Raising Take-up of Welfare Programs: Evidence from a Large French Reform

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July 2024

Abstract

Imperfect take-up of public policies has been a growing concern for modern welfare states. This paper examines the effect on claiming behaviors of two types of interventions, an increase in benefit generosity and an information provision. Exploiting exhaustive administrative data and a large welfare reform of an in-work benefit in France as a quasi-experiment, I find that information provision acted as an effective tool to raise participation while benefit generosity did not. Take-up responses to changes in monetary incentives to claim are not significant and small in magnitude, with an implied take-up elasticity of about 0.1. Instead, raising global awareness about the program led to an increase by 15% of the take-up rate. The marginal enrollee is more likely to be a childless single male but has on average lower income than the usual entrant. Drawing on the empirical findings, I build a theoretical framework to assess the welfare implications of policy interventions that can raise take-up and apply it to the French welfare reform.

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1 Introduction

Welfare programs are key policy instruments to alleviate poverty and redistribute resources to low-income households. Recent studies have also documented the positive effects of these policies on a wide range of other outcomes beyond redistribution, such as health, well-being, social cohesion, and public safety (Hoynes, Miller, et al. 2015; Deshpande and Mueller-Smith 2022). Additionally, these policies have been shown to generate significant long-term benefits in terms of education and income (Aizer et al. 2016; Hoynes, Schanzenbach, et al. 2016), suggesting they might serve as social investments that ultimately pay for themselves. Despite their large positive impact, the take-up of these programs is often far from perfect (Currie 2004; Ko and Moffitt 2022). For instance, in the U.S., the IRS estimates that 22% of eligible households did not claim the EITC in 2021. In the U.K., 33% of eligible households did not take up the WTC in 2019 according to the tax authorities. Non take-up rates as high as 40 to 50% have also been observed in programs in Finland and Germany (Bargain et al. 2012; Harnisch 2019).

Addressing welfare programs' non-participation is nonetheless challenging. There is a lack of evidence and consensus about which policy instruments can be effective in raising take-up. Additionally, improving participation can have ambiguous social welfare effects that should be carefully assessed to ensure that the selected policy intervention is indeed welfare-improving.

Imperfect take-up may be part of a constrained optimum because it may help policy-makers better target resources towards those the most in need. In a first-best world, eligibility criteria of these programs can be set such as to target exactly these individuals and full take-up is unambiguously optimal. In a second-best world though, because of asymmetric information, policy-makers can only condition transfers on observable proxies like income, but cannot directly target individuals based on their private unobserved type, like skills or ability, that determines their need. There is thus a trade-off between granting benefits to some non-targeted individuals (Type I error) and not granting it to some targeted individuals (Type II error). In such a set-up, it may be optimal for the government to use screening instruments such as ordeals[1] (Nichols and Zeckhauser 1982; Besley and Coate 1992a; Besley and

[1]Ordeals are purely unproductive costs that the government imposes on individuals that participate to a program. Under some conditions, they can improve the targeting efficiency of these programs.

Coate 1995) or monitoring technologies (H. Kleven and Kopczuk 2011) to deter non-targeted individuals from applying. In a richer model that accounts for rational as well as behavioral participation decisions, Finkelstein and Notowidigdo (2019) show that the welfare effects of policy interventions that aim at raising take-up depend on the targeting property of this intervention, the mechanisms driving initial non take-up and the fiscal externalities generated by the new enrollees. In other words, what matters for designing appropriate policy responses to imperfect take-up is to understand *who* is not taking up and *why* they do not.

Different barriers can affect participation in welfare programs. Policy-makers can therefore use different type of interventions to try to improve take-up by alleviating these barriers. The empirical literature has emphasized three broad categories of barriers to participation. First, imperfect take-up might be a rational choice where agents choose not to apply to a program if the costs associated with claiming outweigh the benefits.[2] Such costs encompass transaction costs, such as time and money invested in the application process and subsequent interactions with the administration, as well as other disutility costs imposed by the design of the program, such as work requirements, queuing or congestion. In this case, policy-makers can improve take-up by (i) simplifying the system and reducing costs, or (ii) by providing assistance to make navigating the system easier (Deshpande and Li 2019; Castell et al. 2022; Homonoff, Somerville, et al. 2022). Assistance provision is often costly to provide because it requires one-on-one interaction with a caseworker (Finkelstein and Notowidigdo 2019). Policy makers can also increase benefit generosity, keeping the cost constant (Rosenqvist and Selin 2023), but this has an even larger cost, since it benefits to all infra-marginal beneficiaries. Second, individuals may fail to claim due to frictions, like imperfect information about policy rules, lack of awareness of the program's existence or individual behavioral biases. In this case, imperfect take-up is not a rational choice but might be an optimization error. A large literature has tested the use of information provision (e.g. sending a letter to likely eligible individuals informing them about their eligibility) (Bhargava and Manoli 2015; Linos et al. 2022; Finkelstein and Notowidigdo 2019; Cranor et al. 2019). Results are strikingly mixed about the effectiveness of information provision to raise take-up. Last, agents may choose not to participate if claiming is associated to negative feelings such as stigma. To this date, there

[2]Some of these costs might be *ordeals*, as discussed above, while others might be productive costs imposed by the government for a reason (e.g. collecting information about the applicant).

are only a few evidence about the role of stigma in explaining imperfect take-up (Moffitt 1983; Celhay et al. 2022).[3] Overall, these three broad categories of barriers also align with the administrative burdens ("frictions that people face in their encounters with public services") classification proposed by Herd and Moynihan (2018), encompassing compliance, learning, and psychological costs. Finally, a few empirical studies have also investigated the targeting properties of imperfect take-up and of intervention aiming at reducing it (Deshpande and Li 2019; Homonoff and Somerville 2021; Finkelstein and Notowidigdo 2019; Castell et al. 2022). To do so, these studies look at how the average characteristics of claimants changes after a take-up change. Again, evidence are mixed as some find that the marginal claimant appears to be in greater need (e.g. with lower resources or worse socio-economic conditions) while other studies document adverse self-selection patterns. Interestingly, papers that find an adverse self-selection patterns often underscore that while the marginal applicant is negatively selected, there is evidence that the remaining pool of non-participant might be in greater need (Finkelstein and Notowidigdo 2019; Castell et al. 2022)

This paper studies the welfare and policy implications of imperfect take-up by examining the relative effect on take-up behaviors of (i) an increase in benefit generosity and (ii) an information provision. The empirical part of the paper focuses on one of the largest welfare program in France, an in-work benefit called *prime d'activité* (or PA hereafter). Using a recent reform of this program as a quasi-natural experiment, I empirically investigate how take-up behaviors respond to changes in two key policy parameters: the cash transfer amount and the program's visibility. The PA program is an in-work subsidy, akin to the EITC in the U.S., providing monthly cash transfers to low-wage workers. It stands as the second-largest welfare program in France, with an annual spending of 10 billion euros. The take-up rate of the program was estimated to be about 73% in 2016 (DREES 2017).[4] On December 10th 2018, following the "yellow vests" protests in France, the government announced an unexpected increase of the monthly transfer amount by up to 90 euros (+60%) for eligible workers with earnings around the minimum wage. The reform was implemented on January 1st 2019. Interestingly, survey evidence also

[3]Note also that stigma could be viewed as a specific type of cost and therefore be considered in the same category as transaction costs.

[4]This figure comes from an early report by the statistical service of the French Ministry of Social Affairs (DREES). An important caveat is that the methodology used was deemed imperfect by the Ministry itself, and consequently, the estimation has not been carried out for more recent years.

show the existence of an increase in awareness about the program around the reform which is likely due to the extensive media coverage of the reform. While 25% of the population never heard about the program pre-reform, this share decreased by 5 p.p.t. after the reform. Overall, the reform acted as both an increase in the monetary incentives to take up as well as a positive information shock about the existence of the program.

The empirical analysis starts by documenting the remarkably large overall take-up response to the 2019 reform, and then investigate the respective role of the information provision mechanism and monetary incentives mechanism. It also studies the compositional change induced by the take-up response to assess its targeting properties. The empirical analysis relies on the use of exhaustive confidential administrative data from the French social administration (*Caisse Nationale des Allocations Familiales*) which contains information about all claiming individuals. While such data cannot be used to estimate the *level* of take-up, I show that they can be used for inference about *variation* in the take-up rate, after controlling for possible change in eligibility. Because claiming behaviors are observed at a very granular time span (daily level), I use it to estimate the discontinuous increase in enrollment into the program caused by the 2019 reform. The empirical strategy relies on an event-study design that compares enrollment around the date of the reform and the same calendar dates in previous years.[5] This strategy allows me to estimate the overall effect of the reform, i.e. of the bundled treatment including the change in benfit generosity and the information shock. While enrollment trended in a parallel way before Dec. 10th for each year, I show that there is a significant jump after the date of the announcement of the reform in 2019 that is not observed at other year. The number of welfare claims increased by 75% in December 2018 after the announcement and by almost a twofold factor in January 2019 after its implementation. Overall, these effects translate into a 20% increase in the total enrollment. Turning to the analysis of the mechanisms, I use a difference-in-difference strategy to uncover the role of the change in benefit generosity. This strategy exploits the heterogeneous exposure to the benefit increase caused by the reform, across individuals with different amount and type of resources as well as different family composition. Under the hypothesis that exposure to the informa-

[5]Results are robust to the use of enrollment at the same date but in another program (the safety net or RSA) as a control group.

tion shock is uncorrelated with exposure to the benefit increase[6], I estimate that the take-up response to an increase in the benefit generosity only led to a 3% increase in enrollment, an effect that is not statistically significant. This estimate implies an elasticity of the take-up rate to the benefit amount of about 0.1. This lack of response seem to reject the idea that large transaction costs were driving the take-up decision of the marginal entrants. Instead, I provide evidence that the bulk of the response can be explained by the information shock. There is a clear and discontinuous surge in enrollment among individuals not affected by the benefits increase, almost as large as the one from those affected by the benefits increase. Heterogeneity analysis show that those who display the smallest take-up responses are those who were the most likely to be already aware of the existence of the program (e.g. those who had been enrolled into the program in the past, those who had been enrolled in any program in the past..). Lastly, I investigate the targeting effect of this take-up response. Using again an event-study strategy, I show that the reform led to a significant change in the composition of the beneficiaries. On the one hand, marginal entrants are more likely to be a single male without kids. On the other hand, marginal entrants have lower income on average. This result could highlight that this population was the most likely to be unaware of the existence of the program as single male without kids are typically not eligible to many other programs and are thus more likely to have less contact with the welfare system than individuals or couples with kids.[7]

To understand the welfare and policy implications of such reforms, I then construct a theoretical framework. The model is rooted in the standard public finance framework, involving endogenous labor supply decisions and a government setting a non-linear tax-benefit schedule (Mirrlees 1971; Saez 2002; Jacquet et al. 2013; Hansen 2021). I relax the assumption of perfect compliance to social benefits and introduce an endogenous take-up decision driven by two possible parameters; a fixed

[6]This assumption is in line with the survey evidence which indicate that the awareness increase was quite uniform across groups.

[7]An alternative interpretation could be that yellow vests protesters were most likely to belong to this population. Because the reform was adopted as a response to the protests, it might explain that they take up more. I argue that this interpretation is unlikely to hold. First, raising the generosity of the PA program was not one of the protesters demand and there is evidence that it didn't satisfy them. Note also that the government opted for this specific measure at the last minute after having contemplated an increase in the minimum wage instead. Second, I run the analysis on the subsample of individuals living in yellow vests areas (i.e. living zones where at least one roundabout blockade was held) and on the subsample of individuals living in non-yellow vests areas. Results are not statistically different in these two subsamples.

utility cost associated with claiming the benefit and a misperception parameter that can capture a lack of program awareness. These parameters are distributed across the population, resulting in heterogeneous take-up in the population. The model thus captures both a "rational" non take-up decision due to transaction costs and a "behavioral" non take-up decision due to frictions or bias. I derive new welfare formulas for the effect of the benefit increase and of an information shock. These formulas are expressed in terms of sufficient statistics that can be estimated in my empirical setup. This simple model illustrates that accounting for imperfect take-up has important policy implications. First, the welfare effect of transfers now trades off the social value of providing assistance to those who take-up with the social cost of funding this assistance via a general tax increase.[8] In that sense, the targeting efficiency of imperfect take-up (i.e., whether those who take-up are more or less in economic needs than those who do not take-up) becomes a key feature for normative analysis. Second, endogenous take-up creates an additional fiscal externality as changes in the program's parameters (e.g. benefit schedule) or in information frictions can lead to higher or lower take-up when take-up. The elasticity of the take-up rate with respect to the transfer amount and with respect to the percentage of individuals aware of the program become new sufficient statistics for welfare analysis. Calibrating the formulas with my empirical estimates and previous literature estimates, I find evidence that the bundled reform was overall welfare-improving, under a large range of redistributive preferences assumptions. The model allows me to discuss and compare the welfare implications of the two standard types of interventions discussed to raise take-up; administrative burden reduction and information provision. In particular, I show that both interventions can reduce the perceived participation tax and thus induce individuals out of the labor force to start working and taking up. Assuming that the overall participation tax is positive, this generates a positive fiscal externality. While most of the debate about raising take-up has focused on *equity* arguments, this highlights a previously overlooked *efficiency* rationale for raising take-up of in-work benefits.

This paper contributes to the literature exploring the mechanisms and the targeting property of imperfect take-up. Earlier works on this topic relied on correlational analysis. A recent wave of studies has instead provided more credible evidence by relying on randomized experiments testing the effectiveness of various policy inter-

[8]The model assumes perfect compliance to taxes so that at a given income level, everyone pays the same amount of taxes.

ventions aiming at raising take-up (e.g. information provision, stigma reduction, simplification of the claiming process) (Bhargava and Manoli 2015; Finkelstein and Notowidigdo 2019; Bettinger et al. 2012; Goldin et al. 2022). While experimental designs offer clean identification, these studies are typically conducted on smaller samples that do not allow for detailed heterogeneity analysis and sometimes on samples that are not representative of the entire eligible population. By using a large reform as a quasi-experiment and exhaustive data, this paper can help shed light on the heterogeneity of what drives non take-up and for whom. To the best of my knowledge, this is the first paper using a national welfare reform to causally study imperfect take-up and leveraging such a large take-up response. Moreover, I contribute to this literature by clarifying under which conditions administrative enrollment data can be used to identify parameters of interest about take-up. Empirical works relying on observational data typically use a constructed measure of take-up as their outcome of interest, leading to potential measurement error bias. In contrast, studies with experimental designs can directly use enrollment as their outcome. I show that, even absent an experimental design, enrollment data can be used for causal inference about take-up behaviors provided that the analysis controls for the main confounding factor; eligibility variation. This is of practical relevance as administrative social records on welfare program recipients are becoming increasingly available to researchers and could thus be used to study other programs with similar features to the one I study here.

Imperfect take-up is particularly intriguing in the case of in-work benefits. This paper contributes to the growing body of empirical research that focuses on these types of programs. Because they are typically subject to no ordeal costs, lower stigma, and much less monitoring than traditional income support programs, it has been widely speculated that non take-up to in-work benefits must come from information frictions and typically low awareness. Yet, the empirical evidence regarding the effect of information provision in raising participating are mixed. Most of this literature has centered around the EITC in the U.S. (Kopczuk and Pop-Eleches 2007; Bhargava and Manoli 2015; Goldin et al. 2022; Linos et al. 2022). In a large-scale field experiment, Linos et al. (2022) find that information provision and small nudges have no effects on EITC claiming among non-tax filers, while previous findings identified significant effects of these types of interventions on the population of tax filers (Bhargava and Manoli 2015). This disparity could indicate that heterogeneous take-up mechanisms are particularly prevalent in the case of in-work benefits

because they target a broader range of households than standard welfare programs. While the program I study here differs from the EITC on some dimensions, this paper can help understand how imperfect take-up might vary for these types of programs and which dimensions matter for heterogeneity since it exploits a large shock and exhaustive data. Additionally, it is interesting to note that the information provision I study is very different in nature than the standard information letter experiment. I find a large positive effect of untargeted and unpersonalized information provision, while previous literature has found mixed evidence about the effectiveness of targeted and personalized information provision. In future work, I am trying to assess if this is (i) because the at-scale intervention generated a social multiplier effect with information transmission and amplification and (ii) because untargeted information provision has far-reaching effect by allowing to also target "harder to spot" eligible individuals.

Finally, the theoretical framework I developed in the paper contributes to the literature in two ways. First, it enriches the standard optimal taxation framework (Mirrlees 1971; Saez 2002) by allowing for imperfect compliance with respect to monetary transfers. In doing so, it uncovers the important role of take-up responses as another source of possible fiscal externalities that affect the shape of the optimal welfare program. A smaller strand of the literature has investigated the optimal level of take-up when the government can affect participation through ordeals (Besley and Coate 1992b; Nichols and Zeckhauser 1982) or indirectly through complexity generated by monitoring (H. Kleven and Kopczuk 2011). In contrast to them, I investigate how accounting for imperfect take-up affects the optimal non-linear welfare program's schedule rather than trying to derive the optimal level of take-up. Second, the theoretical framework allows me to derive welfare formulas to study the desirability of interventions that might affect take-up, like a benefit increase or an information shock. In doing so, I relate to the paper by Finkelstein and Notowidigdo (2019) who derive similar welfare formulas. My approach follows more closely the standard public finance workhouse model, with endogenous labor supply. By modeling more precisely the main source of fiscal externalities - labor supply and take-up behavioral responses - I am able to highlight a new rationale for why raising take-up might be optimal, beyond equity or targeting efficiency arguments. Moreover, I use a tax perturbation approach which allows me to express welfare formulas in terms of sufficient statistics that can be estimated empirically.

The rest of the paper is organized as follows. Section 2 describes the PA transfer program and its recent 2019 reform. Section 3 describes the data used for the empirical analysis. Section 4 details the empirical strategy used to analyze the variation of take-up following the reforms and to identify the mechanisms. Section 5 provides the results of the empirical analysis. Last, Section 6 details the theoretical analysis of the welfare implications of imperfect take-up.

2 Institutional Context

This section describes the French EITC welfare program and the 2019 welfare reform.

2.1 Description of the program

The French EITC program ("*prime d'activité*" or PA hereafter) is a welfare benefit created in 2016 with the goal of providing financial support to low-income workers and fostering work incentives.[9] It can be characterized as an *in-work benefit* since only families with at least one working member are eligible.[10] Eligibility depends on labor income, total resources and family composition. Since 2016, the program has grown to become one of the largest means-tested cash transfers in France, with about 10 billion euros of annual spending and reaching about 15% of the population in 2022.

Figure 1 represents how the schedule varies with monthly labor income and family composition. In the phase-in range, the benefit increases with labor income. Workers face a negative marginal tax rate of -0.61%, meaning that for each additional euro of labor income, a worker gets to keep 0.61 cents of benefit. In the phase-out range, the benefit then decreases with labor income until reaching zero for those with resources above a certain threshold. While it is a family-level program, each family member with labor income above 0.5 minimum wage is eligible for an additional "individual" bonus which increases the total family transfer. Additional details about the exact formula of the transfer and the legislative parameters can be found in Appendix B. Overall, a single minimum wage worker received about 90 euros (respectively 150 euros for one eligible to housing benefits) per month in

[9] Article L841-1 from the Social Security Code of Law.

[10] Self-employed individuals and public-sector employees are eligible for the program and face the same eligibility rules as private-sector employees.

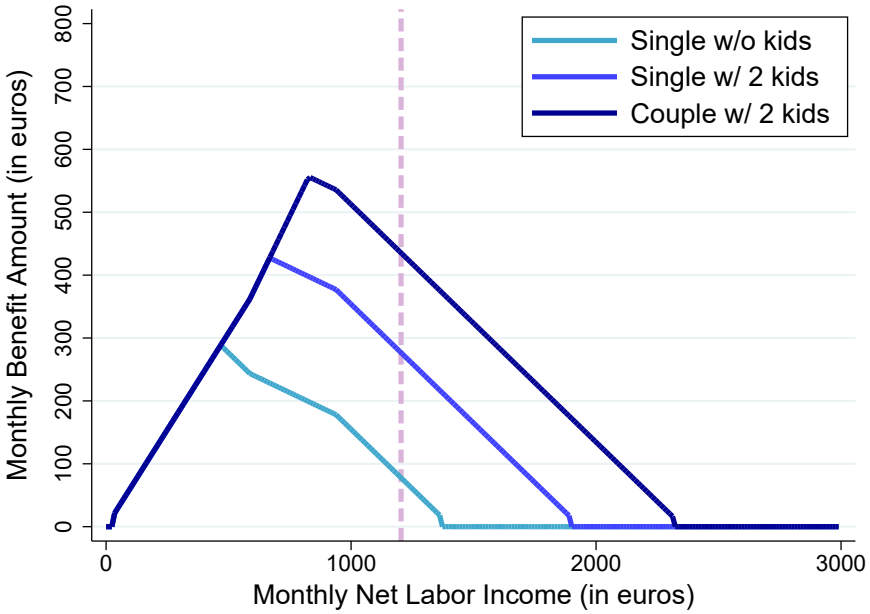

Figure 1: Schedule of the PA program

Source: French Ministry of Social Affairs. Online simulator EDIFIS
Note: Legislation as of 2018. The dashed pink line represents the level of the national minimum wage in France in 2018. All families are supposed to not receive housing benefits. All children are supposed to be at least 6 years old. The test-case "single without kids" is depicted by the light blue line. The test-case "single with two kids" is depicted by the intermediate blue line. It represents the case of a single parent not benefiting from the "isolated parent" bonus nor from the single parent benefit (*Allocation de Soutien Familial*). The test-case "couple with two kids" is depicted by the dark blue line. It represents the case of a couple with only one spouse working.

2018 which represents about 7% (respectively 15%) of her total disposable income (see Figure A.10 of Appendix B). The benefit is not taxable and can be cumulated with other programs (including the safety net, family benefits, or unemployment insurance) though most of these other cash transfers enter the means-testing and thus cannot be fully cumulated.

The program is administered by the French national welfare agency (*Caisse Nationale des Allocations Familialles* or CNAF). Hence, the French EITC is not a tax credit and is not run by the tax administration.[11] The claiming process is an online procedure[12] that takes around twenty minutes. The claimant needs to declare the following information: their social security number, bank account details, current family situation (e.g. number and age of children, marital status), past family resources and monthly individual labor income of the past three months. The procedure is fully declarative, meaning that claimants do not need to provide any documents to prove their declaration. The social administration does verify *ex-post* some of the declarations by using information shared by the tax administration[13] as well as by performing occasional audits.

One specific feature of the program will play a key role in the identification strategy explained in Section 4. Even though benefits are remitted to recipients on a monthly basis, the benefit's amount remains fixed for three months. For each of the past three months before a claim, a fictive benefit amount is computed based on the earnings declared. The amount granted for the three months following the claim is an average of these three fictive benefit amounts. At the end of the third month, beneficiaries must do a recertification procedure in order to keep receiving benefits if eligible. The recertification process asks the claimant to update her family situation if necessary and declare her resources for the past three months for the administration to compute the new amount of benefit. If a claimant is still eligible but fails to re-certify, she does not receive benefits anymore. She can re-apply the following months though. Importantly, the quarterly system implies that whatever

[11]Nonetheless, I use the terminology "French EITC" for simplification and to highlight the similitude with the U.S Earned Income Tax Credit.

[12]Claimants can, if they wish to, go to a local welfare agency office to get the help of a caseworker in making their claim or can use a paper form and send it by regular mail.

[13]This information is imperfect. First, some resources entering the means testing are tax exempted. Second, households declare an annual taxable income but do not provide the monthly decomposition of their income which is key in the eligibility assessment. Third, while France moved to a withholding income tax system in 2019, the previous timing of tax declaration was much later, typically one to two years after the time of the PA claim.

the changes in labor earnings or in family situations during a period of three months, the eligibility to the benefit remains fixed and based on past outcomes. In other words, in the very short-run, eligibility is exogenous.

