairness. The scrutiny exercised on vulnerable individuals, according to Défenseur des droits (2017) is frequently deemed disproportionate, discriminatory, and often lacks legal foundations. The control agents, except those from the Employment agency, have the authority to request various documents directly from banks, employers, energy providers, telecommunications operators, etc., without being bound by professional or banking secrecy. The scoring system used to trigger these controls, relying on the absence and variability of income, tends to disproportionately target the poorest beneficiaries (Quadrature du net 2023). As indicated in the same report, the probability of an RSA beneficiary undergoing controls was 40 points higher than their proportion in the overall population receiving social benefits.

Drawing parallels with the historical context of the Poor Laws reveals striking similarities in the systemic inequalities and barriers to economic advancement faced by marginalised groups (Persky 1997). Just as the Poor Laws entrenched poverty and perpetuated social stratification, "assistaxation" perpetuates cycles of financial hardship and inequality, hindering efforts to achieve economic security and social mobility.

From "deserving Poor" to "Welfare Queens": a backlash against women In her work titled "Backlash Against Welfare Mothers Past and Present," Reese (2005) analyses the intersection of class, race, and gender issues during conservative political periods. Reese (2005) demonstrates political alliances rooted in racist sentiments and patriarchal family values. Whether it's the TANF (Temporary Assistance for Needy Families) in the USA or allowances for single-parent families in the British New Deal, these reforms reinforced the "ethics of marriage," the idea that poor women should marry and remain married to a man. State governments and local administrations commonly used "suitable household" and "suitable mother" rules to deny public assistance to single mothers (Fisher and Reese 2011, 231). The narrative of *"Ending welfare as we know it"* of President Clinton has been highly pervasive and led to a political evolution of the concept of the "deserving poor"[53] (Peterson 1997; Carcasson 2006; Ellwood 2000). In her doctoral thesis, Mangin (2021) demonstrates that the narratives used in debates accompanying welfare reforms are reflected in the legislative corpus of different States, including stigmatising stereotypes like the "*Welfare Queen*", which depicts single mothers living on welfare as manipulative, dishonest, unworthy, lazy, African American women having children to avoid work and challenging dominant sexual norms and gender roles (Jarrett 1996; Foster 2008; Van Doorn and Bos 2019). These stereotypes are also found in the discourse of some social workers and are reflected in their daily interactions with these beneficiaries (Masters, Lindhorst, and Meyers 2014). Similarly, in Great Britain, the image of the irresponsible white working-class single mother who became pregnant at seventeen and had children with multiple men was continually reinforced by government rhetoric and media representation (Herke 2021; Herbst-Debby 2022).

In France, several recent work have documented the large inequalities and unfair treatment of single parents (Le Pape and Helfter 2023); some even proposed and simulated various reform scenario (Allègre, Périvier, and Pucci 2021). Perhaps the most immediate reform would be to remove child support and family support allowance from the income deduced from welfare and in-work benefits, as proposed by Pucci and Périvier (2022). Our contribution further documents the lack of transparency of the system and its impact on single parents, along with activation measures. Single mothers' unfair situation is unlikely to improve so long as the fiscal, social and administrative reference centres on the idea of couples while households get more and more diverse, often falling in between definitions.

[53] See the works of Appelbaum (2001) and Robert A. Moffitt (2015) regarding reforms in the United States or Herke (2021) for a study of the Hungarian case compared to other European Union countries

Appendix

A Details on the French tax benefit system

A.I A short history of monetary-incentives in France

A minimum income scheme from the 1980's In 1985, France introduced a welfare benefit called *Revenu minimum d'insertion* (RMI). The RMI was a differential minimum income imposing a 100% implicit marginal tax rate. When one got a job, a temporary reduction in marginal tax rate was implemented[54] such that if a recipient took a job, only half of their labour income was considered to compute the level of RMI. The reduction was only implemented for the first 750h worked. Gurgand and Margolis (2008) analysed monetary incentives using data from 1996-1998 on recipients of RMI and consider the distribution of potential monthly earnings based on observed wages and working time. Accounting for the welfare earnings top-up programme (*intéressement*), the study finds that gains are almost always positive but generally low, particularly for single mothers. Using census data from 1999, Bargain and Doorley (2011) analyse the causal effect of RMI on employment level of childless single men using the 25-year-old eligibility threshold and show large negative effects. No such effects are found for single mothers.

A first tax credit in the early 2000's Inspired by optimal taxation theories from the US, in-work benefits started being introduced in the French socio-fiscal package in 2001 with the *Prime Pour l'Emploi* (PPE) or Job Bonus. The stated goal was to create a monetary incentive targeting poor workers, all the while combatting the risk of "inactivity traps" created by the *Revenu minimal d'insertion* (RMI), a basic income scheme. Evaluations of PPE quickly underlined that the policy wasn't meeting its goal. In 2002, Cahuc (2002) ascribes this failure to four factors : (1) employment is constrained by demand because of the minimum wage's high value ; (2) incentives for full-time re-employment are already plentiful, conversely to part-time incentives ; (3) while some jobs have been created, PPE incites some women to reduce their working time, since it is degressive above minimum wage and includes the household's incomes in its calculation ; (4) the fact that those benefiting the most from PPE are households belonging to deciles 2 to 4, and not the poorest households, makes its impact on poverty very weak. Stancanelli (2008), utilising various double-difference strategies and data from the Employment Survey, found a negative effect of the PPE on married women's labour market participation (-3 percentage points), a positive effect on cohabiting women (+6 percentage points), and a negligible effect on single women[55]. Aggregate effects on women's labour market participation were modest (around 2,000 job entries). Despite an average take-up of 95%, Vermare et al. (2008) did not find any significant effect of the PPE on employment in the general population, including married women[56].

The activation turn of 2008 In 2007-2008, the in-work benefits has been tested in an experiment and generalised before results were known in 2008 (Bourguignon 2009). The new welfare replaces the former basic income RMI and the temporary allowance for single parents API (*Allocation parent isolé*), replaced by a similar temporary RSA-supplement (*RSA majoré*). Conversely to the report's recommendations, in 2008 RSA and its in-work benefits (*RSA activité*) were adopted[57] with a flat implicit marginal tax rate of 38%, with no difference between part-time and full-time. Another deviation from the report is that the degressivity rate no longer depends on family configuration, which favours families with children (Allègre 2024). To be eligible, one must be at least 25 years old, reside in France, and not be enrolled in school. Recipients are required to actively search for employment and participate in social or employment support programmes, or risk facing sanctions. Temporary supplements for single parents are accessible for 12 consecutive months or up to 18 months from the triggering event. Single parents with *RSA majoré* must also participate in job-search or social support activities unless their youngest child is younger than 3. For such households, there is no age requirement or mandatory activation and RSA-supplement extends until the youngest child turn 3.

[54] Called *intéressement*, top-up programme in the paper.

[55] The negative impact on married women was primarily attributed to a resource condition based on couple income.

[56] They used data from the Tax Income Survey (ERF). Note that the high take-up rate is due to the easier access as it was embedded in tax forms. See Allègre (2024) for a discussion.

[57] While political opposition prevented RSA activité from being financed by the suppression of PPE, the latter's weight de facto diminished due to the freezing of its scale : in 2014, its average amount was 33 euros monthly (Allègre 2024).

Regarding the effects of the in-work benefits (*RSA activité*) introduced in 2009, the improvement in monetary incentives did not necessarily translate into increased employment, as the initial evaluation showed (Bourguignon 2009). Most research find little to no effect in general, but some effects when considering sub-groups. Simonnet and Danzin (2014), using administrative data from Cnaf and a double-difference strategy, observed a positive impact on the employment of single mothers, but not on men. Allègre (2011) showed a negative and significant effect on the labour supply of married women but a positive effect for single women. Bargain and Vicard (2014) used the same identification strategy as Bargain and Doorley (2011) with a notable difference: they looked at 'the youth' and pool genders together. They conclude that the disincentive results of the RMI were weak and concentrated among low-skilled workers, and find no disincentive effects for RSA. While the main results of Bargain and Doorley (2011) was about heterogeneous gendered differences, this analysis did not even report separate estimates by gender, nor between parents and non-parents. Sicsic (2019) explored the elasticity of earned income concerning marginal tax rates in the French socio-fiscal system, revealing a modest elasticity of 0.1 in response to changes in RSA marginal rates.

Reducing welfare stigma through a separate in-work benefits In spite of several issues, the need for a new reform arose outside of economic concerns : while non-take up of RSA approximated 40%, it reached a staggering 68% for RSA activité (Domingo and Pucci 2014). Misinformation played a role (people fear having to reimburse overpayments) but explanations primarily underline the power of stigma: the in-work benefits was associated to RSA, both by name and administrative process, thus making it *repugnant* for poor workers averse to a "hand-out". A report by MP Christophe Sirugue further notes that generally, "*RSA activité and PPE's impact is very weak when it comes to incitations to either (re-)entering or remaining in employment*" (Sirugue 2013).

The 2015 reform - implemented in 2016 - aimed both at combatting this repugnance and at simplifying the social benefits "menu" by merging together RSA activité and PPE into a single in-work benefits : *Prime d'activité* (PA). Just like *RSA activité*, PA degressively declines (by 39%) with earned income, and its amount is the difference between a base amount and the household's ressources. Unlike PPE however, it also includes a progressive individual bonus for every household member earning more than half the minimum wage. While this reform has been analysed as discretely shifting the goal from incitation to distribution (Allègre 2024), it is also worth noting that its budget was based on an estimated take-up of 50% (or a rough average between the two previous schemes). In reality, it reached about 70% as soon as the first year, leading to annual additional expenses of 800 millions euros.

RSA and PA remain tightly linked in their formula and eligibility rules. The main difference is that if a household applies for PA with incomes lower than the RSA, but does not receive RSA already, they have to fill another form for RSA[58]. Conversely, PA is automatically computed for RSA recipients who report positive labour incomes. PA is also open from 18 years old while RSA is still limited to adults older than 25, except single parents[59].

A massive increase of in-work benefits in 2019 The 2019 so-called "Gilets Jaunes" reform was presented as a concession following the eponymous social movement that had shaken France for months, with a notable focus on poor workers' declining purchasing power and living conditions. We provide a detailed explanation of the PA formula and the effect of the reform in Appendix A.III that follows.

While the reform had an explicitly distributive goal, with President Macron promising a € 100 increase of PA's individual bonus, many interpreted it as more of a publicity stunt. Indeed, out of the 100 euros promised, 10 were expected due to the minimum wage's automatic revaluation, and the rest was already planned as early as 2017, initially through successive annual increases (Bozio et al. 2023). A more significant change resides in the enlargement of income eligibility levels by the inclusion of incomes up to 1.5 minimum wage (vs 1.3 previously). Using microsimulation on administrative data from CNAF, Dardier, Doan, and Lhermet (2022) estimate that 83% of the subsequent 43% take-up increase are due to newly eligible beneficiaries, while the remaining is ascribable to the reform's visibility. They also estimate that the reform reduced poverty by 0.6 point, with most gains located in households of the 2nd and 3rd living conditions decile. For families with children, including single parent families,

[58] something many don't know which largely contributes to non take-up of RSA, as documented by Hannafi et al. (2022).

[59] The age threshold allowed Locks and Thuilliez (2023) to demonstrate that RSA reduces homelessness and estimate that the cost of setting the age limit to 18 would be 60% offset by savings in social assistance costs to the homeless.

the poverty rate reduction is over 1 point (Dardier, Doan, and Lhermet 2022). It is note-worthy that a majority (57%) of PA beneficiaries are women, which aligns with their over-representation in part-time employment, and raises questions as to the efficiency of incitations at the intensive margins when they are blind to gender.

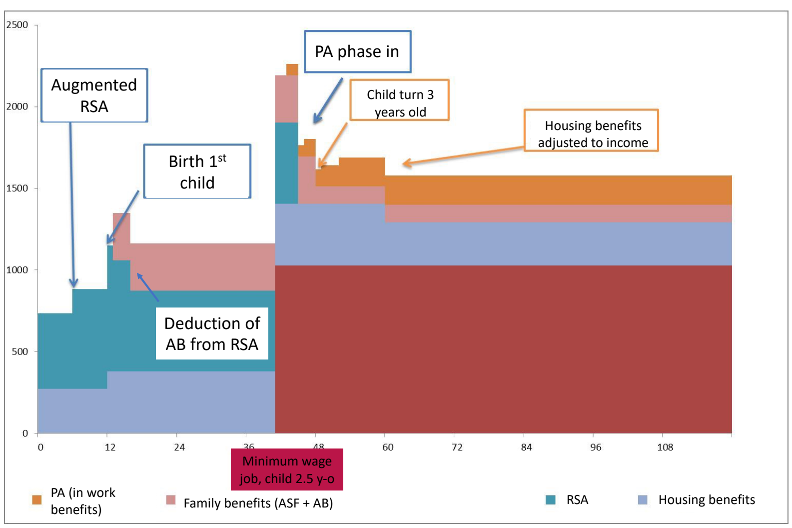
So far, we are not aware of a successful causal evaluation of the effect of this reform. The latter being highly confounded by mass public comunication, it is hard to disintangle the reaction to the reform from better knowledge of the tax-benefit system. Bozio et al. (2023) tried to estimate the effects of the reform using a difference-in-differences design comparing groups most affected by the reform with groups less incentivised. Using micro-simulation and case-studies, they predict different gains for households varying by family size and composition. However, a placebo test on the year before the reform between these groups finds similar estimates and reject the parallel trend assumption.

In summary, French evaluations of the effects of monetary incentives suggest positive effects for singles and single mothers, with relatively limited magnitudes. Effects on other groups are either null or negative on average. While which effects dominate remains essentially an empirical question whose answer is context-dependent, the challenge also stems from identifying and estimating the intensive margin reaction and the effect on wage.

A.II Dynamics of social transfers

An overlooked aspect of the research on the effect of social transfers are their schedules: how and when benefits or tax adjust. While it matters a lot for incentives, the dynamics of adjustment of various social transfers when people's situation change is an under-investigated topic. The main reason may be that nobody really knows how it actually works. Let-us take a simple case-study to illustrate what happens when a single mother gets pregnant and returns to work when her child reaches 2.5 year old.

Figure A.17: Dynamic simulation of the effect of job re-entry at the minimum wage on social transfers in 2018 for a single mother when her child is 2.5 years old. Old internal DSER simulation tool.



