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

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Abstract

Imperfect take-up of public policies has been a growing policy concern leading many governments to seek interventions that could encourage take-up. This paper examines the effect of two types of intervention: an increase in benefit generosity and information provision. Using a national reform of France's in-work transfer scheme and exhaustive social security data, I provide the first at-scale causal evidence of the role of money and information in shaping take-up behaviors. Overall, I find that information provision acts as an effective tool to raise participation while benefit generosity does not. Using a difference-in-differences strategy, I estimate small and non-significant take-up responses to changes in monetary incentives, with an implied take-up elasticity of about 0.1. Instead, I show that the reform acted as an information shock through large media coverage leading to a 7% increase in self-declared awareness of the program. Comparing enrollment between this program and another unaffected welfare program, I show that information provision caused an 18% surge in take-up in the month following the reform, representing almost half a million new claimants. Moreover, this intervention succeeded in encouraging take-up even among populations traditionally perceived as harder to reach. Finally, I develop a theoretical framework to estimate the welfare effects of these interventions in the French set-up and draw broader policy implications. The model highlights that, in the case of in-work transfers, raising take-up might be socially desirable not only for *equity* reasons but also for *efficiency* reasons.

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1 Introduction

In-work transfers have become the main anti-poverty policy tool in modern welfare states. In the United States for example, the Earned Income Tax Credit (EITC) lifts a total of 9.1 million individuals out of poverty, including 4.7 million children (Austin Nichols and Rothstein 2015).[1] Similar prominent in-work benefit schemes have been implemented in many other countries in the past decades. By targeting resources to low-wage working individuals, they can alleviate poverty both *directly*, by improving the living conditions of the working poor population, and *indirectly*, by encouraging work for those out of the labor force (Hoynes and Patel 2018).[2] Additionally, these programs have been found to have long-run, and even inter-generational, positive effects on health, education and income (Chetty, Friedman, et al. 2011; Evans and Garthwaite 2014).

Despite their key role, take-up of in-work transfers is incomplete (Currie 2004). In the U.S., 20% of eligible people do not claim the EITC, causing a total of $7.3 billion benefits to remain unclaimed (TIGTA 2018). In other countries, non take-up rates of in-work benefits tend to be even higher (e.g. 27% in France, 35% in the U.K) (Ko and Moffitt 2022). Various policy interventions have been implemented to encourage take-up such as information outreach, personal assistance with the application process or administrative burden reduction.[3] However, designing successful interventions has proven challenging. First, empirical studies have yielded mixed results regarding the effects of these interventions on take-up. Second, the overall social welfare effect of such interventions remains theoretically ambiguous as they will also impact program fiscal cost and targeting efficiency and can affect individual behaviors such as labor supply.

This paper studies the effects and implications of two types of policy tools aiming to increase in-work benefits participation: (i) information provision and (ii) monetary incentives increase. To do so, I leverage a recent national reform of France's in-work benefit as a natural experimental set-up. Combining the large shock created by the reform with access to new exhaustive administrative data on families enrolled, I provide the first at-scale causal evidence of the effects of information provision and monetary incentives on take-up behaviors. While the causal role of monetary incentives has been relatively less explored, many recent studies have investigated the role of information provision. Using mostly field experiments to test

[1] Across all programs, including in-kind programs like SNAP or Medicaid, the EITC stands as the program with the largest effect on child poverty. Among cash transfer programs, it also represents the largest program with an expenditure of 57 billion dollars and 23 million recipients in 2022 (IRS 2022).

[2] While there was a relative consensus regarding the existence of positive extensive labor supply responses to the EITC, it is worth noting that a recent study by H. Kleven (2024) has challenged this view.

[3] For example, the U.S tax authority sends notices every year to more than half a million "likely eligible" tax filers reminding them to claim the EITC. In France, almost 3 million targeted individuals have benefited since 2014 from a one-hour meeting (*Rendez-vous des droits*) with a case-worker who helps them identify and apply for benefits (including in-work benefit) they might be eligible for.

information outreach effectiveness, some of these studies find positive results (Bhargava and Manoli 2015; Guyton et al. 2017; Manoli and Turner 2014; Matikka and Paukkeri 2022) while others do not (Bettinger et al. 2012; Linos et al. 2022; Cranor et al. 2019; Castell et al. 2022).[4] It has been speculated that this could be due to their focus on different targeted sub-populations (e.g. tax filers versus non tax-filers) or because the latter tended to be larger scale interventions than the former. By using a national reform and exhaustive data, this paper provides new evidence about the heterogeneous effects of such interventions across populations and contexts and whether they can be successfully scaled up or not.[5] Additionally, the paper develops a theoretical framework to assess the welfare implications of both the information provision and the increase in monetary incentives. This framework allows me to empirically estimate the welfare effects of the French reform and to draw broader policy insights.

France implemented a large and salient reform of its in-work benefit program in 2019. This program provides monthly cash transfers to low-wage workers and stands as the second-largest welfare program in the country, with an annual spending of 10 billion euros. Program take-up was estimated at 73% pre-reform (DREES 2017). Following the "yellow vests" protests in the fall of 2018, the government decided on an increase of the in-work benefit to support low-wage workers' living conditions. A minimum wage worker would see her monthly payment go up from 150 to 240 euros, representing an 8% surge in total disposable income. The reform provides an interesting setting to study policy interventions aiming at improving welfare participation. First, it provides quasi-random variation in monetary incentives to claim since exposure to the benefit increase was heterogeneous across families with different family compositions and past levels of labor income. Second, because the reform attracted large media coverage, I argue that it also acted as an indirect information shock. In the national printed press, the number of articles mentioning the program rose by 180%.[6] On the internet, Google searches of the program reached their all-time high in the month following the reform. Because awareness of the program was imperfect pre-reform, such media coverage could help alleviate part of the existing information friction and encourage take-up.

The first empirical analysis seeks to understand the role of monetary incentives in shaping take-up decisions. In a standard neoclassical model where welfare program participation is

[4]To the best of my knowledge, in the extensive literature studying information outreach interventions, there habe been only two studies using natural experiments. Manoli and Turner (2014) study the effects of the EITC notices sent to likely eligible taxpayers using a 2005 computer glitch that caused some of these taxpayers not to receive the notice. Cranor et al. (2019) study the effects of laws mandating employers to provide EITC information notices to their employees.

[5]As emphasized by List (2022), it is critical for policy-making to assess if light-touch policy interventions (e.g. nudges) that have been found to be effective in small-scale experiments can be successfully scaled up. Recent studies have suggested that this is often not the case (Camerer et al. 2018; Bird et al. 2021).

[6]Source: `Europresse.com`.

endogenous but costly, an increase in the benefit amount should encourage take-up (Moffitt 1983). Yet, few papers have investigated the causal role of monetary incentives because credible exogenous variation in benefit level is hard to find. The French welfare reform provides me with such variation. Using a de-trended difference-in-differences (DDD) design, I compare the evolution of claiming behaviors of families whose characteristics make them eligible for the reform-induced benefit increase (treated group) and families whose characteristics make them ineligible for it (control group).[7] The outcome of interest is the number of beneficiaries registered at a given period, in a given local welfare agency[8] and from a given group (treated or control). Exposure to the benefit increase is defined as the difference between the benefit amount one could get under the *actual* reformed system and under a *counterfactual* system without reform.[9] To measure this treatment exposure, I build a micro-simulation tool that simulates in-work transfer eligibility based on observed characteristics. It is important to note that because treatment assignment is not random, treated families tend to earn higher income and come from larger families. However, identification does not require the two groups to be similar in level but instead relies on a standard *parallel trend* assumption. I show that treated and control groups display similar pre-trends in enrollment.

Results find that take-up behaviors are not influenced by changes in monetary incentives. While treated families face a 20% surge in potential benefit, I fail to detect any differential change in claiming after the reform in this group relative to the control one. The estimated causal effect is statistically not significant and small in magnitude (a 2% surge in the take-up rate). This would imply a take-up elasticity with respect to the benefit size of about 0.1.[10] Several possible explanations could help understand this null effect. First, while the reform generated large and salient changes in monetary incentives to claim, individuals could still be inelastic if facing too high information friction. The estimated elasticity should be regarded as a lower bound of the frictionless elasticity. Second, if the claiming costs associated with the program are low and the benefit level already high, the local elasticity could be zero. Compared to standard welfare programs, France's in-work benefit can indeed be seen as a fairly low-burden program, with low stigma attached and a relatively easy and fast application process. This setting is still close to other in-work transfer settings.[11]

[7] The de-trended approach consists in comparing the relative evolution of enrollment across treated and control groups around January 2019 (when the reform was introduced) relative to the same calendar dates in the previous year, i.e. around January 2018 (a placebo event). It allows me to control for observed differential seasonality effects between the two groups.

[8] The French in-work benefit program is run administratively at the local county level (*département*). There are 94 such areas in mainland France with an average number of inhabitants of about 650,000.

[9] By design of the reform, the "pure" control group who sees no change in benefit at all is of a very limited size which makes estimation noisy. In my preferred specification, I compare families who are eligible for no or small benefit increase (less than 15 euros per month) and families who are eligible for a large benefit increase (more than 15 euros).

[10] In a recent study, Rosenqvist and Selin (2023) estimated a 0.25 take-up elasticity in the context of parental leave benefits in Sweden. But comparing elasticities across different types of programs is hard.

[11] For example, in the U.S, in the case of eligible households who already filed a tax return, claiming the EITC is fairly costless.

The second empirical analysis seeks to understand the role of information provision in shaping take-up decisions. The reform might have an effect on take-up not solely through its direct effect on benefit levels but also through its indirect effect that increased media coverage and public attention regarding the program. Identification of such effect is challenging as individual-level exposure to the information shock is unobserved in administrative data. To overcome this challenge, I exploit an additional data source, the General Population Survey, which measures self-reported knowledge of the welfare system in a representative population sample. Analysis of this survey provides three key insights. First, program awareness significantly rose from 75% to 80% in the overall population in the year following the reform.[12] Second, this alleviation of information friction is also found among low-income individuals, a subset of the population that is likely eligible for the program. Third, the increase in awareness is homogeneous across sub-populations, suggesting that exposure to the information shock among eligible individuals was rather uniform. Fourth, while awareness rates were evolving in a parallel way pre-reform across different types of programs, a significant increase in awareness post-reform is only observed for the in-work transfer. To identify the causal effect of the information shock, I thus implement a de-trended difference-in-differences that compares enrollment in the in-work transfer program (treated program) and in the French guaranteed minimum income scheme (control program).

By alleviating part of the existing information friction, the reform successfully increased program participation. Overall, I estimate an 18% take-up increase in response to the information provision. The effects of information provision are large and instantaneous. Using more granular data at the daily level, I find that the flow of entrants increased significantly the day after the reform announcement. Overall, I detect large entry responses in December 2018 after the announcement (+75% flow) and in January (+ 175% flow). These sizable effects on entry translate into a large and permanent increase in the stock of beneficiaries. Overall, by the end of the first month, almost 500,000 new eligible families enrolled in the program in response to the information shock. To ensure that the cross-programs estimation strategy is not capturing the effect of the benefit increase, I run the estimation on the subsample of families who are not affected by this feature of the reform (previous control families) and still find the same large take-up responses. Additionally, survey evidence shows that exposure to the information shock and to the benefit increase are not correlated. Finally, I address the possible role of a decrease in stigma in driving part of the observed responses. While stigma has long been speculated to be a possible driver of non take-up (Moffitt 1983; Celhay et al. 2022), qualitative studies suggest that this is much less prevalent in the context of in-work transfers (Kula et al. 2020). I also show that take-up responses are not significantly higher in places with a lower initial number of beneficiaries (i.e. places that

[12]The share of individuals who declared understanding "a bit" or "well" who is eligible for the program also increased at this time.

might be prone to social stigma). Turning to the heterogeneity analysis, I show that take-up responses are four times larger among first-time entrants than among past beneficiaries who re-enter the program.[13] This suggests that the reform did not act as a reminder that alleviated behavioral friction (e.g. inattention, forgetfulness, procrastination) but rather as an information provision that alleviated information friction (low program awareness and low understanding of eligibility criteria).