2.2 The 2019 reform of the program

Following the "yellow vests" protests in France in late 2018, it was announced that the PA benefit would be sizeably increased as of January 2019 1st to support low-wage workers. This reform was fully unanticipated before its announcement in December 2018. Due to the specific context in which the reform was adopted, it also attracted an unusually large media coverage which might have changed awareness and knowledge of this program.

In September 2018, the government published the annual finance bill for 2019 that included a proposed increase of the carbon tax on fuel. This tax increase is considered to be one of the major factors that contributed to the birth and rise of the yellow vests movement. The yellow vests held regular blockades and protests throughout November and December 2018 with up to 300,000 participants (Boyer et al. 2020). On December 10th 2018, President Emmanuel Macron announced publicly a 10 billion euros plan to address the rising living costs in France for the "working poors" which was at the heart of the protesters' complaints. The main measure was an immediate and large increase of the PA benefit for a global cost of about 2.5 billion euros of additional public spending.[14] The reform was included in an emergency law promulgated on December 24th 2018 (Law n°2015-991) and the PA reform was effectively put into place as of January 1st 2019. It was therefore a largely unanticipated reform.[15]

Figure 2 shows how the reform changed the benefit's schedule for a single person without kids and with only wage income. First, the benefit increased but in a heterogeneous way. The individual bonuses were increased which led to an increase

[14]The plan included three other measures; the cancellation of the carbon tax increase, an extension in the eligibility to an energy voucher and an increase of its amount and the cancellation of an increase in social contributions on pensions for low-income pensioners (Ben Jelloul et al. 2019).

[15]In September 2018, French President Emmanuel Macron initially announced that the PA welfare program would increase annually by 20 euros for the next four years. This measure was part of an anti-poverty plan ("*Stratégie nationale de prévention et de lutte contre la pauvreté*") but did not get a lot of attention by the public. Note also that the planned increase were not to take place on January 1st of each year but on April 1st which is the usual date at which legislative parameters are updated every year.

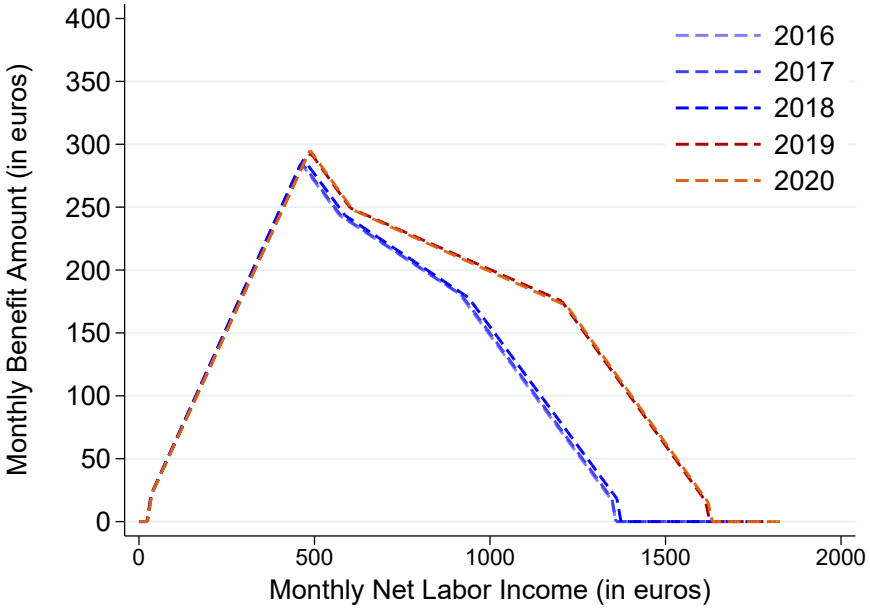

Figure 2: Evolution of the PA program's schedule

Notes: The figure represents the benefit amount served as a function of earnings for a single individual without children and with only labor income as resources.

in the benefit amount for some families with resources above a certain threshold. Beneficiaries with earnings lower than half a minimum wage did not benefit from the increase. Beneficiaries with earnings between 0.5 and 1 minimum wage benefited from an increase between 0 and 90 euros and beneficiaries with earnings above minimum wage benefited from the full 90 euros increase. In practice, exposure to the benefit increase is heterogeneous and depends not only on total labor income but also on family other resources (e.g. capital income, unemployment insurance, pensions and other benefits), family composition and the repartition of labor income between spouses. Second, the reform changed the income thresholds below which one is eligible to the program. Mechanically, some families previously ineligible became eligible to the program following the reform. Third, the reform changed the work incentives faced by the beneficiaries by reducing the participation tax at most income level[16] and changing the marginal tax rate in some earnings range.

The reform might have had an additional indirect effect. Indeed, evidence indicates that it also contributed to increasing the salience and awareness of the program. Since it only existed since 2016, some eligible households might have

[16]In the case of couples, the effects can be more complicated and even lead to an increase in the participation tax.

learned about the existence of the program following the announcement of the reform because of the extensive media coverage of the reform and the attention drawn to the yellow vests crisis and its outcomes.[17] Using survey data, Figure A.1 shows that more than one individual out of five had never heard about the program before the reform.[18] By comparison, the awareness rate for the three other major welfare programs in France (i.e. family benefits, housing benefits, and the safety net) is close to perfect. The figure also shows that the share of people declaring knowing the existence of the French EITC increased by about 5 percentage points after the reform, while staying constant for other programs. Figure A.4 shows the same pattern when looking at higher levels of information (e.g. the share of people declaring knowing "quite well" and "very well" who can be eligible for the program). In Appendix A, I show additional evidence that the reform was of particular salience and might have contributed to an increase in the salience of the program itself. For example, the number of Google searches associated with the benefit reached their peak in December 2018.

In summary, the 2019 reform of the PA program may have prompted individuals to take up the program through two mechanisms: (i) an increase in the monetary incentives to claim and (ii) an indirect positive information (or salience) shock about the program. This paper uses this quasi-experimental framework to assess the effect of these two types of interventions.

3 Data

This section presents the empirical strategy used to estimate the causal effect of the reform on take-up behaviors.

3.1 Identification of the effect of the reform

Before turning to the different mechanisms at play, the analysis first aims at quantifying the overall effect of the 2019 welfare reform on take-up. Because there can be many possible mechanisms potentially affecting all eligible, it is not possible

[17] Before the implementation of the PA benefit, Domingo and Pucci (2013) documented that for 55% of the people not taking up the safety net (RSA), the main source of information about this program was TV and radio, before public administration or charities.

[18] Low awareness rates can still be found even after conditioning on various proxies for eligibility (see Figure A.7).

to identify a clear subset of eligible individuals not affected by the reform which could act as a good control group. Instead, I focus on identifying the *overall* discontinuous change in claiming behavior around the 2019 reform using high-frequency claiming data. This captures the effect of the bundled treatment. Apart from seasonal effects, claiming behaviors to the PA program are highly stable over time. For this reason, I argue that claiming behaviors from previous or next years can provide a valid counterfactual. The empirical strategy thus uses an event-study design that compares the evolution of claiming behaviors around Dec. 10th 2018 (day of the announcement) and Jan. 1st 2019 (day of the implementation) with the evolution of claiming behaviors at the same calendar date of other years for which there was no reform.

$$\log(Y_{smt}) = \mu_s + \lambda_m + \sum_{d \neq 11} \beta_d \times \mathbb{1}_{\{m=d\}} \times T_s + \epsilon_{smt} \tag{1}$$

Equation 3 describes the regression used to estimate the causal increase in take-up caused by the 2019 reform. To estimate this equation, I use daily data about the number of new PA claims made. The outcome variable is the log number of claims at date t. The subscript m denotes the calendar month to which date t belongs. The subscript s denotes the event to which date t belongs. Events are defined as one-year window period of time around the Jan. 1st of a given year. T_s is a dummy that takes the value 1 for dates belonging to the "treated" event (i.e. the Jan. 1st, 2019 event), which are dates between July 1st, 2018 and July 1st, 2019. In the estimation, dates belonging to the "control" event (i.e. the Jan. 1st, 2018 event) are dates between July 1st, 2017 and July 1st, 2018. Using the Jan. 1st, 2020 event leads to similar results. The reference month used in the regression is November. The regression is run at the national level but can be run at a more disaggregated level (e.g. using total number of claims by region or by group of population).

In this strategy, a discontinuous change in the number of new claims at the time of the reform can identify the causal effect of the reform on take-up under two main identification assumptions. First, there should be no anticipation effect. Because of the specific context in which the reform was adopted, this assumption is likely to hold. Before the announcement of the reform on Dec. 10th, no specific focus was put by the media on the PA program[19] and no policy-makers had mentioned a

[19]This can be seen for example in the Google searches represented in Figure A.6.

possible reform of this program. The increase of the PA benefit was not part of the demands made by the "yellow vests" protests and, to the best of my knowledge, the choice of this specific policy tool was made at the last minute by the government. Reassuringly, the empirical analysis shows a significant change in claiming behaviors at the time of the announcement but not before. Second, to ensure that the identification strategy is valid, I need to rule out the possibility that the claiming effect comes from a change in the number of people eligible rather than a change in the number of people taking up conditional on being eligible. As mentioned in Section 2, eligibility is exogenous in the short-run because it is based on past labor supply and fertility choices. Hence, the effects measured between Dec. 10th 2018 and Jan. 31st 2019 cannot capture the effects of an eligibility response (e.g. a positive labor supply response that increased the number of eligible).[20] To ensure that the estimated effect does not capture an exogenous change in eligibility around the date of the reform (e.g. due to changes in economic conditions at that precise date), I perform a placebo test where I use enrollment into the safety net program (RSA) instead of the PA program. I find no significant change in enrollment around Jan. 1st 2019 in this program.[21]

3.2 Identification of the mechanisms

The empirical analysis also wishes to understand what can explain the take-up responses to the 2019 welfare reform. A first natural mechanism would be that families started to take-up more because the net benefit of claiming increased. Indeed, while the costs of claiming were unaffected by the reform, some eligible families saw an increase in the amount of transfer due to the reform. A second possible mechanism would be that families that were previously not taking up due to information frictions (e.g. because they were not aware that the program existed or that they were eligible) instead of costs, would start taking up because the reform participated in alleviating some frictions (e.g. by shifting attention towards the program or creating an information shock).

[20]The effects measured from February onwards can capture a mix of take-up and eligibility responses. I will thus focus on the effects found in December and January when discussing the magnitude of the response.

[21]The safety net is a program for families with no or very low resources. While the PA program and the safety net do not target exactly the same population, there is some overlap in their targeted population and they use the exact same variables to assess eligibility.

3.2.1 Effect of the benefit increase

Since there is heterogeneity in the exposure to the benefit increase, a natural strategy is to compare the evolution of enrollment into the program for families facing a change in incentives versus families facing no changes. I define exposure to the benefit increase as the difference between the amount of benefit a family is eligible for under the actual system and under a counterfactual system without reform, given their pre-reform characteristics. For each family entering the program, I micro-simulate these two eligibility amounts using their declared characteristics. Treated families are those who face a discrepancy between the two amounts when they enter the program.[22] For these families, the reform increased the monetary incentives to take-up the program. Using a difference-in-difference strategy, the analysis will compare the evolution of the log number of claims among the treated group versus the control group. The identifying variation exploited can come from differences in earnings, total resources, and family composition which enter the formula for the benefit amount. As an alternative way to identify the nature of the mechanisms at play, I also perform the event-study design mentioned in the previous subsection for the treated and control groups separately.

$$\log(Y_{gt}) = \lambda_t + \sum_{d \neq 2018-11} \beta_d \times \mathbb{1}_{\{t=d\}} \times T_g + \epsilon_{gt} \tag{2}$$

Equation 4 describes the diff-in-diffs regression. The outcome Y can be the number of claims (effect on the flow) or the number of beneficiaries (effect on the stock). The regression is run at the group level g, where there are only two groups (the treated and the control group). T_g is equal to 1 if the observation belongs the treated group. The coefficients β_d identify the causal effect of being exposed to the benefit increase on take-up, under several identification assumptions. First, the standard parallel trend assumption must hold, i.e. enrollment would have followed similar trends absent the reforms among the two groups. The results presented in the next section will show the event-studies coefficients β_d for all periods, including pre-reform periods. I find no significant pre-trends. Second, as discussed above, one must assume that take-up is driving the change in claiming behaviors and not eligibility. Again, in the very short-run, because of the eligibility rules of

[22] In particular, treated families have an actual benefit amount more than 15 euros larger than the counterfactual no-reform benefit amount. Note that 15 euros is the threshold below which the benefit is not paid to eligible families.

the PA program, I can rule out an eligibility behavioral response. Moreover, the difference-in-difference strategy allows to control for any exogenous fluctuations in the number of eligible. Third, for the diff-in-diffs strategy to identify the role of the sole benefit increase, I need to assume that the treated and the control group are affected similarly by the other possible mechanisms. Recall that exposure to the benefit increase is determined in a complex way by the interaction between several household's characteristics (e.g. labor income, non-labor income, family composition...). Moreover, using the survey data discussed in Section 2, I find no evidence of a correlation between income (or family composition) and declared changed in awareness of the program after the reform.

One concern that needs to be addressed is that for a small subset of PA beneficiaries, enrollment is automatic. When a family claims the safety net (a program that can be cumulated with the PA program), the welfare agency will systemically assess its eligibility to the PA program and enroll the family if deemed eligible. Because these observations come mostly from families with low resources, they are almost all in the control group. These observations are dropped from the sample as their enrollment behavior is passive which threatens the parallel trend assumption.

3.2.2 Effect of the information shock

Beyond the direct effect via the increase in the benefit amount served, the 2019 welfare reform could also have generated a positive take-up response through an increase in information about the program. Defining exposure to this mechanism (which I will call "information shock" for simplicity) is less straightforward than for the benefit increase mechanism. For now, I propose several pieces of evidence suggesting that this mechanism plays a role. First, using the event-study design, I estimate the magnitude of the take-up response among the control group (those not affected directly by the reform). I find very large responses even among this group. Second, I identify various proxies for the initial awareness of families with respect to the PA program (e.g. families ever enrolled in the PA program, families ever in contact with the welfare agency ...) and perform the event-study separately for these subpopulations. I find that those likely to be already aware of the existence of the program respond much less than the others. Third, using separate survey data, I show evidence supporting the fact that there was an increase in awareness and information regarding the PA program around the time of the reform and this in-

crease seems to be pretty uniform in the population. In future work, I will use other data sources and strategies to further confirm the information mechanism. I am currently in the process of accessing detailed TV audience data by sub-population from the leader French company producing such data (*Mediametrie*). This will allow me to link precisely exposure to media coverage of the reform and claiming behaviors. I also plan to analyze peer effects in claiming behaviors by leveraging the precise information I have in my data about the firm in which beneficiaries work and the neighborhood where they live.

3.3 Identification of the targeting effects of the reform

Characterizing *who* are the families that start to take-up due to the reform is key to understand the targeting properties of the reform. To identify the causal effect of the reform on the characteristics of the marginal entrants in the program, I use the same even-study design as described in Equation 3. The outcomes are the average characteristics of new claimants (e.g. family situation, income, nationality, past employment and welfare trajectories...). The event-study design allows to control for the fact that the welfare agency typically updates information about beneficiaries at the beginning of the calendar year and that many new entrants flow into the program at that date.

4 Empirical Framework

This section presents the empirical strategy used to estimate the causal effect of the reform on take-up behaviors.

4.1 Identification of the effect of the reform

Before turning to the different mechanisms at play, the analysis first aims at quantifying the overall effect of the 2019 welfare reform on take-up. Because there can be many possible mechanisms potentially affecting all eligible, it is not possible to identify a clear subset of eligible individuals not affected by the reform which could act as a good control group. Instead, I focus on identifying the *overall* discontinuous change in claiming behavior around the 2019 reform using high-frequency claiming data. This captures the effect of the bundled treatment. Apart from seasonal effects, claiming behaviors to the PA program are highly stable over time. For

this reason, I argue that claiming behaviors from previous or next years can provide a valid counterfactual. The empirical strategy thus uses an event-study design that compares the evolution of claiming behaviors around Dec. 10th 2018 (day of the announcement) and Jan. 1st 2019 (day of the implementation) with the evolution of claiming behaviors at the same calendar date of other years for which there was no reform.

$$\log(Y_{smt}) = \mu_s + \lambda_m + \sum_{d \neq 11} \beta_d \times \mathbb{1}_{\{m=d\}} \times T_s + \epsilon_{smt} \tag{3}$$

Equation 3 describes the regression used to estimate the causal increase in take-up caused by the 2019 reform. To estimate this equation, I use daily data about the number of new PA claims made. The outcome variable is the log number of claims at date t. The subscript m denotes the calendar month to which date t belongs. The subscript s denotes the event to which date t belongs. Events are defined as one-year window period of time around the Jan. 1st of a given year. T_s is a dummy that takes the value 1 for dates belonging to the "treated" event (i.e. the Jan. 1st, 2019 event), which are dates between July 1st, 2018 and July 1st, 2019. In the estimation, dates belonging to the "control" event (i.e. the Jan. 1st, 2018 event) are dates between July 1st, 2017 and July 1st, 2018. Using the Jan. 1st, 2020 event leads to similar results. The reference month used in the regression is November. The regression is run at the national level but can be run at a more disaggregated level (e.g. using total number of claims by region or by group of population).

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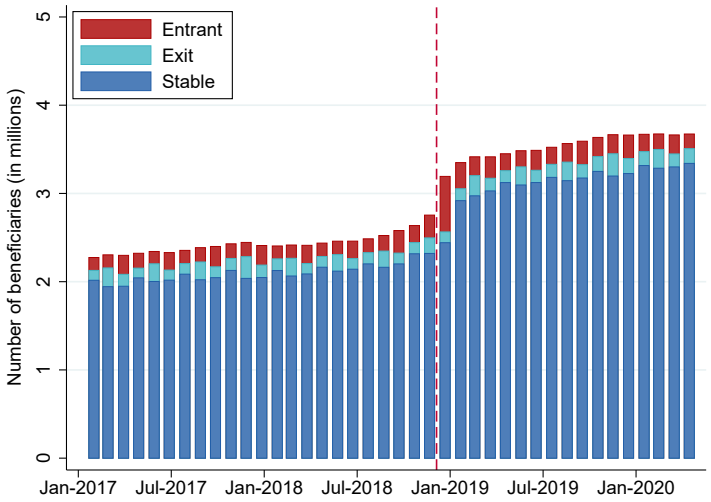
5 Results

This section provides descriptive evidence of the effect of the 2019 reform on the take-up of the PA welfare program in France and presents the results of the empirical analysis.

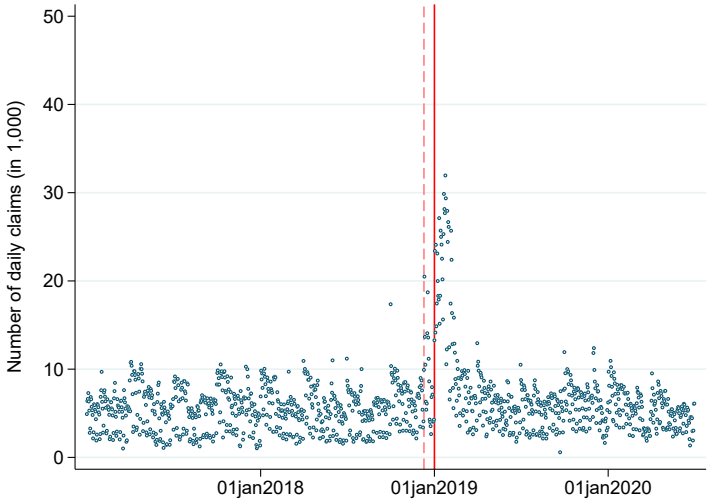
5.1 Descriptive Evidence

Figure 3(a) represents the evolution of the number of families enrolled in the PA welfare program over time. Aggregate enrollment typically remains quite stable from month to month even if there are significant movements in and out of the program.[27] In contrast with this usually stable pattern, enrollment rose sharply in January 2019 (+18%). Figure 3(b) represents the evolution of the number of daily new claims. Right after the announcement of the reform on December 10th and even more after the implementation of the reform on January 1st, the number of daily claims surged by a large amount. Most of the new claims come from first-time claimants, i.e. are not driven by re-entry in the program by past beneficiaries.

[27]The average monthly entry rate in 2018 was about 7% and the average monthly exit rate was about 6%.



(a) Number of beneficiaries



(b) Number of claims

Figure 3: Evolution of enrollment into the PA program

Notes: Sample restricted to eligible beneficiaries not benefiting from the eligibility expansion, i.e. whose simulated eligibility is still positive under the pre-reform system. Panel (a) represents the stock of beneficiaries in a given month. Panel (b) represents the flow of entrants, i.e. the number of daily claims. The dashed line represents the date of announcement and the solid line the date of implementation of the reform

Several explanations could account for this large and instantaneous surge in enrollment after the reform.[28] First, there could have been an exogenous economic

[28] As discussed in Section 3, all empirical results shown in this Section are based on the sample of families not benefiting from the eligibility expansion. Therefore, this explanation cannot explain the observed pattern in the figures. The eligibility expansion did have an effect on aggregate enrollment but I estimate that it represents only about a third of the overall enrollment increase.

shock that caused many to become eligible to the program in January 2019. This could be for example a positive labor market shock that reduced unemployment. To the best of my knowledge, there were no sizeable shocks to the French economy at that precise date. Second, this surge in enrollment might be reflecting a surge in the number of eligible due to a behavioral response in terms of labor supply. As discussed earlier, given the fact that eligibility is fixed in the short-run, the rapidity of the enrollment increase combined with the fact that the reform was largely unanticipated casts doubts about this channel. Third, the enrollment surge could alternatively indicate a large take-up response of a magnitude never seen before. In the rest of this section, I investigate if this is the case and if so, what mechanisms drove the take-up responses.

5.2 Behavioral take-up response to the reform

To estimate the overall take-up response to the reform, I use an event-study design comparing how the number of claims evolved around January 1st, 2019 (the date of the reform) and January 1st, 2018 and 2020. Figure 4 represents the number of daily claims (in log) around the cutoff date, i.e. January 1st. The blue dots depict these figures around Jan. 1st, 2019 for the period July 1st, 2018 to July 1st, 2019. At the time of the reform (both the announcement and the implementation), there is a clear discontinuous increase in the number of claims. As a placebo test, the orange and purple dots display the same figures around January 1st, 2018 and January 1st, 2020. No clear discontinuity emerges at those times. Given the similar evolution of the number of claims before the announcement of the reform, the event-study design seems to provide a valid counterfactual. As an additional placebo test, Figure A.8 in Appendix A represents enrollment into another welfare program (the safety net or RSA) around the same period. Eligibility to the two programs are different but is based on the same set of covariates (with some households can even be eligible for both programs). There is no clear discontinuity in enrollment into this other program. This suggests that the enrollment response in January 2019 is not due to a concomitant aggregate economic shock that led to a surge in the number of eligible. Instead, the response seems to be driven by a change in take-up behavior caused by the reform.