Up to 2018, the statistics department of CNAF used to build what was meant to be a simple case-study simulation tool to model the adjustment to job re-entry in particular. Figure A.17 displays a simulation using the 2018 legal framework for the aforementioned example. Pregnancy starts at $m = 0$, which coincides with her last month of RSA in the quarter - an important detail we discuss right after. Three months later, her physician declares the pregnancy, which triggers the *augmented RSA* in the next quarter and until the child turns 3. When the child is born, housing benefits increase and two more transfers are opened: the *Family support allowance* (ASF) and early childhood benefits[60] (AB). At birth, parents may also receive a Birth allowance[61], a one-time transfer to help with higher spendings around childbirths. In the next quarter, 80% of ASF and the entirety of family benefits are deduced from the augmented RSA. ASF is perceived because we assume that the father does not pay child support. Otherwise, the amount of child support would be entirely deduced from RSA. In this simulation, the mother takes a full-time job when the child turns 2.5, again on the last month of the quarter, and immediately reports her first wages. In this first quarter of employment, she cumulates the full RSA payment with her earned income, and the PA slowly kicks in since in her next quarterly report, she has only been working for a month. The next quarter, she loses the RSA - a full-time minimum wage being higher than the threshold - and the PA increases. Then the child turns three and there are no more early childhood benefits. The reduction of the early-childhood allowance causes an increase of the in-work benefits. Indeed, since social transfers are part of the reference income, they are entirely deduced from RSA and PA payments and their exhaustion is partially offset. The last adjustment occurs two years later when the taxable income of the first year in employment is available and housing benefits are reduced[62].

This figure shows that there are monetary incentives for employment but their adjustments are highly dependent on many other parameters that together, may very well create situations that are hard to track for parents, especially since no detail on computation is provided. This tool has been discontinued due to the challenges of keeping it updated with frequent reforms, changes, increases in minimum wage, and other factors. Moreover, the tool faced difficulties in presenting typical scenarios due to the intricate consequences of any alterations in the timing of events. In the given example, even a minor shift in the month of job re-entry within a quarter significantly impacts the distribution of in-work benefits over two quarters. The intricacies are further heightened by precarious employment situations[63].

In the next appendix, we present the details of the formula for the in-work benefits and the main changes of the 2019 reform.

A.III The in-work benefits (PA): formula eligibility and the 2019 reform

The amount of in-work benefits PA_m for the month m, is computed according to the following formula[64] :

$$PA_m = \underbrace{BPA_m\left(1+\delta_m^f\right)}_{\text{Flat amount }:FPA_m} + \tau_m \tilde{W}_m - \underbrace{\max\left(BPA_m\left(1+\delta_m^f\right), \tilde{Y}_m\right)}_{\text{Reference household incomes}} + \underbrace{\sum_i B\left(\bar{W}_{iq}^a\right)}_{\text{PA supplement}} \tag{18}$$

In words, the amount of in-work benefits is based on the difference between a flat amount and households' incomes to which an individual bonus is added. The flat amount of the *Prime d'activité*, denoted as FPA_m, depends on the current baseline amount BPA_m multiplied by a factor δ_m^f, weighting family composition of the household. BPA_m represents the amount for a single individual without children, and δ_m^f is a weighted sum of household members where the one additional person (child or partner) means $\delta_m^1 = .5$. An additional second person leads to an additional 30% increase in the amount, 40% for single parents. Any person beyond three additional individuals qualifies for a 40% increase. To this flat amount, a fraction τ of the household's earned income $\tilde{W}_{im}$ during month m is added, and the household resources $\tilde{Y}_{im}$ are subtracted[65]. Note that the household's earned income $\tilde{W}_{im}$ is included in the total resources $\tilde{Y}_{im}$ with spouse's incomes and other resources. Importantly, other resources include all other social transfers, such as child support or capital incomes (if any). Moreover, it is the sum of earnings that matters and the composition of incomes between spouses has no influence on the amounts but through the individual supplement. The French administration institutionalised an old-school Beckerian static unitary model (Périvier 2012), as if the household's preferences can be represented using a unique utility function that does not depend on prices, incomes, or any exogenous factor, independently of the number of household members (Pierre-André Chiappori 2017).

[60] AB stands for *allocation de base* which is the amount of cash benefit for low income families with children under 3. Like any other social transfers, they are deduced from the baseline amount of RSA and PA.
[61] Included in the augmented RSA here. It targets both single and coupled parents, with a different income threshold of eligibility
[62] Since 2021, this is no longer the case. The current system defines the reference income from a moving average over earned incomes in the past 12 months.
[63] For instance, households who work for a single quarter will have very different in-work benefits depending on which month they first registered.
[64] *Article L842-3 du Code de la sécurité sociale*
[65] The resource base is considered to be at least equal to the flat amount, hence the max function.

The bonuses are calculated based on the average earned incomes $\bar{Y}_{iq}$ of each member i of the household during the reference quarter q. Parameters BPA_m, τ_m, δ_m^f are set by decree and have evolved over time, using the following formula[66]:

$$B\left(\bar{W}_{iq}\right) = min\left(\bar{B}_m, max\left(0, \bar{B}_m \times \frac{\bar{W}_{iq} - S_{min}}{S_{max} - S_{min}}\right)\right) \quad (19)$$

To understand this formula, let me describe which parameters are set by law and how they define these variables. The legislation defines three legislative parameters by decree regarding the calculation of individual bonuses:

- The first parameter, τ_b, corresponds to the maximum percentage amount of the bonus relative to the base amount. The maximum bonus amount, expressed in euros, is then calculated as $B_m = \tau_b BPA_m$.
- The second parameter, s_{min}, corresponds to the minimal threshold of monthly earnings (in multiples of the gross hourly minimum wage) required to qualify for the bonus. The earnings threshold in euros is thus $S_{min} = s_{min} \times \bar{s}$, with $\bar{s}$ being the amount of the current gross hourly minimum wage.
- The third parameter, s_{max}, corresponds to the threshold of monthly earnings (in multiples of the gross hourly minimum wage) from which the bonus becomes maximum and constant. The earnings threshold in euros is therefore $S_{max} = s_{max} \times \bar{s}$, again with $\bar{s}$ being the amount of the current gross hourly minimum wage.

The bonus amount for which an household is eligible depends on each individual's average income over the reference quarter. It is zero when these earnings are below S_{min} euros. It then increases linearly with the amount of these earnings until it reaches the maximum amount, B_m, when the individual earned incomes equal $\bar{B}_m$.

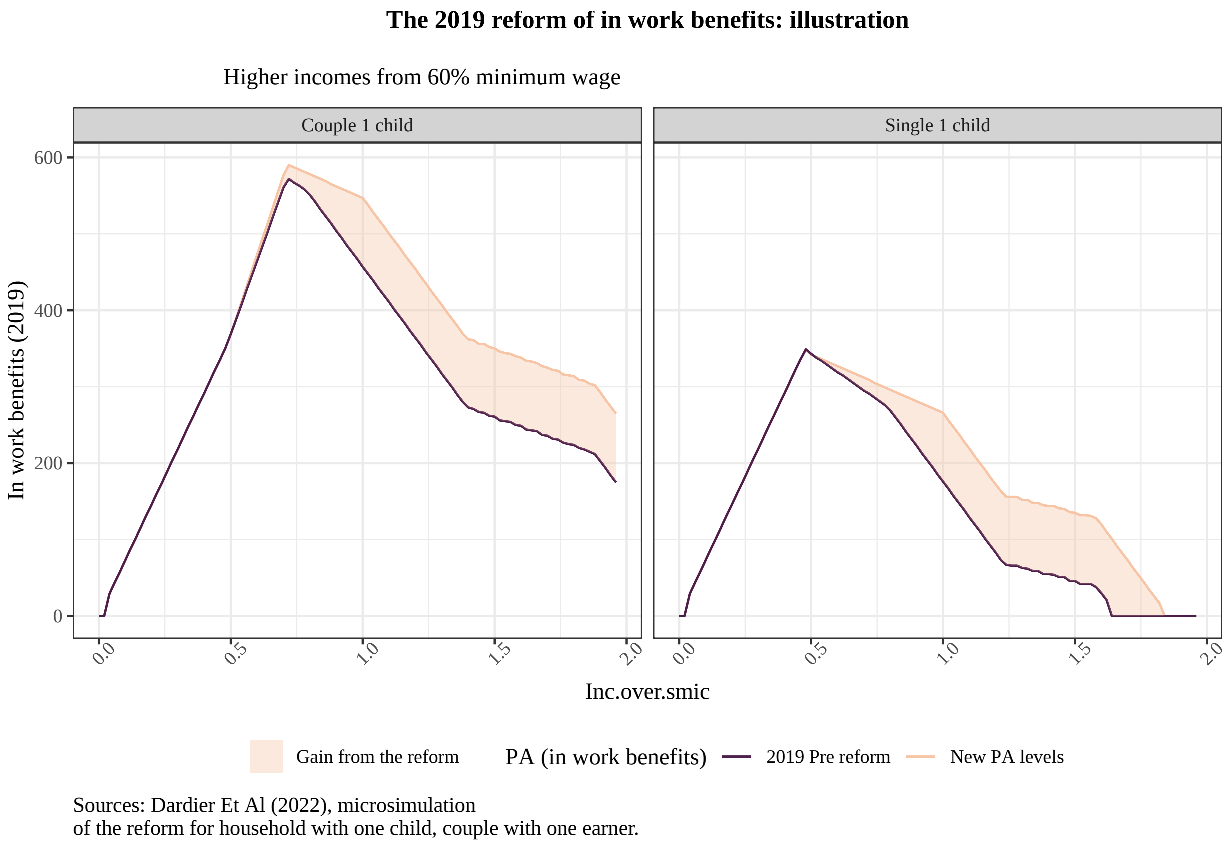
In January 2019, following the "Yellow Vests" movement, a major reform affected three components of this formula:

- Higher baseline amount BPA_m,
- Higher PA bonuses
- Changes in the marginal tax rate on earned incomes, increasing or decreasing depending on the configuration.

An important effect of the reform is the alignment of the reduction in individual bonus at the minimum wage level. Figure A.18 reproduces data from Dardier, Doan, and Lhermet (2022) who used microsimulation to measure the effect of the reform. They estimated that this reform would have led to a 37% increase in the number of households benefiting from the in-work benefits and an average gain of €70 per month at an increased total cost of nearly €4 billion.

[66] *Articles L842-3 et D843-2 du Code de la sécurité sociale.*

Figure A.18: Effect of the 2019 reform of in-work benefits on transfers for parents of one child as simulated by Dardier et Al. 2022



A.IV Variations in the implicit marginal tax rates

Ultimately, the 2019 reform induced large changes in the shape of the in-work benefits and introduces more variations in the marginal tax rate. To see that, we use the EDIFIS simulation model of DREES and compute the implicit marginal tax rate for parents of 1, 2 or 3 children, single or in a single-earner couple.

The IMTR is the variation of disposable income per capita over a marginal change of pre-tax income. We compute the average IMTR over bins of 4% of the minimum wage to smooth the curve. The spikes we see are exit thresholds of various social transfers. After 60% minimum-wage, the unstable aspect of the implicit tax rate is linked to the calculation of housing benefit, in which resources are counted on an annual basis, rounded to the nearest hundred euros. The level of gain in employment therefore depends on whether the extra euros earned tip resources into the next highest hundred euros or not[67]. This poorly designed interaction between these two parameters generates very strange variations in marginal tax rates. For instance in these simulations, a single parent with two children going from 42% to 43% of the minimum wage undergoes an implicit marginal tax rate of 337% and these € 13 additional euros turn into an overall reduction of € 33 of disposable incomes. While the variations may be small in magnitude, they add a lot of noise to a system that is already hard to understand. For households, this means income uncertainty and unexplained variation, especially since these changes occur because of aggregated variations in income over the past 12 months.

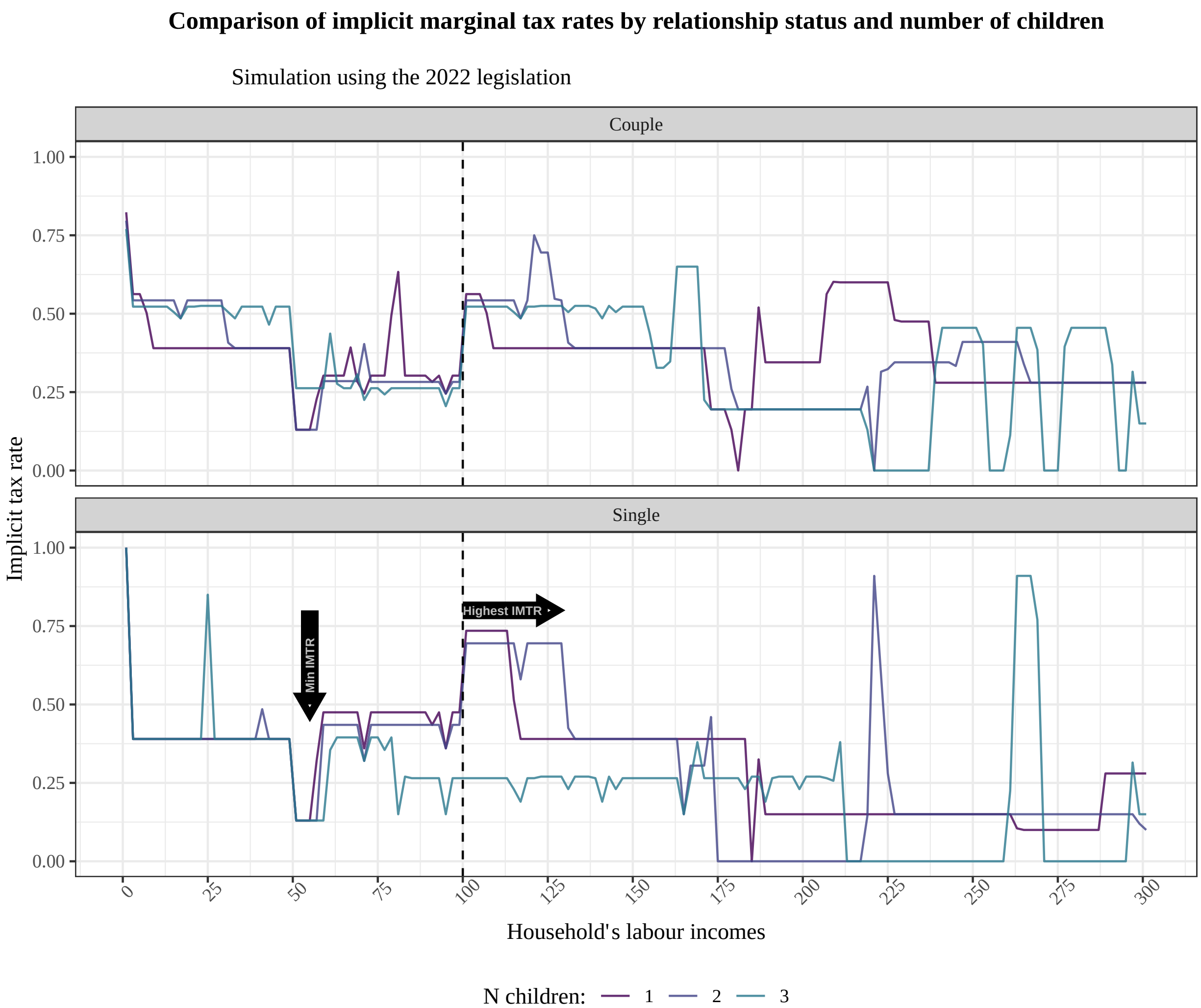
Apart from these small variations, there are four parts of the distribution we should comment. First, for households earning less than 50% minimum wage, the IMTR is higher four couples than single parents. Second, individual bonus of the PA starts at 50% minimum wage and creates a sharp drop in the IMTR up to 60%. Third, from this point, housing benefits start to decrease and, importantly, the IMTR is about 50% for single parents and roughly 38%

[67] See https://evaluation.securite-sociale.fr/home/financement/29-assurer-un-revenu-disponible.html

for couples, local variation aside. Fourth, the IMTR sharply increases at the full-time minimum wage for couples and single parents, but couples only reach 52% i.e. roughly the same level as for single parents from 60% to 100%. The latter face an IMTR of about 70% up to 115% to 130% of the minimum wage.