In the third part of the empirical analysis, I investigate the targeting property of the information shock. Understanding who responds to take-up encouragement interventions is key for welfare analysis. Indeed, previous work has shown that imperfect take-up might be optimal depending on who self-selects out and why(Albert Nichols and Zeckhauser 1982; H. Kleven and Kopczuk 2011).[14] To address this targeting effect, I estimate how the reform affected the average characteristics of the marginal entrant, following the same strategy as earlier. Marginal entrants are disproportionately single-males with no children, individuals with lower average income, and individuals with no past contact with the welfare administration. These effects are at odds with previous evidence that suggested that information provision was more effective (or even, only effective) among populations that have been in closer contact with the administration (Linos et al. 2022). In terms of magnitude, the take-up response estimated is also much larger than the effects found in previous studies.[15]. A possible explanation is that this paper evaluates quite a different type of information intervention. This paper investigates the role of general and untargeted information provision, similar to a national information campaign broadcasted in various media. Previous studies focus on tailored and targeted information provision, typically information letters about one's own eligibility sent to likely eligible individuals. The latter could be less effective if (i) government have a hard time identifying who is possibly eligible or not among non-claimants and (ii) if untargeted information provision benefits from a social multiplier effect whereby there is information diffusion within the population, and possibly transmitting through non-eligible people who got informed.[16]

[13]Movements in and out of the program are quite common. More than two-thirds of program entries in a given month, pre-reform, are actually re-entry into the program by past beneficiaries.

[14]These papers show that, in policy set-up with asymmetric information regarding individual types, it may be optimal for the government to use screening instruments such as ordeals (i.e. purely unproductive costs imposed on individuals that participate to a program) or monitoring technologies to deter non-targeted individuals from applying. As a result, some individuals self-select out of the program but this is efficient if the barriers to participation are designed in a way that these individuals are also those that the government did not want to enroll.

[15]As underlined before, a significant number of papers found null effects. The most successful information intervention in the context of in-work transfer schedules is by far Bhargava and Manoli (2015). Estimating the at-scale effects of a reminder notice that would target all tax-filers not claiming the EITC, with simplified content and benefit display, they estimate an 11% increase in take-up (from 75 to 83%).

[16]Using newly-accessed data that allows me to observe two types of networks (neighbors and co-workers) and link it to the social security data, I plan to investigate this second channel to expand this paper's analysis.

To understand the welfare and policy implications of take-up interventions, I develop a theoretical framework for optimal policy design in the presence of endogenous program participation. The model is rooted in the standard public finance framework with heterogeneous agents, endogenous labor supply decisions, and a social planner setting a non-linear transfer schedule (Mirrlees 1971; Saez 2002; Jacquet et al. 2013; Hansen 2021). I introduce a new endogenous take-up decision and two possible mechanisms; a fixed utility cost of claiming the benefit[17] and a misperception parameter that can capture the lack of program awareness. These parameters are distributed across the population, resulting in heterogeneous take-up. 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. Using tax perturbation approaches, I derive new welfare formulas that capture the overall effects of (i) a benefit increase and (ii) an information shock (modeled as a shock to the distribution of the misperception parameter). These formulas are expressed in terms of key sufficient statistics that can be estimated using the empirical setting of the French welfare reform. Calibrating the model using the empirical results, I find that both the information provision and the benefit increase introduced by the 2019 French reform were welfare-improving, under mild redistributive preferences. Assuming that the social value of consumption for eligible is 20% larger than that of non-eligible families, the reform generated a 10 million euros welfare gain in the first month (about 3% of monthly spending).

Importantly, the framework highlights the role of two rationales for encouraging take-up. First, depending on who starts claiming and why they did not claim in the first place, take-up interventions can improve program targeting efficiency. Second, in the context of in-work transfers, take-up interventions, like information provision or administrative burden reduction, can reduce the perceived participation tax and thus encourage some individuals out of the labor force to start working and taking up. Assuming that the overall participation tax is positive, these responses generate 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. So far, the empirical framework has only helped identify pure take-up responses to the information intervention (which generates negative fiscal externalities). To shed light empirically on the importance of this efficiency motive, I use new data that links social security and matched employer-employee data to estimate joint labor supply and take-up decisions. The estimation strategy relies on an instrumental variable approach. Using an instrument for eligibility to the in-work transfer, I estimate how employment evolves around the reform between groups that are more likely to be eligible for the program (and thus more likely to be affected by the intervention) and groups that are less likely to be eligible.

[17]The participation cost can reflect costs associated with the application process (e.g time and money invested in the process, disutility for doing paperwork) but also disutility from being enrolled (e.g. stigma).

This paper contributes to the large 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 interventions 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; Deshpande and Li 2019; Homonoff and Somerville 2021). While experimental designs offer clean identification, these studies are typically conducted on smaller samples that do not allow for detailed heterogeneity analysis and on samples that are often 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.[18] I also contribute to the specific strand of the literature that focuses on in-work transfer programs, for which typical factors like stigma or high costs due to monitoring are less likely to drive take-up. Focusing also on the French context, Castell et al. (2022) investigates how providing personal assistance to potential claimants via an individual meeting with a case-worker and personalized information via an online simulator can affect claiming behaviors for various welfare programs, including the French in-work benefit. They find strong effects of the assistance intervention but not effects of the information intervention. But 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; Cranor et al. 2019). 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). Instead of studying information letters targeted at identified families likely eligible, this paper investigates the effects of a very indirect information shock, that happened through reform media coverage. The general and untargeted information provision was successful at improving program awareness and in results take-up. This implies that *how* information is provided (i.e. by whom, to whom, and through which channels) matters for take-up responses and for reaching harder-to-reach populations.

Another contribution of this paper is to attempt to bridge the gap between the optimal transfer program literature and the normative literature on take-up. The first studies optimal transfer program design in a set-up where the social planner needs to account for endogenous earnings responses to the transfer schedule (Mirrlees 1971; Saez 2002; Hansen 2021). However, at odds with real-life setups, this literature assumes perfect take-up of transfers. The second literature investigates reasons why imperfect take-up might be part

[18] Very few papers have studied the effects of national reform on aggregate take-up rates. To the best of my knowledge, this is the first paper to use a national reform to causally study the determinants of take-up. Fuchs et al. (2020) study descriptively a large reform in Austria that led to a decrease in non take-up from 53% to 30%.

of the second-best optimum. This can arise when governments can use ordeals as efficient self-targeting mechanisms (Besley and Coate 1992; Albert Nichols and Zeckhauser 1982) or when using monitoring technologies that generate complexity for claimants (H. Kleven and Kopczuk 2011). Yet, these papers fail to account for the role of endogenous earnings in shaping optimal policies, even though take-up and labor supply are joint decisions when transfers are means-tested. The model I develop thus helps to intersect both frameworks and allows me to derive new insights. In doing so, I relate to the paper by Finkelstein and Notowidigdo (2019) who also derive welfare formulas for the effects of interventions aiming to increase take-up. Contrary to their model which has a general fiscal externality term, I choose to model more explicitly the fiscal externalities - labor supply and take-up behavioral responses - which allows me to highlight the overlooked efficiency rationale for why raising take-up might be optimal. Moreover, the tax perturbation approach I use provides me with welfare formulas expressed in terms of sufficient statistics that can be estimated empirically.

2 Institutional Background & Data

This section describes the institutional background of France's in-work benefit and the welfare reform implemented in 2019. It then describes the main datasets used in the empirical analysis.

2.1 Institutional Background

The French in-work benefit program ("*prime d'activité*" or PA hereafter) is a transfer program created in 2016 to provide financial support to low-income workers and foster work incentives.[19] It stands as the second largest means-tested cash transfer in France, with about 10 billion euros of annual spending and reaching about 15% of the population. In 2022, the average monthly transfer is around 182 euros. The program is similar in terms of size and schedule to in-work transfers implemented in many OECD countries, such as the EITC in the U.S or the WTC in the U.K.

Eligibility Eligibility status and benefit amount served depend on labor income, total resources, and family composition. Importantly, the benefit amount a family is eligible for depends on the labor income of the past three months. The program features a phase-in region and a phase-out region (see Figure A.1). However, contrary to many in-work transfer programs, it is not a tax credit and is instead run by the national welfare agency (*Caisse Nationale des Allocations Familialles* or CNAF).[20] It is a family benefit, as means-testing is assessed by summing all family member resources, but it also features some "individual

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

[20] Nonetheless, I sometimes refer to the program as the "French EITC" for simplification and to highlight the similitude with the U.S program.

bonuses" that solely depends on individual labor income. More details about the benefit schedule and formula can be found in Appendix A.

Program's take-up Take-up of the program is incomplete and was estimated to be 73% in 2016 (DREES 2017). To understand what barriers might explain imperfect take-up, I provide additional details about the program. First, the in-work benefit can be seen as a relatively "low-burden" program. The claiming process is an online procedure that takes around twenty minutes.[21] 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 does not require the claimant to provide any documentation.[22] Initial claiming is fairly easy but beneficiaries must re-certify every three months to remain enrolled. Frequent re-certification allows the government to better adjust benefit levels to the current situation of beneficiaries, it also imposes a burden on claimants. Second, while social stigma can prevent take-up, it is generally considered to be relatively limited in this context since in-work transfers are often less associated with feelings of being "assisted" by society than standard safety net programs. Moreover, the online claiming process ensures anonymity. Third, information or behavioral friction could affect take-up. In 2018, 25% of the adult population declared having never heard about the program.[23] Such low awareness rates are found even among low-income individuals who are more likely to be eligible for the program. There is also evidence that among those aware, there is a low understanding of the program (e.g. eligibility criteria, benefit formula..).[24]

The 2019 welfare reform The reform proposed an increase in benefit level for a subset of beneficiaries with earnings around the full-time minimum wage. Figure 1 shows how the reform changed the benefit schedule for a single person without kids and with only wage income. The benefit increased but in a heterogeneous way. Beneficiaries with earnings lower than half a full-time minimum wage (about 600 euros per month) did not benefit from the increase. Beneficiaries with earnings between 0.5 and 1 minimum wage (between 600 and 1,200 euros per month) benefited from an increase between 0 and 90 euros. Beneficiaries with earnings above minimum wage (above 1,200 euros per month) benefited from the full 90 euros increase. In practice, exposure to the benefit increase is heterogeneous and depends on family composition and the split of labor income between spouses as well. The reform also expands eligibility by pushing up the income eligibility threshold. Mechanically, some families previously ineligible became eligible to the program following the reform. Finally, the

[21] 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.

[22] The social administration does verify *ex-post* part of the declarations by using information shared by the tax administration as well as by performing occasional audits.

[23] Source : General Population Survey (*Baromètre d'opinion DREES, 2018*) and author's own computation.

[24] See more details in Appendix B.

reform changed the work incentives faced by beneficiaries since it reduces the participation tax at most income level[25] and changes the marginal tax rate in some earnings range.

Additionally, the reform might have contributed to improving the salience and awareness of the program. Since it only existed since 2016, some eligible households might have 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.[26] Using survey data, Figure A.3 shows that more than one individual out of five had never heard about the program before the reform.[27] 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.5 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 B, 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 in-work benefit 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.

2.2 Data

The main data source used in this project is the panel of Social Security Records (ALLSTAT). These restricted-access data are produced by the French social administration (CNAF) and used to administrate the main cash transfer programs in France, namely family, housing and social benefits (including the PA program). The ALLSTAT files are monthly data at the family level. A unique identifier allows to follow families across periods and to construct a monthly panel for the period January 2017 to December 2020.

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

[26] 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.

[27] Low awareness rates can still be found even after conditioning on various proxies for eligibility (see Figure A.9).

Population and Sample of interest The data are exhaustive and thus feature all French families benefiting from at least one welfare program administered by the CNAF institution[28] which represents a sample of about 15 million families (i.e. almost half of the French families). In the analysis, I will focus on the unbalanced sample of families who benefited from the PA program at least once between January 1st 2017 and January 1st 2020. By definition, the data only sample families that are eligible to some transfers and made a claim. It has two important implications. First, my sample does not contain eligible families who never claimed the PA benefit. Second, even for those who did take up at least once, there is uncertainty about their situation whenever they disapear from the sample; it could mean that they are not eligible at that period or it could mean that they are not taking up at that period. Section 3 explains how I deal with these empirical challenges of endogenous sample selection to study take-up behaviors. Importantly, I remove from my sample families who entered the program after the 2019 welfare reform due to the eligibility expansion.