Figure 5 displays the estimated coefficients of the event-study regression described by Equation 3. The reform led to a large and significant increase in the number

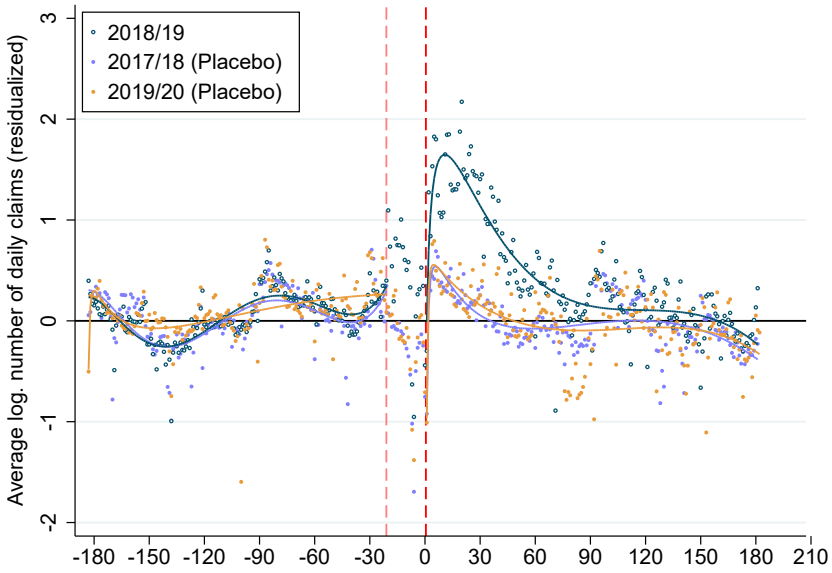


Figure 4: Evolution of claiming behaviors

Note: Daily total log number of claims after removing a day-of-the-week and first-day-of-month x quarter fixed effects. The x-axis represents the number of days relative to the cut-off date (January 1st of a given year). The solid lines represent polynomial fits of degree four, before and after the January 1st event (excluding the announcement period, i.e. Dec. 10th-Dec. 31st). The orange and purple dots and lines serve as placebo tests (control events around Jan. 1st, 2018 and Jan. 1st, 2020).

of new claims of about 75% in December 2018 and 175% in January 2019. In aggregate, this implies that the take-up increase led to an extra 422,000 families entering the program (as of the end of January 2019). Because this effect translates into an expansion by 16% of the stock of beneficiaries, it suggests that the rise in take-up had a significant impact on the public spending devoted to the program. I also estimate a significant increase in enrollment in February and March, but these estimates could also reflect other behavioral responses than pure take-up responses. In particular, they could capture, in part, labor supply response that generates an increase in the share of families eligible and ultimately enrolled in the program.

Overall, the results indicate that the take-up response was huge and had a sizeable impact on the aggregate number of beneficiaries. This naturally raises the question of how much did it affect the take-up rate of this program. The empirical strategy used cannot directly speak to this question because it relies on enrollment data rather and not on take-up data. Instead, I can answer this question by using the only available estimate of the PA take-up rate in the literature (27% according to DREES 2017). Taking at face value this estimate and combining it with the

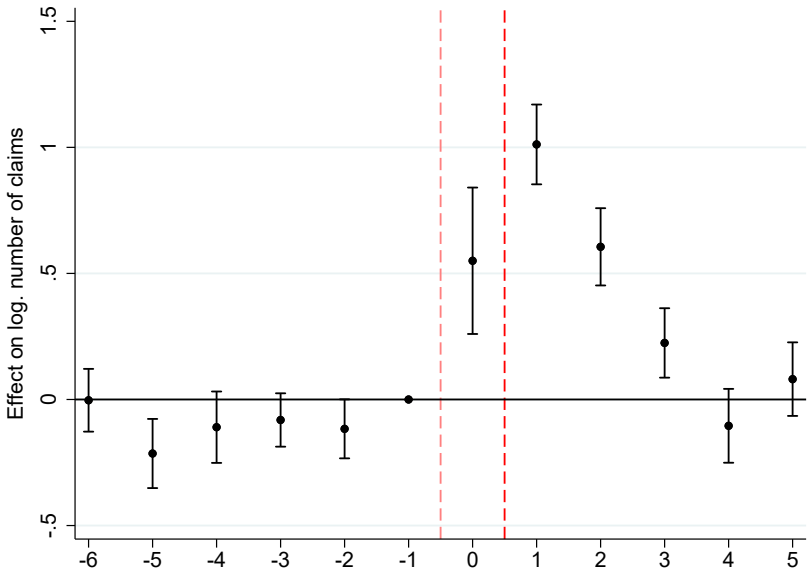


Figure 5: Effect of the reform on take-up - Estimation

Note: The x-axis represents the number of months relative to December 2018. The figure plots the coefficient of an event-study. The period Dec. 1st to Dec. 9th is excluded from the regression sample (so that the December coefficient fully captures the effect of the announcement of the reform).

estimates of the reform effect on enrollment, a back-of-the-envelope computation would lead to the conclusion that the reform led to a cut by half of the non take-up rate.[29]

5.3 Mechanisms

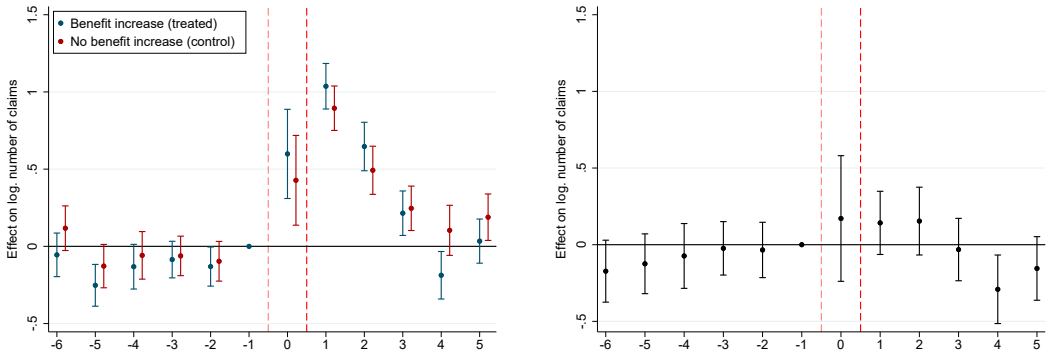
In this section, I investigate the role of two possible mechanisms for driving the take-up response: (i) the increase in the benefit amount served and (ii) the indirect positive information shock accompanying the announcement and the implementation of the reform.

5.3.1 Effect of monetary incentives

To identify the role of the transfer increase on take-up behavior, I use a diff-in-diffs strategy comparing families exposed to the benefit increase and family not exposed. The treatment is a continuous variable as families could experience a benefit increase between zero and 90 euros per working member. For simplification, I define the treatment as a binary variable equal to one when the benefit increase

[29]There was about 2.7 million of beneficiaries in November 2018. Assuming that the non take-up rate is 27%, the total number of eligible should be about 3.7 million. Since the reform caused about 0.42 million of families to start taking up, this closed the gap by a bit less than half.

faced is higher than 15 euros per month and zero otherwise.[30] Assuming that enrollment in these two groups would have evolved parallel absent the reform, the diff-in-diffs strategy captures the causal effect of being exposed to a higher benefit amount on take-up decisions.



(a) Event-study regression coefficients

(b) Triple-diff regression coefficients

Figure 6: Effect of the benefit increase on enrollment

Notes: Panel (a) represents the coefficients of an event-study design comparing the log. number of claims around January 2019 versus around January 2018. The x-axis denotes the number of months relative to December. The red and blue dots represent the estimated coefficients in two subsamples, the "treated" and the "control" group. Treated families are families that experience an increase in the amount of benefit served at the time of entry, i.e. that face a larger benefit amount under the current system than under a counterfactual system without reform. Panel (b) represents the coefficients of a triple-difference estimation comparing the log. number of claims around January 2019 versus January 2018 and among the treated versus the control group.

Figure 6(a) represents the coefficients of the event-study strategy used in the previous section, estimated separately among the treated and the control group. It shows that the relative increase in entry is larger among families exposed to an increase in the benefit amount served than for families facing no changes. Figure 6(b) shows the results of the triple-diff strategy comparing enrollment across years and across treated and control groups. The causal effect of being exposed to the benefit increase is small relative to the overall magnitude of the reform, representing an enrollment increase of about 20% in December and January 2019. This would translate to a 3% increase in the stock of beneficiaries. Moreover, the effects are not significant. Assuming that the cost of claiming remained stable around the reform,

[30]The PA benefit is only paid to families if they are eligible to at least 15 monthly euros. I chose this threshold because it provides a sense of what is a "negligible" amount according to the administration.

this indicates that an increase in the monetary incentives to take-up has only a limited effect on take-up, suggesting that for most of the new entrants, take-up was not a rational cost-benefit choice. To derive an elasticity of the take-up rate, I need to look at the treatment effect on the stock of beneficiaries. I find that , on average, treated families saw an increase by about 30% of the PA benefit amount served between November and January and an increase by about 3% of their enrollment relative to the control families. Hence, the estimated elasticity of the take-up rate with respect to the benefit amount served is about 0.1. In other words, when the benefit served increases by 1%, it leads to an increase in enrollment (via an increase in take-up under our identification assumptions) by about 0.1%. The elasticity estimation helps to assess the relatively low magnitude of the effect of monetary incentives on take-up behavior.

5.3.2 Effect of the information shock

Figure 6(a) also indicates that there was a discontinuous increase in enrollment even among those not exposed to a significant change in the monetary incentives to take up. Assuming that the information shock is orthogonal to the exposure to the benefit increase, we see that the bulk of the response cannot be rationalized by the change in monetary incentives. I argue that the residual effect measured could be linked to heightened awareness of the program. The announcement of the reform and the way it was extensively covered by the media could increased awareness about the existence of the program (see Figure 1A.1.2). Conditional on already knowing about the program, it could have increased the amount of information people have about the program (see Figure A.1) and thus help eligible individuals perceive their own eligibility to the program. Several pieces of evidence point towards this explanation. Figure 7 shows that, among the control group, the increase in take-up was much larger among first-time entrants than among past beneficiaries, who are less likely to be not taking up due to a lack of information/awareness. Additionally, we see in Figure 6(a) that the increase in the number of claims made starts on December 10th 2018, at the announcement, consistent with the idea that part of the new entrants are not motivated by the increased benefit amount. Additional work is being carried out to better identify this mechanism.

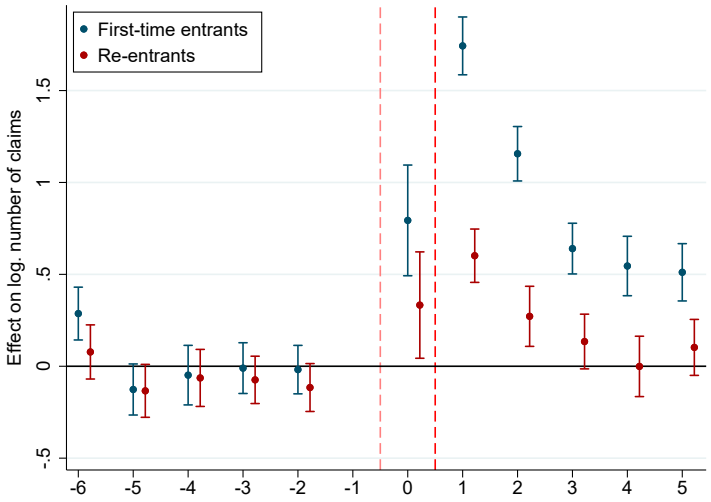


Figure 7: Evolution of log. number of claims across subgroups

Notes: Sample restricted to families not exposed to the benefit increase.

5.4 Targeting properties

Turning to the results about the targeting property of imperfect take-up, I investigate how the reform affected the characteristics of the marginal enrollee. In other words: who are the families that started to take-up as a response to the reform? Are they different from the average pool of entrants? To answer this question, I use an event-study that compares how the average characteristics of newly enrolled families evolved around January 2019 using past years as control groups. Figure A.9 represents the coefficients of these regressions on several outcomes separately for the "treated" group (those affected by a benefit increase) and for the "control" group (those unaffected by a benefit increase). On average, the two groups seem to display quite opposite patterns. Overall, it is hard to conclude whether the take-up response was driven by better-off or worse-off families as the results are quite mixed. On the one hand, new entrants have on average lower income. On the other hand, new entrants come on average from groups generally thought to be in a more favorable socio-economic conditions (e.g. non-foreigners, single male, non past beneficiaries of the safety net...).

6 Theoretical Model & Welfare Analysis

When discussing the design of an optimal transfer program or the welfare implications of reforms, economists often rely on the standard public finance framework

(Mirrlees 1971; Saez 2002). This framework makes two crucial assumptions. First, the tax and benefit system is fully integrated and described by a unique tax, that can be negative to capture means-tested transfers. Second, individuals fully comply to both taxes and benefits, which rules out incomplete take-up of benefits. These features are at odds with real-life policies. Taxes and transfers are often regulated by different laws and managed by separate administrations. Moreover, empirical evidence shows that a significant share of eligible households do not receive the transfers they are eligible for.

In this section, I expand the standard framework to account for endogenous program take-up decisions. The theoretical framework and the methodology used follow the work from Hansen (2021). The key difference between our two models is the introduction of endogenous participation in the welfare program. The model helps answer two sets of questions. In Section 6.2, I investigate how incomplete take-up affects the optimal design of welfare programs. In particular, I revisit the standard question of whether an optimal system should feature negative tax rates at the bottom (i.e. an in-work benefit)[31]. In Section 6.3, I study the welfare effects of reforms that affect take-up, similar to those studied empirically in the paper, and express it in terms of key sufficient statistics. I then calibrate the model using the empirical results to estimate the welfare effect of the 2019 reform.

6.1 Conceptual framework

Individuals. Individuals have heterogeneous skills ω and fixed cost of work δ. Types are known by individuals but are private information to the government. Individuals face a classic consumption-leisure trade-off problem and have to choose consumption level c and earnings level y to maximize their utility given their budget constraints. For simplicity, utility is assumed to be quasi-linear and additively separable: $U(c, y; \omega, \delta) = c - h(y; \omega) - \mathbb{1}_{y>0}\delta$.

$$h(0; \omega) = 0,\ h_y \geq 0,\ h_{yy} \geq 0,\ h_\omega \leq 0. \tag{A1}$$

$$h_{y\omega} \leq 0. \tag{A2}$$

[31] I use the terminology "in-work benefit" hereafter to designate a transfer that implements a system with negative participation taxes and negative marginal tax rates at the bottom. In Hansen (2021), this is called an EITC.

Individuals face a disutility of earning income y which is denoted by $h(y)$ and is assumed to be increasing and convex in y and decreasing in ω (A1). I also assume a single-crossing property (A2) that ensures that individuals with higher abilities ω have lower marginal disutility of work. Individuals also face a utility cost equal to the fixed cost of working δ whenever they choose $y > 0$.

The Population. There is a continuum of agents of mass one. Individuals are characterized by their skills type ω and fixed cost of work δ. Agents also differ along two other dimensions that will drive the heterogeneity in non take-up behaviors; their cost of claiming social benefits γ and their perception of these benefits θ. Different possible interpretations of these parameters are discussed in Section 6.2. An individual i thus has type $\{\omega; \delta; \gamma; \theta\} \in \Omega \times \Delta \times \Gamma \times \Theta \subseteq \mathbb{R}^4_+$. For convenience, I assume that the distributions of types are bounded, i.e. $\Delta = [\underline{\delta}; \overline{\delta}]$, $\Omega = [\underline{\omega}; \overline{\omega}]$ and $\Gamma = [\underline{\gamma}; \overline{\gamma}]$. For simplicity, I also assume that θ is a dummy and therefore $\Theta = \{0, 1\}$. The joint distribution of type is denoted by $F : \Omega \times \Delta \times \Gamma \times \Theta \mapsto [0; 1]$ and the density is denoted by f. For now, I assume that type distributions are not correlated. This assumption will be relaxed in future work.[32]

The Tax and Benefit System The tax system and the benefit system are considered as separate systems contrasting with the standard model. Taxes are positive transfers made from consumers to the government while benefits are positive transfers received by the consumers from the government. Agents are assumed to fully comply with taxes. However, in Section 6.2, I relax the assumption of perfect compliance in the benefits system to account for imperfect take-up. To simplify the exposition, I assume that taxes are set to zero[33], meaning that an individual who chooses not to take up benefits is actually facing the *laissez-faire* situation.

The benefit schedule is assumed to be a smooth, continuously differentiable and concave function of income y[34] that is denoted $B(.)$ (A3). An individual earning

[32]Note that with orthogonal type distribution and quasi-linear preferences, there is no role for ordeals (Nichols and Zeckhauser 1982). Under this assumption, claiming costs do not act as a self-targeting mechanism, whereby high-skilled would be relatively more deterred from reducing labor supply and applying than low-skills.

[33]In many countries, households with sufficiently low income to receive benefits often do not pay positive personal income taxes. More generally, as long as taxes do not interact with benefits (e.g. like in the French system), this assumption does not change the main insights of the model.

[34]In many programs, eligibility is also a function of family size and composition. The simplified framework presented here captures, in part, this feature of programs if we assume a unitary household model and that benefits amounts are set such that benefit per consumption unit is

income y and receiving benefits faces a marginal tax rate $\tau(y) := -B'(y)$, and a participation tax $T^P(y) := B(0) - B(y)$. Since taxes are assumed to be zero, the participation tax and the marginal tax rate conditional on not taking up benefits are zero. Note that the benefit schedule described is flexible enough to encompass various types of schedules such as negative income tax (or guaranteed minimum income) as well as earning subsidy (or in-work benefit).

$$B(y) \geq 0 \text{ and } B_{yy} \leq 0 \text{ on the domain where } B(y) > 0. \tag{A3}$$

The labor force participation rate of individuals of type ω is denoted by $r(\omega) \equiv E[y^*(\omega, \delta, \gamma, \theta) > 0|\omega]$. The elasticity (respectively semi-elasticity) of labor supply at the intensive (respectively extensive) margin for individuals with skills ω, are denoted $\epsilon(\omega)$ and $\eta(\omega)$ respectively.

$$\varepsilon(\omega) = \frac{\partial dy(\omega)}{\partial(1-\tau(y(\omega))} \frac{1-\tau(y(\omega))}{y(\omega)} \tag{5}$$

$$\eta(\omega) = \frac{\partial r(\omega)}{\partial[\bar{y}(\omega) - T^P(y(\omega))]} \frac{1}{r(\omega)} \tag{6}$$

Social Planner. The social planner's objective is to set a transfer system $B(.)$ that maximizes a social welfare function given its resource constraint. Due to asymmetric information, the government can only observe individual income y but not individual private types $\{\omega; \delta; \gamma; \theta\}$. The social welfare function W is a function of the indirect utility $V(\omega, \delta, \gamma, \theta)$ and is defined as follows:

$$W = \int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \alpha(\omega) \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) V(\omega, \delta, \gamma, \theta = j|B) \; d\gamma \; d\delta \; d\omega. \tag{7}$$

The welfare weights α are assumed to vary only with individuals' skills and not with fixed cost of working δ or with type γ. The government's resource constraint is:

$$\int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \alpha(\omega) \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) B(y^*(\omega, \delta, \gamma, \theta = j)) \; d\gamma \; d\delta \; d\omega \leq E. \tag{8}$$

where $y^*(\omega, \delta, \gamma, \theta)$ is the optimal earning choice of a given individual when facing the benefit system B and E is an exogenous ceiling for public spending.

constant.

6.2 Model with imperfect take-up

In this section, I relax the assumption of perfect compliance to social transfers and extend the model to account for imperfect take-up. There are three new features in the model. First, individuals have a new binary choice variable p which captures the decision to take-up or not the benefit B. Second, individuals face a heterogeneous fixed cost γ of taking up the benefit.[35] Third, individuals might misperceive the benefit schedule due to imperfect information or cognitive bias. The perceived schedule is $\theta B(.)$ where θ denotes the heterogeneous initial knowledge type of an individual.[36] The model thus encompasses two types of non take-up, one of them being a privately optimal choice made by agents facing a costly take-up decision while the other is an optimization mistake that is the result of information frictions.

I denote by $q(\omega) \equiv E[p^*(\omega, \delta, \gamma, \theta) = 1|\omega]$ the take-up rate of individuals of type ω and by $\bar{q}$ the aggregate take-up rate in the population $\int_{\underline{\omega}}^{\bar{\omega}} q(\omega)d\omega$.[37] I define two semi-elasticities of the take-up rate, one with respect to the benefit amount one faced whenever working and the other one with respect to the demogrant $B(0)$:

$$\xi(\omega) = \frac{\partial q(\omega)}{\partial B(\bar{y}(\omega))} \frac{1}{q(\omega)} \tag{9}$$

$$\xi_0(\omega) = \frac{\partial q(\omega)}{\partial B(0)} \frac{1}{q(\omega)}. \tag{10}$$

I also introduce a new concept of labor force participation which will be key for the welfare analysis. Let $\tilde{r}(\omega)$ denote the labor force participation rate *conditional* on taking up. It is the share of individuals who have strictly positive earnings y among individuals who choose to participate in the program (i.e. with $p^* = 1$).

[35] Heterogeneous fixed cost of taking up can reflect different perceived costs (e.g. stigma feelings), different effective transaction costs (e.g. due to commuting time) or different effective opportunity costs (e.g. value of the time spent on the application). Because of quasi-linearity, the fixed cost γ can be interpreted as a utility cost or a monetary cost.

[36] For simplicity, I assume that $\theta \in \{0, 1\}$, i.e. agents are either fully unaware of the existence of the social benefits or have perfect knowledge of its schedule. In their model, Finkelstein and Notowidigdo (2019) use a continuous misperception parameter such that individuals perceive $(1 + \varepsilon)B$. However, when calibrating their model empirically, they find that ε is very close to -1, suggesting that full unawareness is not an unrealistic assumption.

[37] Note that the take-up rate is defined here as the share of individuals receiving a positive amount of transfer among the overall population (not among the fraction of the population that is eligible to the transfer). Indeed, throughout this section I do not need to distinguish between eligible and non-eligible individuals not receiving the transfer. To be more accurate, q could be referred to as the benefit participation rate.

Similar to the elasticity $\eta(\omega)$ defined previously, I denote by $\tilde{\eta}(\omega)$ the extensive-margin labor supply elasticity conditional on take-up.

The utility maximization problem of an individual is now:[38]

$$\begin{cases} \max_{\{c_i, y_i, p_i\}} & U_i = c_i - h(y_i, \omega) - \mathbb{1}_{y_i>0}\delta - p_i\gamma \\ s.t. & c_i \leq y_i + p_i\theta B(y_i) \end{cases}$$

Working and claiming are jointly determined because each of these discrete decisions affects the overall return of the other decision. Conditional on working, the individual chooses an earning level that equates the marginal benefit of working with the marginal cost of working. The marginal benefit of working depends on whether or not the individual take-up. Let us denote $y_1^T(\omega)$ (respectively $y_2^T(\omega)$) the optimal earning choice of an individual of type ω conditional on working and taking up (respectively not taking up). These optimal choices are characterized by the following first-order conditions:

$$1 + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{11}$$

$$1 = h_y(y_2^T(\omega), \omega). \tag{12}$$

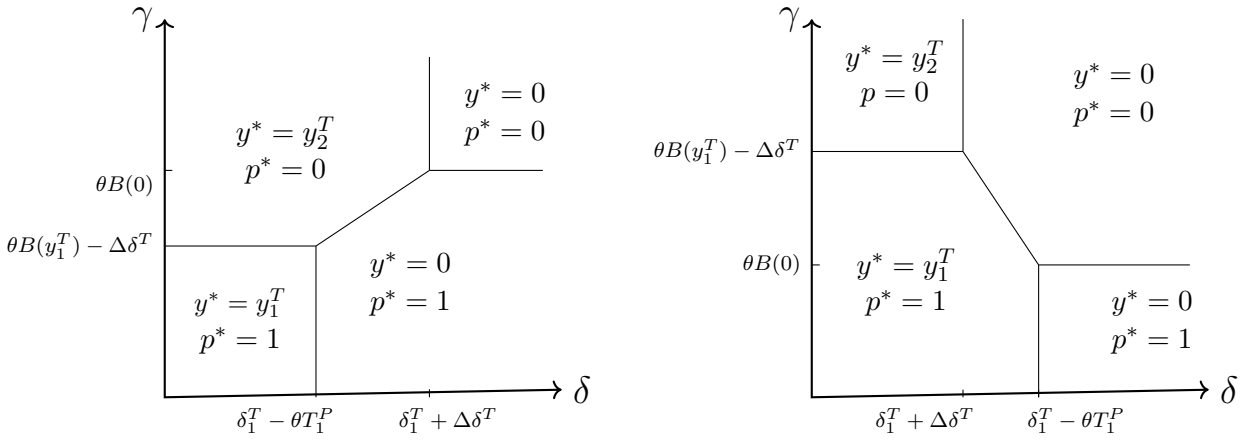
The overall solution of the individual's problem is:

Proposition 1. *The solution to the individual problem is:*

$$\{y^*, p^*\} = \begin{cases} \{y_1^T(\omega),\ 1\} & \textit{if} & \gamma < \theta B(0) - \theta T^P(y_1^T) - \Delta\delta^T \\ & \textit{and} & \delta < \delta^T(y_1^T) - \theta T^P(y_1^T) - \max\{0; \gamma - \theta B(0)\} \\ \{y_2^T(\omega),\ 0\} & \textit{if} & \gamma > \theta B(0) - \theta T^P(y_1^T) - \Delta\delta^T \\ & \textit{and} & \delta < \delta^T(y_1^T) + \Delta\delta^T + \min\{0; \gamma - \theta B(0)\} \\ \{0, 1\} & \textit{if} & \gamma < \theta B(0) \\ & \textit{and} & \delta > \delta^T(y_1^T) - \theta T^P(y_1^T) + \max\{0; \gamma - \theta B(0) + \theta T^P(y_1^T) + \Delta\delta^T\} \\ \{0, 0\} & \textit{if} & \gamma > \theta B(0) \\ & \textit{and} & \delta > \delta^T(y_1^T) + \Delta\delta^T - \min\{0; \gamma - \theta B(0) + \theta T^P(y_1^T) + \Delta\delta^T\} \end{cases}$$

with $\delta^T(y) = y - h(y)$ *and* $\Delta\delta^T = \delta^T(y_2^T) - \delta^T(y_1^T)$

[38] Note that this problem is the ex-ante decision problem of an individual with misperceptions θ. If the individual takes up, she does receive $B(y)$ ex-post.