This brings us to the last important difference to notice. There are sharp differences by number of children. In particular, single parents of 3 children do not receive welfare or in-work benefits from 75% of the minimum wage. For them, social transfers depend mostly on housing benefits on the one hand, and family allowance on the other. Housing benefits increase with the number of children, and for parents of three with low or no income, there is an additional family allowance called *Complément familial*. All are entirely deduced from RSA and PA, explaining the little amounts and early exit.

Figure A.19: implicit marginal tax rate for single and couples parents by number of children

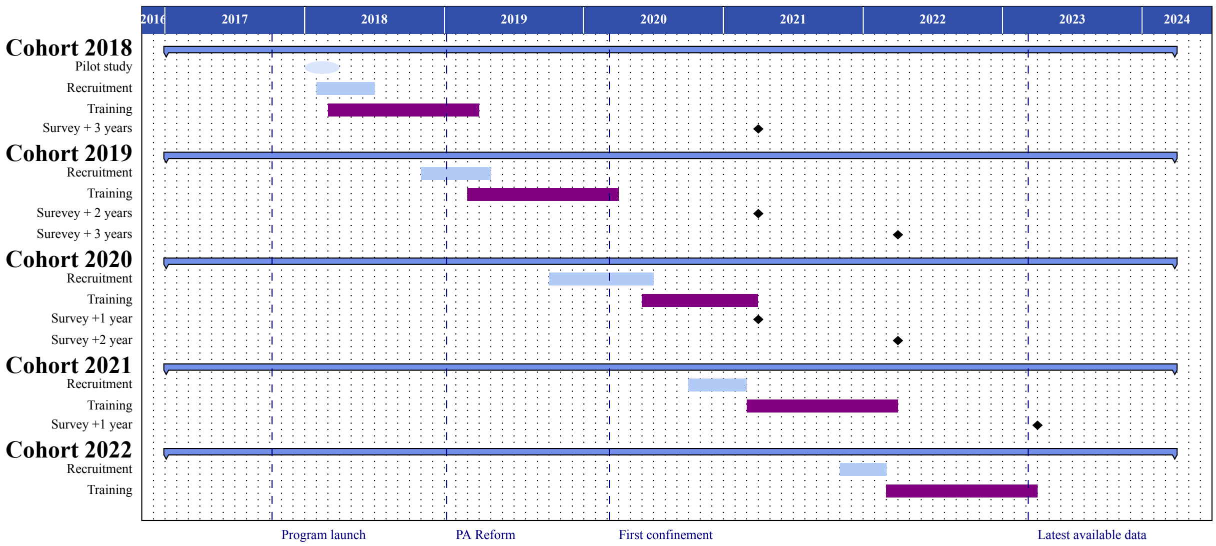


Sources: DREES, EDIFIS.
Note: The implicit marginal tax rate is directly computed in the simulation. We use an average over bins of 2pp of the minimum wage.
Spikes with more than 100% marginal tax rate occuring at the exit thresholds of each transfers
removed for clarity.

B The Reliance experiment

A) Timeline of the experiment

Figure B.20: Timeline of the experiment



B.I Recruiting participants

A) Invitation letter and invitation leaflet for the first cohort

Figure B.21: Model letter for recruiting the first cohort

ALLOCATIONS FAMILIALES
Caf de Meurthe-et-Moselle

DÉPARTEMENT MEURTHE & MOSELLE

GROUPE Caisse des Dépôts

Laxou, le 15 Février 2018

MODELE

Dossier suivi par
ARELIA – Dispositif RELIANCE
Tél : [redacted]

Tout sur le RSA en Meurthe et Moselle :
www.insertion.meurthe-et-moselle.fr

Objet : RSA – Information collective RELIANCE

Madame,

Vous êtes bénéficiaire du Revenu de Solidarité Active (RSA).

La Caisse d'Allocations Familiales, le Conseil Départemental de Meurthe-et-Moselle ainsi que la Caisse des Dépôts et Consignation mettent en place une **action d'accompagnement destinée aux chefs de familles monoparentales**. Dans ce cadre, vous avez été identifié(e) pour y participer.

Vous êtes invité(e) à une réunion d'information collective présentant cette action intitulée RELIANCE dont l'objectif est de favoriser, à terme, votre accès à un emploi ou une formation. Au cours de cette rencontre, un temps vous sera réservé afin d'évoquer votre situation, vos projets et vos modalités d'organisation.

Celle-ci aura lieu le :

Date : **Lundi 19 Mars 2018 à 09H30 à 11H00**

Lieu : **ARELIA RELIANCE**
9-11 rue Robert Schuman, 3ème étage
54500 VANDOEUVRE LES NANCY

(Voir plan au dos)

Merci de vous organiser pour vous rendre disponible. Néanmoins, ***en cas de difficultés de garde****, nous pouvons vous accueillir en présence de vos enfants.*

Si vous ne pouvez pas venir à cette réunion, nous vous demandons de téléphoner au [redacted] **réception de cette lettre**, pour nous en informer.

Dans l'attente de vous rencontrer, je vous prie de recevoir, Madame, l'expression de nos salutations distinguées.

Pour le Président du Conseil Départemental et par délégation,

[redacted]
Responsable du Service Economie Solidaire et Insertion

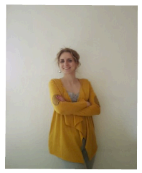
Reliance est un dispositif porté par l'association Arélia, en partenariat avec Ulis et Ecoval

arélia

Figure B.22: Presentation leaflet 1/2

Figure B.23: Presentation leaflet 2/2

ATELIER : ACCES AUX DROITS/ MODE DE GARDE/ RAPPORT A SOI/ SANTE



- Accès aux droits et au numérique : faire le point sur l'ouverture des droits et actualisation informatique
- Recherche de mode de garde (Branche famille CAF)
- Vie quotidienne et organisation : comment appréhender le changement à venir et trouver des solutions adaptées
- Rapport à soi et aux autres : (parentalité, conjugalité, féminité, masculinité, santé)
comment être en harmonie avec soi et les autres au sein du changement.

ATELIER : CONSTRUCTION DU PROJET PROFESSIONNEL/ PROJET DE VIE

- Détermination ou émergence d'un projet personnel (connaissance de soi, plan d'actions)
- Phase d'exploration (découverte des métiers et formations)
- Immersion en milieu professionnel (stage, mise à disposition, PMSMP)
- Recherche d'emploi intensive et/ ou formation professionnelle

ATELIER RECS : RESEAU D'ECHANGES, DE COMPETENCES ET DE SAVOIRS/ CITOYENNETE ET BIEN-ETRE

- RECS :
  - Valoriser les compétences et les savoirs dans le cadre d'échange
  - Favoriser l'entraide, la cohésion et le savoir-être ensemble
- Atelier citoyenneté : être acteur dans la société
- Atelier bien-être : prendre soin de soi (relaxation, gestion du stress, socio esthéticienne, art thérapie….)
- Atelier créatif : développement des capacités, valorisation par le biais d'activités ludiques et manuelles

Version nov 2018 Ne pas jeter sur la voie publique

B) Invitation letter for the second cohort

Figure B.24: Letter in the 2019 and subsequent cohorts

Laxou, le 09 Novembre 2018

Madame XXXXXXXX XXXX
XXXXXXX
XXXXXXX

54000 NANCY

Dossier suivi par
ARELIA – Dispositif RELIANCE
Tél : [redacted]

Tout sur le RSA en Meurthe et Moselle :
www.insertion.meurthe-et-moselle.fr

Objet : RSA – Information collective RELIANCE

«Civ»,

La Caisse d'Allocations Familiales, le Conseil Départemental de Meurthe-et-Moselle ainsi que la Caisse des Dépôts et Consignation organisent une **action d'accompagnement destinée aux chefs de familles monoparentales** appelée RELIANCE. Dans ce cadre, vous avez été identifié(e) pour y participer.

L'objectif de cette action d'insertion est de vous soutenir dans la construction et la réalisation d'un projet professionnel personnalisé. Elle comprend également de nombreux ateliers organisés autour de l'accès aux droits, à la santé, à la citoyenneté, à la garde d'enfants…

Une information détaillée du dispositif vous sera présenté le :

Date : **Lundi 10 Décembre 2018 de 09H30 à 11H00**

Lieu : **ARELIA RELIANCE**
9-11 rue Robert Schuman, 3ème étage
54500 VANDOEUVRE LES NANCY

(Voir plan au dos)

Je vous informe que **votre participation à cette réunion s'inscrit dans vos obligations relatives à la législation du dispositif RSA.**

Merci de vous organiser pour vous rendre disponible. Néanmoins, **en cas de difficultés de garde**, nous pouvons vous accueillir en présence de vos enfants.

Si vous ne pouvez pas venir à cette réunion, nous vous demandons de téléphoner au [redacted] **réception de cette lettre**, pour nous en informer.

Dans l'attente de vous rencontrer, je vous prie de recevoir, Madame, l'expression de nos salutations distinguées.

Pour le Président du Conseil Départemental et par délégation,

[redacted]
Responsable du Service Economie Solidaire et Insertion

Reliance est un dispositif porté par l'association Arélia, en partenariat avec Ulis et Ecoval

C) Adaptation of the programme during the pandemic

The Covid-19 epidemic and the sanitary restrictions implemented in France from March 2020 onwards had a significant impact on the course of support and, more importantly, on the living conditions, prospects, and opportunities for this particularly vulnerable population.

The year 2020 was marked by the impact of lockdown measures, curfews, and activity restrictions in certain sectors, to varying extents. The exceptional measures taken by authorities to support businesses and households likely mitigated the recessive effects of the health crisis to some extent. However, the economy has been durably affected, and vulnerable populations have been particularly impacted (Duvoux and Lelièvre 2021).

The first lockdown was implemented just as the participants of the 2020 cohort were starting their support journey. One challenge for the Reliance team was to maintain a connection despite the lockdown. To achieve this, a coordinator was assigned to regularly call all participants and answer their inquiries during extended time slots, including evenings and weekends. Social media, particularly a Facebook group, was utilised, with the coordinator regularly sharing diverse content ranging from recipes and activities to job-related quizzes, documentation, and information, etc.

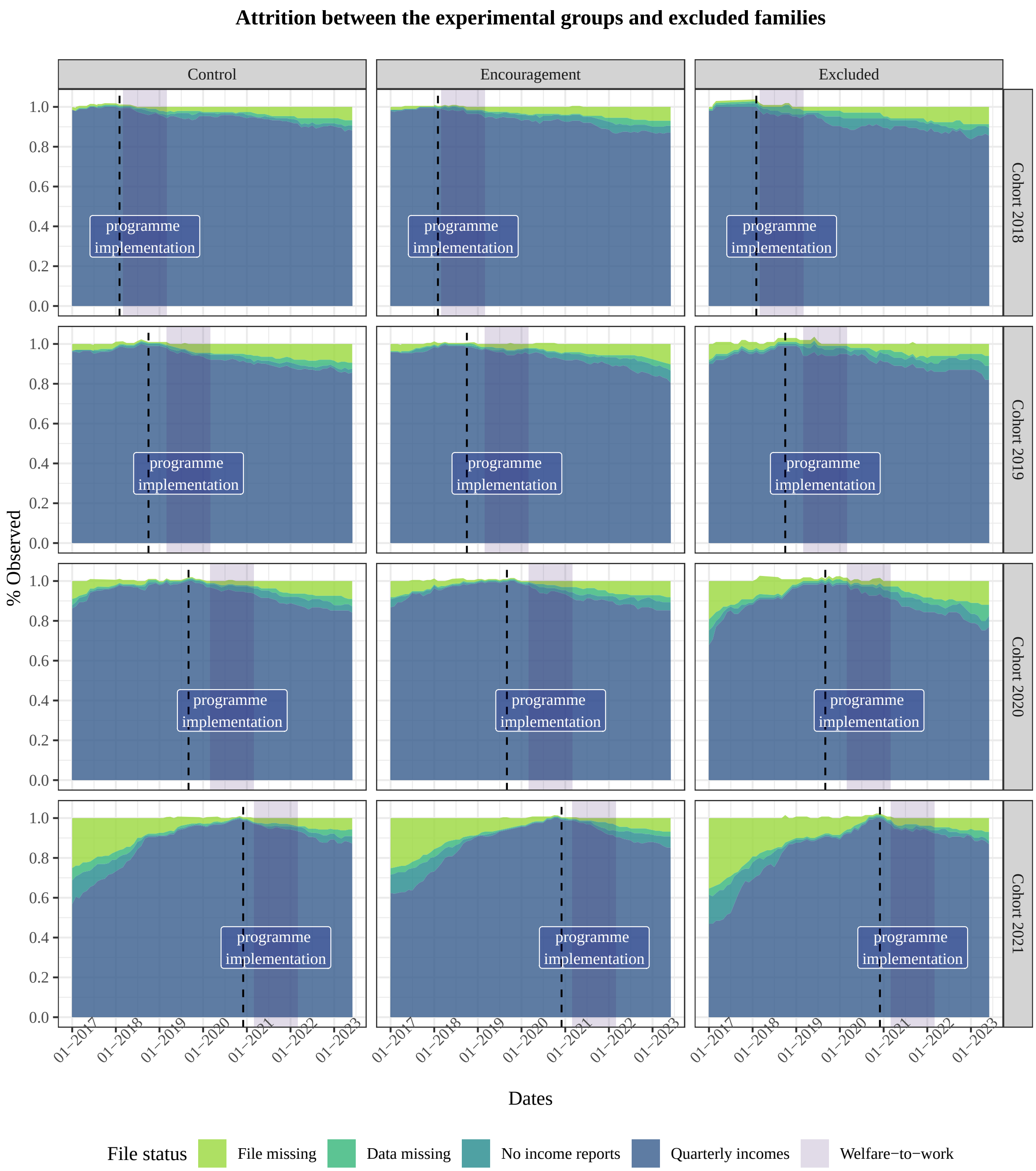
The Reliance team also identified limited access to digital resources for some participants. Consequently, computer equipment (laptops, tablets) was provided for free to certain participants with the support of the Caf and the Departmental Council. Social workers also arranged for food parcels to be delivered to homes and printed important documents for beneficiaries who no longer had access to printing facilities. A mask manufacturing plant offered employment opportunities for participants to make masks, and volunteer sewing workshops for mask-making were organised.