Variables The files contain both information declared by families on a regular basis to the administration (e.g. family resources, family composition and some socio-demographic variables) and outcome variables computed by the administration such as the benefits amounts granted. I will use in particular all the variables needed to assess the eligibility to the PA benefit as well as the amount of benefits received monthly by each family. I define the dummy variable of being a PA beneficiary based on receiving a strictly positive amount of benefit in a given month. Families not present in the social records in a given month are considered as not enrolled in the PA program for this month. I will also use the precise date of claim in the empirical analysis. While the dataset contains monthly information, I have access to the date (JJ/MM/YYYY) where a claim for the PA program was made. I make several imputations to this variable. First, within a spell in the program, if several date of claims are recorded, I keep the earliest one (these cases include recording error, families who start claiming the safety net while enrolled in the PA program). Second, the precise date of claim is not always updated for families re-entering the program (especially when re-entering shortly after their last spell). For these families, I observe the month and year of entry but not the day in the month. I impute it using the observed distribution of entry in the program in this given month. This procedure relies on the assumption that the dates of entry and of re-entry display a similar timing pattern within a given month.

Descriptive Statistics Every month, around 15 million of families (i.e. half of all French families) receive some transfers from the social administration. Table 1 describes the characteristics of these families, as well as the characteristics of the subsample of those enrolled in the in-work benefit program, for the month of October 2018. Among the families benefiting

[28]The CNAF administrates the benefits of nearly all French households except in a few cases which are mainly households working in the agricultural sector and thus belonging to a separate social security institution. I do not have access to information on these households' benefits.

from the program, 50% are single individuals with no kids, 25% are single parents with kids (with the vast majority being women) and 25% are couples.

2.3 The General Opinion Survey

To complement the information provided by the administrative social records, I also use a representative annual survey conducted by an agency linked to the French Ministry of Social Affairs.[29] This data source provides information about factors that are typically unobserved in administrative data such as the awareness and knowledge individuals have about welfare programs.

Population and Sample of interest The survey samples every year around 3,000 to 4,000 individuals representative of the adult population in France. All the results provided will adjust for the weights provided by the survey. I restrict the sample of interest to individuals aged 18 to 65 years old as older individuals are not eligible to the in-work transfer and tend to have very different average levels of knowledge and awareness. The survey is a repeated cross-section as individuals are only surveyed once.

Variables The survey contains about several hundreds of questions about people's perceptions and opinions about various topics such as inequalities, poverty risks, the housing situation, the pension system, the health system, other public policies and some current social debates. It also asks the interviewee about her own situation providing rich socio-demographic information. I will mostly use two sets of questions, relating to knowledge and opinions about welfare programs, as well as the socio-demographic information. First, regarding information, I will use a set of questions asked about the interviewee's knowledge of different policies (family benefits, housing benefits, welfare programs including the PA program since 2016, subsidized health insurance programs etc.). The first question asks "*Have you heard of [program YYYY]?*". If the answer is yes, the second question asks "*Do you know who can benefit from it?*" and allows the answers; "*Yes, quite precisely*", "*Yes, but approximately*" or "*No*". I will use these questions to construct a proxy of information with three levels of knowledge. One limitation of this data source is that these questions are only asked once every two years (2014, 2016, 2018 and 2020) because the survey rotates some questions every year. However, since the 2019 reform was only announced during December 2018 and the survey interviews people in October and November of a given year, I will consider that the information about the PA program reported in the 2018 wave of the survey could not be affected by the potential information shock linked to the 2019 reform.

[29] The survey is called *Baromètre d'Opinion* and is conducted by the *Direction de la Recherche, des Études, de l'Évaluation et des Statistiques* (DREES).

3 Empirical Framework

This section presents the empirical strategy used to estimate the causal effect of two treatments, the monetary incentives change and the information provision, on take-up behaviors of the reform.

3.1 Identification using administrative data

A key challenge when using Social Security administrative data to study take-up behaviors is that such data only sample families whenever they do take-up, by definition. Besides, when families are not observed in the data, it is impossible to distinguish if it is because they are not eligible for the program or because they are eligible but not taking up. Therefore, it is not possible to measure take-up behaviors or estimate take-up rates with such data. However, because Social Security administrative data allows to observe enrollment, I outline a simple empirical framework to show how such data can be used to identify the causal treatment effect of interventions on take-up, under some identification assumptions.

Conceptual Framework Consider a social benefits program that can be represented by a mapping $g(.)$ linking individual-level characteristics represented by a multidimensional vector X (e.g. family composition, family resources, work history, place of residence...) to a continuous benefit entitlement e. Individuals need to claim the benefit in order to receive it. At the individual level[30], take-up of the program is a discrete choice variable p. Take-up can be inferred from (i) the amount of benefit an individual is eligible for (denoted $e \equiv g(X)$) and (ii) the amount of benefit actually received by the individual (denoted by b). An agent *takes up* the benefits program if she is eligible for a strictly positive amount of transfer e and receives a strictly positive amount of benefit b.[31] An agent *does not take up* the benefits program if she is eligible for a strictly positive amount of transfer $\tilde{e}$ but receives no benefit. When an agent is not eligible for the program, the take-up decision is not defined. At the aggregate level, considering a population of size N, I denote by B the aggregate number of beneficiaries and by $\tilde{E}$ the aggregate number of eligible in the population. The take-up rate in the overall population is $\bar{p} = \mathbb{E}[\mathbb{1}_{b>0}|\mathbb{1}_{\tilde{e}>0}]$, it is the share of eligible individuals who do take up the benefit.

Identification In Social Security data, one can observe individual-level enrollment b and compute aggregate enrollment B Proposition 1 states that, for marginal changes, the variation in the log take-up rate is simply the difference between the variation in the log number of beneficiaries enrolled and the variation in the log number of eligible, under the assumption

[30] As many welfare programs, the in-work benefit program is a family-level program. For simplicity, in this framework, I will not distinguish between the "individual" and the "family" or "household" levels.

[31] I will assume that in that case, there are no mistakes from the social administration and no possibilities for the agent to fraud, hence $e = b$.

that all beneficiaries are eligible (i.e. there is no fraud)[32]. This implies that the causal effect of a treatment on take-up $\bar{p}$ can be identified by an empirical strategy that uses enrollment as an outcome provided that this strategy controls for any variation due to variation in the number of eligible.

Proposition 1. *If* $p(b > 0|\tilde{e} = 0) = 0$, *then* $d\log(B) = d\log(\bar{p}) + d\log(E)$

The treatment effect on enrollment behaviors can identify the treatment effect on take-up behaviors under the assumption that eligibility is exogenous to the treatment. I now discuss the likelihood that this assumption holds in my empirical set-up. First, the fact that the reform expanded the eligibility range to higher-income families (see Figure 1) is an obvious violation of this assumption. Enrollment is likely to increase because of the entry of these new eligibles into the program, even at a constant take-up rate. In the remainder of this paper, I will thus exclude from the analysis any families entering the program because of the eligibility expansion. I simulate for each family their eligibility using the current reformed eligibility rules and under a counterfactual system with unchanged eligibility rules. Any family that is eligible under the former but not under the latter is excluded from the analysis sample. Second, the identification assumption would also be violated if families strategically attempt to become eligible for the program in response to the reform (e.g. positive extensive-margin labor supply responses). I argue that in the very short-run, eligibility remains exogenous because it is based on past outcomes (e.g. past three months earnings).[33] Since the reform was unanticipated, any enrollment response observed before February 2019 cannot reflect an eligibility response but must reflect a take-up response.

Unit of observation & Outcome of interest Throughout the analysis, I will use the number of families enrolled in a given month and in a given local welfare agency[34] as the outcome of interest. Because the administrative data only sample families when enrolled, I only have access to an unbalanced panel of families. To identify the causal effects of the reform, I will be able to identify family-level exposure to the benefit increase. Yet, the analysis will be run at the more aggregated welfare agency level in order to have a balanced panel. Observations will be weighted by the total population in the local area.

[32]Estimates of social fraud are even scarcer than estimates of non take-up in the literature but tend to be an order of magnitude below the later. In France, the social administration estimated the total amount of fraud to social benefits in 2018, detected or undetected, to represent about 2.3 billion euros, i.e. 3.2% of total spending (Cour des Comptes 2020). Using a back-of-the-envelope calculation, given that total annual PA spending was roughly 5 billion euros in 2018 and for a take-up rate of 73%, one can estimate the unclaimed benefit to the sole PA program (which is only one of many social benefits) to be about 1.8 billion euros due to non take-up.

[33]Labor market frictions might be an additional reason why one would not expect earnings and thus eligibility to adjust instantaneously. On the contrary, a take-up response can be very immediate since it only consists in making an online claim that takes less than an hour.

[34]There is one local welfare agency by local regions (*départements*), i.e. a total of 94 ones in my sample.

3.2 Identification of the effects of monetary incentives

To identify the causal effect of a change in monetary incentives to claim on claiming behaviors, the analysis leverages the differential exposure to the 2019 benefit increase across families.

Definition of treatment exposure 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. Figure 1 represents the change in the benefit schedule generated by the reform, in the case of a single individual with no kids. I define exposure 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.[35]

Estimation strategy A standard difference-in-differences strategy would rely on the standard *parallel trends* assumption. Treated families are expected to trend parallel to control families absent the reform. However, descriptively, treated families seem to display different seasonal effects than control. This is plausible since treated families are on average richer than control families and can therefore face differential seasonal job market trends for example. In particular, enrollment among control families decreases by a larger amount every January (as well as every July) relative to treated families. Since this differential trend seems constant over time, I can control for it.

Using a de-trended difference-in-differences strategy, the analysis compares the evolution of excess claims (compared to the previous year's same calendar date) among the treated group versus the control group around the time of the reform.[36] Therefore, it relies on a *relative parallel trend* assumption: the excess enrollment should trend the same across the two groups.

$$\log(Y_{jgkt}) = \sum_{\substack{d=-5 \\ d\neq -1}}^{d=6} \beta_d \times \mathbb{1}_{\{m=d\}} \times \mathbb{1}_{\{k=1\}} \times T_g + \lambda_{gk} + \lambda_{gt} + \lambda_{kt} + \mu_j + \varepsilon_{jgkt} \tag{1}$$

[35] In the preferred specification, treated families are those facing a benefit increase of more than 15 euros. This is because, by the design of the reform, there are few "pure" control families and thus expanding the control group allows to gain in precision during the estimation. Note that 15 euros is the threshold below which the benefit is not paid to eligible families.

[36] The de-trended difference-in-difference is similar in spirit to a triple difference strategy. The first exploits variation in one group-dimension and two time-dimension. The second exploit variation in two group-dimensions and one time-dimensions.

Equation 1 describes the estimated regression. The outcome Y can be the number of claims (effect on the flow) or the number of beneficiaries (effect on the stock). Y_{jgkt} is the outcome in local area j, treatment group g, event time t and event k. There are only two groups g (the treated and the control group). T_g is equal to 1 if the observation belongs to 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. excess enrollment would have followed similar trends absent the reforms among the two groups. The results presented show the event-studies coefficients β_d for all periods, including pre-reform periods. 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 the PA program, I can rule out an eligibility behavioral response. Moreover, the difference-in-differences 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, I find no evidence of a correlation between income (or family composition) and declared changes in awareness of the program after the reform (see Table A.3).

3.3 Identification of the effects of information provision

The 2019 welfare reform could also have generated a positive take-up response through an increase in information about the program.

Definition of treatment exposure Identifying who is affected by this "information shock" is less straightforward than for the monetary incentives mechanism. Using the General Population Survey, I find that general self-reported awareness and knowledge of the in-work transfer increased after the reform. This suggests that the reform successfully alleviated some pre-existing information friction, though the relationship is not causal. Additionally, survey evidence indicates that the increase in awareness and knowledge is quite uniform in the population. There is no evidence that the information shock affected very differentially specific subset of the population of eligible families. In the empirical analysis, I will thus assume that all eligible families were exposed to the information provision.[37]

Estimation strategy Survey evidence indicates that the information shock is specific to the in-work transfer. Figure 2 plots the evolution of self-reported awareness rate for several

[37]This does not imply that the information provision is successful in improving intermediary outcomes such as awareness and knowledge of the program for all but rather that they all received some treatment exposure.

programs. While awareness rates evolved in a parallel way before the reform, there is a clear and significant increase in self-declared awareness of the in-work transfer relative to the other programs. Therefore, I estimate a similar de-trended difference-in-differences as in Equation 1 which, this time, compares enrollment behaviors in the in-work transfer program (treated program) and in the safety net program (control program). Indeed, this program is assumed to be unaffected by the information provision intervention as we fail to detect any change in awareness, contrary to the in-work transfer program. I run this analysis separately on the subsample of families affected by the benefit increase and those not affected. The estimated coefficients on the subsample of control families capture the causal effect of the information provision.