(a) Case when $\theta T_1^P > -\Delta\delta^T$ (b) Case when $\theta T_1^P < -\Delta\delta^T$

Figure 8: Individual optimal earnings and take-up decision

The solution $\{y^*, p^*\}$ is represented in the (δ, γ) space by Figure 8. For individuals with sufficiently large or low costs of working δ and of taking up γ, take-up and labor supply decisions are in fact disjoint. However, for some individuals with intermediate values of δ and γ, a new trade-off arises. The trade-off can take two different forms depending on the shape of the benefit schedule around $y_1^T(\omega)$ and $y_2^T(\omega)$. Let $\Pi(\omega|y > 0) \equiv B(0) - T_1^P(y_1^T(\omega)) - \gamma - \Delta\delta^T(\omega)$ be the utility premium of take-up conditional on working and $\Pi(\omega|y = 0) \equiv B(0) - \gamma$ be the utility premium of take-up conditional on not working. The utility premium of take-up is lower conditional on working than conditional on not working whenever $T_1^P + \Delta\delta^T$ is positive. In that case, there is a trade-off between working and not taking up versus not working and taking up (see Figure 8(a)). The utility premium of take-up is higher conditional on working than conditional on not working whenever $T_1^P + \Delta\delta^T$ is negative. In that case, there is a trade-off between working and taking up versus not working and not taking up (see Figure 8(b)). Finally, one can note that individuals who are unaware of the program ($\theta = 0$) have the same solution as in a *laissez-faire* situation. There would be only two areas in the graphical representation of the solution. If $\delta \geq \delta^T(y_2^T)$, the individual does not work and does not take up, and if $\delta < \delta^T(y_2^T)$, the individuals earn y_2^T and does not take up.

Accounting for imperfect take-up has important implications for the welfare effects of reforms of the transfer system and thus for the optimal welfare program. In Section C.3 of Appendix C, I re-derive the necessary and sufficient conditions

for the introduction of negative participation and marginal tax rates to be welfare-improving provided by Hansen (2021) in a set-up with imperfect take-up. I highlight two key differences between this set-up and the perfect compliance set-up. First, when assessing the welfare effects of the transfer of resources introduced by the reform, one needs to account for the fact that these reforms only affect those who take-up but are likely funded by taxes that are paid by everyone (assuming perfect compliance to taxes). Depending on whether those who take up are those with the highest economic needs or those with the lowest economic needs, the reform can have very different implications. This implies that the optimal design of a welfare program depends on the forces driving non take-up, and whether these forces act as efficient self-screening mechanisms or as inefficient barriers. Second, any change in the schedule generates an additional fiscal externality due to take-up responses, on top of the standard fiscal externalities generated by labor supply responses. This new fiscal externality is negative and depends on a new key sufficient statistics, the take-up elasticity with respect to the transfer amount ξ.

6.3 Welfare analysis of the 2019 French welfare reform

In this section, I use the theoretical framework built in the previous sections to express the welfare effects of a reform similar to the 2019 PA reform in France which is the focus of the empirical analysis in this paper. In the framework, the reform is implemented sequentially. First, the reform creates a positive information shock (i.e. a shock on the distribution of the parameter θ). Second, the reform introduces an increase in the amount of the in-work benefit received by working agents (i.e. a change in the benefit schedule B).

6.3.1 Additional assumptions

To model accurately the French system and its 2019 reform, I start by introducing three additional assumptions about the initial transfer system in place before the reform.

The transfer system has non-negative marginal tax rates $\tau(y)$ and non-negative participation taxes $T^P(y)$ at all income levels y. Figure 11(a) represents the initial budget constraint implemented by this benefit schedule (solid red line). This initial system is close to the benefit system in place in France in late 2018, before the implementation of the welfare reform.

$$\tau(y) \geq 0 \text{ and } T^P(y) \geq 0 \ \forall \ y. \tag{A4}$$

I assume that there are two separate transfer schedules, B_0 and B. B_0 is a guaranteed minimum income program that provides a transfer of $B(0)$ to any agents out of the labor force and then phases out at a rate of 100%. Therefore B_0 is similar to the guaranteed minimum income in France (*Revenu de Solidarité Active* or RSA). B is an in-work benefit program that provides a positive transfer only to working agents. It phases in until an income threshold equal to $B(0)$ and phases out until an income threshold of y_b. Therefore B is similar to the French EITC program (*Prime d'activité* or PA).[39]

$$B_0(y) = \begin{cases} B(0) & \text{for } y = 0 \\ B(0) - y & \text{for } y \in [\underline{y}, B(0)] \\ 0 & \text{for } y \in [B(0), \bar{y}] \end{cases}$$

$$B(y) = \begin{cases} 0 & \text{for } y = 0 \\ \tau_0 \ y & \text{for } y \in [\underline{y}, B(0)] \\ B(0) - (1 - \tau_0)y & \text{for } y \in [B(0), y_b] \\ 0 & \text{for } y \in [y_b, \bar{y}] \end{cases} \tag{A5}$$

Last, I assume full take-up of the guaranteed minimum income program B_0. In particular, I assume that there is perfect awareness about this schedule and zero cost associated with benefiting from this program. Survey evidence presented in Section 2 indicates that this program is well-known. Almost all individuals declare at least having heard about the program. However, the costs associated with benefiting from the program are likely not zero and are generally thought to be larger than the cost of claiming the in-work benefit.[40] In this section, p denotes the decision to take-up the in-work benefit B conditional on being eligible.

The budget constraint of a type θ individual is: $c \leq y + B_0(y) + p\theta B(y)$. (A6)

[39] It is also similar to the Earned Income Tax Credit in the U.S. and other programs across countries.

[40] This is a simplification assumption that will be relaxed in future work since it is not in line with existing estimates. This assumption should lead to an over-estimation of the positive welfare effects of the reform since it under-estimate the true fiscal externality imposed by individuals who start working and taking up.

6.3.2 Solution of the agent's problem

Conditional on working, the individual chooses an earning level that equates the marginal benefit of working with the marginal cost of working. The marginal benefit of working depends on whether or not the individual take-up. Let us denote $y_1^T(\omega)$ (respectively $y_2^T(\omega)$) the optimal earning choice of an individual of type ω conditional on working and taking up (respectively not taking up). These optimal choices are characterized by the following first-order conditions:

$$1 + B_0'(y_1^T(\omega)) + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{13}$$

$$1 + B_0'(y_2^T(\omega)) = h_y(y_2^T(\omega), \omega). \tag{14}$$

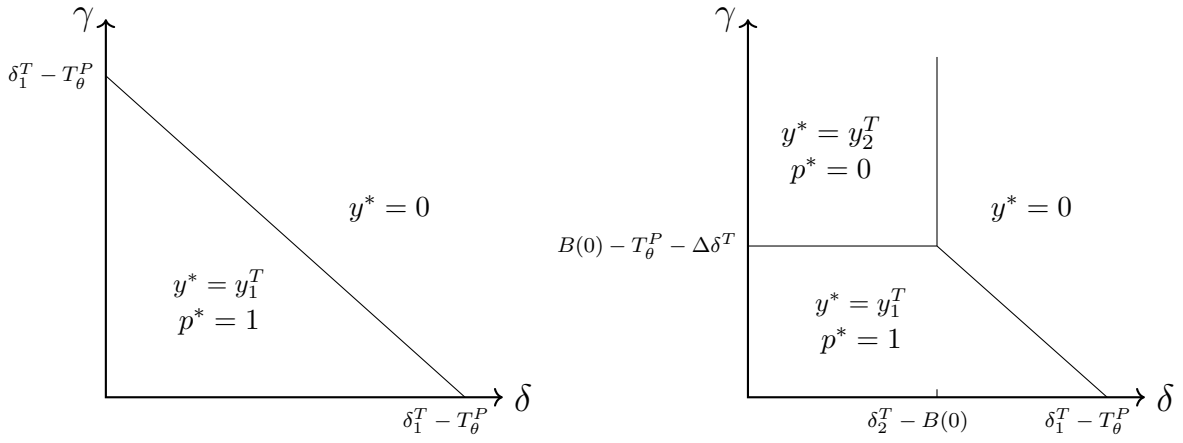
The two main policy parameters are $B_0(0)$ the amount of guaranteed minimum income received by all non-working agents under (A6) and $T_\theta^P(y) \equiv B_0(0) - B_0(y) - \theta B(y)$, the perceived participation tax at income level y.

Proposition 2. *Under Assumptions (A4)-(A6), the solution to the individual problem is:*

$$\{y^*, p^*\} = \begin{cases} \{y_1^T(\omega),\ 1\} & \text{if} \quad \gamma < \min\{B(0)\ ;\ \delta_2^T(\omega) - \delta\} - \Delta\delta^T(\omega) - T_\theta^P(y_1^T(\omega)) \\ \{y_2^T(\omega),\ 0\} & \text{if} \quad \gamma > B(0) - T_\theta^P(y_1^T) - \Delta\delta^T \text{ and } \delta < \delta_2^T(\omega) - B(0) \\ \{0, 0\} & \text{if} \quad \delta < \delta_2^T(\omega) - \max\{B(0)\ ;\ \Delta\delta^T(\omega) + T_\theta^P(y_1^T(\omega)) + \gamma\} \end{cases}$$

with $\delta^T(y) = y - h(y)$ *and* $\Delta\delta^T = \delta^T(y_2^T) - \delta^T(y_1^T)$

Figure 9 represents the individual's solution $\{y^*, p^*\}$ in the (δ, γ) space. Two cases arise depending on the skills parameter ω. Figure 9(a) represents the solution for low-skills individuals whose optimal earning decision is $y_2^T(\omega) < B(0)$, conditional on not taking up the in-work benefit. Because they face a 100% marginal tax rate for $y < B(0)$ due to the perception of the safety net B_0, their optimal earning choice is $y_2^T = 0$. In other words, working and not taking up the in-work benefit is a dominated choice for these individuals. The figure shows that their optimal choice is either not to work ($y^* = 0$) or to work and take-up ($y^* = y_1^T$ and $p^* = 1$). This choice is driven by the relative cost of working δ versus claiming the in-work benefit γ. Figure 9(b) represents the solution for higher-skills individuals whose optimal earning decision is $y_2^T(\omega) > B(0)$, conditional on not taking up the in-work



(a) Case when $y_2^T(\omega) = 0$

(b) Case when $y_2^T(\omega) > B(0)$

Figure 9: Individual optimal earnings and take-up decision

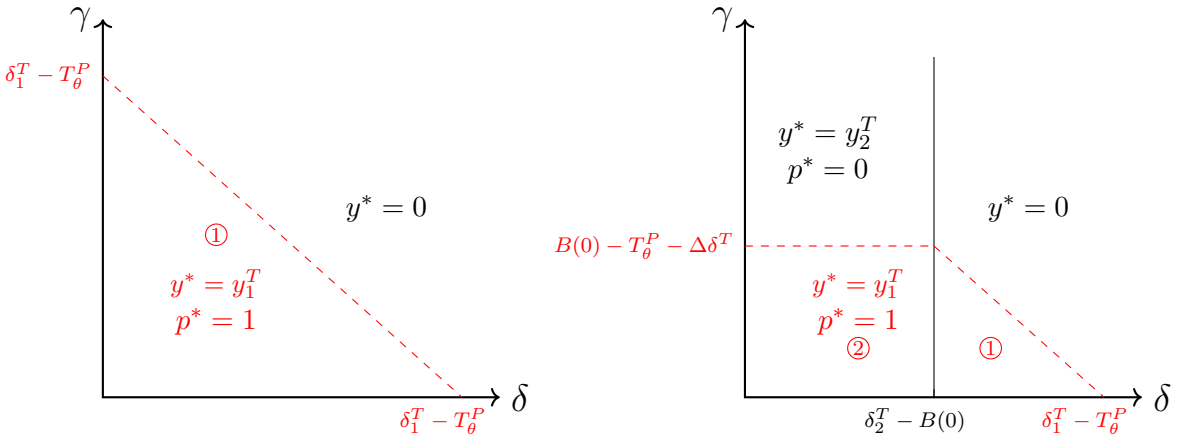
benefit. Compared to the former case, an additional solution arises for individuals with low cost of working δ and large cost of claiming γ where they can choose to work but not take-up the in-work benefit, even if eligible. The trade-off driving the take-up decision, conditional on working is solely driven by the absolute value of the claiming cost γ. And the trade-off driving the work decision, conditional on not taking up is solely driven by the absolute value of the cost of working δ.

6.3.3 Welfare effect of the positive information shock

Given a transfer system that satisfies Assumptions (A4)-(A6), Figure 10 represents the solution of the individual's problem for agents that are unaware of the in-work benefit program ($\theta = 0$). Figure 10(a) represents the case of individuals individuals with low skills such that $y_2^T(\omega) = 0$. Figure 10(b) represents the case of individuals with intermediate skills such that $y_2^T(\omega) > B(0)$.[41] Individuals who are not aware of the in-work benefit B naturally never take up. Depending on their type, some of the individuals who would have worked and take-up under perfect awareness end up working more ($y_2^T(\omega) > y_1^T(\omega)$) and not taking up while others end up not working at all but claiming the guaranteed minimum income B_0.

I now investigate the effects of an information shock, defined as a positive shock on the distribution of types θ. Suppose that the initial share of individuals aware

[41] Recall that the y_2^T cannot be strictly positive and lower than $B(0)$ because this is a dominated range.



(a) Case when $y_2^T(\omega) = B(0)$ (b) Case when $y_2^T(\omega) > B(0)$

Figure 10: Individual optimal earnings and take-up decision

Lecture: The solid black lines represent the initial solution (when $\theta = 0$) and the dashed red lines represent the solution after the information shock (when $\theta = 1$).

of the program was p_θ and that after the reform it is $p_\theta + \Delta\theta$.[42] Some individuals were not working when unaware of the in-work benefit program because they were overestimating their true participation tax. If their claiming cost is relatively small compared to their labor force participation cost, these individuals display a positive labor supply response at the extensive margin and positive take-up response (effect ① in Figures 10(a) and 10(b)). Some individuals were previously working but not taking up because they were also unaware of the program. If their claiming cost is sufficiently small, they display a positive take-up response and a negative intensive margin labor supply response (effect ② in Figure 10(b)). Overall, the model predicts that after a positive information shock $\Delta\theta$, the take-up rate and the labor force participation rate increase.

Proposition 3. *The marginal welfare effect of an information shock $\delta\theta$ is:*

$$W_\theta(\theta) = \bar{q}(\theta) \int_{\underline{y}}^{\bar{y}} f_y(y) \left[\tilde{\eta}^\theta(y)(\alpha(\omega^T(y))dV_1(y) + T_\theta^P(y)) + \xi^\theta(y)(\alpha(\omega^T(y))dV_2(y) - B(y))\right] dy$$

Proposition 3 provides the effect of a small shock $\Delta\theta$ on welfare. The information shock affects welfare through two channels, the fiscal externalities created by individual behavioral responses and the effect of these responses on individual private

[42] For now, to be in line with the empirical set-up, I assume that the information shock is uniform and is orthogonal to individuals' types.

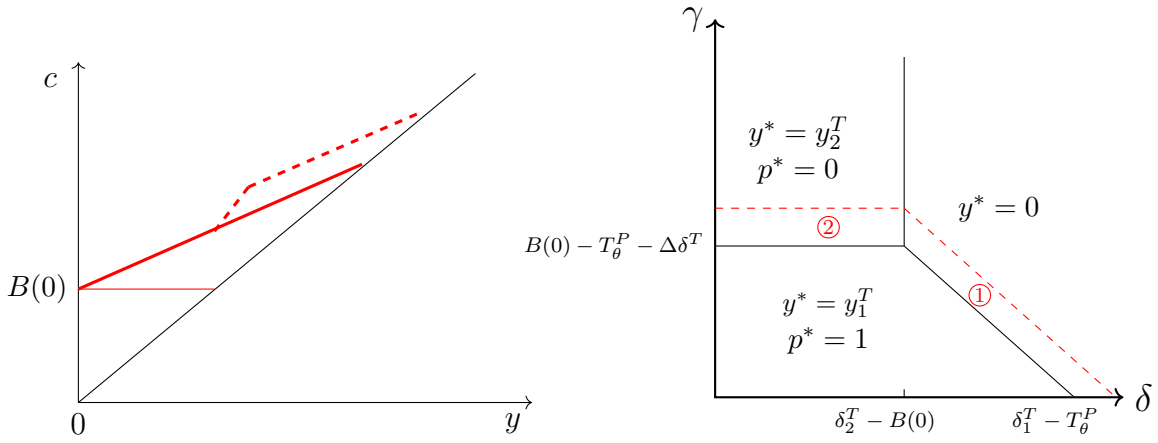
welfare.[43] The first fiscal externality is a decrease in public spending by an amount $T_\theta^P(y)$ due to the positive take-up response among individuals who start working (effect ①). This behavioral response also triggers an increase in private welfare by an amount denoted $dV_1(y) \equiv (\delta^T(y) - \delta) + (B(y) - \gamma)$ for these individuals. This first effect is governed by the "awareness" labor force participation elasticity $\tilde{\eta}^\theta$, which can be defined as the percentage change in the share of individuals working, conditional on taking up, caused by a one percentage change in the share of individuals aware of the benefit. The second fiscal externality is an increase in public spending by an amount $B(y)$ due to the positive take-up response among individuals already working (effect ②). This behavioral response also triggers an increase in private welfare by an amount denoted $dV_2(y) \equiv (B(y) - \gamma)$ for individuals who respond. This second effect is governed by the "awareness" take-up elasticity ξ^θ which captures the percentage change in the take-up rate caused by a one percentage change in the share of individuals aware of the benefit. The elasticities are sufficient statistics that can be estimated empirically using the 2019 welfare reform. The participation tax $T^P(y)$ and benefit level $B(y)$ are known. However, dV_1 and dV_2 are not directly observable.

6.3.4 Welfare effect of the benefit increase

I now consider a welfare reform that increases the in-work benefit B served for individuals with earnings above a certain threshold, such as the one depicted in Figure 11(a). Formally, a tax reform is an object (τ, h) that replaces the initial transfer $B(y)$ by $B(y) + \tau h(y)$. In this section, I restrict the analysis to reforms where $h(0) = 0$ as is the case of the 2019 reform.

After a benefit increase such as the one described above, two types of behavioral responses can arise. First, because the participation tax T_θ^P has decreased, there can be a positive take-up and extensive labor supply response among individuals previously not working but who were aware of the program and had a relatively low cost of claiming compared to their cost of working (effect ① in Figure 11(b)). Second, because the benefit amount that can be claimed has increased, *conditional* on labor supply choices, there can be a positive take-up response among individuals previously working but not claiming the benefit who are aware of the benefit and

[43] Behavioral responses affect private welfare because the standard envelope theorem argument does not apply anymore. Indeed, individuals responding to an information shock were not making privately optimal choices before the shock.



(a) Description of the reform

(b) Effect of the reform

Figure 11: Effect of the benefit increase

Lecture: In Panel (a), the solid red line represents the initial budget constraint. The dashed red line represents the change in the budget constraint introduced by the reform. In Panel (b), the solid black lines represent the initial individual solution and the dashed red lines represent the solution after the reform.

have a low enough cost of claiming (effect ② in Figure 11(b)). This response can also imply an intensive margin labor supply response which sign depends on the sign of $B'(y_2^T)$.

Proposition 4. *The marginal welfare effect of the reform* (τ, h) *around* $\tau = \bar{\tau}$ *is:*

$$W_\tau(\bar{\tau}, h) = \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y) \left[h(y)\alpha(\omega^T(y)) - R_\tau(\bar{\tau}, h) \right] dy$$

$$\text{where } R_\tau(\bar{\tau}, h) = \left[h(y) \left[1 + \tilde{\eta}(y)(\bar{\tau}h(y) - T^P(y)) + \xi(y)(\bar{\tau}h(y) + B(y)) \right] + h'(y)\varepsilon(y)y \frac{B'(y) + \bar{\tau}h'(y)}{1 + B'(y)} \right]$$

Formula 4 provides the marginal effect of the reform on total welfare. As is standard in the literature, it can be decomposed into different components. First, the reform generates a direct welfare effect due to the marginal increase in the transfer by an amount $h(y)$ for low-income agents who work and take up. This effect is captured by the term $h(y)\alpha(\omega^T(y))$, where $\alpha(\omega^T(y))$ represents the social welfare weight associated to this private welfare gain. The reform also generates a change in public spending, captured by the term $R_\tau(\bar{\tau}, h)$, the marginal effect on public spending of a reform (τ, h) around $\bar{\tau}$. As is standard in the literature, this effect can be split into (i) the direct mechanical effect on public spending

of changing the transfer schedule by an amount $h(y)$ and (ii) an indirect fiscal externality effect due to behavioral responses. The welfare formula is expressed in terms of observable parameters and three key sufficient statistics; the labor supply elasticities at the intensive and extensive margin, and the take-up elasticity with respect to the benefit amount.

6.3.5 Welfare formulas for large reforms

In the previous subsections, I have provided welfare formulas for the marginal effects of various reforms. However, actual reforms, like the 2019 French welfare reform, can rarely be considered as small. Large reforms can be seen as a sequence of small reforms evaluated at different starting points. Following H. J. Kleven (2021), I use the trapezoid approximation to express the welfare effect of a large reform as the average of the marginal welfare effects evaluated at the pre-reform situation ($W_\tau(0,h)$) and at the post-reform situation ($W_\tau(\tau,h)$) multiplied by the size of the reform (τ). I denote by $\bar{q}_0$, f_0 and α_0, the pre-reform take-up rate, income distribution conditional on take-up and welfare weights. I denote by $\bar{q}_1$, f_1 and α_1, the post-reform take-up rate, income distribution conditional on take-up and welfare weights. I use the same methodology to evaluate the welfare effect of the information shock, i.e. of a non-small change in the awareness rate $p(\theta = 1)$.