The qualitative evaluation highlighted that contact was generally well maintained, and those interviewed felt supported. However, activities initiated before the lockdown were put on hold (such as driver's licence exams, workplace internships, access to rights, etc.), and the Reliance team noticed a greater level of "disengagement," particularly less participation in collective activities despite the implemented safety measures. FORS (2020) notes explanations from participants, such as fear of rejoining groups in the current health context or overwhelming anxiety. Overall, the lockdown period was anxiety-inducing for Reliance's supported individuals, who once again found themselves isolated, potentially exacerbating their difficulties. However, FORS also highlights that this period helped re-motivate some participants despite the context.

B.II Attrition

A) Descriptive statistics

Figure B.25: Share of income data available across time and cohorts



Sources: ALLSTAT 2017−01−01 to 2023−06−01
Proportions of the baseline population observed or not at each date
in the experimental group and excluded families.

B.III Balance check at the time of random assignment

Table B.3: Balance of main variables of interest the month before randomisation

	Control (N=828)		Encouragement (N=843)		Diff. in Means	Std. Error
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*		
Share labour income >0	0.08	0.28	0.08	0.27	-0.01	0.02
Share receive RSA	0.98	0.15	0.98	0.14	0.00	0.01
Mean monthly total household's incomes	1394.69	493.21	1398.98	497.86	4.37	24.08
Mean monthly household's incomes per CU	708.59	150.11	712.19	157.06	3.70	11.51
Mean monthly total social transfers	1297.67	497.91	1292.00	481.42	-7.10	17.99
Mean yearly taxable income N-2	1424.11	2871.59	1599.39	3315.34	178.27	206.54
Favourable assessment	0.66	0.47	0.69	0.46	0.03	0.03
Mean distance (km) to the programme	3.32	1.86	3.49	2.01	0.17	0.12
Share French	0.81	0.39	0.84	0.37	0.03	0.02
Share higher education	0.51	0.50	0.53	0.50	0.02	0.03
Share lower education	0.25	0.43	0.24	0.43	0.00	0.03
Share unknown education	0.24	0.43	0.23	0.42	-0.01	0.02
Mean age	36.04	7.95	36.14	7.75	0.08	0.46
Mean age youngest child	7.13	5.56	7.16	5.49	0.03	0.34
Mean age oldest child	11.41	6.28	11.32	6.12	-0.11	0.34
Share with children under 2	0.31	0.46	0.29	0.45	-0.02	0.03
Share with children 3 to 5	0.33	0.47	0.32	0.47	-0.01	0.03
Share with one child over 16	0.32	0.47	0.30	0.46	-0.03	0.03
Share receive family allowance	0.57	0.50	0.56	0.50	-0.01	0.01
Share receive family supplement	0.17	0.38	0.17	0.38	0.00	0.03
Share receive housing benefit	0.89	0.31	0.88	0.32	-0.01	0.02
Share receive family support allowance	0.65	0.48	0.64	0.48	-0.01	0.02

Table B.3: Balance of main variables of interest the month before randomisation

	Control (N=828)		Encouragement (N=843)		Diff. in Means	Std. Error
	Mean	*Std. Dev.*	*Mean*	*Std. Dev.*		
Share Receive child support	0.21	0.40	0.21	0.40	0.00	0.02
Share receive Early childhood allowance	0.31	0.46	0.29	0.45	-0.02	0.03

* = p<.1, ** = p<.05, *** = p<.01
Sources: ALLSTAT, cohorts 2018 to 2021 one month before randomisation.
Notes : mean and mean differences are weighted within-block averages.
Standard errors account for block randomisation.

B.IV Participation across cohorts

Table B.4: Average effects of encouragement on participation by cohort

	sample				
	Full sample	*Cohort 2018*	*Cohort 2019*	*Cohort 2020*	*Cohort 2021*
Encouragement	0.389***	0.279***	0.363***	0.421***	0.472***
	(0.021)	(0.027)	(0.027)	(0.039)	(0.045)
Num.Obs.	1671	395	397	386	493
R2	0.286	0.195	0.249	0.300	0.354
R2 Adj.	0.254	0.156	0.214	0.266	0.329
Std.Errors	by: strataXc	by: strataXc	by: strataXc	by: strataXc	by: strataXc
FE: strataXc	X	X	X	X	X

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

Sources: Sample: Cohorts 2018 to 2022 at the time of randomisation.
OLS regressions of participation on encouragement, recentered by within-block propensity scores and blocks x cohort FE. Cluster robust standard errors adjusted by blocks x cohorts in parenthesis.

C Data preparation and cleaning

C.I Main databases: characteristics and pre-treatment

Description of the source files The source file contains three databases built from the `ALLSTAT FR6` monthly records from the National family allowance fund matched with design variables. All baseline table contains 175 columns.

- `BaselineClean` contains 2662 observations of the 5 cohorts, including those with unfavourable initial assessment and the 53 households from the pilot study. It was extracted from the complete dataset at the month before random assignment.
- `Reliance` contains 207636 individual × months observations, with missing values when unobserved
- `RelianceNoMissing` contains 194264 individual × months observations where lost households have been dropped.

We load and match two additional sources from INSEE: monthly minimum income and consumer price index for the bottom 20% of the national income distribution with reference value in 2015. Since many social transfers depend on the full-time minimum wage-level, it is useful to also define labour incomes as share of a full-time minimum wage. Then, we can use these variables with simulations of the tax-benefit system and use the *static* implicit marginal taxation rate in bunching estimates.

We drop the data of a file whose separation occurred the month of sampling from provisional datasets and whose status was only stabilised 2 months after random assignment. This household was in the encouragement group of the 2019 cohort and did not participate.

Pre-treatment of baseline characteristics There are 32 (1.5%) housholds whose initial incomes are missing, but whose total disposable incomes indicate that they have no income. We therefore impute 0 for these files. There are also 6 households who have no children in custody at that month. They all have one child with intermittent patterns of custody. We assign sample means for children age at baseline.

All monetary measures are converted in 2015 values, and we *winsorise* the .1% with extreme values, most likely due to misreports. We create dummies for the covariates used in the analysis and *distributional dummies* $i.e.$ for fractile of income distributions.

We centre all continuous covariates by the sample mean in their block. This transformation is useful when using models in long difference as it retrieves the average of the outcome at the observed month while removing baseline differences between blocks.

This baseline table is then matched to the full dataset by their Unique id.

Main databases The main database contains data from January, 2017 to ‘r June, 2023 for 2662 households, among which 548 were excluded from the experimental sample and 53 from the pilot. Removing them from the 207636 observations leaves us with 161694 observations.

Since we can only observe the first cohort from -13 months from random assignment and the fourth cohort up to 30 months after, we only keep observations of cohort 2018 to 2021 between these relative months from random assignment. This leaves us with a dataset of 103066 observations.

Last, two blocks with 5 have less than two observations due to the two files that we removed because they were ineligible. We remove their 317 observations.

The final database contains 102749 observations for 1666 households.

Post-treatment and aggregated databases We construct three databases with aggregated outputs or limited time frame.

- Average income from 18 to 30
- Total income from 18 to 30
- Binned worked-month $\times$ individuals

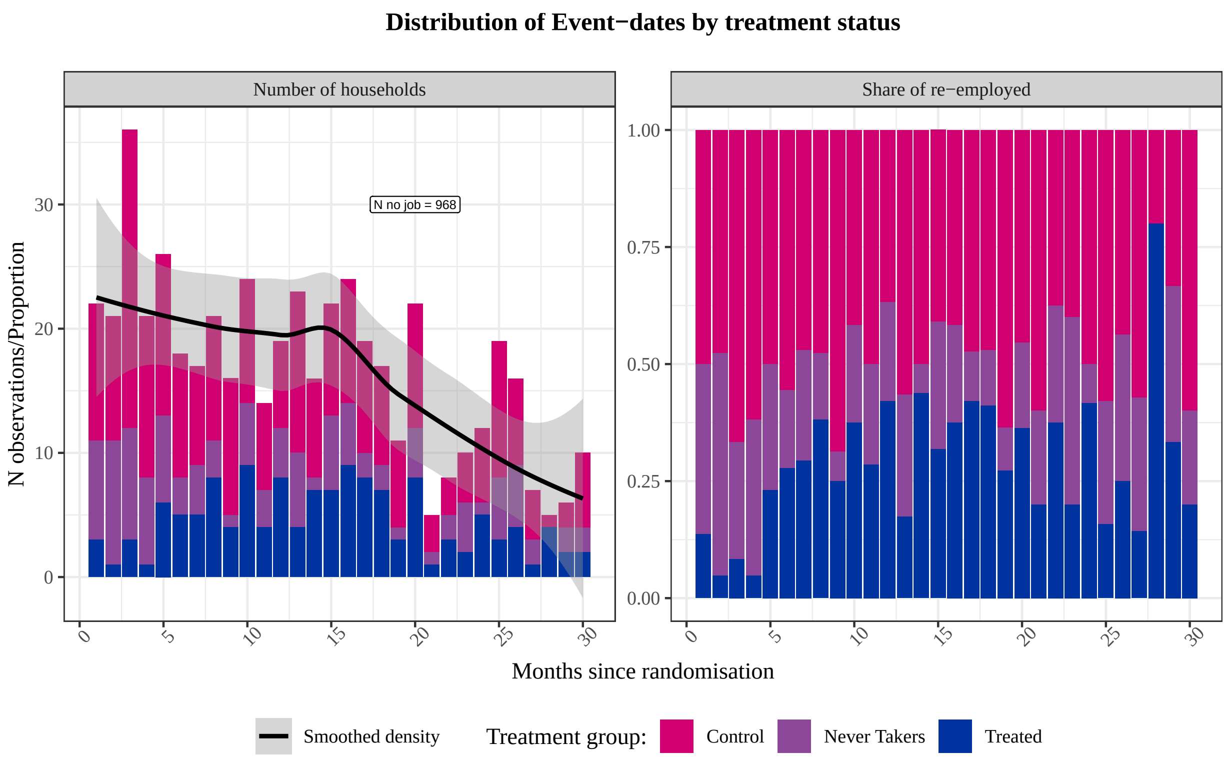
The two first datasets contain one observation per individual and compute the relevant aggregate outcomes. Some statistics are computed over the entire periods, others over months with positive labour incomes.

The last database records every wage in bins of 5% of the full-time minimum-wage over the same number of months for all households. This database contains as many different bins of income individuals ever recorded, and how many times they did. We use them for the bunching analysis.

D Additional descriptive statistics

D.I Distribution of dates of first job re-entry

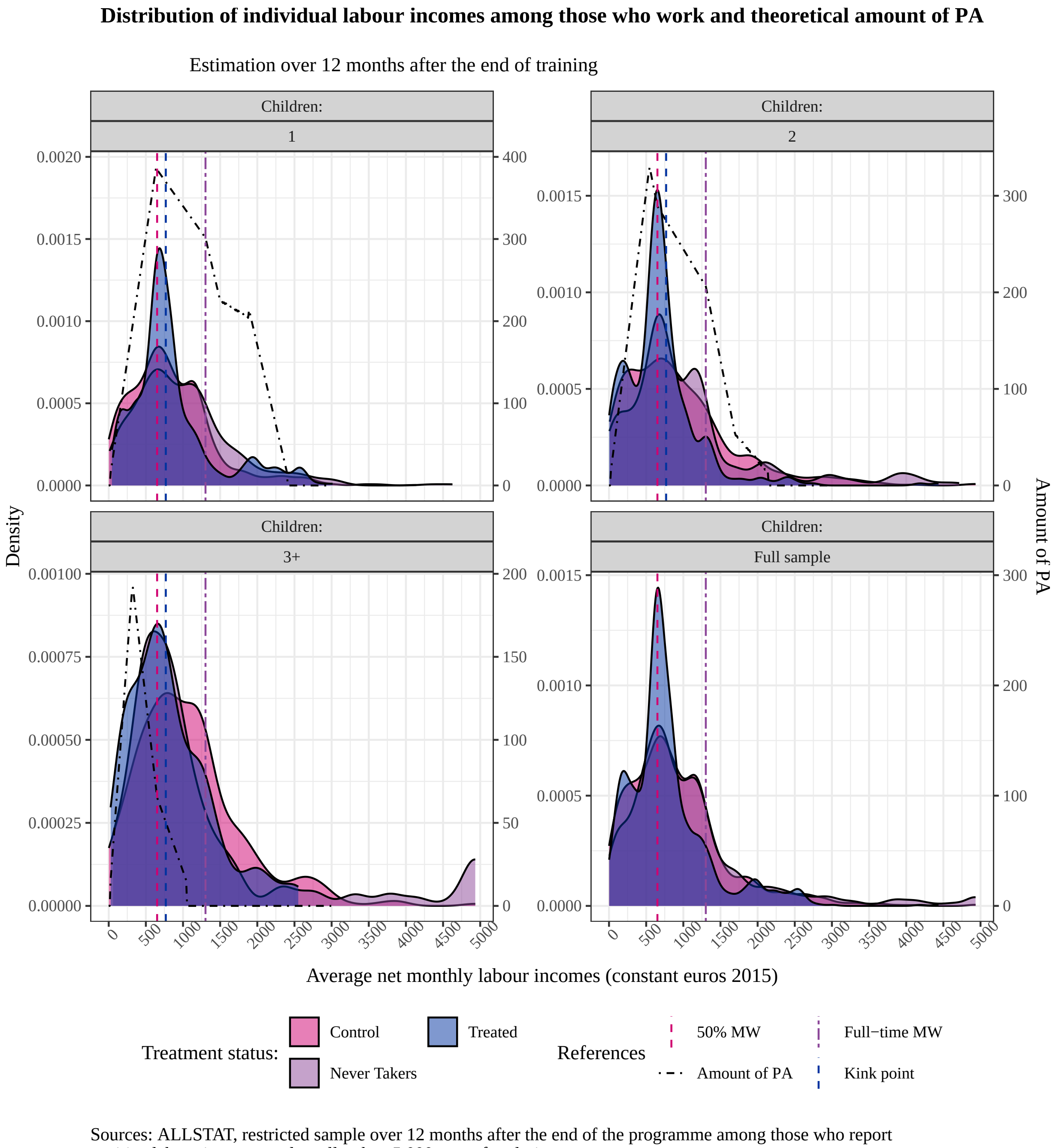
Figure D.26: Compliers mostly start jobs in the last 6 months of the programme



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021 from 0 to 30 months from random assignment.
We omit the bar with 0 event for clarity and report their number in the label of the top panel.
Stacked bar chat of the number of households with first job re−entry at the month since random assignment.
Smoothed density use local polynomial regressions of the number of observation on group−time.