The empirical strategy relies on several identification assumptions. First, an excess parallel trend assumption in terms of enrollment in the in-work transfer and the safety net. Empirical results will estimate pre-trends to provide supporting evidence regarding this assumption. Second, for the enrollment effect to identify a take-up effect, there should be no concomitant shock that could lead to a differential change in the number of eligible between the two programs. Third, because the reform acts in two ways, via information and via money, I need to make some further parametric assumptions. Exposure to the two shocks should be orthogonal. Table A.3 estimates the change in awareness rate around the reform across several subgroups using the survey data. It shows that exposure to the information shock is uncorrelated to characteristics that drive exposure to the benefit increase (e.g. income and family composition). Moreover, the information treatment effect and the money treatment effect should be additive and the information treatment effect should be homogeneous across families differentially affected by the benefit increase. This last point is in line with survey evidence as mentioned before.

3.4 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 strategy as described above as described in Equation 1 on outcomes that represent the average characteristics of new claimants (e.g. family situation, income, nationality, past employment and welfare trajectories...).

4 Empirical Results

This section provides the results of the empirical analysis of the effects of the 2019 welfare reform on take-up behaviors.

4.1 Descriptive Evidence

Aggregate enrollment in the in-work transfer program typically remains quite stable from month to month, despite significant movements in and out of the program.[38] In contrast with this usually stable pattern, Figure 3(a) shows that total enrollment rose sharply in the first month after the reform from about 2.7 million to 3.6 million families enrolled. Enrollment remained permanently higher afterward. To see this from another perspective, Figure 3(b) represents the evolution of the number of new claims (i.e. enrollment flow). New applications for the in-work transfer program were about three times larger than usual in January 2019. This could be due to the eligibility expansion implemented by the reform.[39] In both figures, I thus represent the same enrollment evolution focusing on the subsample of families enrolled who did not benefit from the eligibility expansion. There is still a clear large enrollment increase at the time of the reform. Out of the 700,000 new families enrolled in January 2019, only one-third can be attributed to the mechanical effect of the eligibility expansion. Throughout the rest of the analysis, I will focus on the subsample of families not affected by the eligibility expansion.

Several other explanations could account for the remaining large and instantaneous surge in enrollment after the reform. First, there could have been a concomitant economic shock that caused many to become eligible and in turn claim the program. 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, the 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. Finally, the enrollment surge could indicate a sizeable increase in take-up behaviors from eligible families. The rest of this section investigates if this is the case and if so, what mechanisms can explain these take-up responses.

4.2 Take-up responses to the change in 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 families not exposed. Figure 4(a) represents the raw average evolution of the outcome of interest, the number of families enrolled in a given month. Enrollment stock of treated and control families evolved in a parallel way before the reform. Interestingly, the figure also shows that both groups

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

[39] The reform increased the income threshold below which families can be eligible for the program. As shown by Figure 1, in the case of single individuals without children, those with monthly earnings between 1,400 and 1,600 euros became eligible after the reform.

experienced an increase in enrollment following the reform. Turning to the estimation, Figure 4(b) represents the coefficients of the de-trended difference-in-differences regression.

The causal effect of the benefit increase on enrollment behaviors is not statistically significant. Pre-trends are slightly significant and indicate an overall upward trend in the excess enrollment of the treated relative to the control. In that sense, the estimated effects are likely to be an upper bound, if anything, of the true treatment effect. Finally, it is important to underline that the enrollment effect is also small in economic magnitude, with an estimated increase by 1.8% in January 2019. Scaling up this effect, we observe that, even if significant, it would have only explained less than 10% of the aggregate enrollment surge observed. To derive an elasticity of the take-up rate, I estimate the treatment effect on the log transfer amount (Figure A.11). On average, treated families saw a potential benefit increase by about 20%. 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 by about 0.1% of the take-up rate (even if this estimate is not statistically significant).

Assuming that the cost of claiming remained stable around the reform, this indicates that an increase in the monetary incentives to take-up had no effects on take-up. In other words, for the marginal entrant, non take-up was not a rational cost-benefit choice.

4.2.1 Take-up responses to the information shock

Figure 4(a) suggested that there was a discontinuous increase in enrollment even among those not exposed to a significant change in the monetary incentives to take up. Moreover, intermediary outcomes like program awareness seem to have reacted around the time of the reform. For this reason, I investigate another possible mechanism, an information shock produced by the reform and its large media coverage.

To identify this effect, I use a diff-in-diffs strategy comparing families enrolling into the in-work transfer program (treated program) and families enrolling into another program unaffected by the information shock (control program). Figure 5(a) represents the raw average evolution of enrollment in these two programs. Figure 5(b) represents the coefficients of the de-trended difference-in-differences regression. While enrollment in both programs was trending in parallel ways befrore the reform, enrollment into the in-work transfer program surged by 18% following the reform. We also note that there is a smaller but visible enrollment response in December, which is plausible given that the reform was announced on December 11th. This analysis was performed using all families enrolling into the in-worker benefit. But importantly, the same results are found when focusing on enrollment behaviors from families not affected by the monetary incentives change. Looking at the effects on

the flow of entry rather than the stock, Figure A.10 shows that the increase in take-up was much larger among first-time entrants than among past beneficiaries. This is in line with an information provision that alleviates information friction such as lack of awareness of the program or misperception about eligibility. It rejects the hypothesis that the reform only acted as a salience shock which reminded well-informed people to claim.

4.3 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 the same strategy as before but using average characteristics as my outcome. Figure 6 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 similar patterns. Overall, it is hard to conclude whether the take-up response was driven by better-off or worse-off families. 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...). However, it is also interesting to note that these are populations that are less in contact with the welfare agencies, either because they are eligible to less programs (e.g. they are not eligible to family benefits) or because they are more marginalized. I find that almost half of the new entrants have never been in contact with the welfare agency in the past. This stands in contrats with previous results from the literature in the U.S. These studies typically find large positive effects on EITC claiming of information outreach on tax-filers but no effects on non tax-filers. Here, I find large positive effects on the information shock, even among harder-to-reach population.

5 Theoretical Model

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 5.2, 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)[40]. In Section 5.3, 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.

5.1 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: $U(c, y; \omega, \delta) = c - h(y; \omega) - \mathbb{1}_{y>0}\delta$.

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

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

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 5.2. 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

[40] 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.

$F: \Omega \times \Delta \times \Gamma \times \Theta \mapsto [0;1]$ 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.[41]

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 5.2, 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[42], 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[43] that is denoted $B(.)$ (A3). An individual earning 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.

$$\varepsilon(\omega) = \frac{\partial dy(\omega)}{\partial(1-\tau(y(\omega)))} \frac{1-\tau(y(\omega))}{y(\omega)} \tag{2}$$

$$\eta(\omega) = \frac{\partial r(\omega)}{\partial[\bar{y}(\omega) - T^P(y(\omega))]} \frac{1}{r(\omega)} \tag{3}$$

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 informa-

[41] Note that with orthogonal type distribution and quasi-linear preferences, there is no role for ordeals (Albert 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.

[42] 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.

[43] 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 constant.

tion, 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:

$$W = \int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \alpha(\omega) \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) V(\omega, \delta, \gamma, \theta = j|B) \; d\gamma \; d\delta \; d\omega. \tag{4}$$

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:

$$\int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \alpha(\omega) \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) B(y^*(\omega, \delta, \gamma, \theta = j)) \; d\gamma \; d\delta \; d\omega \leq E. \tag{5}$$

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.

5.2 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.[44] 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.[45] 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 $\int_{\underline{\omega}}^{\bar{\omega}} q(\omega) d\omega$.[46] 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 $B(0)$:

[44] 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.

[45] 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.

[46] 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.

$$\xi(\omega) = \frac{\partial q(\omega)}{\partial B(\bar{y}(\omega))} \frac{1}{q(\omega)} \tag{6}$$

$$\xi_0(\omega) = \frac{\partial q(\omega)}{\partial B(0)} \frac{1}{q(\omega)}. \tag{7}$$

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 $p^* = 1$). 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:[47]

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

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:

$$1 + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{8}$$

$$1 = h_y(y_2^T(\omega), \omega). \tag{9}$$

The overall solution of the individual's problem is:

[47] 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.

Proposition 2. *The solution to the individual problem is:*

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

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

The solution $\{y^*, p^*\}$ is represented in the (δ, γ) space by Figure A.12. 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 12A.12.3). 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 12A.12.4). 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 D.4 of Appendix D, 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.

5.3 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).

5.3.1 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 15A.15.1 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.

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

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).[48]

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

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

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

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.[49] In this section, p denotes the decision to take-up the in-work benefit B conditional on being eligible.

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

5.3.2 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:

$$1 + B_0'(y_1^T(\omega)) + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{10}$$

$$1 + B_0'(y_2^T(\omega)) = h_y(y_2^T(\omega), \omega). \tag{11}$$

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.

[49] 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.

Proposition 3. *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 A.13 represents the individual's solution $\{y^*, p^*\}$ in the (δ, γ) space. Two cases arise depending on the skills parameter ω. Figure 13A.13.1 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 13A.13.2 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 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.

5.3.3 Welfare effect of the positive information shock

Given a transfer system that satisfies Assumptions (A4)-(A6), Figure A.14 represents the solution of the individual's problem for agents that are unaware of the in-work benefit program ($\theta = 0$). Figure 14A.14.1 represents the case of individuals individuals with low skills such that $y_2^T(\omega) = 0$. Figure 14A.14.2 represents the case of individuals with intermediate skills such that $y_2^T(\omega) > B(0)$.[50] 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 of the program was

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

p_θ and that after the reform it is $p_\theta + \Delta\theta$.[51] 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 14A.14.1 and 14A.14.2). 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 14A.14.2). Overall, the model predicts that after a positive information shock $\Delta\theta$, the take-up rate and the labor force participation rate increase.

Proposition 4. *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 4 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 welfare.[52] 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 $dV_1(y) \equiv (\delta^T(y) - \delta - T^P(y) - \gamma)$ 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.

[51] 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.

[52] 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.

5.3.4 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 15A.15.1. 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^P_θ 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 15A.15.2). 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 have a low enough cost of claiming (effect ② in Figure 15A.15.2). This response can also imply an intensive margin labor supply response which sign depends on the sign of $B'(y_2^T)$.

Proposition 5. *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 5 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.

5.3.5 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 6. *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 7. *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:*

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

5.3.6 Calibration

I now turn to the empirical welfare analysis of the 2019 welfare reform using the welfare formulas from Proposition 6 and Proposition 7. 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.[53] 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 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 4, I have estimated that the average take-up

[53] 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.

elasticity with respect to the benefit amount was around 0.1.[54] 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 insurance system in Norway, beneficiaries would adjust their earnings such that the (intensive-margin) earning elasticity increases from 0.06 to 0.15. To the contrary, Chetty and Saez (2013) find no effect of information provision about the EITC marginal tax incentives on labor supply at the intensive margin. Nyman et al. (2023).Liebman and Luttmer (2015)

- $\xi^{theta}(y)$ (the take-up semi-elasticities with respect to a change in awareness). In Section 4, 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.

[54] 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.

- $\alpha_0(y)$ and $\alpha_1(y)$ (the welfare weights sequences). These welfare weights represent the social value of transferring one euro from the overall population[55] 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).[56] 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 $\alpha_0 = \alpha_1 = 1.086$.[57]

5.3.7 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 A.20 presents the results of the sensitivity analysis that varies the parameters.

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[58] 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

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

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

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

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

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 A.20 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.[59] 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.[60]

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.[61] Indeed, what drives labor supply and take-up decision in the model is 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).[62] 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.

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

[60] 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.

[61] 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 burdens.

[62] 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.