Proposition 5. *The total welfare effect of the reform (τ, h) can be approximated in the following way:*

$$W(\tau,h) = \frac{\tau}{2}\left[\bar{q}_0 \int_{\underline{y}}^{\bar{y}} f_0(y)\left(h(y)\alpha_0(y) - R_\tau(0,h)\right)dy + \bar{q}_1 \int_{\underline{y}}^{\bar{y}} f_1(y)\left(h(y)\alpha_1(y) - R_\tau(\tau,h)\right)dy\right]$$

Proposition 6. *The total welfare effect of an increase by $\Delta\theta$ of the fraction of individuals with type $\theta = 1$ can be approximated in the following way:*

$$\begin{aligned} W(\theta) = &\frac{\Delta\theta}{2}\left[\bar{q}_0 \int_{\underline{y}}^{\bar{y}} f_0(y)\left(\tilde{\eta}^\theta(y)(\alpha_0(y)dV_1(y) + T_\theta^P(y)) + \xi^\theta(y)(\alpha_0(y)dV_2(y) - B(y))\right)dy\right] \\ + &\frac{\Delta\theta}{2}\left[\bar{q}_1 \int_{\underline{y}}^{\bar{y}} f_1(y)\left(\tilde{\eta}^\theta(y)(\alpha_1(y)dV_1(y) + T_\theta^P(y)) + \xi^\theta(y)(\alpha_1(y)dV_2(y) - B(y))dy\right)\right] \end{aligned}$$

6.3.6 Calibration

I now turn to the empirical welfare analysis of the 2019 welfare reform using the welfare formulas from Proposition 5 and Proposition 6. I detail below how I calibrate the parameters of these formulas.

- $B(y), T^P(y), B'(y)$ (the tax-benefit parameters). For each observation, I micro-simulate their benefit amount, participation tax, marginal tax rate, given their income and family situation. To also get the changes in these parameters caused by the reform, I simulate it under the pre-reform system and under the post-reform system.

- τ (the amplitude of the parametric reform). I calibrate τ to be the average change in B in the overall population of beneficiaries of the PA program.

- $h(y)$ (the direction of the parametric reform). For each observation, I calibrate h to be the observed change in B divided by τ.

- $\Delta\theta$ (the magnitude of the information shock). $\Delta\theta$ represents the absolute change in the fraction of individuals aware of the existence of the benefit (i.e. of $p(\theta = 1)$). Using the results obtained from the DREES survey, I calibrate this parameter to be 5 p.p.t.

- $\bar{q}_0 f_0(y)$ and $\bar{q}_1 f_1(y)$ (the pre-reform and post-reform income distribution conditional on taking up). I use the number of observations of beneficiaries (i.e. the total number of observations in the administrative welfare data with strictly positive amounts of PA transfer declared) at each income level to calibrate these parameters. The pre-reform distribution is the one present of November 2018, to avoid capturing the effect of responses that started in December 2018. The post-reform distribution is the one of January 2019.

- $\tilde{\eta}(y)$ (the labor force participation semi-elasticities with respect to the net-of-tax income). As most of the literature has estimated elasticities, I modify the formulas to express them in terms of elasticities rather than semi-elasticities.[44] I calibrate the elasticity of the labor force participation conditional on take-up to be 0.2. In a recent paper, H. Kleven (2024) estimates a non-significant employment elasticity for all EITC reforms except for the 1993 reform where the mean elasticity is 0.63. There is however an ongoing debate on the magnitude of this parameter in the literature. Therefore, I perform sensitivity analysis by making this parameter vary between 0 and 0.6, a range large enough to encompass most of the existing estimates of this parameter, to the best of my knowledge. For now, I also assume constant elasticity (and hence decreasing

[44] Let $\tilde{\eta}^r(y)$ be the regular elasticity and $\tilde{\eta}(y)$ be the semi-elasticity. Then we have that $\tilde{\eta}(y) = \tilde{\eta}^r(y)/(y - T^P(y))$, where the net-of-tax income $y - T^P(y)$ can easily be calibrated using the information described above.

semi-elasticity) across income groups. As this parameter is likely context-specific and heterogeneous across groups, I plan to estimate the labor supply responses to the 2019 reform in future work to better calibrate the parameter.

- $\varepsilon(y)$ (the earning elasticities at the intensive margin). Sicsic (2022) estimates the intensive margin elasticity in the context of France for the period 2005 to 2015. He finds an average elasticity of 0.1 when focusing on in-work benefit reforms, somehow smaller than the overall elasticity considering all reforms which is about 0.2 to 0.3. I calibrate the intensive margin earning elasticity to be 0.1. I perform sensitivity analysis by making this parameter vary between 0 and 0.4.

- $\xi(y)$ (the take-up semi-elasticity). Note that what I label as a take-up rate semi-elasticity is instead an enrollment rate semi-elasticity (i.e. the percentage increase in the share of the population enrolled in the program following a one euro increase in the benefit amount). Therefore, it fits with what I have estimated empirically and the welfare analysis doesn't require to make any assumption about the baseline take-up rate in the population. In Section 5.3, I have estimated that the average take-up elasticity with respect to the benefit amount was around 0.1.[45] Again, I perform a slight modification of the welfare formula to make appear the take-up elasticity rather than semi-elasticity. I perform sensitivity analysis by making this parameter vary between 0 and 0.4.

- $\tilde{\eta}^{\theta}$ (the labor force participation semi-elasticities with respect to a change in awareness). For now, I calibrate this parameter to be zero as there is no empirical estimates of this parameter in the literature and I haven't estimated it yet. Note that instead of having a labor force participation elasticity with respect to the share of aware individuals in the population, the model could feature a labor force participation elasticity with respect to the net-of-tax income that is context-specific and depends on the awareness rate in the population. Hence, an information shock would increase the parameter $\tilde{\eta}(y)$. Closer to this framework, Kostøl and Myhre (2021) find that following the receipt of a letter detailing the tax incentives created by the disability insur-

[45] While the empirical analysis pointed to a non-significant response of claiming behaviors to changes in the benefit amount, I nevertheless use the point estimate found in the empirical analysis. Given that take-up responses to an increase in program generosity generate a larger fiscal externality than take-up responses to an increase in program awareness, this should lead to an underestimation of the positive welfare effects of the reform.

ance system in Norway, beneficiaries would adjust their earnings such that the (intensive-margin) earning elasticity increases from 0.06 to 0.15.

- $\xi^{theta}(y)$ (the take-up semi-elasticities with respect to a change in awareness). In Section 5.3, I have shown that following the information shock, that increased the share of individuals aware of the benefit by 5 p.p.t in the population, enrollment increased by 17%. Hence, I calibrate the parameter $\xi^{\theta}(y)$ to be 3.4. I assume that this elasticity does not vary across income groups.

- $dV_1(y)$ and $dV_2(y)$ (the private welfare gains of individuals responding to the awareness shock). As I have set $\tilde{\eta}^{\theta}(y)$ to zero, I also set $dV_1(y)$ to zero. The private welfare gains of individuals starting to take-up (conditionally on already working) is $dV_2 = B(y) - \gamma$. Claiming costs γ are private types that are not observed. I assume that the average claiming cost in a given income group is 12 euros, which corresponds approximately to the opportunity cost of spending one hour on the application for a minimum wage worker in France. On the one hand, this estimate is likely overestimating the time cost because the application takes about twenty minutes in practice. On the other hand, by focusing solely on the opportunity time cost I'm likely under-estimating the true cost of applying, which is likely including the disutility for doing paperwork or possible stigma feeling.

- $\alpha_0(y)$ and $\alpha_1(y)$ (the welfare weights sequences). These welfare weights represent the social value of transferring one euro from the overall population[46] to a low-income household with earnings y and who take up the program. In my main calibration, I use the following method to assign welfare weights. I assume that there are two types of individuals in the population, low-type (i.e. low ω) and high-type (i.e. high ω).[47] Hence, all households taking up face the same welfare weight, which is the welfare weight assigned to low-type households: $\alpha_0(y) = \alpha_0$ and $\alpha_1(y) = \alpha_1$, for all y. I assume that the social planner values the consumption of low-type individuals 10% more than the consumption of high-type individuals. In sensitivity analyses, I vary this percentage (including setting it to zero which implies utilitarianism preferences). Under these social preferences, we have that $\alpha_0 = \alpha_1 = 1.086$.[48]

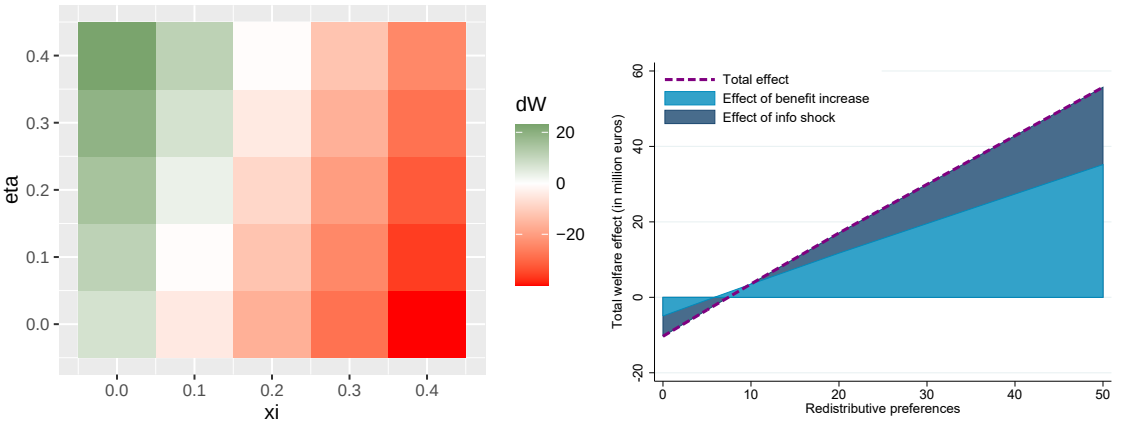
[46]The average welfare weight in the population is 1.

[47]Finkelstein and Notowidigdo (2019) also use a discrete 2-type case for their numerical simulation.

[48]In France, there was a total of 28.9 million of households in 2018. There were 2.7 million

6.3.7 Numerical simulation

Overall, using my preferred calibration, I find that the 2019 reform has increased welfare by 3.55 million euros for the month of January 2019. This represents a welfare gain of about 0.8% of the pre-reform monthly spending devoted to the PA program. The benefit increase (i.e. the parametric reform) generated a welfare increase by 3.5 million euros and the information shock accounts for a welfare increase by 0.05 million euros. Figure 12 presents the results of the sensitivity analysis that varies the parameters.



(a) Welfare effects by elasticities

(b) Welfare effects by welfare weights

Figure 12: Sensitivity analysis

Note: In Figure (a), the welfare weights are chosen such that the consumption of low types is valued 10% more by the social planner than the consumption of high types (preferred calibration). In Figure (b), the parameter ξ is set to 0.1 and the parameter $\tilde{\eta}$ to 0.2 (preferred calibration).
Lecture: In Figure (b), if the government values consumption of low-type households by 20% more than the one of high-type, the total welfare effect of the reform amounts to 16.7 million euros.

Welfare effect of the benefit increase Decomposing the effects of the parametric reform further, I estimate that its mechanical cost was 98 million of euros[49]

beneficiaries of the PA program, and, under the assumption that the baseline take-up rate is 73% (DREES 2017), there were 1 million eligible households not taking up. I consider that all eligible households are low-type which represents a total of 2.7 + 1 = 3.7 million households. Conversely, all non-eligible households are high-type, which represents a total of 28.925 - 3.7 = 25.2 million households. We can solve for the welfare weights under the condition that the average welfare weight in the population is one.

[49]All costs and effects are expressed for the month of January 2019. The overall cost of the reform for year 2019 is much larger.

and that the extra cost due to fiscal externalities is 5 million of euros. The reform generates positive extensive-margin labor supply responses which represent a *decrease* in cost by 8 million, and intensive-margin labor supply responses which represent an additional cost of 1 million euros. Moreover, the positive take-up responses represent an increase by 12 million euros for public spending. Accounting for the positive welfare effects of redistributing benefits towards low-income beneficiaries, a welfare gain of 106 million of euros, the net total effect is an increase in welfare of 3.33 (106 - 98 - 5) million euros.

Welfare effect of the information shock Regarding the effects of the information shock, I find that the take-up responses to the shock generated a cost increase of 68.58 million but these responses also generated welfare gains for those starting to take up amount 68.63 million. We note that new beneficiaries' welfare gains are almost the same as the fiscal externalities they generate. On the one hand, the welfare gains of new beneficiaries should be higher than the fiscal externality because the social planner values by more than one euro, a transfer of one euro to these households (see the welfare weight calibration). On the other hand, new beneficiaries must bear the costs of application γ which reduces their welfare gains compared to the fiscal externality. Figure 12 shows that the information shock is welfare-improving for redistributive preferences such that the government values the consumption of low-types at least 8% more than the consumption of high-types. As expected, the magnitude of the welfare gains generated increases with the strength of redistributive preferences. If consumption of low-types households is valued by society 50% more than consumption of high-types, the information provision can generate a welfare gain of 20.5 million, i.e. almost 5% of the PA public spending.

Policy implications discussion In this section, I have investigated the welfare effects of two interventions: an increase in transfer amount and a positive information shock. Contrary to information provision, raising the amount of transfer is rarely discussed as a policy tool to improve take-up of a program. This is mainly because it imposes a large mechanical cost as it benefits to marginal as well as infra-marginal beneficiaries. Instead, what policy-makers and economists traditionally have in mind is administrative burden reduction (i.e. a negative shock on the γ parameter in the model discussed above). I now discuss and compare the welfare implications of information provision and administrative burden reduction.

First, regarding information provision, my theoretical results have shown that it generates welfare gains that are (i) increasing in social redistributive preferences, (ii) increasing in the magnitude of the optimization error due to unawareness (i.e. increasing in transfer amount eligibility and decreasing in the size of application costs/administrative burden) and (iii) increasing in the magnitude of the elasticity of take-up with respect to the awareness rate in the population (as long as redistributive preferences are strong enough and admin burden small enough to make take-up responses generate positive welfare gains). Thus what is key is to know *who* are the individuals facing information frictions (and thus what is the social value of transferring them additional resources), as well as to understand how much transfer they are eligible for and how large is their cost of taking up. In my setup, costs are likely to be small on average, as the PA program tends to be easier to navigate than most welfare programs (e.g. low stigma, online application, no documents to send, short application time). But in cases where average costs are too high - or if we expect that those facing larger information frictions are also those facing larger costs -, it might be preferable to start by tackling the issue of too high administrative burden first. Information provision can also generate welfare gains by raising jointly employment and take-up.[50] Indeed, in the case of in-work benefits, information frictions might lead individuals to overestimate their participation tax thus causing a negative distortion of employment. In this case, irrespective of the redistributive preferences, reducing information frictions to encourage work and take-up of the in-work benefit unambiguously creates a first-order welfare gain. This rationale for why raising take-up might be efficient has been overlooked in the literature. It could however be first-order when thinking about in-work benefits specifically.[51]

Second, my results can also speak about the welfare effects of administrative burden reductions. Indeed, while I have not derived welfare formulas for this type of intervention, it is easy to see that it has very similar properties than a benefit increase.[52] Indeed, what drives labor supply and take-up decision in the model is

[50]Note that I have abstracted from this in my empirical welfare analysis because of a lack of information to calibrate the parameters.

[51]I'm carrying out additional analysis to try to estimate this effect empirically. It requires to have panel information about new claimants to know whether they were working or not before claiming, which is not available in the welfare administrative records I use. However, new data have been released which could allow me to investigate this by linking welfare administrative records with matched employer-employee panel data.

[52]I assume here that administrative burdens are costs imposed on beneficiaries and that they are well-perceived by individuals, meaning that individuals optimize perfectly when facing such

the net benefit $(B(y) - \gamma)$. A decrease in administrative burden generates three effects. First, it can generate a negative fiscal externality due to positive take-up responses coming from working individuals, of the same magnitude than what an increase in transfer does. Second, it can generate a positive fiscal externality due to positive take-up and employment responses coming from non-working individuals, of the same magnitude as an increase in transfer does. Third, it generates a positive welfare gain for infra-marginal beneficiaries who face lower costs. Note that contrary to the benefit increase, this welfare gain for infra-marginal beneficiaries has no fiscal costs as long as administrative burdens acted as pure ordeals (i.e. deadweight costs that did not serve any purpose apart from potentially acting as a mechanism for self-screening). [53] Both the information provision and the administrative burden reduction can generate positive fiscal externalities by encouraging those out of work to start working and taking up the in-work benefit. Whether one is more effective in doing so than the other depends on the distribution of types in the population. Both can also generate take-up responses from those already working, but these can improve social welfare only in the case of information provision. Indeed, by the envelope theorem, there will be no first-order private gains for those induced to take up due to a reduction in administrative burden as they were already optimizing their behavior. Finally, we note that administrative burden reductions has very different distributive properties than information provision: the former will mostly benefit to those already taking up while the latter will mostly benefit to those who are induced to take-up.

7 Conclusion

This paper studies the issue of imperfect take-up to welfare programs by focusing empirically on the case of a French in-work benefit. Using exhaustive administrative data, I document the very large and unexpected take-up response triggered by the 2019 reform of the program. According to a pre-reform estimation made by the French Ministry of Social Affairs, 27% of eligible families were not claiming the program. I show that aggregate enrollment increased by 20% due to the take-up response to the reform which suggests a cut by half of the non take-up rate. Using this

burdens.

[53] Some costs borne by applicants are not deadweight costs but costs linked to monitoring or information collection from the social planner. The welfare implications of decreasing such costs are of course different.

unique natural experiment, I investigate what drove the take-up responses (*mechanisms*) and who were the families responding (*targeting properties*). Results are consistent with imperfect take-up being an optimization mistake driven by imperfect information rather than a rational choice driven by high claiming costs. Indeed, exploiting the heterogeneous change in benefit amount created by the reform, I find a fairly small elasticity of the take-up rate with respect to the monetary amount of the benefit (about 0.1). Instead, I show evidence suggesting that the reform might have acted as an information shock. Using survey data, I find that the share of eligible individuals aware of the existence of the program increased by 5 p.p.t (+7%) after the reform. Heterogeneity analysis are consistent with the idea that families not aware of the existence of the program learned about the existence of the program and started to take-up. Regarding the targeting properties of imperfect take-up, I find mixed evidence regarding the socio-economic conditions of the new entrants. While new entrants come from groups typically considered as less in need than others (e.g. single males without kids, non-foreigners..), they are also poorer on average. Overall, the results do not support strongly the view that barriers to take-up (like information frictions) might be efficient self-targeting mechanisms.

Using the insights from the empirical analysis, I build a theoretical framework with endogenous take-up decisions to assess the welfare effects of interventions aimed at raising take-up. I express the welfare formulas in terms of sufficient statistics that can be estimated within the empirical framework. Calibrating the formulas with my empirical estimates and previous literature estimates, I find evidence that the bundled reform was overall welfare-improving, under a large range of redistributive preferences assumptions. The model allows me to discuss and compare the welfare implications of the two standard types of interventions discussed to raise take-up; administrative burden reduction and information provision. In particular, I show that both interventions can reduce the perceived participation tax and thus induce individuals out of the labor force to start working and taking up. Assuming that the overall participation tax is positive, this generates a positive fiscal externality. While most of the debate about raising take-up has focused on *equity* arguments, this highlights a previously overlooked *efficiency* rationale for raising take-up of in-work benefits.

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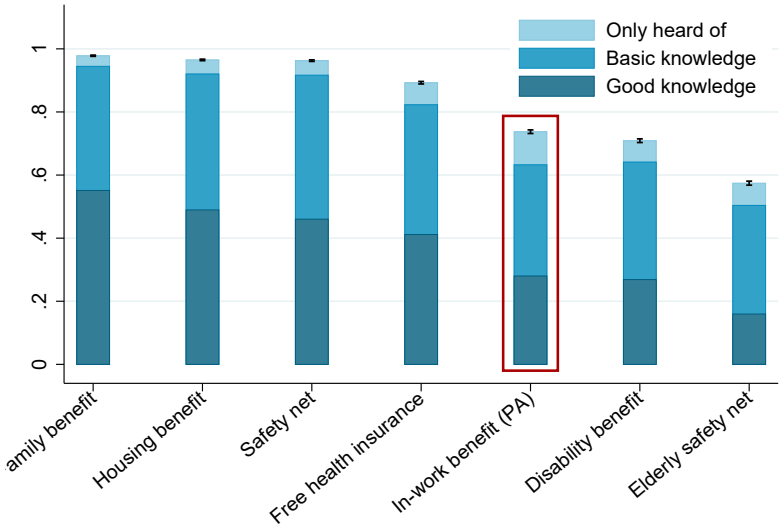
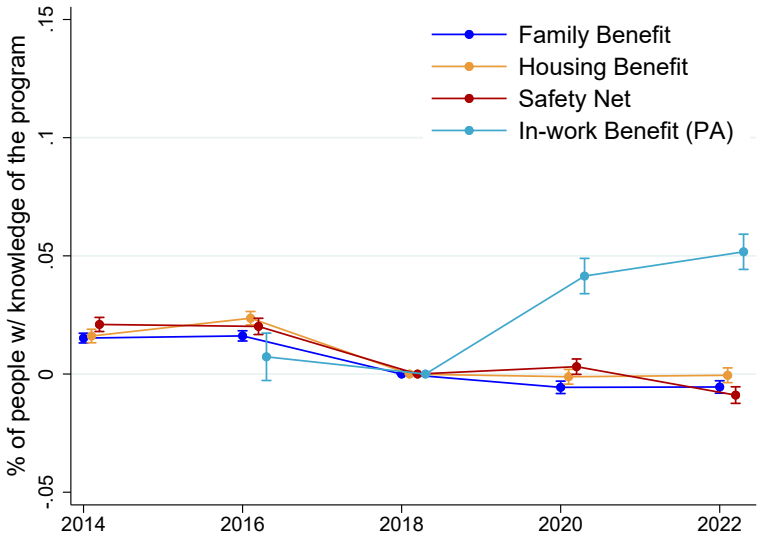
Appendix

A Additional Graphs



A.1.1 Knowledge in 2018



A.1.2 Knowledge's evolution

Figure A.1: Knowledge about welfare programs

Sources: General Opinion Survey (*Baromètre d'opinion*) and author's own computations.
Notes: Sample restricted to individuals aged 18 to 65 years old. Information is missing for the PA program in 2014 since the program was introduced in 2016. Graph (b) represents the event-studies coefficients of a regression of the share of individuals who heard about a given program on yearly dummies.

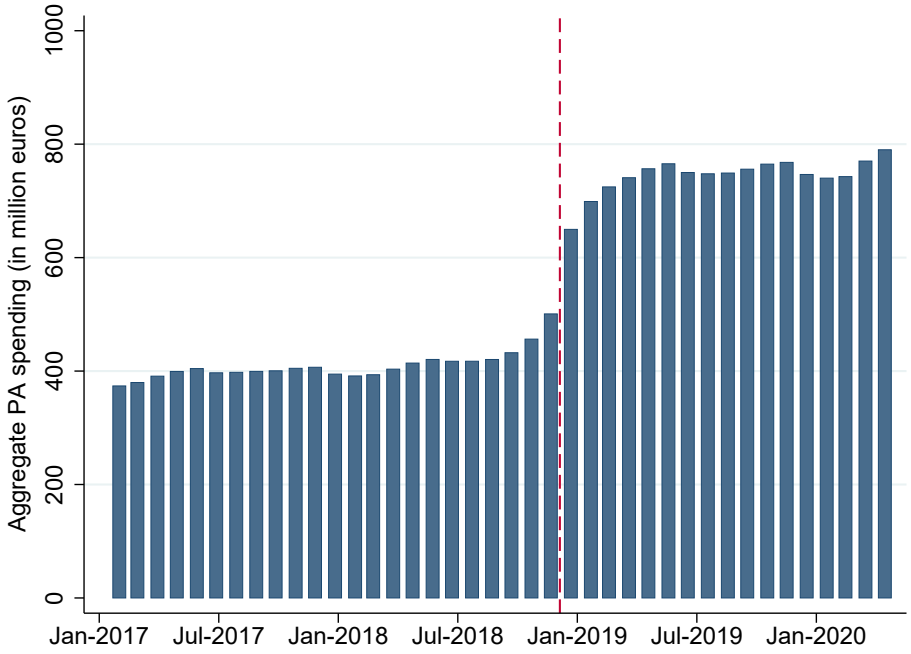


Figure A.2: Evolution of aggregate PA spendings

Sources: Social records data ALLSTAT (2017-2020).