D.II Bunching at the individual labour income

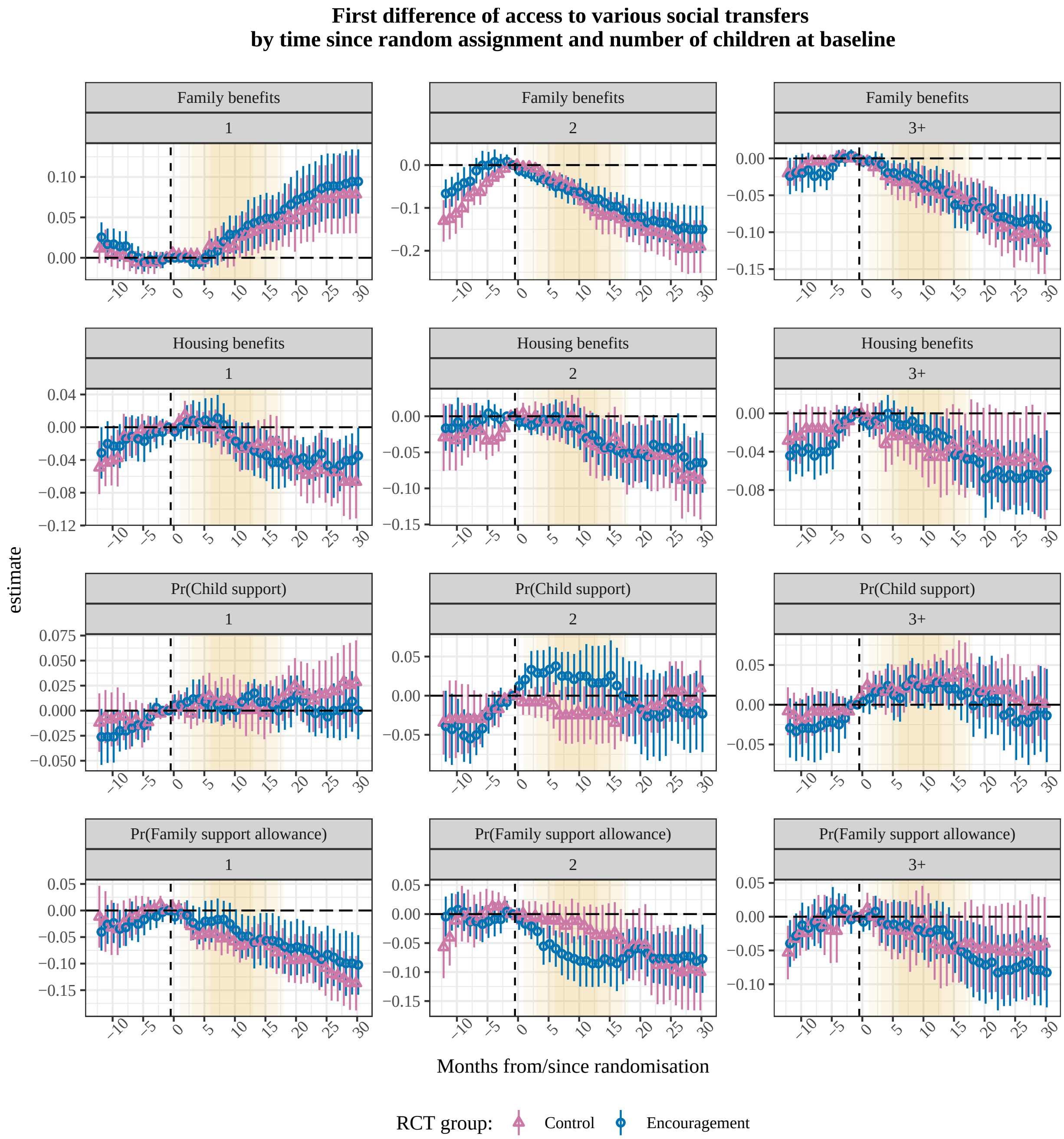
Figure D.27: Bunching of individual labour incomes at the sweet spot



Sources: ALLSTAT, restricted sample over 12 months after the end of the programme among those who report positive labour incomes and smaller than 5 000 euros for clarity.
Notes: Kernel density of individual labour income for those with positive labour incomes.
The PA reference line indicates the theoretical amount of in−work benefits received for single parents by number of children and net labour income based on the EDIFIS model using the 2022 legislation.
Kink points indicate the level of income that minimises the implicit marginal tax rate.

D.III Mean differences in access to different sources of incomes

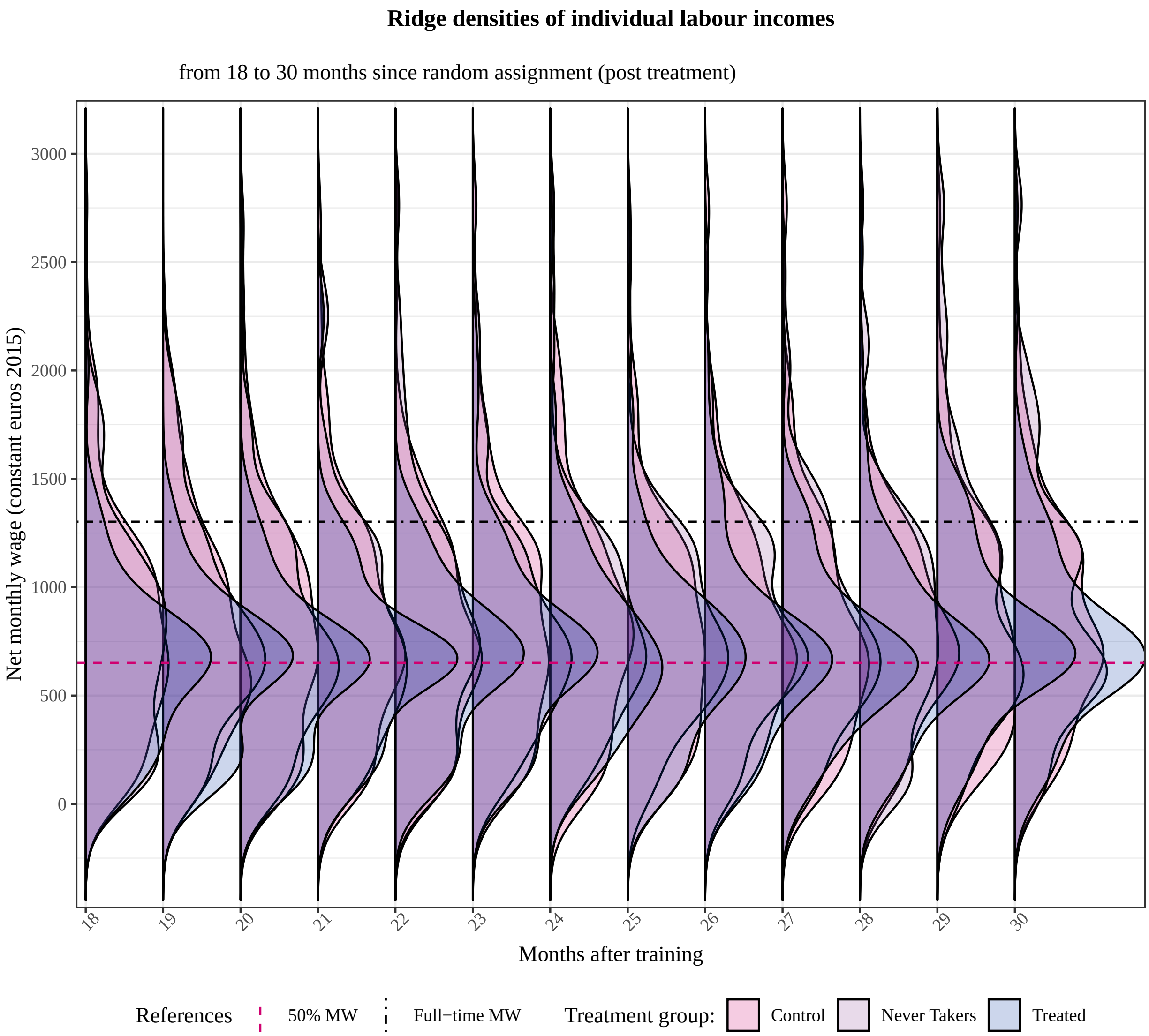
Figure D.28: Change in child support for mothers of 2, slower decrease of family benefits for mothers of 3 or more



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021 from −12 to 30 months from random assignment.
Means and point−wise 95% confidence intervals estimated by OLS regressions on month x group x encouragement dummies without intercept, using cluster−robust standard errors adjusted at the block x cohort level and inverse instrument propensity score weighting.
Rows correspond to different outcomes from separate regressions. Columns display results by number of children at baseline.

D.IV Monthly Densities of individual labour income from 18 to 30 months after random assignment

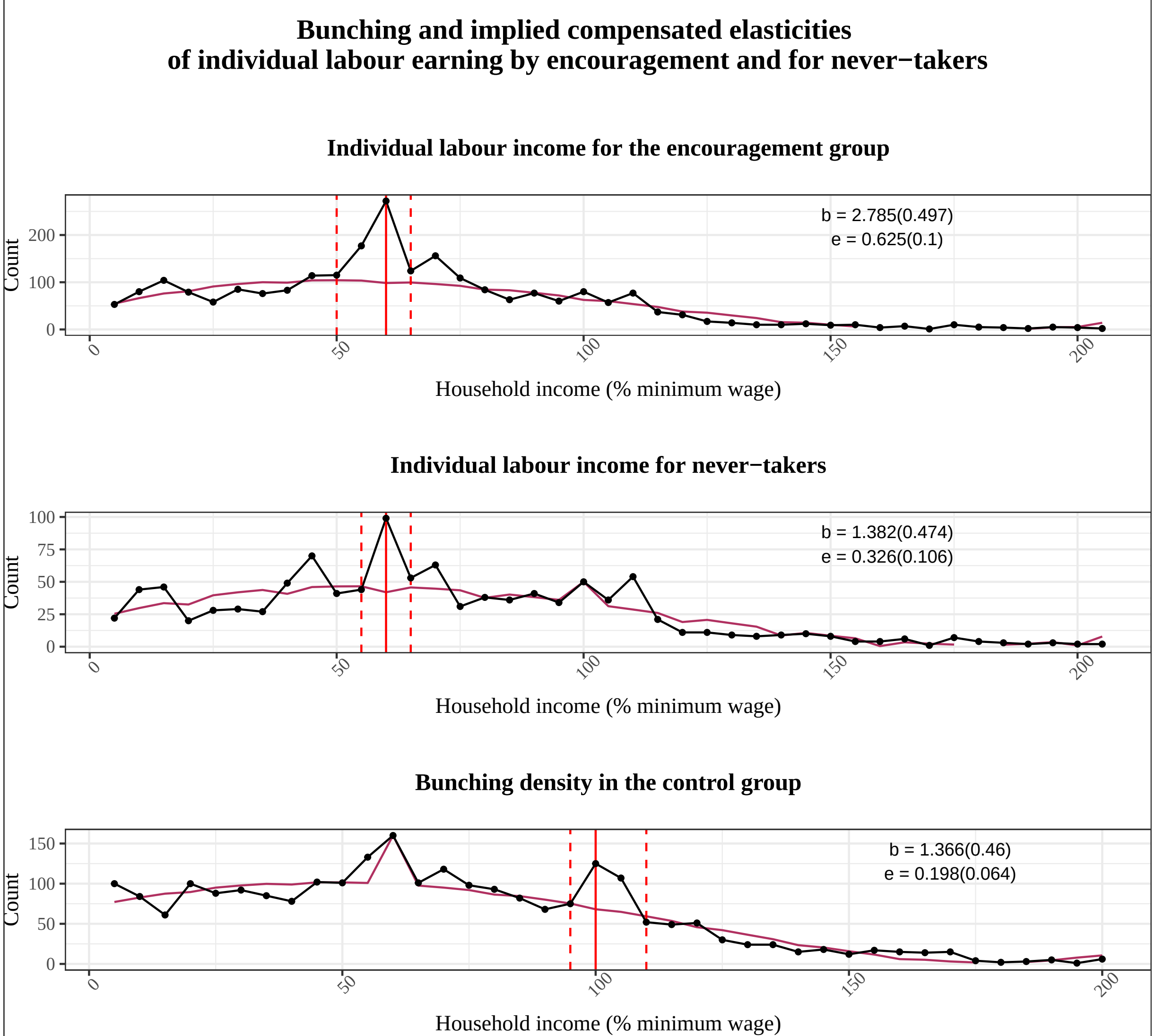
Figure D.29: Densities of individual labour incomes over the year after the end of training by number of children at baseline



Sources: ALLSTAT, cohorts 2018:2021, restricted sample over months 16 to 28 among those who report positive labour income. Densities are scaled separately for each month of observation.