6 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 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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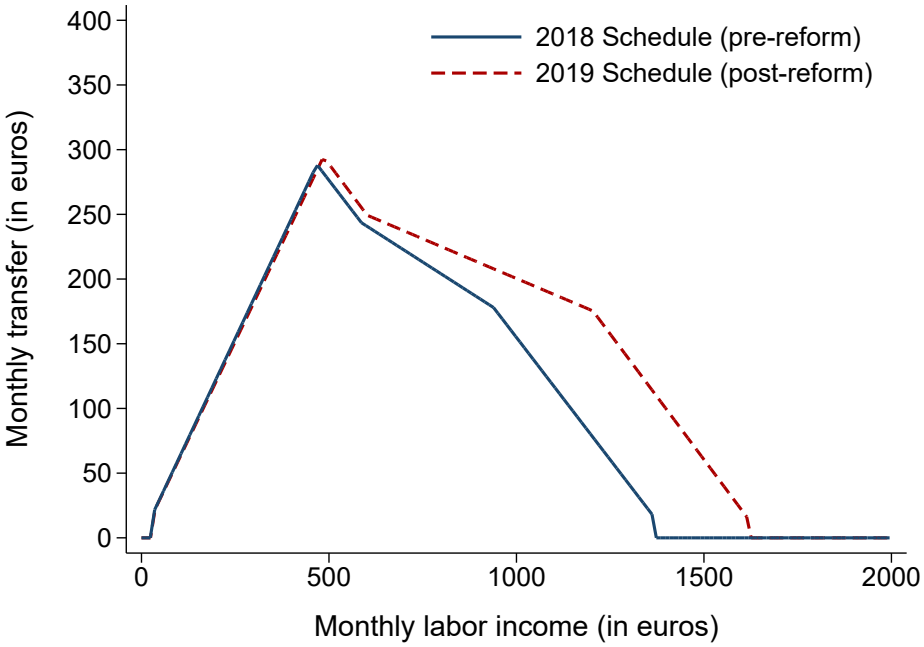
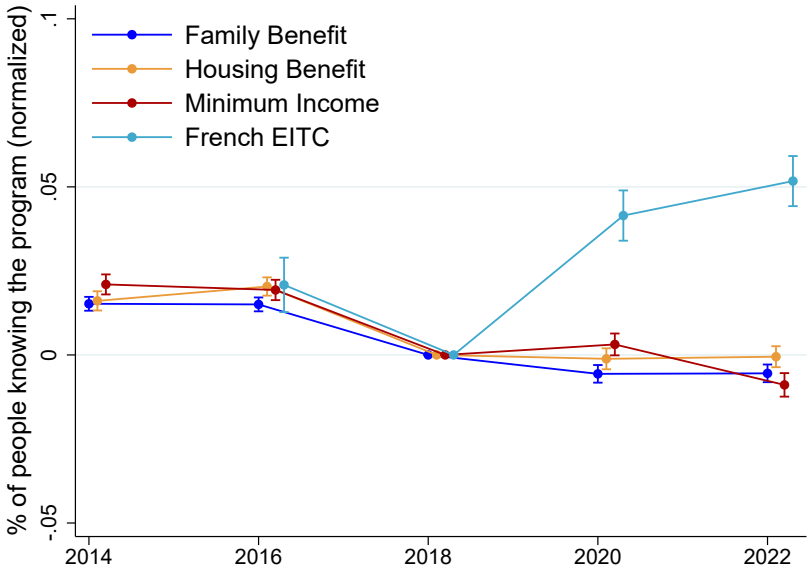
Figure 1: In-work benefit's schedule evolution (2018-2019)



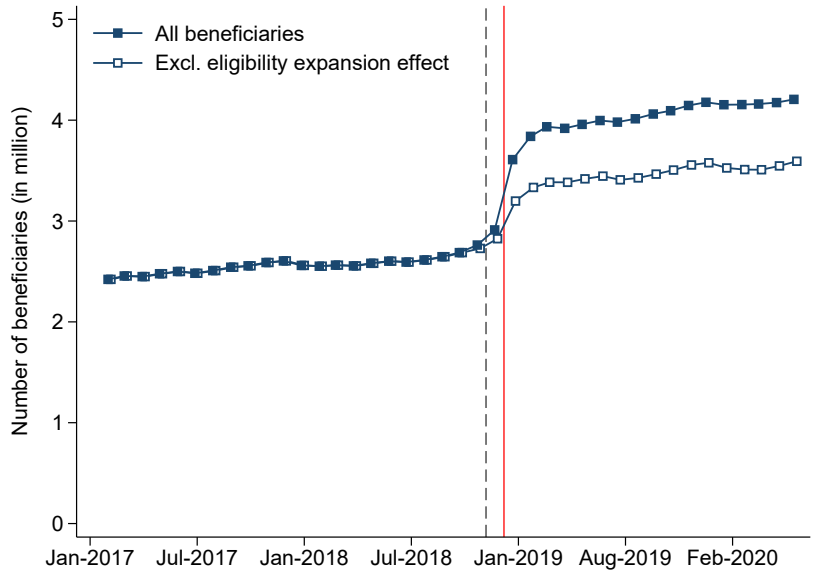
Notes: The figure represents the benefit amount served as a function of labor earnings for a single individual without children and receiving no non-labor income.
Source: Maquette Edifis, DREES, Ministry of Health and Social Affairs.

Figure 2: Program awareness evolution (2014–2022)

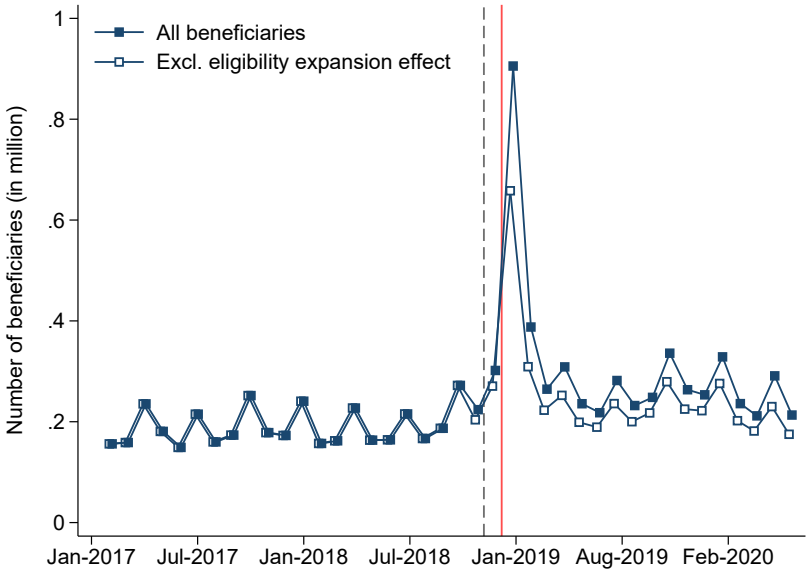


Notes: The figure represents the coefficients of a simple event-study regression which estimates the percentage point change in self-reported awareness of the program for different years relative to its level in 2018. Awareness is a dummy equal to one for individuals answering positively to the question "Have you ever heard about program [*program's name*]?". Regression is weighted to be representative of the population of individuals in the 18-65 years old bracket
Source: General Opinion Survey (*Baromètre d'opinion de la DREES*).

Figure 3: In-work benefit's enrollment (2017-2020)



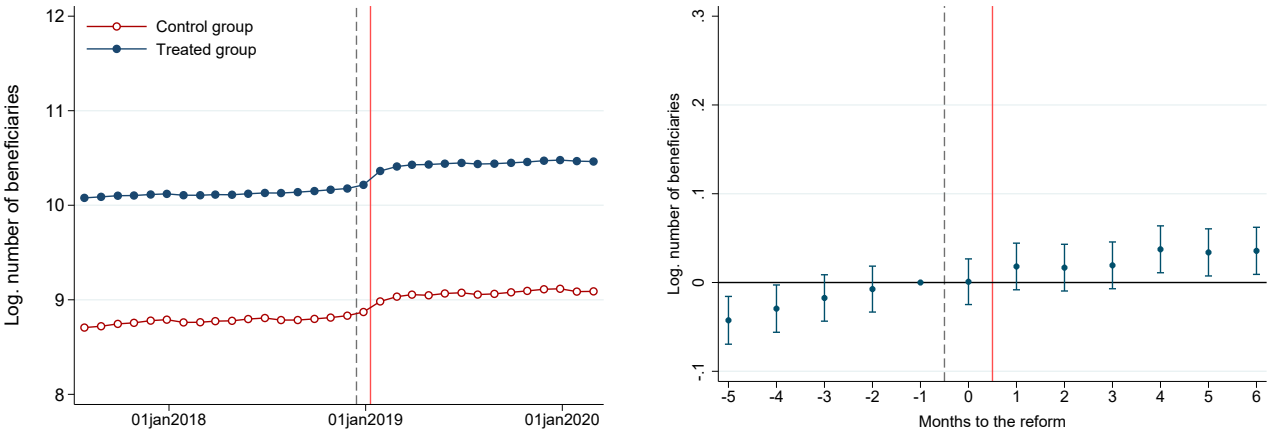
(a) Enrollment stock



(b) Enrollment flow

Notes: This figure plots the evolution of the number of families enrolled in the French in-work benefit program (PA). Panel (a) plots the *stock* of beneficiaries in a given month. Panel (b) plots the *flow* of entrants, i.e. the number of families who started claiming the program in a given month. The dashed blue line represents the date of announcement (Dec. 10th 2018) and the solid line represents the date of implementation of the reform (Jan. 1st 2019). In each panel, a separate line (empty square) represents the same statistics for a subsample that excludes families benefiting from the eligibility expansion caused by the 2019 reform. Using micro-simulation techniques, I simulate eligibility under the actual system and a counterfactual system with no reform in place. The subsample excludes families who are eligible under the former but not under the latter system.
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Figure 4: Effect of the increase in monetary incentives

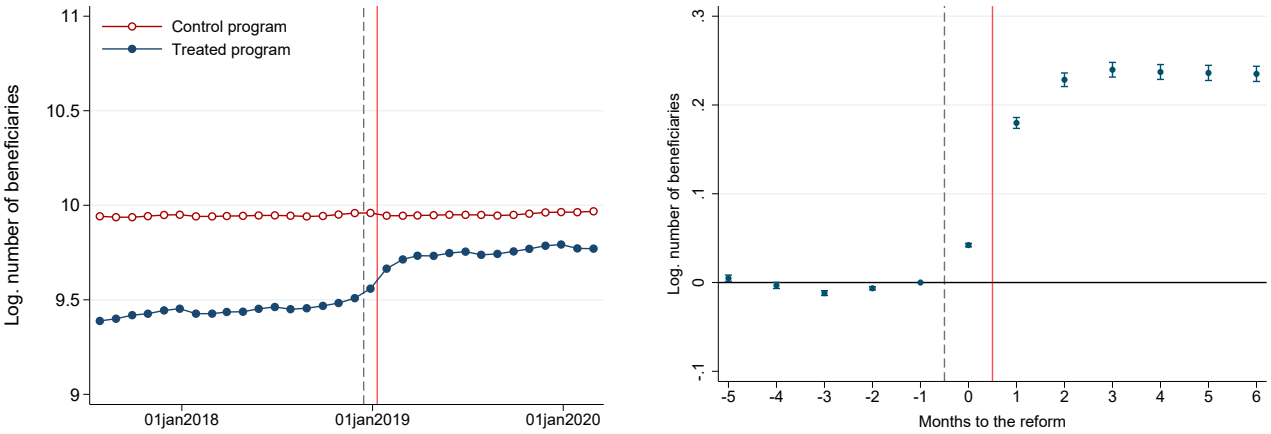


(a) Enrollment (raw data)

(b) Diff-in-diffs coefficients

Notes: Panel (a) plots the evolution of the raw average log. number of families enrolled in local welfare agencies for the group of treated and control families separately. A family is labeled as "treated" if it faces a difference of more than 15 euros between its current benefit amount and the counterfactual benefit amount it could get under a system with no reform in place. Panel (b) plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Section 3.2. The x-axis denotes the number of months relative to the announcement month (December 2018). The sample is restricted to beneficiaries who are not in the eligibility expansion range. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Figure 5: Effect of the information shock

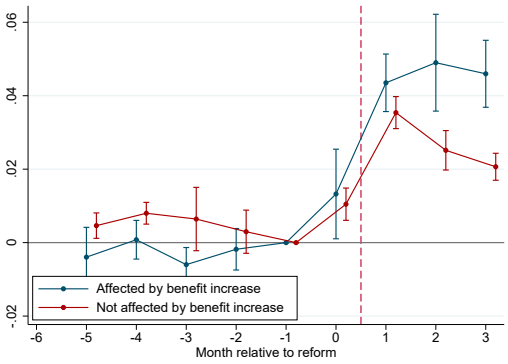


(a) Enrollment (raw data)