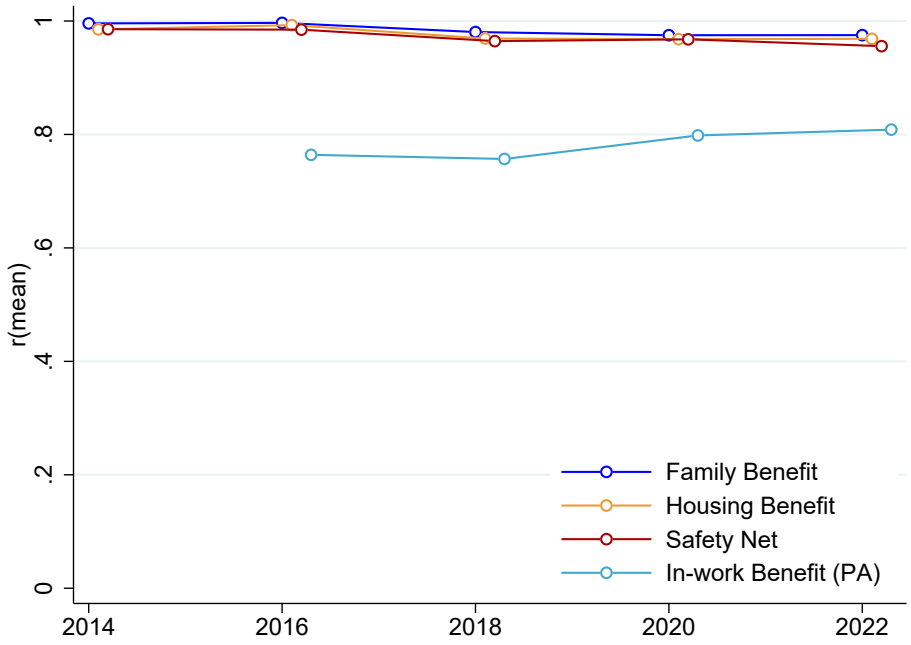
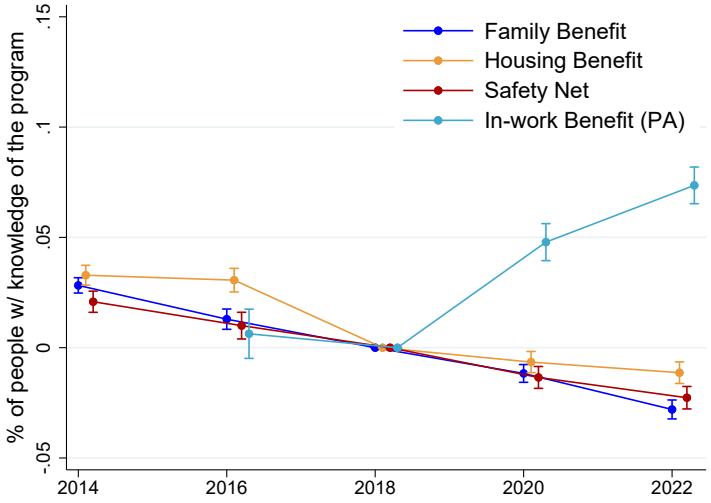
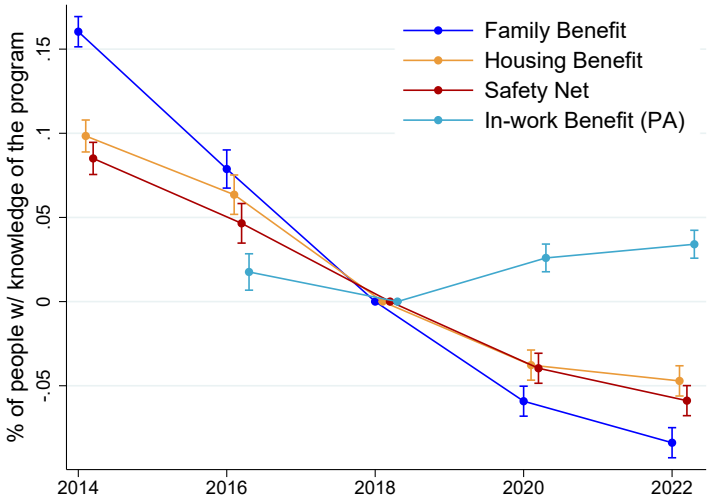


Figure A.3: Evolution of programs' knowledge

Sources: General Opinion Survey (*Baromètre d'opinion*) and author's own computations.
Notes: Sample restricted to individuals aged 18 to 65 years old.



A.4.1 Intermediate Knowledge



A.4.2 Good Knowledge

Figure A.4: Knowledge about welfare programs

Sources: General Opinion Survey (*Baromètre d'opinion*) and author's own computations.
Notes: Sample restricted to individuals aged 18 to 65 years old. The graphs represent the event-studies coefficients of a regression of the share of individuals who heard about a given program on yearly dummies. Panel (A) represents the results for the outcome variable being a dummy equal to 1 if one answers "Yes, but approximately" to the question "Do you know who can benefit from the PA program?". Panel (B) represents the results for the outcome variable being a dummy equal to 1 if one answers "Yes, quite precisely" to the question "Do you know who can benefit from the PA program?".

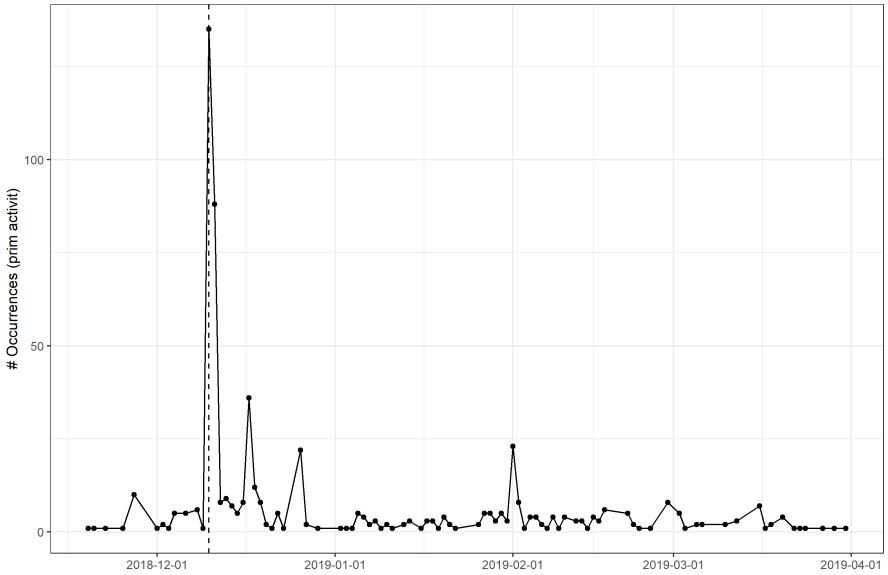


Figure A.5: PPA benefit in yellow vests Facebook groups' comments

Sources: Database from Boyer et al. (2020).
Lecture: Number of occurrences of the pair of adjacent words "prim" and "activit" in discussions on Facebook pages associated to "Gilets Jaunes".

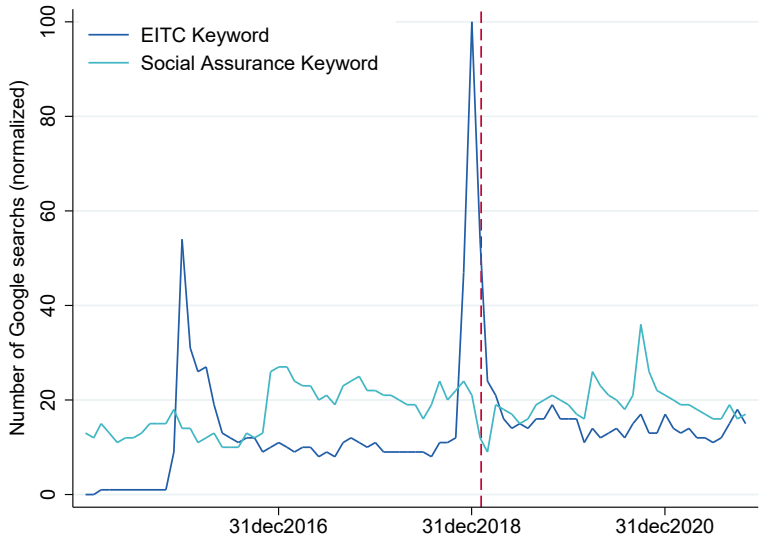


Figure A.6: Google Trends associated with the PPA and the RSA benefits

Sources: Monthly Google Trends.
Notes: The keywords used are "prime d'activité" (PA) and "revenu de solidarité active" (RSA). The number of searches is normalized to 100 at the date with the highest number of research for the topic in the region and period considered.

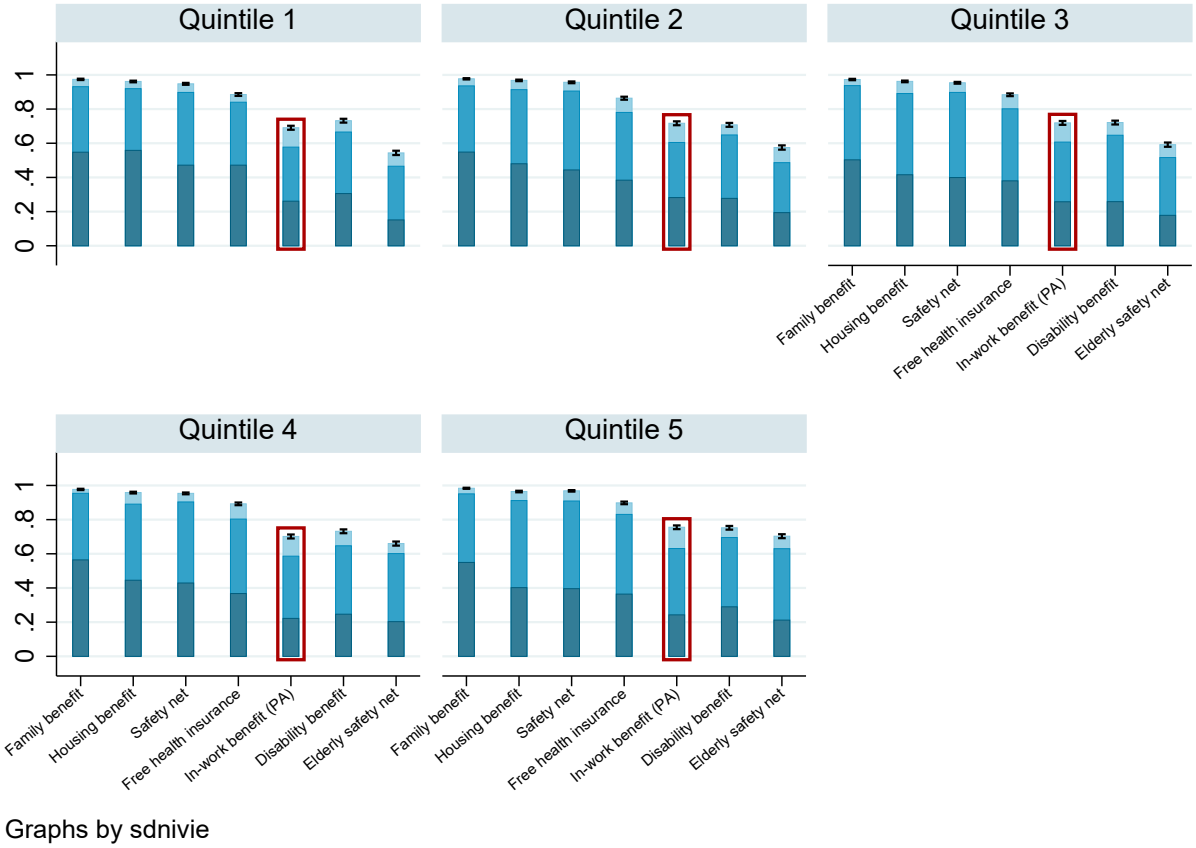


Figure A.7: Knowledge level by quintile of disposable income

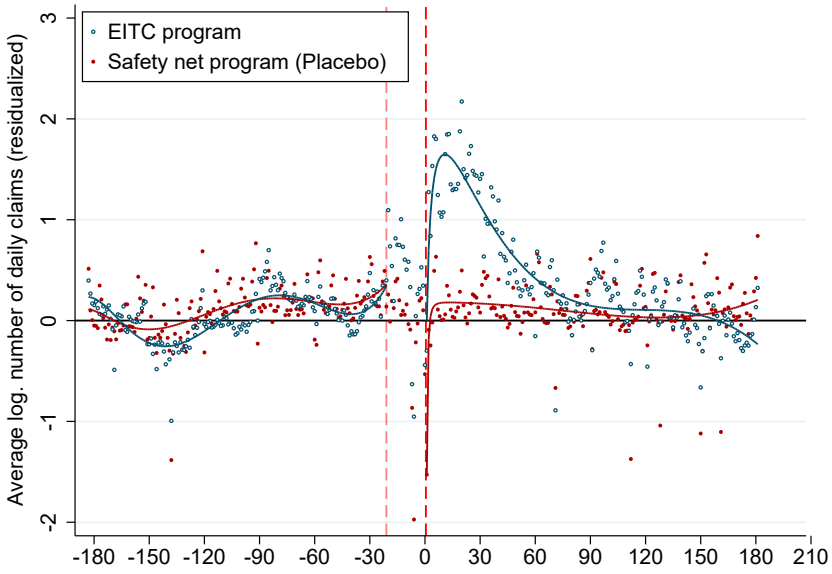
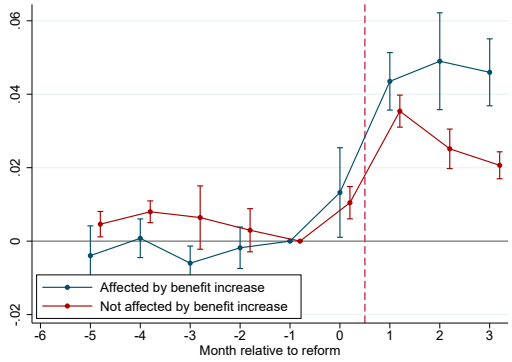
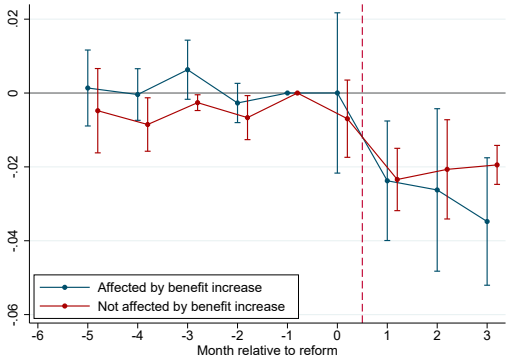


Figure A.8: Discontinuity in log number of daily claims

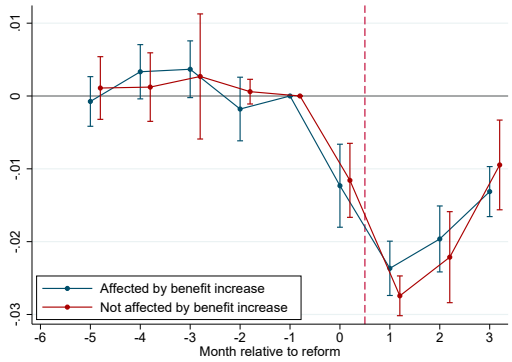
Note: Daily total log number of claims after removing a day-of-the-week fixed effect. The x-axis represents the number of day relative to the cut-off date (January 1st of a given year). The solid lines represent polynomial fits of degree four, before and after the January 1st event (excluding the announcement period, i.e. Dec. 10th-Dec. 31st). The red dots serve as a placebo tests.



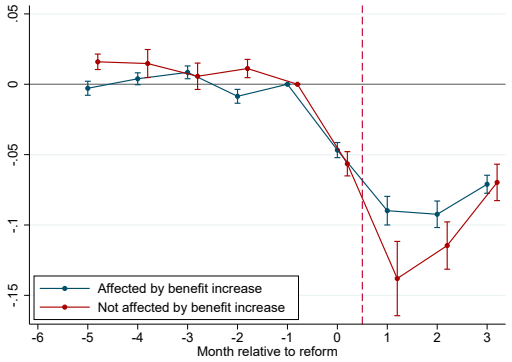
A.9.1 Share of single male



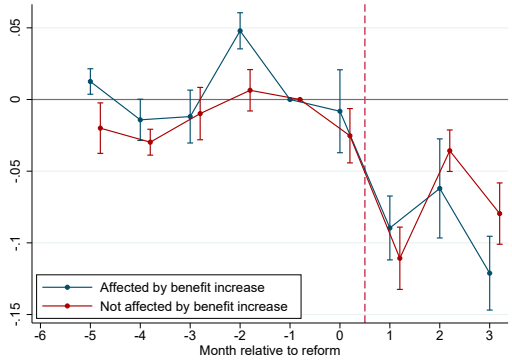
A.9.2 Share of couples



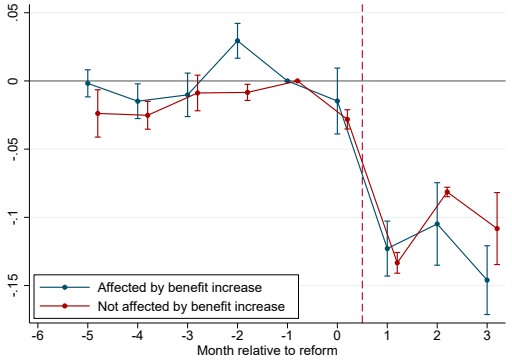
A.9.3 Share of non-EU foreigners



A.9.4 Share of past beneficiaries of the safety net



A.9.5 Average log monthly wage earnings



A.9.6 Average log taxable income

Figure A.9: Difference-in-difference estimation on average characteristic

B Additional Details about the Institutional Setting

Equation 15 provides the exact formula of the PA transfer in a given month. Table 1 and Table 2 detail the legislative parameters used to compute this formula over the 2016-2020 period. The base amount depends solely on the family composition of the household captured by the parameter δ^f. The benefit is augmented by a fraction τ_t of the total household's labor income R^a at month t. The means-testing part consists in subtracting from the amount of the benefit the total household's resources R including labor income R^a as well as any other types of income (e.g. family/housing/unemployment benefits, pensions, capital income). If these resources are lower than the base amount, they are assumed to be equal to the base amount. Hence, when the family's resources R are lower than the base amount, the benefit phases in at rate τ_t. When the resources are higher than the base amount, the benefit phases out at rate $1 - \tau_t$. Finally, the benefit is augmented by individual bonuses granted for each working family member as a function of their labor earnings R_i^a. The detailed formula for the bonus of an individual i is provided by Equation 16.[54]

$$B_t = \underbrace{MB_t\left(1+\delta_t^f\right)}_{\text{Base amount}} + \tau_t R_t^a - \underbrace{\max\left(MB_t\left(1+\delta_t^f\right), R_t\right)}_{\text{Means-Testing}} + \underbrace{\sum_i Bonus\left(R_{it}^a\right)}_{\text{Individual bonuses}} \tag{15}$$

$$Bonus\left(R_{it}^a\right) = \min\left(\tau_b MB_t, \max\left(0, \tau_b MB_t \times \frac{R_{it}^a - s_{min}\bar{w}}{s_{max}\bar{w} - s_{min}\bar{w}}\right)\right) \tag{16}$$

Table 1: Legislative parameters

	Base Amount (MB) in euros	Phase-in rate (τ)	Max. amount of indiv. bonus (τ_b) in % of MW	Income floor for indiv. bonus (s_{min}) in % of hourly MW	Income ceiling for indiv. bonus (s_{max}) in % of hourly MW
01/2016 - 04/2016	524.16	0.62	0.12782	59	95
04/2016 - 04/2017	524.68	0.62	0.12782	59	95
04/2017 - 04/2018	526.25	0.62	0.12782	59	95
04/2018 - 08/2018	531.51	0.62	0.12782	59	95
08/2018 - 01/2019	551.51	0.61	0.12782	59	95
01/2019 - 04/2020	551.51	0.61	0.29101	59	120
04/2020 - 04/2021	553.16	0.61	0.29101	59	120
04/2021 - 09/2021	553.71	0.61	0.29101	59	120

[54] The parameter $\bar{w}$ represents the hourly legal minimum wage.

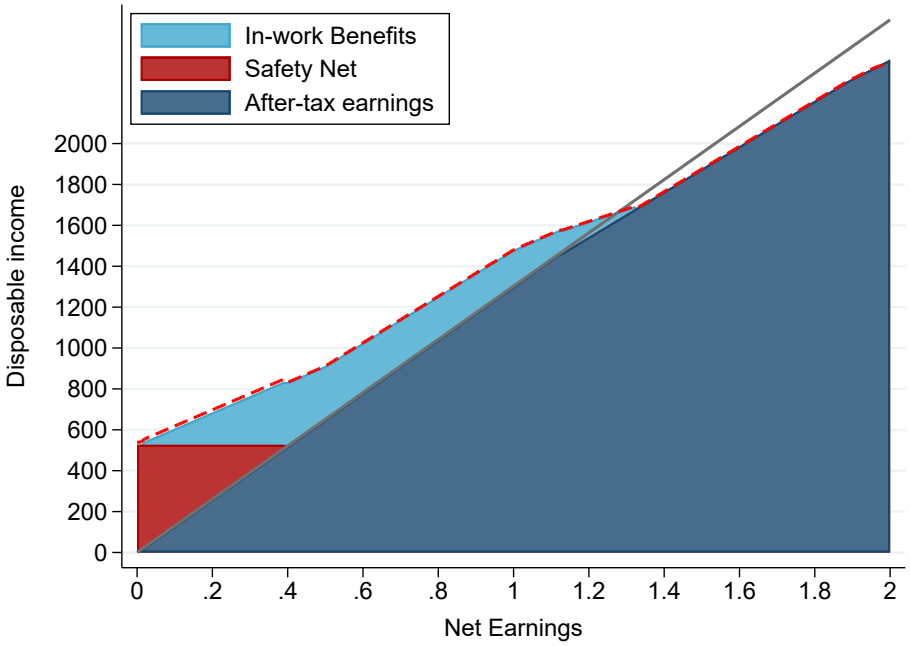


Figure A.10: The budget constraint

Source: French Ministry of Social Affairs. Online simulator EDIFIS
Note: Test-case of a single home-owner individual with only wage earnings as resources. Legislation as of 2022.