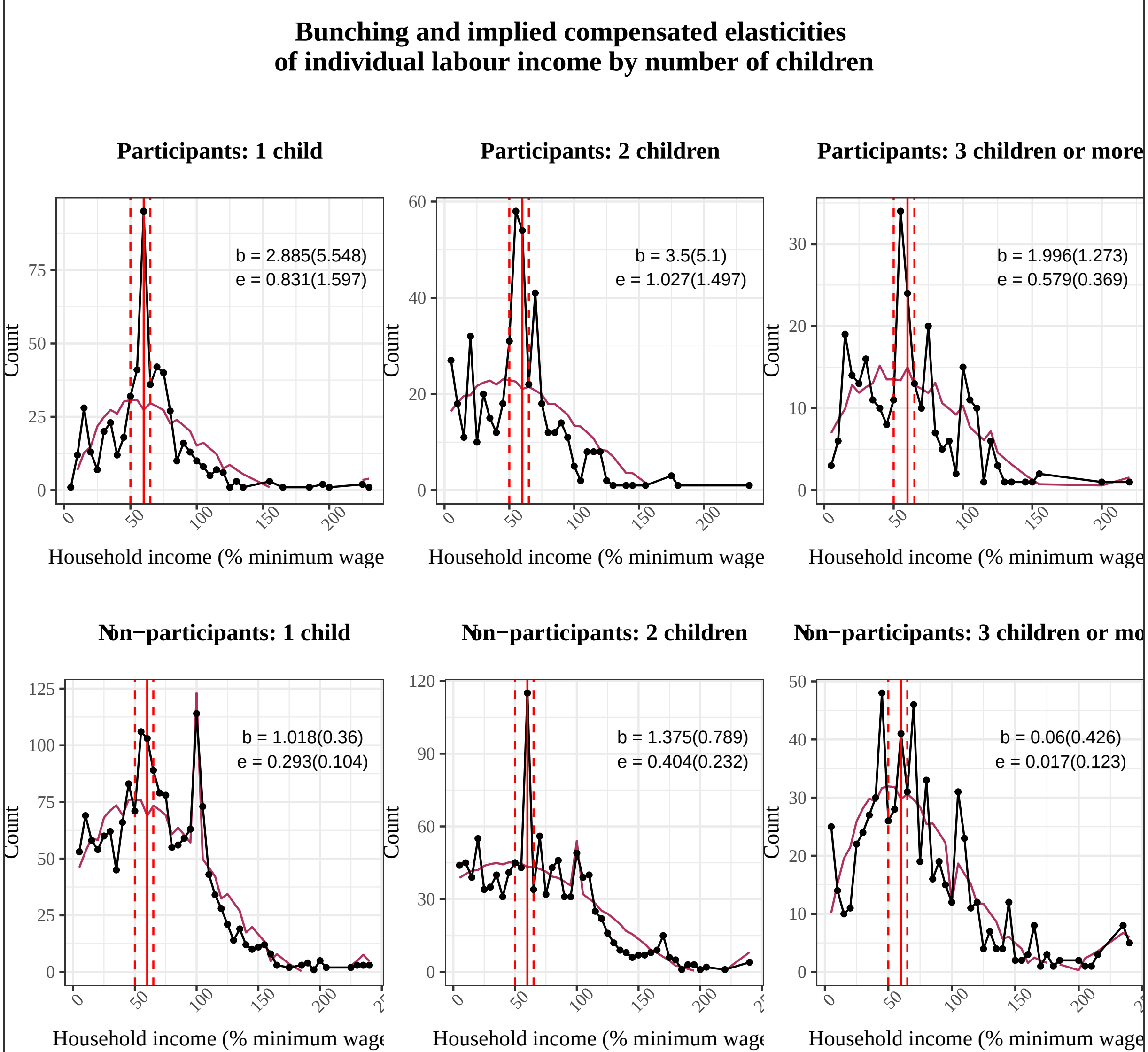
E Additional bunching estimates

Figure E.30: Comparing bunching of individual earnings between encouragement groups and never-takers



Sources: ALLSTAT 2017−01−01 to 2023−06−01 . Observations from 18 to 30 months after random assignment.
The database is collapsed into frequencies of individual labour incomes falling into bins of 5% minimum wage.
The statistical units are individual x months with positive labour incomes.
The top panel is from sub−sample of participants, the bottom panel is from the control group and never−takers.
Counterfactual densities estimated using polynomial of degree 4 , with round number dummies for every 20% SMIC corresponding to round number of days worked.
The bunching regions is delimited by the dashed red lines and we use the average
IMTR for single parents with 1,2,3 chidren presented in the table at the begining of the section.
Elasticities use parametric restriction and standard errors are computed by bootstrap using 499 replications.

Figure E.31: Bunching of individual incomes by number of children at baseline

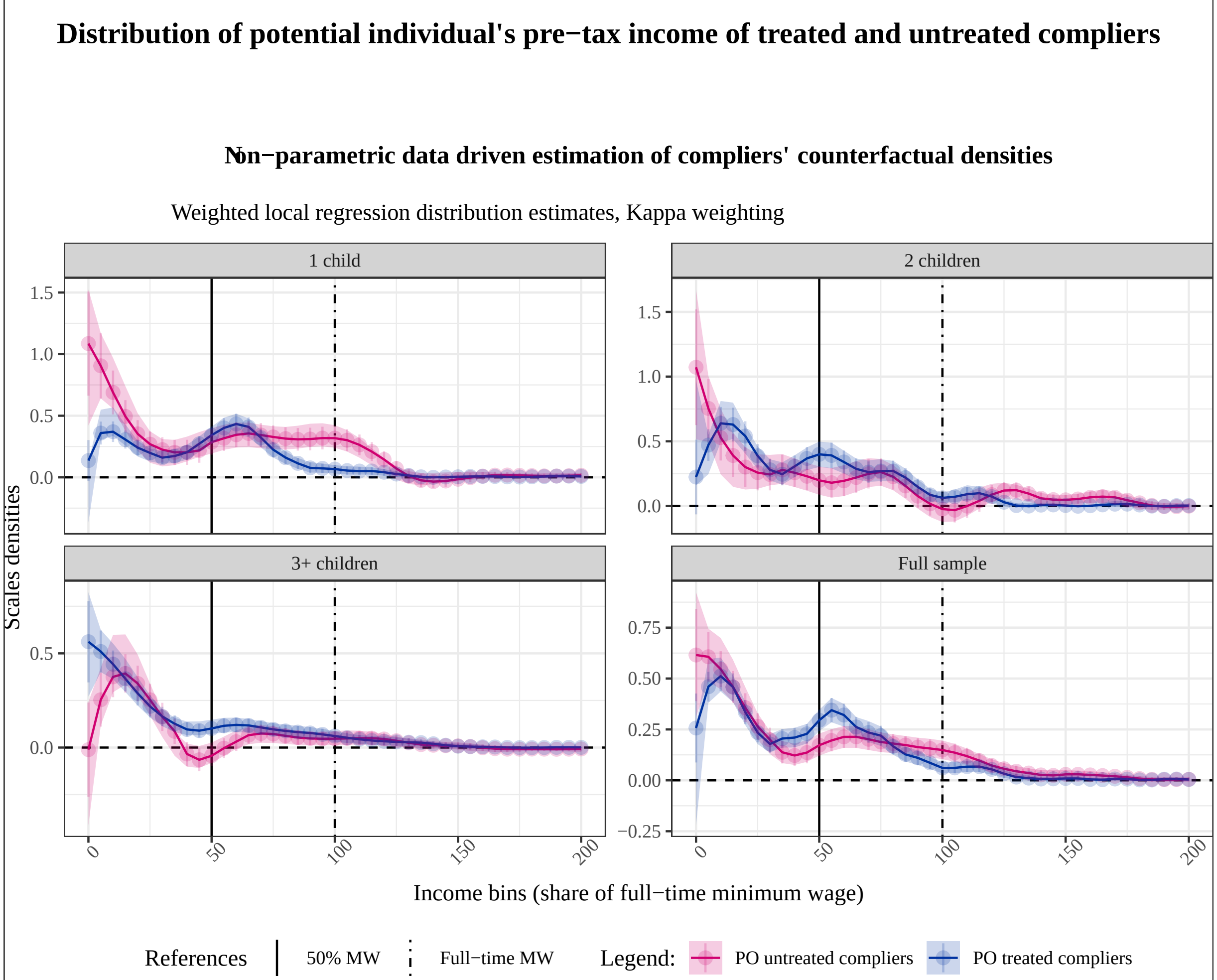


Sources: ALLSTAT 2017−01−01 to 2023−06−01 . Observations from 18 to 30 months after random assignment.
The database is collapsed into frequencies of individual labour incomes falling into bins of 5% minimum wage.
The statistical units are individual x months with positive labour incomes.
The top panel is from sub−sample of participants, the bottom panel is from the control group and never−takers.
Counterfactual densities estimated using polynomial of degree 4 , with round number dummies for every 20% SMIC corresponding to round number of days worked.
The bunching regions are delimited by the dashed red lines and we use the average IMTR for single parents presented in the table in the Bunching section for elasticities.
Elasticities use parametric restriction and standard errors are computed by bootstrap using 499 replications.

F Additional estimations of counterfactual densities

F.I Counterfactual densities by individual pre-tax incomes

Figure F.32: Counterfactual densities of compliers' individual income

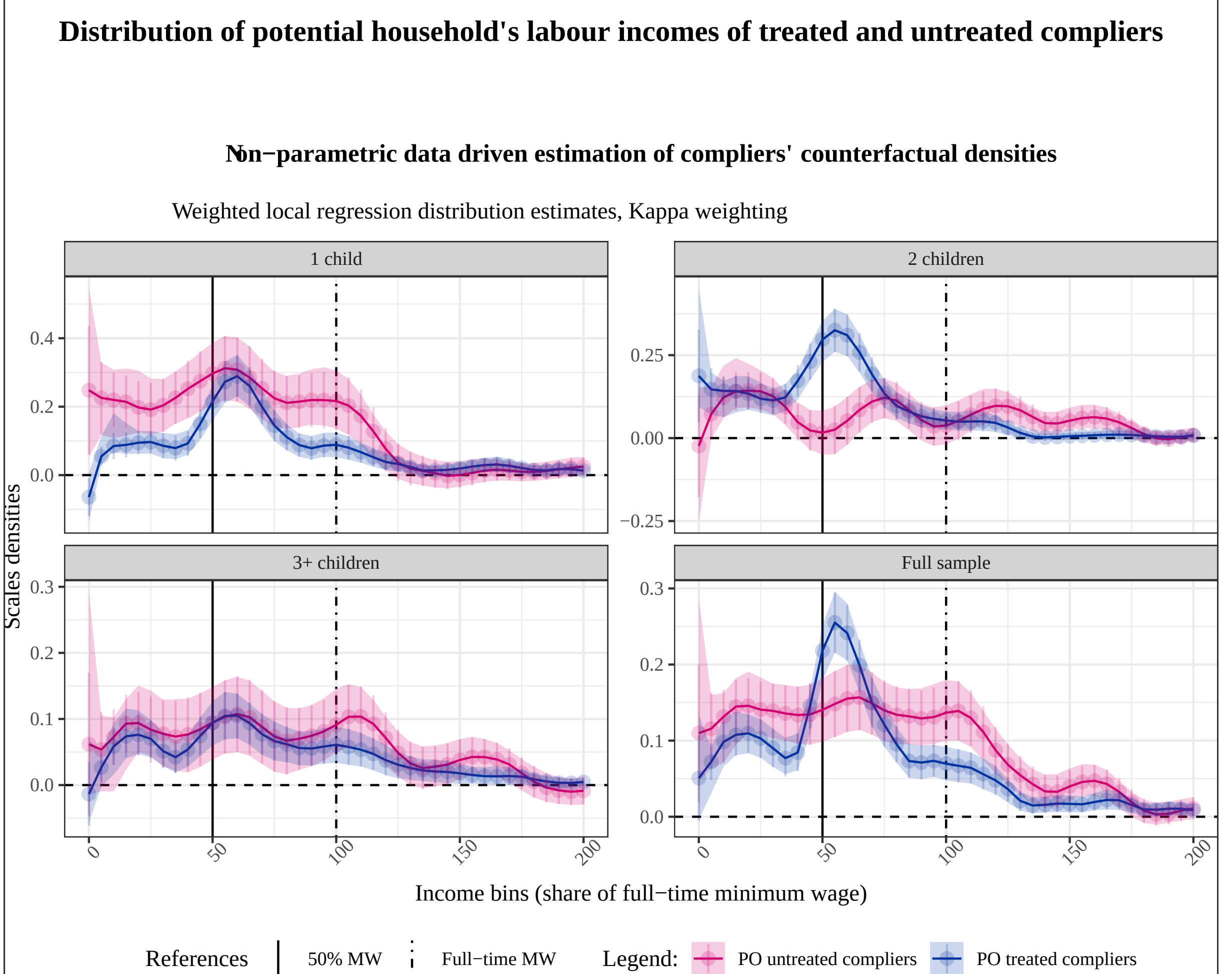


Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
The dependent variable is the individual's pre−tax income as share of full−time minimum wage.
Notes: Estimations of the potential density functions for compliers following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 3 , simultaneous 95% confidence intervals use polynomial of order 4 , 1 degree higher for biais correction.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re−centred instrument and block fixed effect, with T = D for Y(1) and T=(1−D) for Y(0).