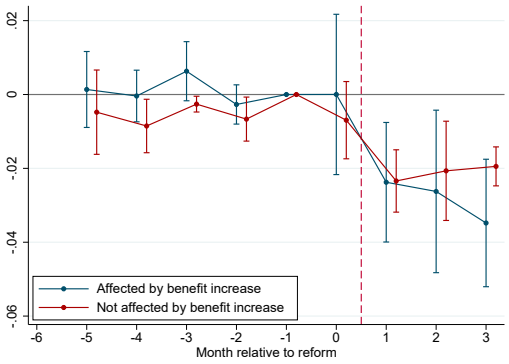
(b) Diff-in-diffs coefficients

Notes: Panel (a) plots the evolution of the raw average log. number of families enrolled in local welfare agencies for the in-work transfer (treated program) and the safety net (control program) separately. Panel (b) plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Section 3.3 including all beneficiaries of the in-work transfer (regardless of their exposition to the benefit increase). The x-axis denotes the number of months relative to the announcement month (December 2018). The sample is restricted to beneficiaries who are not in the eligibility expansion range. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

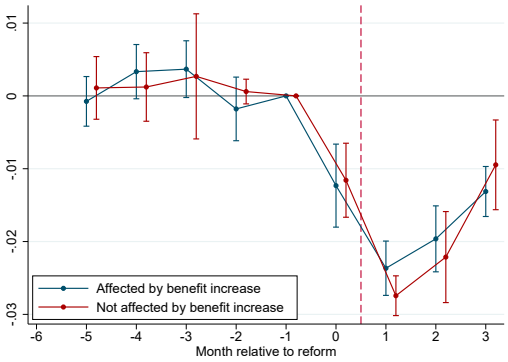
Figure 6: Difference-in-difference estimation on average characteristic



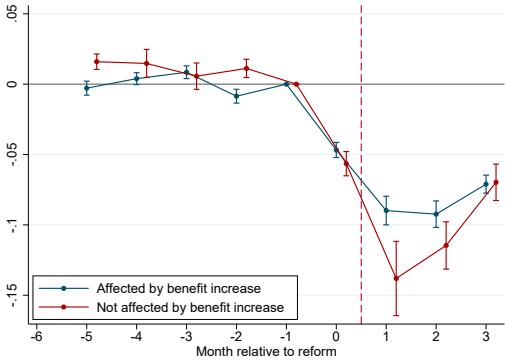
(a) Share of single male



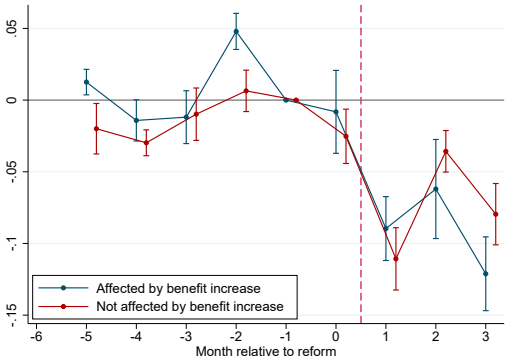
(b) Share of couples



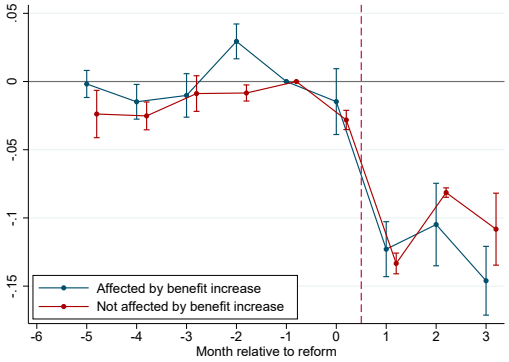
(c) Share of non-EU foreigners



(d) Share of past beneficiaries of the safety net



(e) Average log monthly wage earnings



(f) Average log taxable income

Table 1: Descriptive Statistics

	Any Program (1)	PA program (2)
Demographics		
Single Women	0.35	0.49
Single Men	0.23	0.26
Couples	0.42	0.26
Number of children	0.88	0.76
Age of the family head	40.89	37.47
Income (in euros per adult in family)		
Labor income	10796.01	8,457.77
Unemployment Insurance	784.55	920.75
Pensions	995.06	207.53
Total taxable income	11124.99	8,281.10
Total benefits received	254.76	363.65
Number of obs. (in millions)	16.22	2.65

Notes: This table presents descriptive statistics for the month of October 2018. In column (1), the statistics are presented for the sample of families receiving any transfer from the welfare agency. In column (2), the statistics are presented for the sample of families receiving the in-work transfer program.
Source: Administrative social data, ALLSTAT-FR6, 2018/10.

Appendices

A Details on the Institutional Setting

Benefit schedule. Figure A.1 represents the schedule of the in-work benefit. 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 A. Overall, a single minimum wage worker received about 90 euros (respectively 150 euros for one eligible to housing benefits) per month in 2018 which represents about 7% (respectively 15%) of her total disposable income (see Figure A.2). 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.

Benefit formula. Equation 12 provides the exact formula of the PA transfer in a given month. Table A.1 and Table A.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^a_i. The detailed formula for the bonus of an individual i is provided by Equation 13.[63]

$$B_t = \underbrace{MB_t\left(1+\delta^f_t\right)}_{\text{Base amount}} + \tau_t R^a_t - \underbrace{\max\left(MB_t\left(1+\delta^f_t\right), R_t\right)}_{\text{Means-Testing}} + \underbrace{\sum_i Bonus\left(R^a_{it}\right)}_{\text{Individual bonuses}} \tag{12}$$

$$Bonus\left(R^a_{it}\right) = \min\left(\tau_b MB_t, \max\left(0, \tau_b MB_t \times \frac{R^a_{it} - s_{min}\bar{w}}{s_{max}\bar{w} - s_{min}\bar{w}}\right)\right) \tag{13}$$

Political Background. 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 on December 11th 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

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

of additional public spending.[64] Note that the President in his December 10th speech did not mention the in-work transfer explicitly. It was the following day that the government announced that the increase was going to take place through this specific program. Also note that before the announcement of the reform on Dec. 10th, no specific focus was put by the media on the PA program[65] and no policy-makers had mentioned a 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. The reform was then 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.[66]

Table A.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

Table A.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

[64] 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).

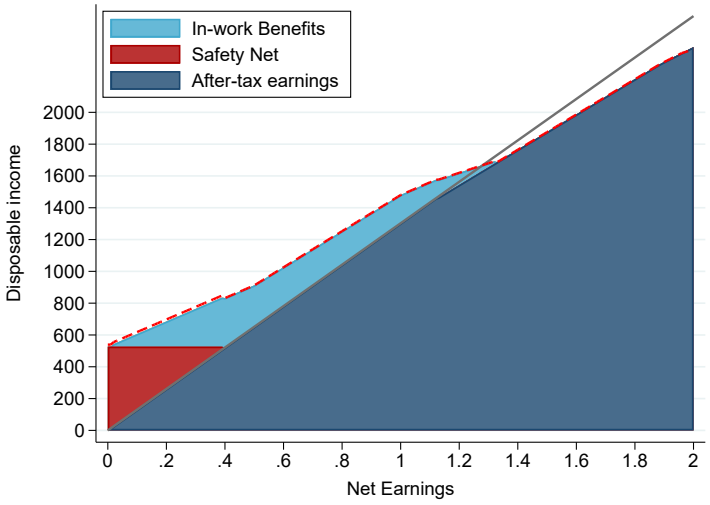
[65] This can be seen for example in the Google searches represented in Figure A.8.

[66] 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 A.1: Schedule of the in-work benefit (PA)

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.
Source: Maquette Edifis, DREES, Ministry of Health and Social Affairs.

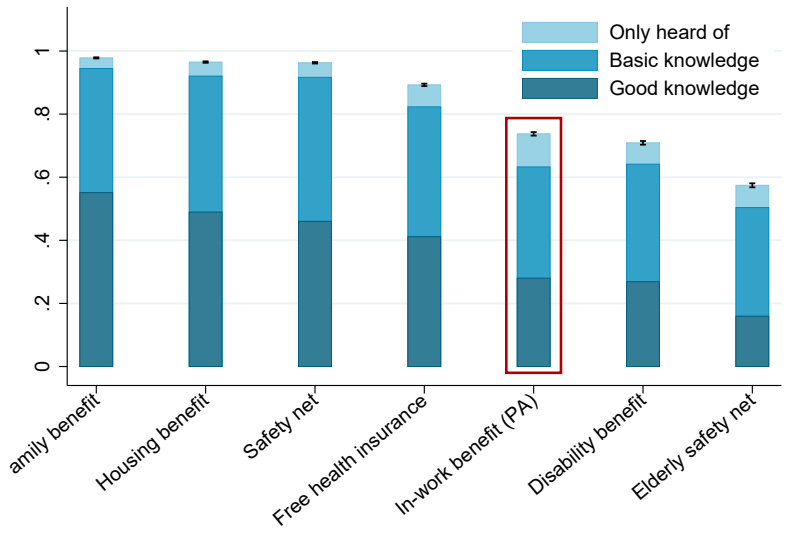
Figure A.2: Budget constraint



Note: Test-case of a single home-owner individual with only wage earnings as resources. Legislation as of 2022.
Source: Maquette Edifis, DREES, Ministry of Health and Social Affairs.

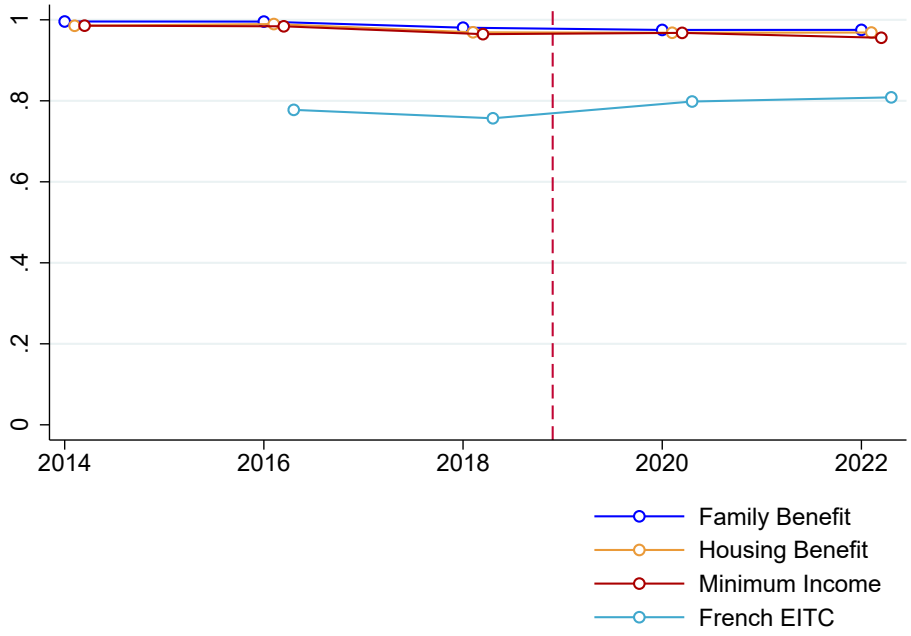
B Characterizing the information shock

Figure A.3: Knowledge about welfare programs



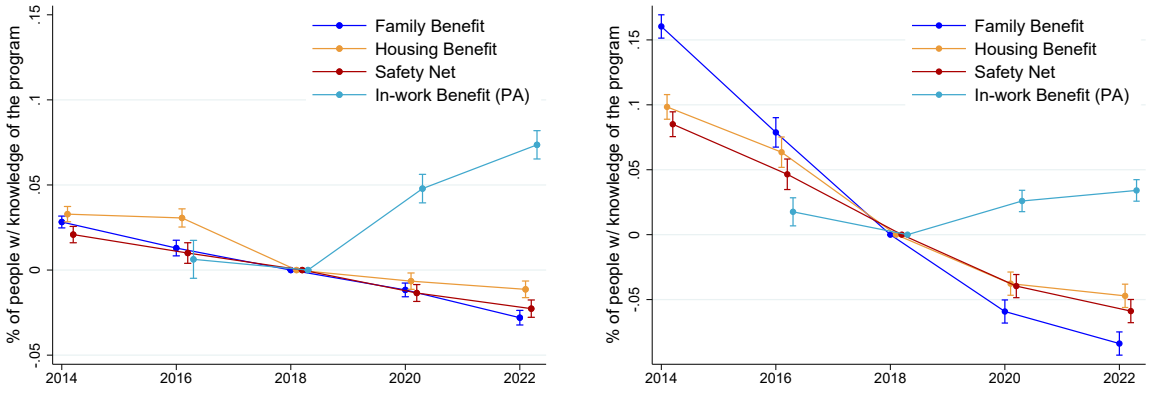
Sources: General Opinion Survey (*Baromètre d'opinion*) and author's own computations.
Notes: Sample restricted to individuals aged 18 to 65 years old. Information is missing for the PA program in 2014 since the program was introduced in 2016. Graph (b) represents the event-studies coefficients of a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

Figure A.4: 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.