Table 2: Base amount and family composition

Adults	Children	Family coefficient δ^f	Total base amount $(1+\delta^f)MB$
1	0	0 %	551.51
1	1	50 %	827.26
1	2	80 %	992.72
1	3	120 %	1213.32
2	0	50 %	827.26
2	1	80 %	992.72
2	2	110 %	1158.17
2	3	150 %	1378.77

C Appendix to the Theoretical Model

C.1 Preliminary Proofs

I start by stating and proving a lemma that provides an expression for the effects of a small reform of the benefit schedule in terms of sufficient statistics. A reform is an object (τ, h) that replaces the benefit schedule $B(y)$ by $B(y) + \tau h(y)$ where $h(.)$ is a flexible function. Lemma 1 characterizes the marginal effect of a reform (τ, h) on public spending. Corollary 1 characterizes the marginal effects of a reform (τ, h) around $\tau = 0$ on public spending, which is a specific case of the previous Lemma.[55]

Lemma 1. *Consider a generic reform (τ, h) that replaces $B(y)$ by $B(y)+\tau h(y)$ where $h(.)$ is continuously differentiable and weakly convex. At any level $\tau = \bar{\tau}$, the marginal increase in public spending due to this reform is given by:*

$$\begin{aligned} R_\tau(\bar{\tau}, h) = \; & \bar{q}(\bar{\tau}) \left[F_{y,\bar{\tau}}(0)h(0) + \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y)h(y)\, dy \right] + \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y)h'(y)\varepsilon(y)y \frac{B'(y) + \bar{\tau}h'(y)}{1 + B'(y)}\, dy \\ & + \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y)(h(y) - h(0))\tilde{\eta}(y)(\bar{\tau}h(y) - \bar{\tau}h(0) - T^P(y))\, dy \\ & + \bar{q}(\bar{\tau})F_{y,\bar{\tau}}(0)h(0)\xi_0(B(0) + \tau h(0))\, dy + \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y)h(y)\xi(y)(B(y) + \bar{\tau}h(y))\, dy \end{aligned}$$

Corollary 1. *The marginal increase in public spending due to a reform (τ, h) around $\tau = 0$ is given by:*

$$\begin{aligned} R_\tau(0, h) = \; & \bar{q} \left[F_y(0)h(0) + \int_{\underline{y}}^{\bar{y}} f_y(y)h(y)dy + \int_{\underline{y}}^{\bar{y}} f_y(y)h'(y)\varepsilon(y)y \frac{B'(y)}{1 + B'(y)} dy \right] \\ & + \bar{q} \left[\int_{\underline{y}}^{\bar{y}} f_y(y) \left[h(0) - h(y) \right] \tilde{\eta}(y)T^P(y) + F_y(0)h(0)\xi_0(0)B(0) + \int_{\underline{y}}^{\bar{y}} f_y(y)h(y)\xi(y)B(y)\, dy \right] \end{aligned}$$

Proof of Lemma 1. The total public spending generated by individuals of type ω under the new system (where the benefit is $B(y) + \tau h(y)$) is:

$$R(\tau, h|\omega) = q(\omega, \tau) \left[(1 - \tilde{r}(\omega, \tau)) \left[B(0) + \tau h(0) \right] + \tilde{r}(\omega, \tau) \left[B(y(\omega, \tau)) + \tau h(y(\omega, \tau)) \right] \right]$$

where $q(\omega, \tau)$ is the *unconditional* take-up rate among type ω individuals, $\tilde{r}(\omega, \tau)$ is the labor force participation rate *conditional* on taking up and $y(\omega, \tau)$ is the optimal earning level for individuals *conditional* on working and taking up. Because the model assumes away taxes for simplification, note that individuals who do not claim benefits do not generate any public spending.
The marginal effect on public spending of a small increase in τ is given by:

$$\begin{aligned} R_\tau(\tau, h|\omega) = \; & q(\omega, \tau) \left[(1 - \tilde{r}(\omega, \tau))h(0) + \tilde{r}(\omega, \tau)h(y(\omega, \tau)) \right] \\ & + q(\omega, \tau)\tilde{r}(\omega, \tau)y_\tau \left[B'(y(\omega, \tau)) + \tau h'(y(\omega, \tau)) \right] \\ & + q(\omega, \tau)\tilde{r}_\tau(\omega, \tau) \left[\tau h(y) - \tau h(0) - T^P(y(\omega, \tau)) \right] \\ & + q_\tau(\omega, \tau) \left[(1 - \tilde{r}(\omega, \tau)) \left[B(0) + \tau h(0) \right] + \tilde{r}(\omega, \tau) \left[B(y(\omega, \tau)) + \tau h(y(\omega, \tau)) \right] \right]. \end{aligned}$$

Using the elasticities concept introduced in Section 6.1, the derivatives of $y(\omega, \tau)$, $q(\omega, \tau)$ and $\tilde{r}(\omega, \tau)$ can

[55] The proofs in this subsection rely on the general framework described in Section 6.1 and 6.2. They do not require the additional simplifying assumptions made in Section 6.3.

be expressed in the following way:

$$y_\tau(\omega,\tau) = h'(y(\omega,\tau))\frac{\partial y(\omega,\tau)}{\partial\left[1+B'(y(\omega,\tau))\right]} = h'(y(\omega,\tau))\varepsilon(\omega)\frac{y(\omega,\tau)}{1+B'(y(\omega,\tau))}$$
$$q_\tau(\omega,\tau) = h(0)\frac{\partial q(\omega,\tau)}{\partial B(0)} = h(0)q(\omega,\tau)\xi_0(\omega) = h(y(\omega,\tau))q(\omega,\tau)\xi(\omega)$$
$$\tilde{r}_\tau(\omega,\tau) = \left[h(y(\omega,\tau)) - h(0)\right]\frac{\partial\tilde{r}(\omega,\tau)}{\partial\left[y(\omega,\tau) - T^P(y(\omega,\tau))\right]} = \left[h(y(\omega,\tau)) - h(0)\right]\tilde{\eta}(\omega)\tilde{r}(\omega)$$

The marginal effect on public spending around τ is then:

$$\begin{aligned}
R_\tau(\tau,h|\omega) = \; & q(\omega,\tau)\left[(1-\tilde{r}(\omega,\tau))h(0) + \tilde{r}(\omega,\tau)h(y(\omega,\tau))\right] \\
& + q(\omega,\tau)\tilde{r}(\omega,\tau)h'(y(\omega,\tau))\varepsilon(\omega)y(\omega,\tau)\frac{B'(y(\omega,\tau)) + \tau h'(y(\omega,\tau))}{1+B'(y(\omega,\tau))} \\
& + q(\omega,\tau)\left[h(y(\omega,\tau)) - h(0)\right]\tilde{\eta}(\omega)\tilde{r}(\omega,\tau)\left[\tau h(y(\omega,\tau)) - \tau h(0) - T^P(y(\omega,\tau))\right] \\
& + h(0)q(\omega,\tau)\left[\xi_0(\omega)(1-\tilde{r}(\omega,\tau))(B(0)+\tau h(0))\right] \\
& + h(y(\omega,\tau))q(\omega,\tau)\left[\xi(\omega)\tilde{r}(\omega,\tau)(B(y(\omega,\tau)) + \tau h(y(\omega,\tau)))\right].
\end{aligned}$$

Let g_ω denote the unconditional skill distribution. We can write the aggregate marginal public spending effect as:

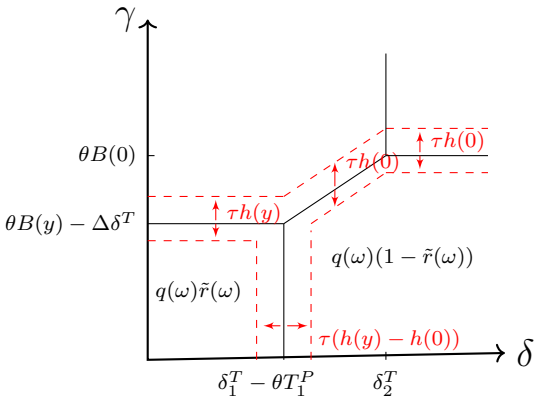
$$\begin{aligned}
R_\tau(\tau,h) = & \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\tau)\left[(1-\tilde{r}(\omega,\tau))h(0) + \tilde{r}(\omega,\tau)h(y(\omega,\tau))\right]d\omega \\
& + \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\tau)\left[\tilde{r}(\omega,\tau)h'(y(\omega,\tau))\varepsilon(\omega)y(\omega,\tau)\frac{B'(y(\omega,\tau)) + \tau h'(y(\omega,\tau))}{1+B'(y(\omega,\tau))}\right]d\omega \\
& + \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\tau)\left[h(y(\omega,\tau)) - h(0)\right]\tilde{\eta}(\omega)\tilde{r}(\omega,\tau)\left[\tau h(y(\omega,\tau)) - \tau h(0) - T^P(y(\omega,\tau))\right]\; d\omega \\
& + \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)h(0)q(\omega,\tau)\left[\xi_0(\omega)(1-\tilde{r}(\omega,\tau))(B(0)+\tau h(0))\right]\; d\omega \\
& + \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)h(y(\omega,\tau))q(\omega,\tau)\left[\xi(\omega)\tilde{r}(\omega,\tau)(B(y(\omega,\tau)) + \tau h(y(\omega,\tau)))\right]\; d\omega
\end{aligned}$$

Under Assumptions (A2), (A3) and an additional assumption that the reform is such that $h(.)$ is continuously differentiable and weakly convex, the optimal income choice $y(\omega,\tau)$ conditionally on working and taking up is strictly increasing in ω. I denote $\omega^T(y) = y^{-1}(y)$. I define as $F_{y,\tau}$ the income distribution conditional on taking up, implemented by the transfer system $B(y) + \tau h(y)$. We then have $F_{y,\tau}(0) = \int_{\underline{\omega}}^{\bar{\omega}} q(\omega,\tau)g_\omega(\omega)(1-\tilde{r}(\omega,\tau))d\omega$ and $F_{y,\tau}(y) = F_{y,\tau}(0) + \int_{\underline{\omega}}^{\omega^T(y)} q(\omega,\tau)g_\omega(\omega)\tilde{r}(\omega,\tau)d\omega$. Substituting the integral in terms of skills ω by integrals in terms of income y, we find that:

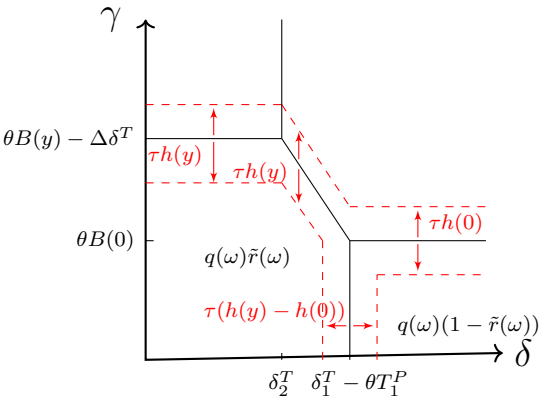
$$\begin{aligned}
R_\tau(\tau,h) = \; & \bar{q}(\tau)\left[F_{y,\tau}(0)h(0) + \int_{\underline{y}}^{\bar{y}} f_{y,\tau}(y)h(y)\; dy\right] \\
& + \bar{q}(\tau)\int_{\underline{y}}^{\bar{y}} f_{y,\tau}(y)h'(y)\varepsilon(y)y\frac{B'(y) + \tau h'(y)}{1+B'(y}\; dy \\
& + \bar{q}(\tau)\int_{\underline{y}}^{\bar{y}} f_{y,\tau}(y)(h(y)-h(0))\tilde{\eta}(y)(\tau h(y) - \tau h(0) - T^P(y))\; dy \\
& + \bar{q}(\tau)F_{y,\tau}(0)h(0)\xi_0(B(0)+\tau h(0))\; dy + \bar{q}(\tau)\int_{\underline{y}}^{\bar{y}} f_{y,\tau}(y)h(y)\xi(y)(B(y)+\tau h(y))\; dy\; \square
\end{aligned}$$

Some extra intuitions The function R_τ illustrates the three standard effects of a small tax reform well known in the optimal taxation literature; a mechanical effect of the reform (first line), an indirect effect due to behavioral responses in terms of labor supply at the intensive margin (second line), and an indirect effect due to behavioral responses at the extensive margin (third line). Accounting for

imperfect take-up changes two things. First, these standard effects are scaled down to account for the fact that individuals who do not take up do not generate any public spending, regardless of their earnings. Importantly, the income distribution that matters for the first three effects is f_y, the income distribution conditional on taking up. Second, there is an additional increase in public spending caused by behavioral take-up responses (fourth line). Conditional on working, take up increases government spending by $B(y_1^T(\omega))$. Conditional on not working, take up increases government spending by $B(0)$. The formula thus depends on two new sufficient statistics, $\xi(\omega)$, the semi-elasticity of the take-up rate with respect to $B(y_1^T(\omega))$ and $\xi_0(\omega)$), the semi-elasticity of the take-up rate with respect to $B(0)$. In the model outlined in Section 6.1, the fiscal externalities can be represented graphically. Figure A.11 represents how the optimal earning and take-up decisions move with a reform (τ, h).



A.11.1 Case when $\theta T_1^P > -\Delta\delta^T$



A.11.2 Case when $\theta T_1^P < -\Delta\delta^T$

Figure A.11: Effect of the reforms on behaviors

C.2 Proofs of Section 6.3

In this section, I provide the proofs for the results of the welfare analysis of the 2019 reform presented in Section 6.3.

Proof of Proposition 3. The total public spending generated by individuals of type (ω, θ) is:

$$R(\omega,\theta) = q(\omega,\theta)\tilde{r}(\omega,\theta)\left[B(y(\omega,\theta)) + B_0(y(\omega,\theta))\right] + q(\omega,\theta)(1-\tilde{r}(\omega,\theta))B(0)$$

where $q(\omega,\theta)$ is the share of individuals taking up the benefit[56], $y(\omega,\theta)$ is the optimal earning level for individuals *conditional* on working and taking-up and $\tilde{r}(\omega,\theta)$ is the labor force participation rate *conditional* on taking up. For simplicity, I drop the other types, γ and δ, in the notations. Note that among individuals not taking up the in-work benefit B, earning an income $y < B(0)$ and taking up the safety net B_0 is a dominated choice by assumptions (A4)-(A6).
The marginal effect on public spending of a small increase $d\theta$ of the share of individuals aware of the program is given by:

$$\begin{aligned} R_\theta(\omega,\theta) = \; & q(\omega,\theta)\tilde{r}_\theta(\omega,\theta)\left[B_0(y(\omega,\theta)) + B(y(\omega,\theta))\right] - q(\omega,\theta)\tilde{r}_\theta(\omega,\theta)B(0) \\ & + y_\theta(\omega,\theta)q(\omega,\theta)\tilde{r}(\omega,\theta)\left[B_0'(y(\omega,\theta)) + B'(y(\omega,\theta))\right] \\ & + q_\theta(\omega,\theta)\tilde{r}(\omega,\theta)\left[B_0(y(\omega,\theta)) + B(y(\omega,\theta))\right] \end{aligned}$$

Individuals who adjust their labor supply at the intensive margin due to an information shock necessarily are individuals who were initially working and were initially not taking up the in-work benefit because they were not aware of it. As explained above, by assumptions (A4)-(A6), they necessarily had earnings such that $y > B(0)$ and hence were facing the laissez-faire budget constraint. Therefore, any intensive labor supply response has no first-order effect on public spending because $B'(y)$ and $B_0'(y)$ are zero initially for these individuals. Overall, after an information shock $d\theta$, there is an increase in public spending due to an increase in the take-up rate among working individuals and a decrease in public spending due to the decrease in the perceived participation tax T_θ^P that triggers a positive take-up and labor supply response. The expression thus simplifies to:

$$R_\theta(\omega,\theta) = q_\theta(\omega,\theta)\tilde{r}(\omega,\theta)B(y(\omega,\theta)) - q(\omega,\theta)\tilde{r}_\theta(\omega,\theta)T_\theta^P(y(\omega,\theta))$$

The derivatives of $q(\omega,\theta)$ and $r(\omega,\theta)$ are given by:

$$\begin{aligned} q_\theta(\omega,\theta) &= q(\omega,\theta)\xi^\theta(\omega) \\ \tilde{r}_\theta(\omega,\theta) &= \tilde{r}(\omega,\theta)\tilde{\eta}^\theta(\omega) \end{aligned}$$

Let g_ω denote the unconditional skill distribution. We can write the aggregate marginal public spending effect as:

$$R_\theta(\theta) = \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\theta)\tilde{r}(\omega,\theta)\left[\xi^\theta(\omega)B(y(\omega,\theta)) - \tilde{\eta}^\theta T_\theta^P(y(\omega,\theta))\right]d\omega$$

Under Assumptions (A2) and (A3), the optimal income choice $y(\omega,\theta)$ conditionally on working and taking up is strictly increasing in ω. I define f_y as the density of individuals earning income y and taking up in a transfer system $B_0(y) + B(y)$. Finally, let $\bar{q}(\theta)$ denote the aggregate take-up rate in the population $\int_{\underline{\omega}}^{\bar{\omega}} q(\omega,\theta)d\omega$. Using these notations and substituting the integral in terms of skills ω by integrals in terms of income y, we find that:

$$R_\theta(\theta) = \bar{q}(\theta)\int_{\underline{y}}^{\bar{y}} f_y(y)\left[\xi^\theta(y)B(y) - \tilde{\eta}^\theta T_\theta^P(y)\right]dy$$

The information shock leads to behavioral responses that have first-order effects on private welfare because it allows agents to correct privately sub-optimal behaviors due to the imperfect awareness of the budget

[56] By extension, and due to Assumption (A6), we assume that among non working agents, $q(\omega,\theta) = 1$, i.e. all agents take up.

constraint. Therefore, assuming that the increase in public spending is funded via a lump-sum tax on all agents in the economy, the marginal effect of the small reform on welfare is:

$$\begin{aligned} W_\theta(\theta) &= \bar{q}(\theta) \int_{\underline{y}}^{\bar{y}} f_y(y)\alpha(\omega^T(y)) \left[\xi^\theta(y)dV_1(y) + \tilde{\eta}^\theta(y)dV_2(y)\right] dy - R_\theta(\theta) \\ &= \bar{q}(\theta) \int_{\underline{y}}^{\bar{y}} f_y(y) \left[\tilde{\eta}^\theta(y)(\alpha(\omega^T(y))dV_1(y) + T^P_\theta(y)) + \xi^\theta(y)(\alpha(\omega^T(y))dV_2(y) - B(y))\right] dy \end{aligned}$$

where $\alpha(\omega^T(y))$ denotes the average welfare weight of agents with earnings y taking up the benefit, $dV_2(y)$ denotes the private welfare gains of those who started to take-up but were initially already working and $dV_1(y)$ the private welfare gains of those who started to take-up but were initially not working. Using the envelope theorem, we can express the private welfare gains in terms of the primitive of our model: $dV_1(y) = (\delta^T(y) - \delta) + (B(y) - \gamma)$ and $dV_2(y) = (B(y) - \gamma)$ □

Proof of Proposition 4. By direct application of Lemma 1 and given that $h(0) = 0$ in the case of the 2019 reform, we can derive the marginal increase in public spending due to the reform (τ, h) around $\tau = \bar{\tau}$:

$$R_\tau(\bar{\tau}, h) = \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}(y) \left[h(y)\left[1 + \tilde{\eta}(y)(\bar{\tau}h(y) - T^P(y)) + \xi(y)(\bar{\tau}h(y) + B(y))\right] + h'(y)\varepsilon(y)y\frac{B'(y) + \bar{\tau}h'(y)}{1 + B'(y)}\right] dy$$

where $f_{y,\tau}$ is the income distribution implemented by the schedule $B(y) + \tau h(y)$.
The marginal effect of the small reform on social welfare W_τ is simply the sum of the mechanical effect it has on individual welfare (an increase in consumption by $\tau h(y)$) weighted by the social welfare weights α minus the cost of the increase in public spending $(R_\tau(\bar{\tau}, h))$, which is assumed to be funded via a lump-sum tax on all agents in the economy.

$$W_\tau(\bar{\tau}, h) = \bar{q}(\bar{\tau}) \int_{\underline{y}}^{\bar{y}} f_{y,\bar{\tau}}\alpha(\omega_T(y))h(y)dy - R_\tau(\bar{\tau}, h)$$ □

C.3 Welfare effects of introducing an in-work benefit

In this section, I revisit partially the question of the optimal transfer program at the bottom of the income distribution (Saez 2001). More precisely, I re-derive the necessary and sufficient conditions for the introduction of an in-work benefit (negative marginal tax rates and participation tax at the bottom) to be welfare-improving. This derivation follows closely the one by Hansen (2021) which uses the two-bracket tax perturbation approach. I provide insights into the policy implications of imperfect take-up by comparing the new conditions obtained with the previous results from Hansen (2021) that relied on a perfect-compliance model.

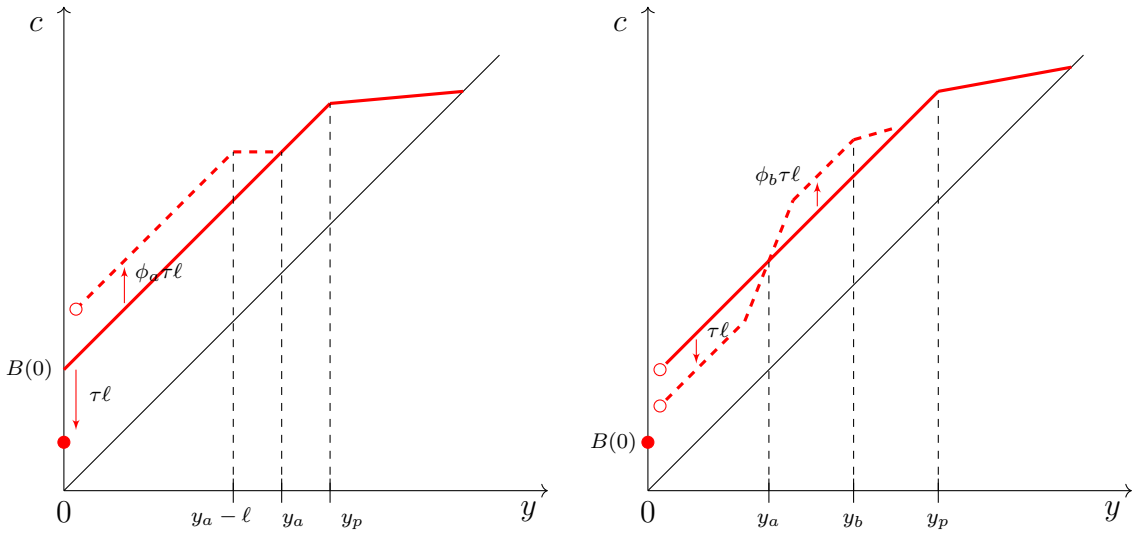
Because the welfare analysis focuses on the introduction of a small in-work benefit, I assume that the initial system has non-negative participation tax rates everywhere (A4). Consistent with Hansen (2021), I also assume that there is an initial linear benefit schedule with a constant participation tax and hence a zero marginal tax rate below a certain threshold of income y_p (A4bis). This schedule is similar to a negative income tax with a demogrant $B(0)$ phasing out at some rates τ. Starting from this system, is it optimal to introduce a small in-work benefit? By deriving the effect of this small reform on welfare, I identify the conditions under which it is welfare-improving.

$$B \text{ is such that } T^P(y) = t_p \text{ for all } y \in (0, y_p). \tag{A4bis}$$

Figure A.12 represents the effect of such reform on the individual's budget constraint. The reform is introduced in two steps. Figure 12A.12.1 represents the effect of the introduction of negative participation taxes. Formally, it introduces two changes: an increase by $\phi_a\tau\ell$ of the benefit amount served for individuals with income in $(0, y_a - \ell)$ and a decrease of demogrant $B(0)$ by $\tau\ell$ for those not working.

Hence, the participation tax initially set to zero becomes negative and equal to $-(1+\phi_a)\tau\ell$ for low-income workers. Figure 12A.12.2 represents the effect of introducing negative marginal tax rates. Formally, it consists of reducing the benefit of workers with income in $[\underline{y}, y_a - \ell]$ by an amount $\tau\ell$ and increasing the benefit of workers with income in $[y_a + \ell, y_b]$ by an amount $\phi_b\tau\ell$. This effectively creates a negative marginal tax rate of $-\tau$ in the range of income $[y_a - \ell, y_a]$ and of $-\tau\phi_b$ in the range of income $[y_a, y_a + \ell]$. The parameters ϕ_a and ϕ_b are calibrated such as to make the reforms budget neutral (before accounting for possible behavioral responses).

Before turning to the analysis, I introduce some notations. Under Assumptions (A2) and (A3), the optimal income choice conditionally on working and taking up, $y_1^T(\omega)$, is strictly increasing in ω. Hence, the tax and benefit system implements an income distribution F_y conditional on taking up with density f_y on the interval $[\underline{y}, \bar{y}]$ and a mass point $F_y(0) = \int_{\underline{\omega}}^{\bar{\omega}} f_\omega(\omega)(1 - \tilde{r}(\omega))d\omega$. I also introduce two more notations. Let $\hat{\alpha}(0) = E_{\Omega\times\Delta\times\Gamma\times\Theta}[\alpha(\omega)|y^*(\omega,\delta,\gamma,\theta) = 0, p^* = 1]$ denote the average welfare weight of agents not working and taking up. Similarly let $\hat{\alpha}(y_1, y_2)$ denote the average welfare weight of agents with earnings in $[y_1, y_2]$ and taking up the benefit. Finally, I introduce $\hat{\varepsilon}(y_1, y_2)$, $\hat{\tilde{\eta}}(y_1, y_2)$, $\hat{\xi}(y_1, y_2)$ and $\hat{\xi}_0(y_1, y_2)$ the average elasticities for individuals with earnings in $[y_1, y_2]$.