Weights are computed for each sub−group separately: $\kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)}$ $\kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)}$

F.II Counterfactual densities of household's incomes

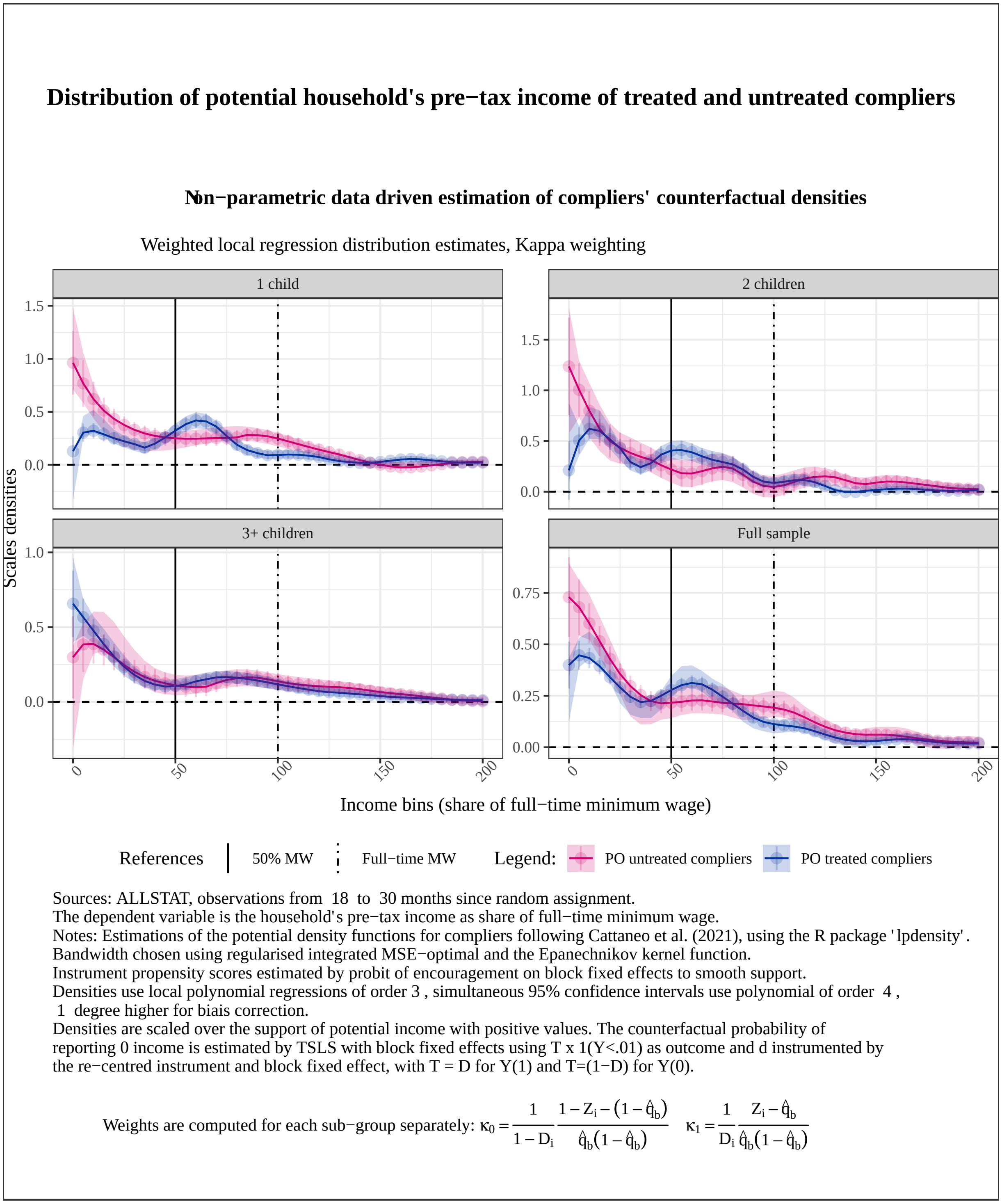
Figure F.33: Counterfactual densities of compliers' household labour incomes



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
The dependent variable is the household's labour income as share of full−time minimum wage.
Notes: Estimations of the potential density functions for compliers following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 3 , simultaneous 95% confidence intervals use polynomial of order 4 , 1 degree higher for biais correction.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re−centred instrument and block fixed effect, with T = D for Y(1) and T=(1−D) for Y(0).

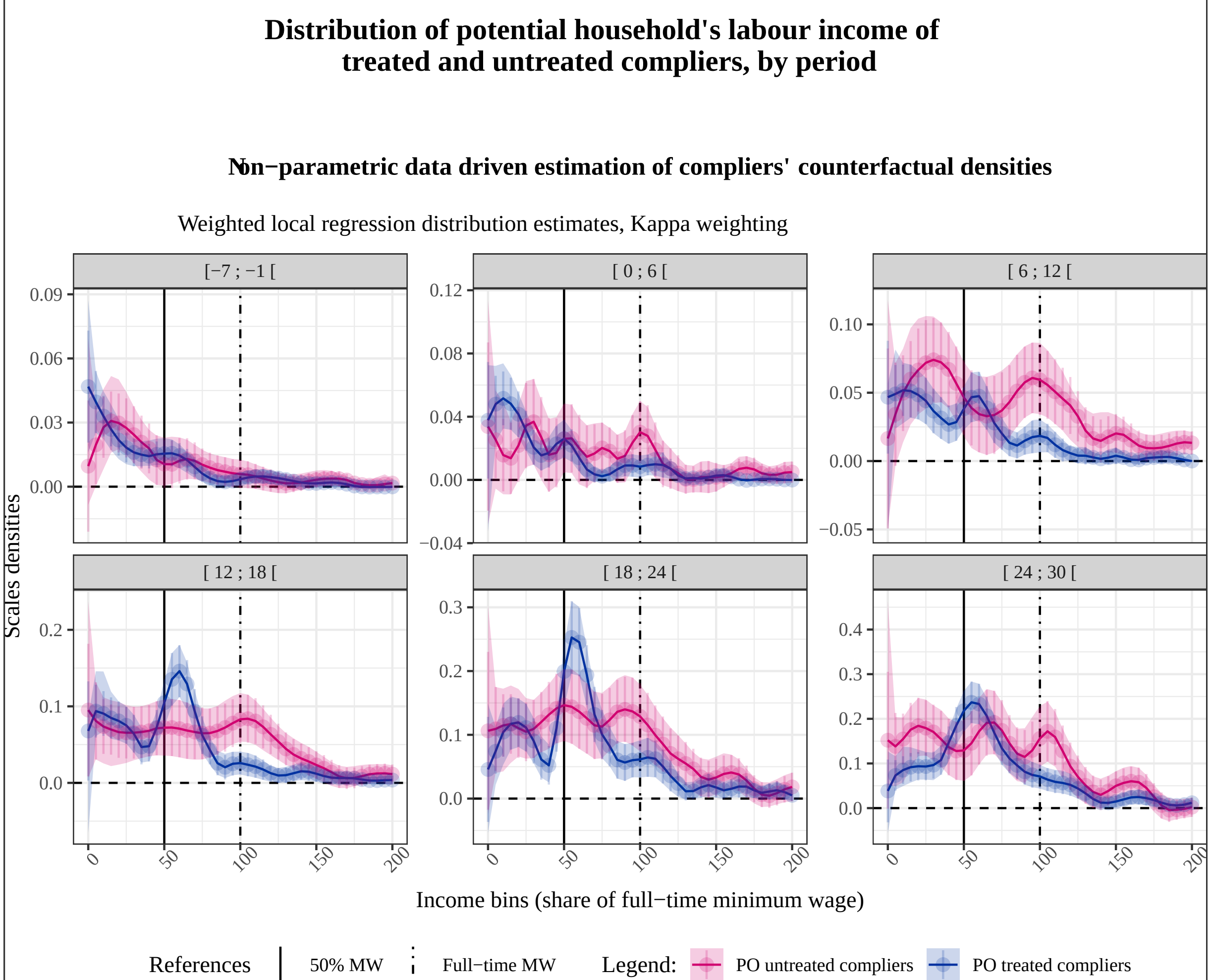
Weights are computed for each sub−group separately: $\kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)}$ $\quad \kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)}$

Figure F.34: Counterfactual densities of compliers' household pre-tax income



F.III Evolution of bunching over 6 month periods

Figure F.35: Evolution of potential household's labour income by 6 month periods

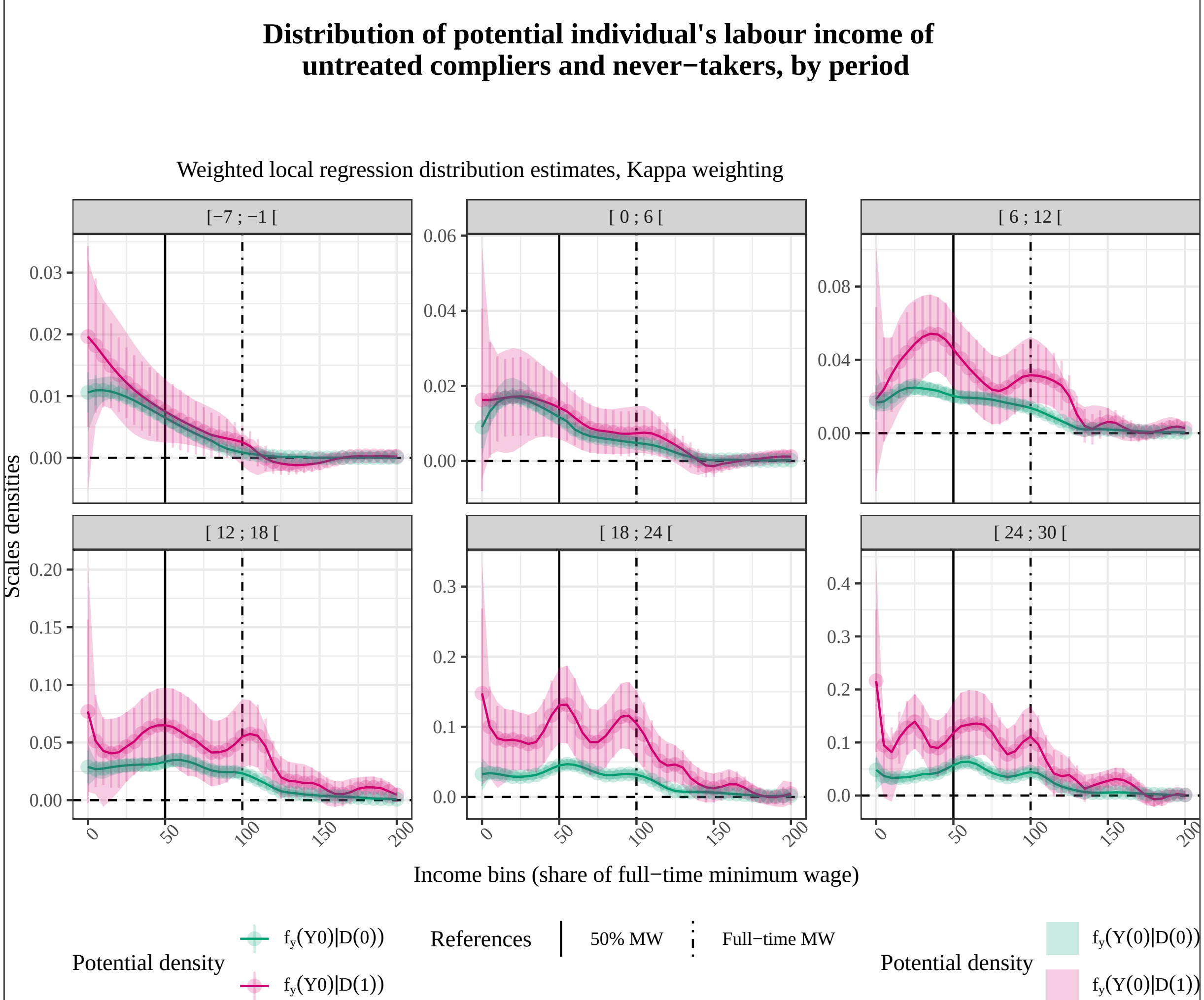


Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
The dependent variable is the household's labour income as share of full−time minimum wage.
Notes: Estimations of the potential density functions for compliers following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 3 , simultaneous 95% confidence intervals use polynomial of order 4 ,
1 degree higher for biais correction.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.000001) as outcome and d instrumented by the re−centred instrument and block fixed effect, with T = D for Y(1) and T=(1−D) for Y(0).

Weights are computed for each sub−group separately: $\kappa_0 = \frac{1}{1-D_i}\frac{1-Z_i-(1-\hat{q}_b)}{\hat{q}_b(1-\hat{q}_b)}$ $\kappa_1 = \frac{1}{D_i}\frac{Z_i-\hat{q}_b}{\hat{q}_b(1-\hat{q}_b)}$

F.IV Comparisons of individual labour incomes with never-takers

Figure F.36: Comparing potential individual labour incomes of untreated compliers and never-takers

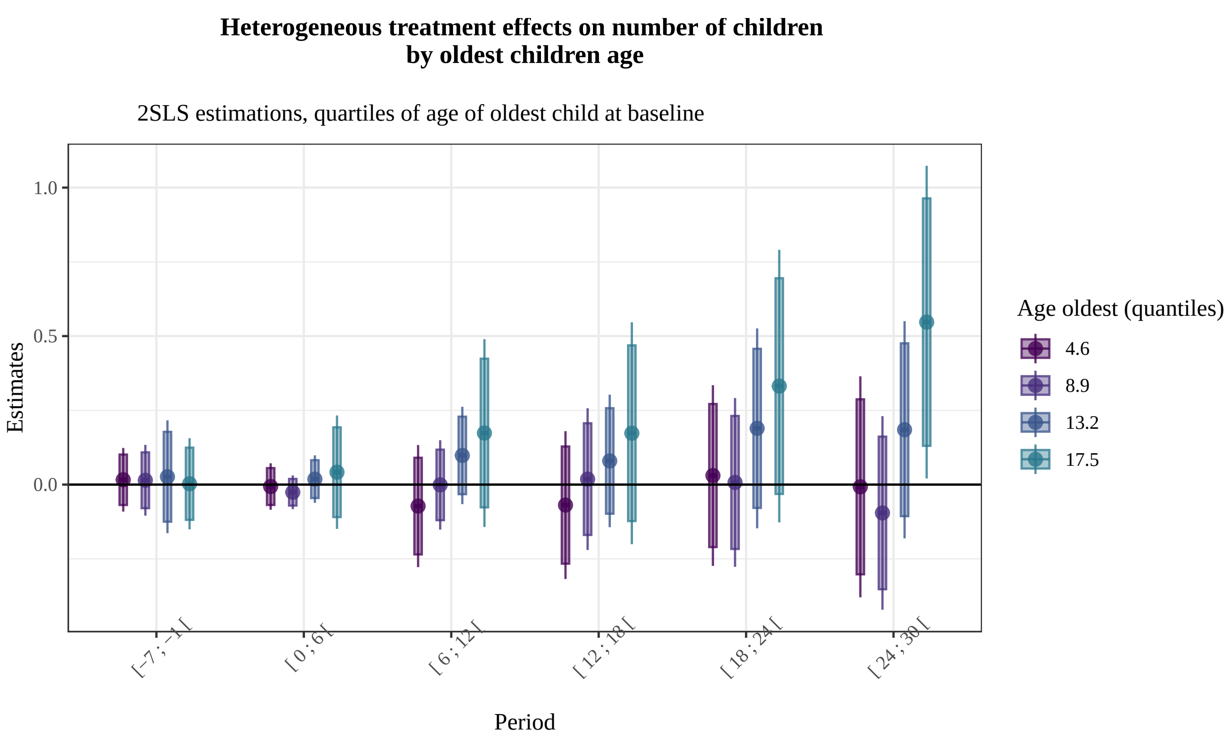


Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
Notes: Estimations of the potential density functions following Cattaneo et al. (2021), using the R package 'lpdensity'.
Bandwidth chosen using regularised integrated MSE−optimal and the Epanechnikov kernel function.
Instrument propensity scores estimated by probit of encouragement on block fixed effects to smooth support.
Densities use local polynomial regressions of order 2 , simultaneous confidence intervals use polynomial of order 3 , 1 higher to correct bias.
Densities are scaled over the support of potential income with positive values. The counterfactual probability of reporting 0 income is estimated by TSLS with block fixed effects using T x 1(Y<.00001) as outcome and d instrumented by the re−centred instrument and block fixed effect, T=(1−D) for Y(0).