Figure A.5: Knowledge about welfare programs

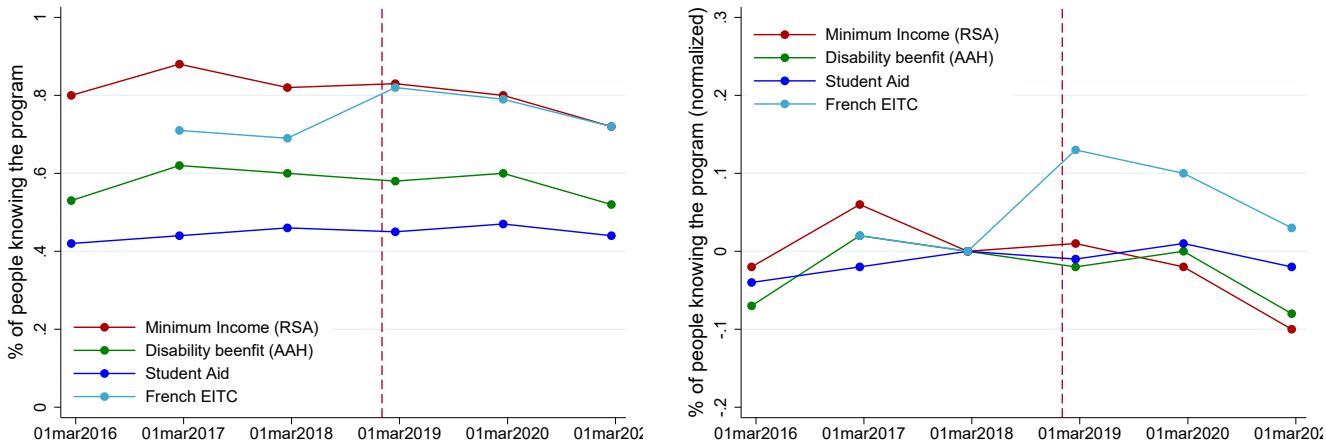


A.6.1 Intermediate Knowledge

A.6.2 Good Knowledge

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.6: Awareness rate (other survey data)



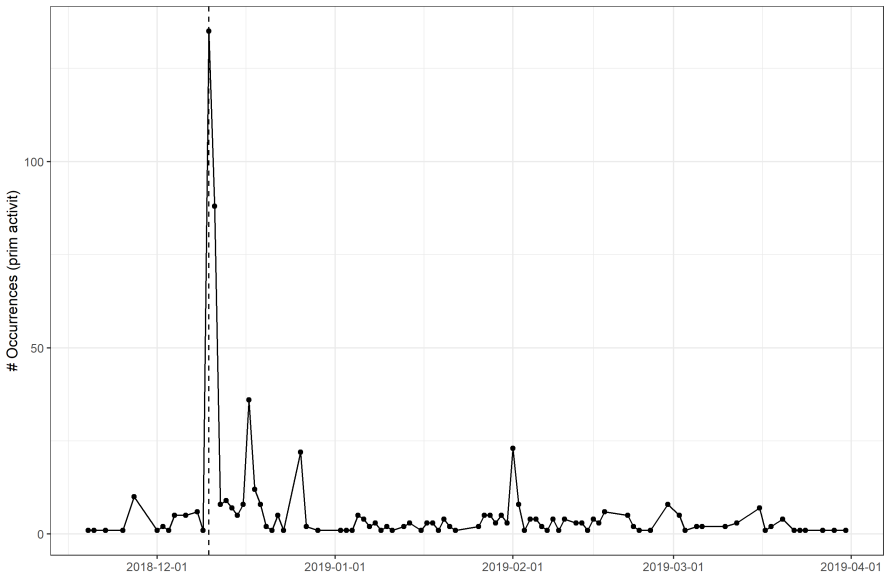
Source: Survey "Baromètre DJEPVA sur la jeunesse" (CREDOC-INJEP, 2016-2021).
Population: Representative sample of 18-30 years old individuals.

Table A.3: Heterogeneity in awareness changes

	Pre-reform (2018)	Post-reform (2020)	Difference
Full population	0.76	0.80	0.03***
	(0.43)	(0.40)	
By income per capita			
< 600 euros	0.78	0.83	0.05
	(0.41)	(0.38)	
600-1200 euros	0.73	0.76	0.03
	(0.45)	(0.43)	
1200-2000 euros	0.77	0.81	0.05**
	(0.42)	(0.39)	
> 2000 euros	0.76	0.79	0.03
	(0.42)	(0.40)	
By family category			
Single w/o kids	0.72	0.76	0.04*
	(0.45)	(0.43)	
Single parent	0.81	0.87	0.06**
	(0.39)	(0.34)	
Couple	0.77	0.81	0.03
	(0.42)	(0.39)	
N	2,104	2,787	

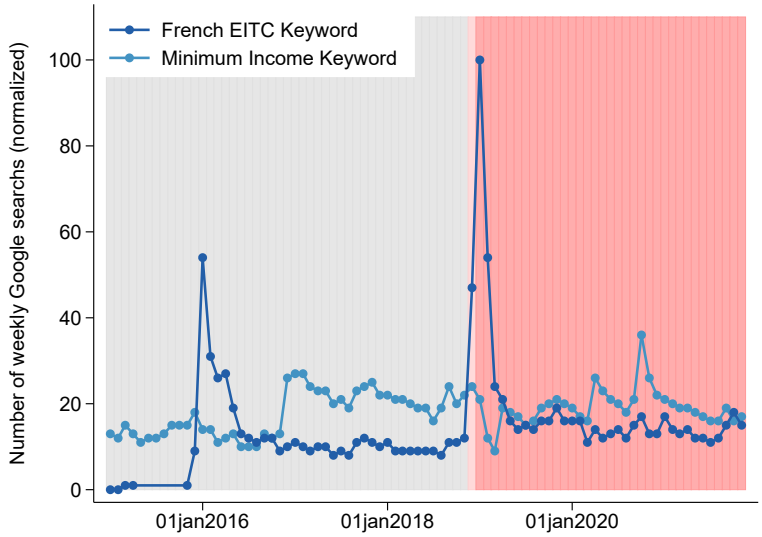
Source: Baromètre d'opinion DREES, 2000-2020. Sample of 18-65 years old individuals.
Notes: The outcome variable is a dummy indicating that individuals declare having heard about the PA program.

Figure A.7: PPA benefit in yellow vests Facebook groups' comments



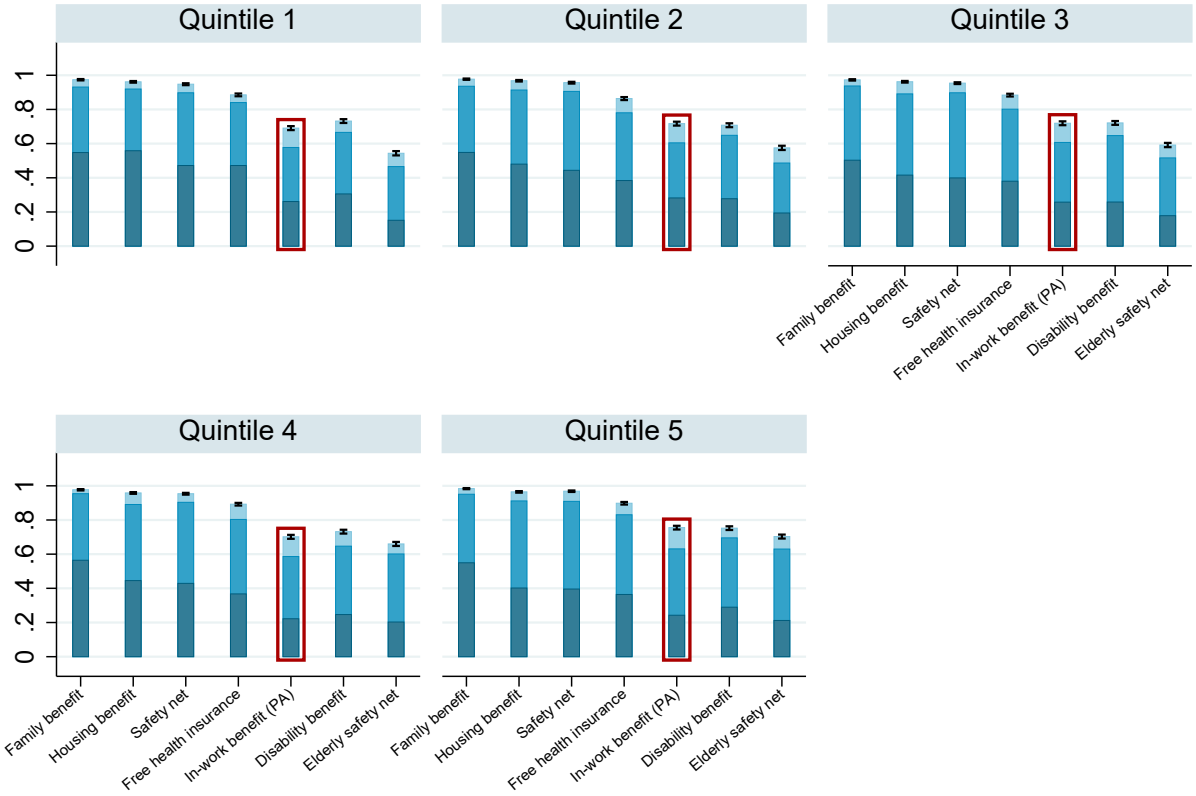
Sources: Database from Boyer et al. (2020).
Lecture: Number of occurrences of the pair of adjacent words "prim" and "activit" in discussions on Facebook pages associated to "Gilets Jaunes".

Figure A.8: Google Trends associated with the PPA and the RSA benefits



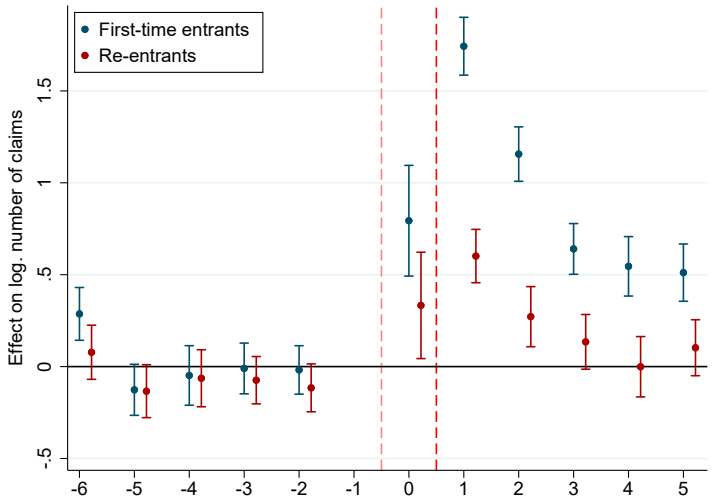
Sources: Monthly Google Trends.
Notes: The keywords used are "prime d'activité" (PA) and "revenu de solidarité active" (RSA). The number of searches is normalized to 100 at the date with the highest number of research for the topic in the region and period considered.