A.12.1 Introduction of negative participation tax A.12.2 Introduction of negative marginal tax rates

Figure A.12: Tax perturbation approach

Lecture: The black line represents the 45-degree line. The solid red line represents the pre-reform budget constraint and the dashed red line represents the after-reform budget constraint.

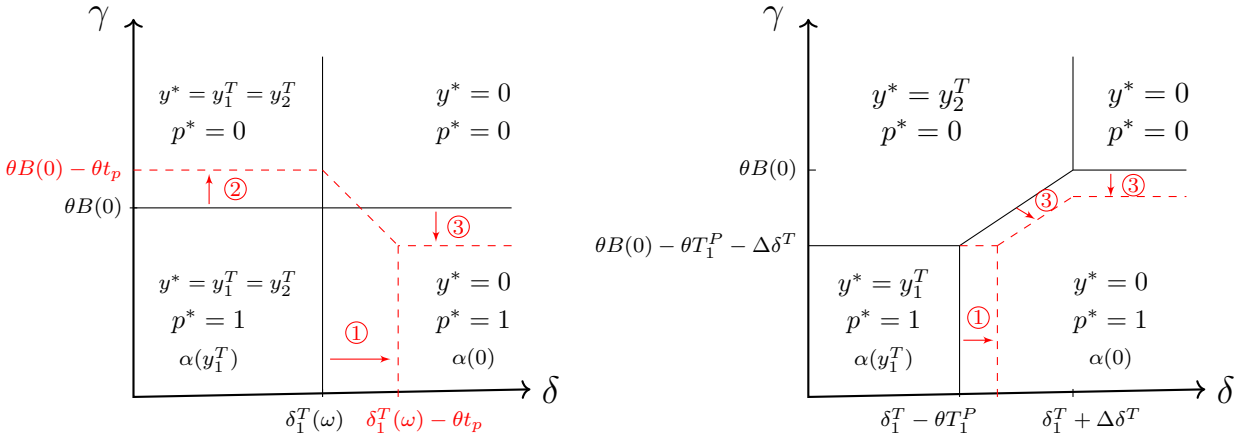
Introducing negative participation taxes. I start by deriving the necessary and sufficient condition for the introduction of small negative participation taxes at the bottom of the income distribution to be welfare improving. Lemma 2 expresses this condition for a system where the participation tax is initially set to zero at the bottom and for a small reform (τ and ℓ tend to zero).

Lemma 2. *Let the pre-reform benefit system B satisfy Assumptions (A4) and (A4bis), with an initial participation tax $T^P(y) = t_p = 0$ for all incomes below y_p. The introduction of a small negative participation tax for incomes below $y_a < y_p$ is welfare-increasing if and only if*

$$\int_{y_p}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy + F_y(0)B(0)\left[\xi_0 - \hat{\xi}(\underline{y}, y_a)\right] > F_y(0)\left[\hat{\alpha}(0) - \hat{\alpha}(\underline{y}, y_a)\right].$$

The reform is welfare-improving whenever the total fiscal externality created by the reform (on the left-hand side of the formula) is larger than the mechanical welfare loss created by the reform (on the right-hand side of the formula).

The mechanical welfare effect represents the social cost of redistributing resources from non-working agents (captured by the welfare weight $\hat{\alpha}(0)$) towards the low-income workers (captured by the average welfare weight $\hat{\alpha}(\underline{y}, y_a)$). Since the reform is calibrated to be budget neutral, the net mechanical social loss of this transfer is $\overline{F}_y(0)(\hat{\alpha}(0) - \hat{\alpha}(\underline{y}, y_a))$. Contrary to the framework with perfect take-up, the mechanical effect of the reform only affects individuals initially taking up the benefit. It is thus expressed in terms of welfare weights *conditional* on take-up and it is scaled by $F_y(0)$ which is the number of non-working agents *conditional* on take-up.[57] These conditional parameters might be different than the unconditional ones depending on the correlation between types. This raises the question of the targeting properties of imperfect take-up. Are those taking up the highest-skilled or the lowest-skilled? What matters for the welfare effect here is however the differential welfare weights between low-income workers and the unemployed. The relevant question is thus: is take-up correlated with skills, in a way that depends on employment status?



A.13.1 Individuals with $y_1^T(\omega) < y_p$

A.13.2 Individuals with $y_1^T(\omega) > y_p$

Figure A.13: Behavioral responses to the introduction of negative participation taxes

Lecture: The dark solid lines represent the situation under the initial tax-benefit system. The dashed red lines represent the situation under the new tax-benefit system after implementing the reform described in this section.

The fiscal externalities terms represent the extra tax revenue generated by the reform due to behavioral responses. They are represented in Figure A.13. Because the reform has decreased the participation tax rate at all income levels, a fraction of individuals initially not working will choose to work (see effect ① in Figure 13A.13.2). The key fiscal externality comes from agents with potential income above y_p who faced a positive participation tax in the initial tax-benefit system. The introduction of a negative participation tax at the bottom leads to a positive fiscal externality because it increases tax revenue (i.e. decreases public spending). The elasticity of labor supply *conditional* on take-up, $\tilde{\eta}(y)$, is the key sufficient statistics to measure this externality. There is no first-order fiscal externality generated by extensive labor supply from agents with income below y_p, since they faced an initial participation tax of zero. There is also no first-order fiscal externality due to intensive labor supply responses since the initial tax system features a zero marginal tax rate.

A new fiscal externality arises when accounting for endogenous take-up decisions. First, some agents with earnings in $[\underline{y}, y_a]$ will start taking up the benefit because $B(y)$ has increased by an amount $\phi_a \tau \ell$ in this range. This take-up behavioral response (illustrated by effect ② in Figure 13A.13.1) generates a negative fiscal externality because it increases public spending. Note that the model also predicts a joint labor supply and take-up responses, whereby some agents not working and not taking up will start working

[57] In the framework with perfect take-up, the mechanical effect is exactly the same as here but expressed in terms of the unconditional parameters. See Lemma 1 in Hansen (2021).

and taking up.[58] Second, because $B(0)$ has decreased by an amount $\tau\ell$, a fraction of non-working agents will stop taking up. This take-up behavioral response (illustrated by effect ③ in Figure 13A.13.1 and Figure 13A.13.2) generates a positive fiscal externality as it decreases public spending. The elasticities of the take-up rate $\xi(y)$ and $\xi_0(y)$, with respect to $B(y)$ and $B(0)$ respectively, are the key sufficient statistics to measure fiscal externality due to take-up responses. Importantly, these elasticities are zero if take-up is solely the outcome of low awareness ($\theta = 0$) and not the outcome of too large take-up costs (γ). The overall fiscal externality due to take-up is $F_y(0)B(0)\left[\xi_0 - \hat{\xi}(\underline{y}, y_a)\right]$. It is positive whenever the elasticity of the take-up rate is more sensitive to changes of the demogrant (ξ_0) than to the changes of the in-work benefit amount ($\hat{\xi}(\underline{y}, y_a)$).

Introducing negative marginal tax rates. After studying the introduction of a negative participation tax, I study the effect of introducing negative marginal tax rates.

Lemma 3. *Let the pre-reform benefit system B satisfy Assumptions (A4) and (A4bis), with an initial participation tax $T^P(y) = t_p$ for all incomes below y_p. The introduction of a negative marginal tax around y_a is welfare-increasing if and only if*

$$-t_p\left[\hat{\eta}(\underline{y}, y_a) - \hat{\bar{\eta}}(y_a, y_b)\right] + \bar{B}\left[\hat{\xi}(\underline{y}, y_a) - \hat{\xi}(y_a, y_b)\right] > \left[\hat{\alpha}(\underline{y}, y_a) - \hat{\alpha}(y_a, y_b)\right].$$

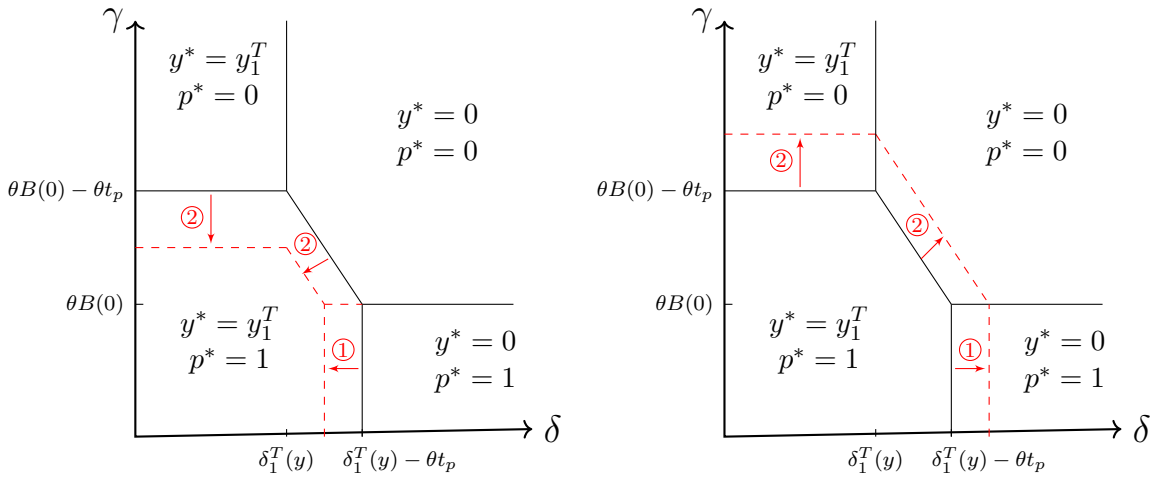
Again, the reform is welfare-improving whenever the total fiscal externality created by the reform (on the left-hand side of the formula) is larger than the mechanical welfare loss created by the reform (on the right-hand side of the formula).

The mechanical welfare effect represents the social cost of redistributing resources from working agents with lower earnings (captured by the average welfare weight $\hat{\alpha}(\underline{y}, y_a)$) towards working agents with higher earnings (captured by the average welfare weight $\hat{\alpha}(y_a, y_b)$). Since the reform is calibrated to be budget neutral, the net mechanical social loss of this transfer is $\hat{\alpha}(\underline{y}, y_a) - \hat{\alpha}(y_a, y_b)$. Recall that, as for Lemma 2, these are welfare weights put on workers conditional on taking up. Hence, accounting for imperfect take-up might affect the welfare effect of the reform, depending on the correlation between types.

The behavioral responses generating the fiscal externalities are represented in Figure A.14. The reform has increased the participation tax rate for households with income below y_a and decreased it for households with income in $[y_a, y_b]$. This generates two extensive labor supply responses of opposite signs. A fraction of households with income below y_a are induced to stop working (see effect ① in Figure 14A.14.1). This response creates a positive fiscal externality (i.e. a decrease in public spending) since the participation tax is initially negative (A5). Conversely, a fraction of households with income in $[y_a, y_b]$ are induced to start working (see effect ① in Figure 14A.14.2). This response creates a negative fiscal externality (i.e. an increase in public spending) since the participation tax is initially negative. The differential of average elasticities of labor supply *conditional* on take-up between the two groups $(\hat{\eta}(y_a, y_b) - \hat{\bar{\eta}}(\underline{y}, y_a))$ is the key sufficient statistic to measure this externality. Note that because the marginal tax rates are zero in the initial system, intensive labor supply responses do not generate a first-order change in public spending.

On top of the standard fiscal externality due to extensive labor supply responses, there is a new fiscal externality due to take-up responses. On the one hand, low-income households with earnings in $[\underline{y}, y_a]$ face a decrease in benefit that leads to a decrease in take-up (illustrated by effect ② in Figure 14A.14.1). This generates a positive fiscal externality because it decreases public spending by an amount $\bar{B}$. On the other hand, households with earnings in $[y_a, y_b]$ face an increase in benefit that leads to an increase in take-up (illustrated by effect ② in Figure 14A.14.2). This generates a negative fiscal externality because it increases public spending by an amount $\bar{B}$. The differential of average elasticities of take-up between the two groups $(\hat{\xi}(\underline{y}, y_a) - \hat{\xi}(y_a, y_b))$ is the key sufficient statistic to measure this externality.

[58] For simplicity, I have assumed that taxes are set to zero in this economy. Hence, the results do not depend on whether take-up responses come from initially working or not-working individuals since, by definition, agents who do not take up generate the same zero public spending regardless of their work decision.



A.14.1 Individuals with $y_1^T(\omega) < y_a$

A.14.2 Individuals with $y_1^T(\omega) \in (y_a, y_b)$

Figure A.14: Behavioral responses to the introduction of negative marginal tax rates

Lecture: The dark solid lines represent the situation under the initial tax-benefit system. The dashed red lines represent the situation under the new tax-benefit system after implementing the reform described in this section.

Conclusion Taking stock of the results, the introduction of an in-work benefit is welfare-improving under some conditions. First, it generates a transfer from low-income workers to workers with slightly higher incomes. This generates a welfare loss that increases with the steepness of the welfare weight slope with respect to income. If society values equally redistribution to all workers eligible for the in-work benefit, there is no mechanical welfare loss associated with introducing an in-work benefit. Second, the reform generates fiscal externalities due to extensive labor supply responses and take-up behavioral responses. If the semi-elasticities of participation to the labor force *conditional* on take-up are weakly decreasing with income or if the share of workers eligible to the in-work benefit is sufficiently small, the extensive labor supply responses generate a positive fiscal externality. Regarding take-up responses, the sign of the fiscal externality depends on the income-gradient of the semi-elasticities of take-up. If the take-up rate semi-elasticities are weakly decreasing with income, take-up responses generate a positive fiscal externality. In conclusion, accounting for imperfect take-up has two key implications. It means that the relevant parameters to account for are the welfare weights and the labor supply elasticities *conditional* on take-up. In a sense, empirical works implicitly tend to estimate elasticities locally and thus on this population. Regarding the welfare weights, the implication is important as it depends on whether non take-up is correlated with skills or not. The second implication is that there is a new fiscal externality due to endogenous take-up responses that can have first-order implications when thinking about an optimal schedule.

C.3.1 Proofs for this section

Proof of Lemma 2 Let us define a tax reform (τ, h_a) depicted in Figure 12A.12.1 that replaces $B(y)$ by $B(y) + \tau h(y)$ with:

$$h_a(y) = \begin{cases} -\ell \quad \text{for } y = 0 \\ \phi_a \ell \quad \text{for } y \in [\underline{y}, y_a - \ell] \\ \phi_a(y_a - y) \quad \text{for } y \in [y_a - \ell, y_a] \\ 0 \quad \text{for } y \in [y_a, \bar{y}] \end{cases}$$

Using Lemma 1, the marginal public spending effect of the reform (τ, h_a) around $\tau = 0$ is:

$$
\begin{aligned}
R_\tau(0,h_a) = \; & \bar{q}\left[-\ell F_y(0) + \phi_a \ell \int_{\underline{y}}^{y_a-\ell} f_y(y)dy + \phi_a \int_{y_a-\ell}^{y_a} f_y(y)(y_a-y)dy\right] \\
& - \bar{q}\left[\phi_a \int_{y_a-\ell}^{y_a} f_y(y)\varepsilon(y)y\frac{B'(y)}{1+B'(y)}dy\right] \\
& - \bar{q}\left[\ell\int_{\underline{y}}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy + \phi_a\ell\int_{\underline{y}}^{y_a-\ell} f_y(y)\tilde{\eta}(y)T^P(y)dy + \int_{y_a-\ell}^{y_a} f_y(y)\phi_a(y_a-y)\tilde{\eta}(y)T^P(y)dy\right] \\
& + \bar{q}\left[-F_y(0)\ell\xi_0(0)B(0) + \phi_a\ell\int_{\underline{y}}^{y_a-\ell} f_y(y)\xi(y)B(y)\;dy + \int_{y_a-\ell}^{y_a} f_y(y)\phi_a(y_a-y)\xi(y)B(y)\;dy\right]
\end{aligned}
$$

The second-order derivative of public spending R with respect to τ and ℓ, evaluated around $\tau = 0$ and $\ell = 0$ is:

$$
\begin{aligned}
R_{\tau\ell}(0,h_a)|_{\ell=0} = \; & \bar{q}\left[\phi_a(F(y_a)-F(\underline{y})) - F_y(0) - \phi_a f_y(y_a)\varepsilon(y_a)y_a\frac{B'(y_a)}{1+B'(y_a)}\right] \\
& - \bar{q}\left[\int_{\underline{y}}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)\;dy + \phi_a\int_{\underline{y}}^{y_a} f_y(y)\tilde{\eta}(y)T^P(y)\;dy\right] \\
& + \bar{q}\left[\phi_a\int_{\underline{y}}^{y_a} f_y(y)\xi(y)B(y)dy - F_y(0)\xi_0 B(0)\right]
\end{aligned}
$$

Because of Assumption (A4), the initial participation tax and marginal tax rate for all income below y_p are zero. It also implies that $B(y) = B(0)$ for all incomes y below y_p. Moreover, if we calibrate the reform to be budget neutral (i.e. $\phi_a = F_y(0)/[F_y(y_a) - F_y(\underline{y})]$), the sum of the mechanical effects is zero. Hence the second-order derivative simplifies to:

$$
\begin{aligned}
R_{\tau\ell}(0,h_a)|_{\ell=0} &= -\bar{q}\left[\int_{y_p}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy - \phi_a\int_{\underline{y}}^{y_a} f_y(y)\xi(y)B(y)dy + F_y(0)\xi_0 B(0)\right] \\
&= -\bar{q}\left[\int_{y_p}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy + F_y(0)B(0)\left(\xi_0 - \hat{\xi}(\underline{y},y_a)\right)\right]
\end{aligned}
$$

where $\hat{\xi}(\underline{y},y_a) = \int_{\underline{y}}^{y_a}\frac{\partial q(\omega_T(y))}{\partial B(\bar{y})}\frac{1}{q(\omega_T(y))}$ is the average take-up rate elasticity of individuals with earnings in $[\underline{y},y_a]$ and $\omega_T(y) = y_T^{-1}$.

Assuming that the increase in public spending is funded via a lump-sum tax on all agents in the economy[59], the marginal effect of the small reform on welfare is:

$$
W_\tau(0,h_a) = -R_\tau(0,h_a) + \bar{q}\left[F_y(0)\hat{\alpha}(0)h(0) + \int_{\underline{y}}^{\bar{y}} f_y(y)\alpha(\omega_T(y))h(y)dy\right]
$$

where $\hat{\alpha}(0) = E_{\Omega\times\Delta\times\Gamma\times\Theta}\left[\alpha(\omega)|y^*(\omega,\delta,\gamma,\theta) = 0, p^* = 1\right]$ is the average welfare weight of agents not working and taking up. Defining $\hat{\alpha}(y_1,y_2) = E_{\Omega\times\Delta\times\Gamma\times\Theta}\left[\alpha(\omega)|y^*(\omega,\delta,\gamma,\theta)\in[y_1,y_2], p^* = 1\right]$, we can express the second-order derivative of the welfare function as:

[59]The tax is levied on all agents, including those not taking up, as I have assumed perfect compliance to taxes.

$$W_{\tau\ell}(0,h_a)|_{\ell=0} = \bar{q}\left[F_y(0)(\hat{\alpha}(\underline{y},y_a)-\hat{\alpha}(0)) + \int_{y_p}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy + F_y(0)B(0)\left(\xi_0 - \hat{\xi}(\underline{y},y_a)\right)\right]$$

Overall, the welfare effect of a small reform (τ, h_a) around $\tau = 0$ and $\ell = 0$ is approximately given by $W(\tau,h_a) - W(0,h_a) \simeq \tau\ell W_{\tau\ell}(0,h_a)|_{\ell=0}$. Such a reform is welfare improving if and only if the condition stated in Lemma **??** is satisfied. □

Proof of Lemma 3 Let us define a tax reform (τ, h_b) depicted in Figure 12A.12.2 that replaces $B(y)$ by $B(y) + \tau h_b(y)$ with:

$$h_b(y) = \begin{cases} 0 & \text{for } y = 0 \\ -\ell & \text{for } y \in [\underline{y}, y_a - \ell] \\ y - y_a & \text{for } y \in [y_a - \ell, y_a] \\ \phi_b(y - y_a) & \text{for } y \in [y_a, y_a + \ell] \\ \phi_b \ell & \text{for } y \in [y_a + \ell, y_b] \\ \phi_b(y_b + \ell - y) & \text{for } y \in [y_b, y_b + \ell] \\ 0 & \text{for } y \in [y_a, \bar{y}] \end{cases}$$

In the same spirit as for the proof of Lemma 2, one can show using Lemma 1 that the marginal public spending effect of the reform (τ, h_b) around $\tau = 0$ and $\ell = 0$ is:

$$\begin{aligned} R_{\tau\ell}(0,h_b)|_{\ell=0} = {} & \bar{q}\left[-(F_y(y_a) - F_y(\underline{y})) + \phi_b(F_y(y_b) - F_y(y_a))\right] \\ & + \bar{q}\left[(1+\phi_b)f_y(y_a)\varepsilon(y_a)y_a\frac{B'(y_a)}{1+B'(y_a)} - \phi_b f_y(y_b)\varepsilon(y_b)y_b\frac{B'(y_b)}{1+B'(y_b)}\right] \\ & + \bar{q}\left[\int_{\underline{y}}^{y_a} f_y(y)\tilde{\eta}(y)T^P(y)\ dy - \phi_b\int_{y_a}^{y_b} f_y(y)\tilde{\eta}(y)T^P(y)\ dy\right] \\ & + \bar{q}\left[-\int_{\underline{y}}^{y_a} f_y(y)\xi(y)B(y)\ dy + \phi_b\int_{y_a}^{y_b} f_y(y)\xi(y)B(y)\ dy\right] \end{aligned}$$

Because of Assumption (A4), the initial participation tax for all income below y_p is constant (and equal to t_p) and the marginal tax rates for all income below y_p are zero. It also implies that $B(y) = \bar{B}$ for all incomes below y_p initially. Moreover, if we calibrate the reform to be budget neutral (i.e. $\phi_b = [F_y(y_a) - F_y(\underline{y})]/[F_y(y_b) - F_y(y_a)]$), the sum of the mechanical effects is zero. Hence the second-order derivative simplifies to:

$$R_{\tau\ell}(0,h_b)|_{\ell=0} = \bar{q}\left[F_y(y_a) - F_y(\underline{y})\right]\left[t_p\left[\hat{\tilde{\eta}}(\underline{y},y_a) - \hat{\tilde{\eta}}(y_a,y_b)\right] + \bar{B}\left[\hat{\xi}(y_a,y_b) - \hat{\xi}(\underline{y},y_a)\right]\right]$$

Assuming that the increase in public spending is funded via a lump-sum tax on all agents in the economy, the marginal effect of the small reform on welfare is:

$$W_\tau(0,h_b) = -R_\tau(0,h_b) + \bar{q}\left[F_y(0)\hat{\alpha}(0)h(0) + \int_{\underline{y}}^{\bar{y}} f_y(y)\alpha(\omega_T(y))h(y)dy\right]$$

Hence we can express the second-order derivative of the welfare function as:

$$W_{\tau\ell}(0,h_b)|_{\ell=0} = \bar{q}\left[F_y(y_a) - F_y(\underline{y})\right]\left[\left[\hat{\alpha}(y_a,y_b) - \hat{\alpha}(\underline{y},y_a)\right] - t_p\left[\hat{\tilde{\eta}}(\underline{y},y_a) - \hat{\tilde{\eta}}(y_a,y_b)\right] - \bar{B}\left[\hat{\xi}(y_a,y_b) - \hat{\xi}(\underline{y},y_a)\right]\right].$$

Overall, the welfare effect of the small reform (τ, h_b) around $\tau = 0$ and $\ell = 0$ is approximately given by $W(\tau,h_b) - W(0,h_b) \simeq \tau\ell W_{\tau\ell}(0,h_b)|_{\ell=0}$. Such a reform is welfare improving if and only if the condition

stated in Lemma **??** is satisfied. □