Weights for untreated compliers: $\kappa_0 = \frac{1}{1 - D_i} \frac{1 - Z_i - (1 - \hat{q}_b)}{\hat{q}_b (1 - \hat{q}_b)}$

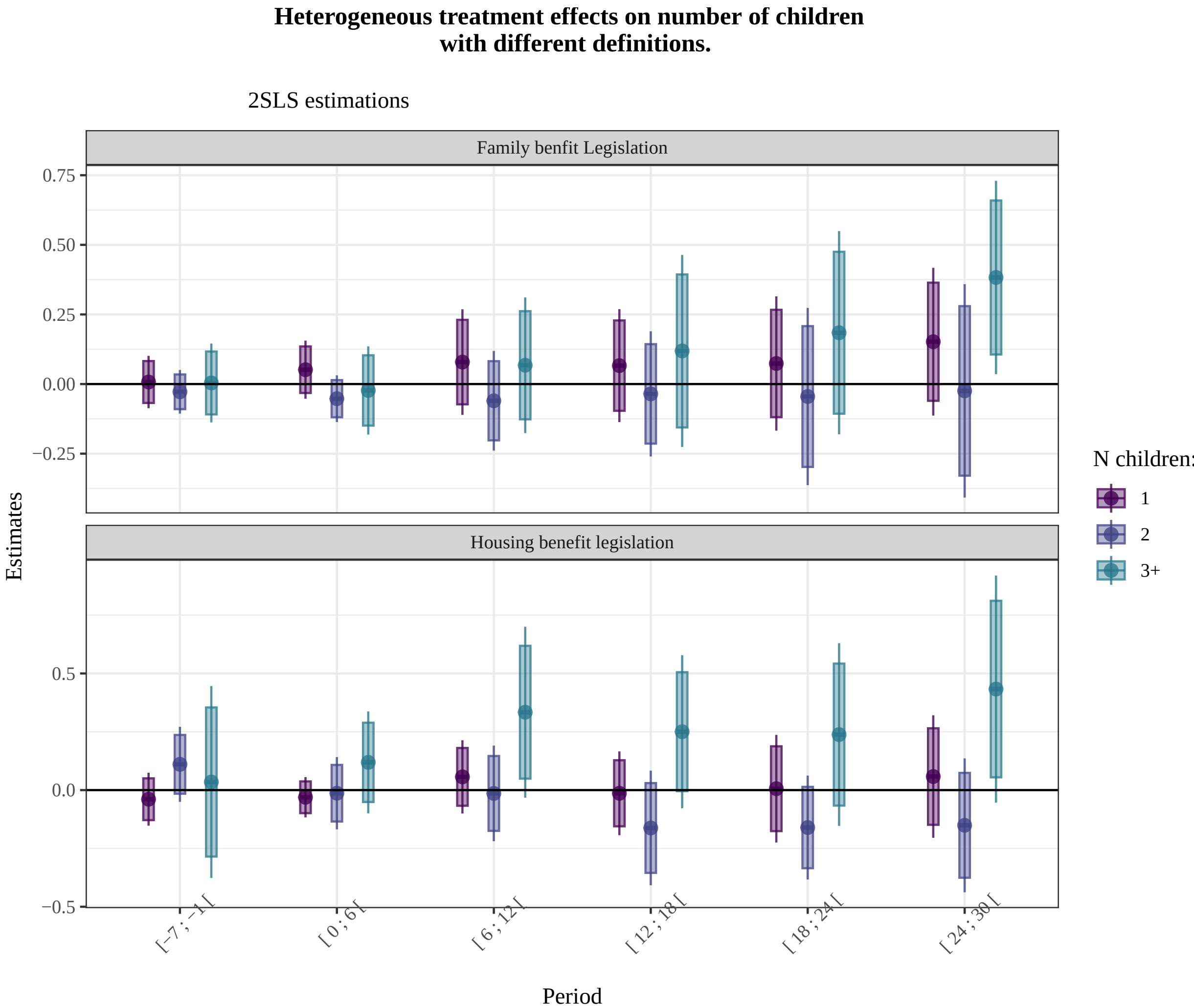
G Other estimations on family structure

Figure G.37: Heterogenous treatment effects on number of children, by quartile of oldest children at baseline



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021.
Notes: The dependent variable is the number of children under responsibility as used for family allowance benefits.
Cluster−robust standard errors at the block x cohort level.
− Error bars indicate 95 % pointwise confidence intervals and extended lines account for FWER by subgroup.
− All models include blocks x cohort x relative months fixed effects and covariates listed in the paper.

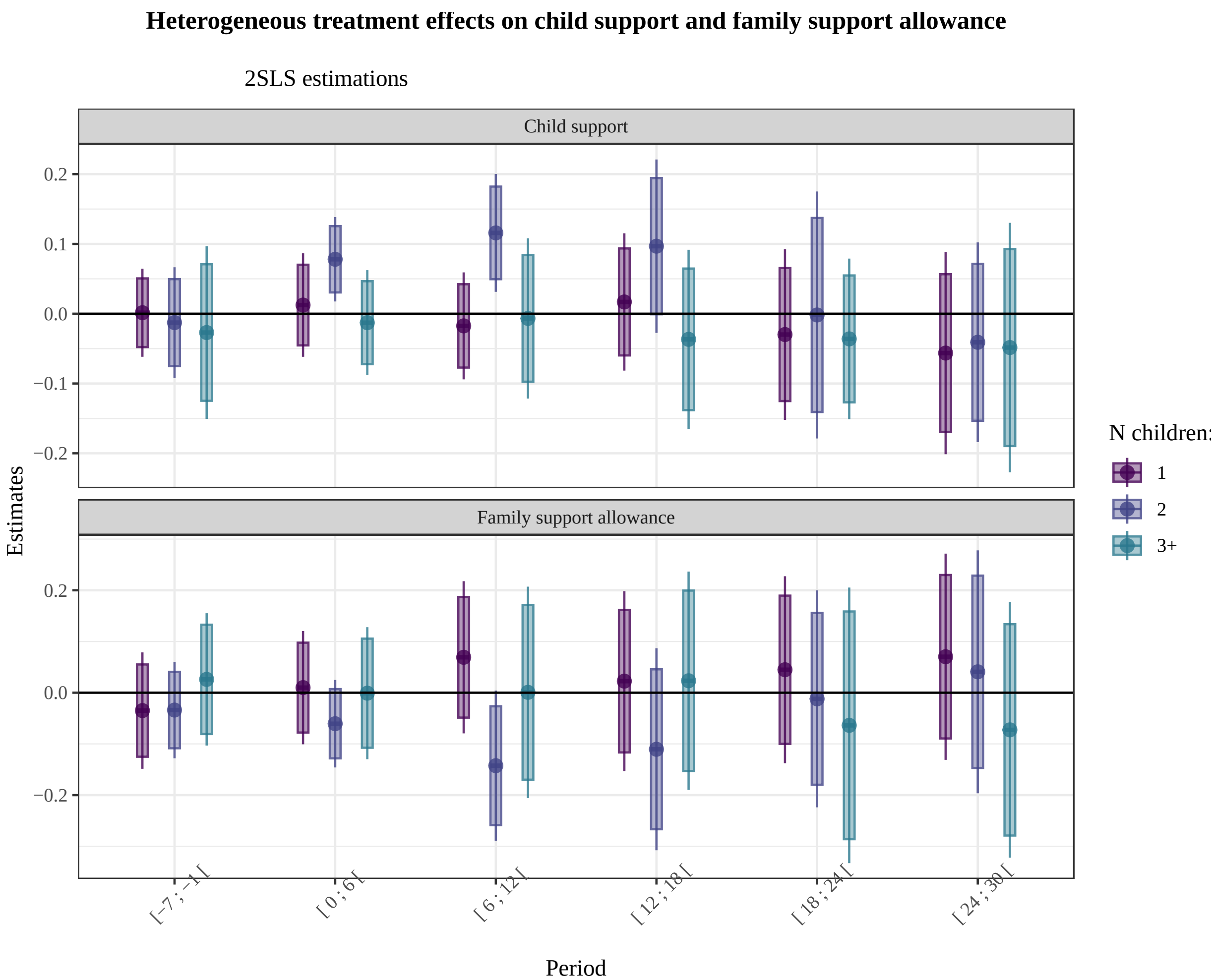
Figure G.38: Effect on different definition of number of children



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021.
Notes: The dependent variable is the number of dependent children either defined by familiy benefits or housing benefits.
Cluster−robust standard errors at the block x cohort level.
− Error bars indicate 95 % pointwise confidence intervals and extended lines account for FWER by subgroup.
− All models include blocks x cohort x relative months fixed effects and instrument propensity score weighting.

G.I Differential effects on child support and family support allowance

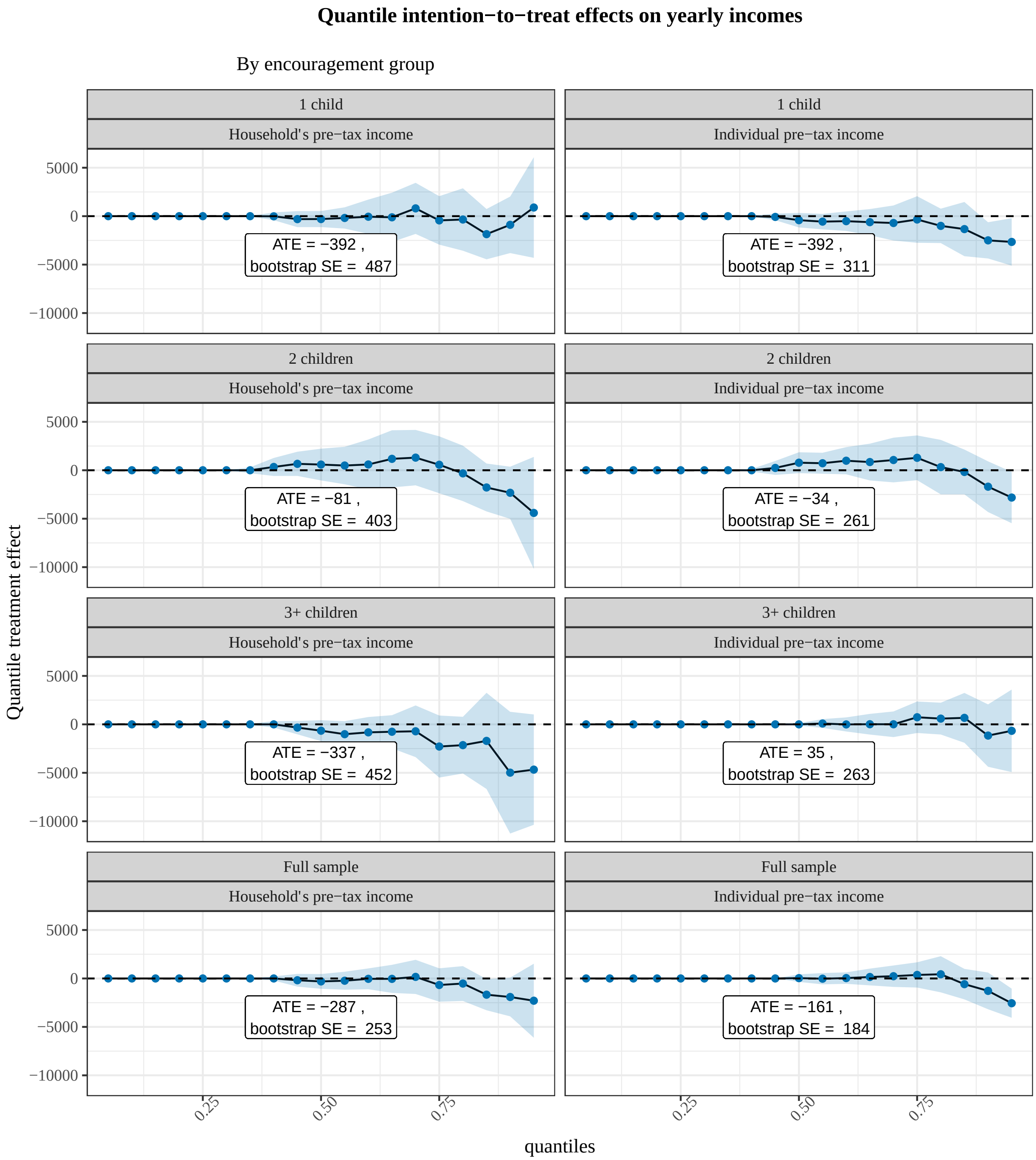
Figure G.39: Perfect substition of ASF and child support for parents of 2



Sources: ALLSTAT 2017−01−01 to 2023−06−01 cohorts 2018 to 2021.
Notes: The dependent variables are long difference between the date and the month before random assignment.
Child support is paid by non−custodial parents and deduced from other social transfers.
Family support allowance is paid in substitute of child support.
Cluster−robust standard errors at the block x cohort level.
− Error bars indicate 95 % pointwise confidence intervals and extended lines account for FWER by subgroup.
− All models include blocks x cohort x relative months fixed effects and instrument propensity score weighting.

G.II Quantile treatment effects on yearly pre-tax incomes

Figure G.40: Quantile treatment effects on cumulative pre-tax incomes over the year after the end of the programme



Sources: ALLSTAT, observations from 18 to 30 months since random assignment.
Notes: Estimations of the quantile intention−to−treat effect controling for blocks x cohort by inverse−propensity score weightingfollowing Firpo (2007). 95% Confidence intervals estimated by bootstrap.

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