Figure A.9: Knowledge level by quintile of disposable income



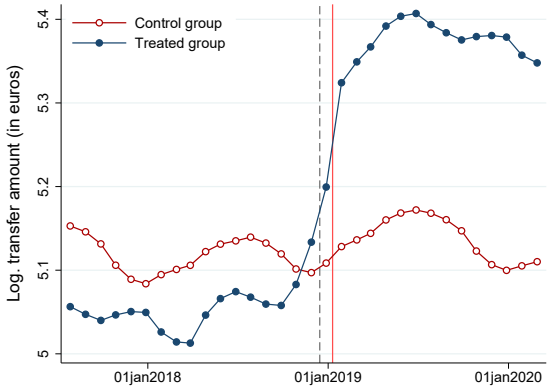
C Additional Graphs

Figure A.10: Evolution of log. number of claims across subgroups

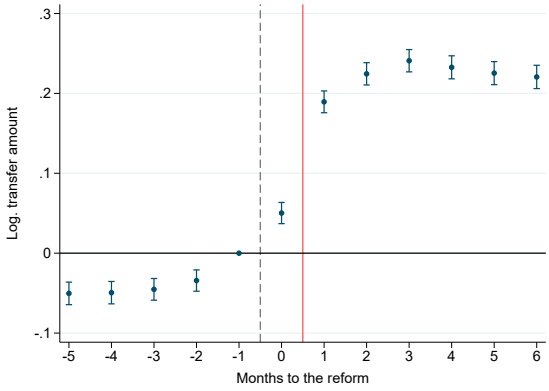


Notes: Sample restricted to families not exposed to the benefit increase.

Figure A.11: Effect of the increase in monetary incentives – First stage



A.12.1 Transfer amount (raw data)

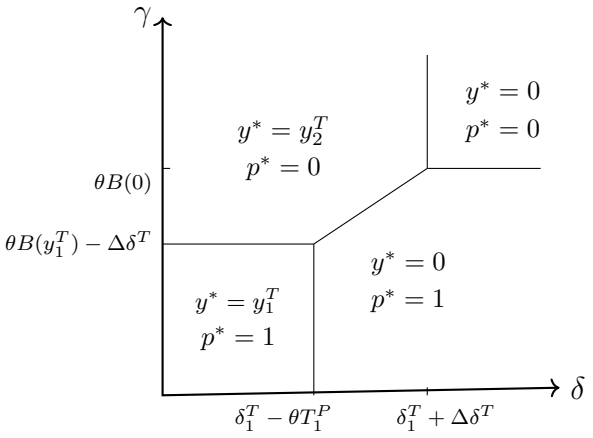


A.12.2 Diff-in-diffs coefficients

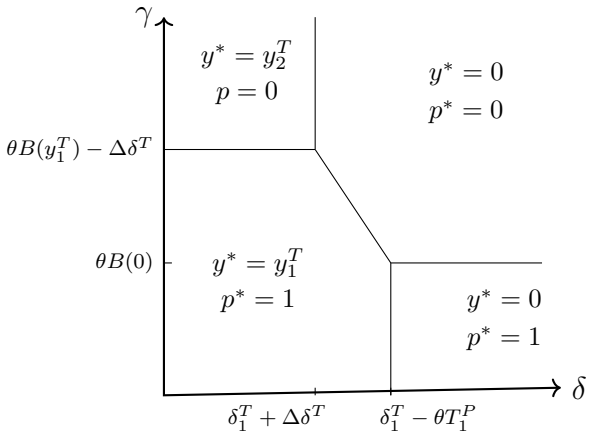
Notes: Panel (a) plots the evolution of the raw average log. transfer received by families in local welfare agencies for the group of treated and control families separately. A family is labeled as "treated" if it faces a difference of more than 15 euros between its current benefit amount and the counterfactual benefit amount it could get under a system with no reform in place. Panel (b) plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Section 3.2. The x-axis denotes the number of months relative to the announcement month (December 2018). The sample is restricted to beneficiaries who are not in the eligibility expansion range.
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

D Appendix to the Theoretical Model

D.1 Figures

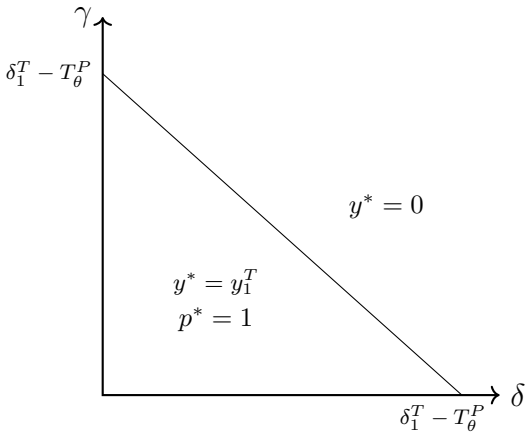


A.12.3 Case when $\theta T_1^P > -\Delta\delta^T$

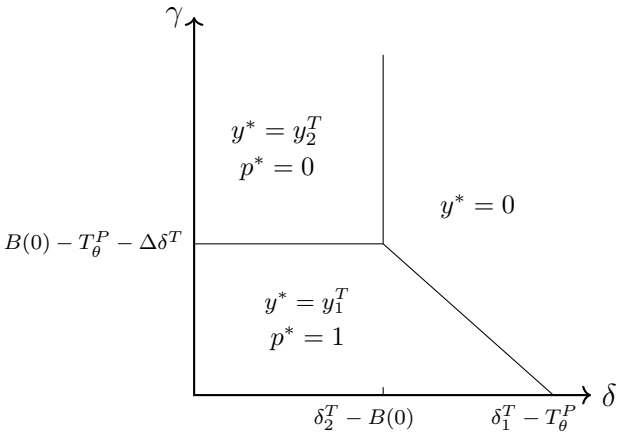


A.12.4 Case when $\theta T_1^P < -\Delta\delta^T$

Figure A.12: Individual optimal earnings and take-up decision



A.13.1 Case when $y_2^T(\omega) = 0$



A.13.2 Case when $y_2^T(\omega) > B(0)$

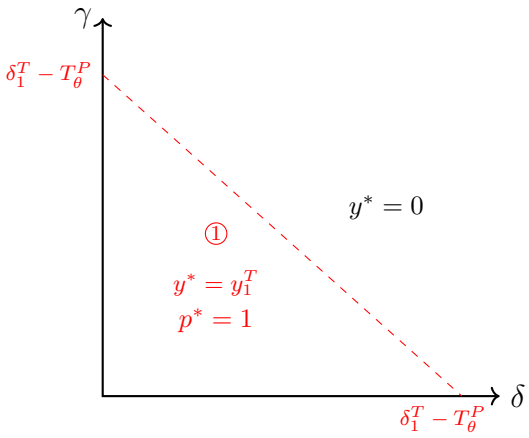
Figure A.13: Individual optimal earnings and take-up decision

D.2 Preliminary Proofs

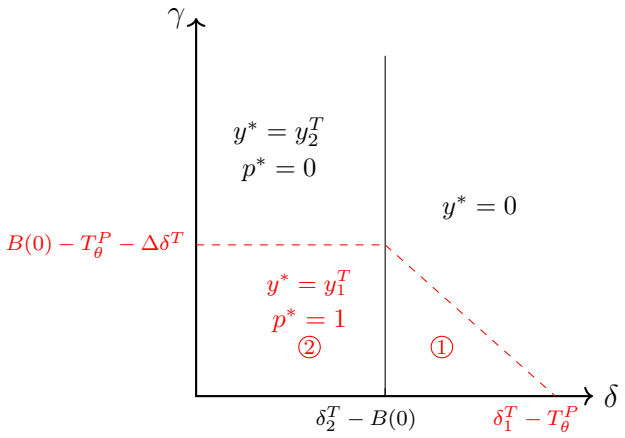
I start by stating and proving a lemma that provides an expression for the effects of a small reform of the benefit schedule in terms of sufficient statistics. A reform is an object (τ, h) that replaces the benefit schedule $B(y)$ by $B(y) + \tau h(y)$ where $h(.)$ is a flexible function. Lemma 1 characterizes 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.[67]

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*

[67] The proofs in this subsection rely on the general framework described in Section 5.1 and 5.2. They do not require the additional simplifying assumptions made in Section 5.3.



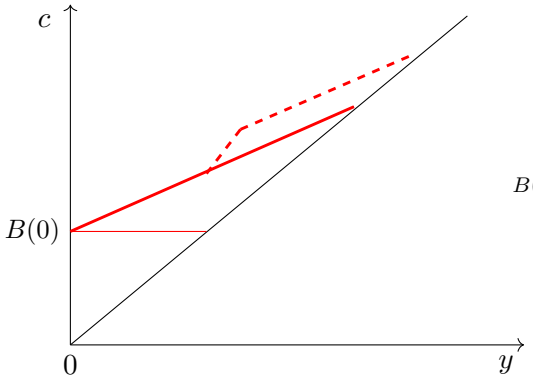
A.14.1 Case when $y_2^T(\omega) = B(0)$



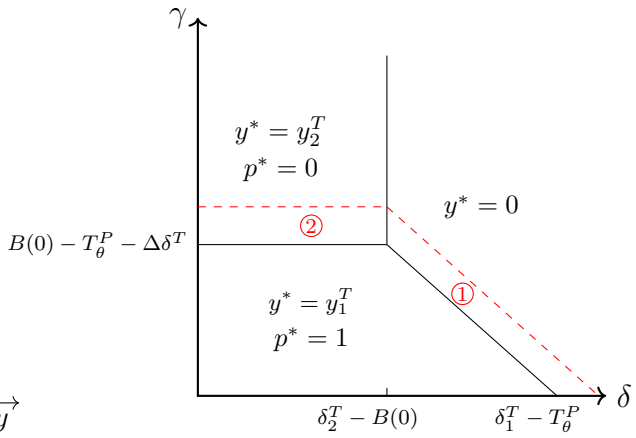
A.14.2 Case when $y_2^T(\omega) > B(0)$

Figure A.14: 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 $\theta = 1$).



A.15.1 Description of the reform



A.15.2 Effect of the reform

Figure A.15: 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.

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 5.1, the derivatives of $y(\omega,\tau)$, $q(\omega,\tau)$ and $\tilde{r}(\omega,\tau)$ can be expressed in the following way:

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

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:

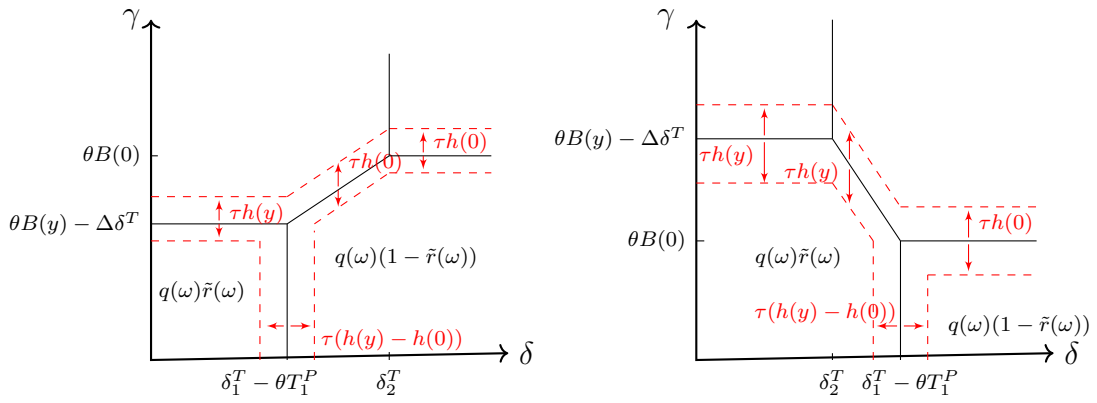
$$\begin{aligned} R_\tau(\tau,h) = & \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\tau)\left[(1-\tilde{r}(\omega,\tau))h(0) + \tilde{r}(\omega,\tau)h(y(\omega,\tau))\right]d\omega \\ & + \int_{\underline{\omega}}^{\bar{\omega}} g_\omega(\omega)q(\omega,\tau)\left[\tilde{r}(\omega,\tau)h'(y(\omega,\tau))\varepsilon(\omega)y(\omega,\tau)\frac{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

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 5.1, the fiscal externalities can be represented graphically. Figure A.16 represents how the optimal earning and take-up decisions move with a reform (τ, h).



A.16.1 Case when $\theta T_1^P > -\Delta\delta^T$ A.16.2 Case when $\theta T_1^P < -\Delta\delta^T$

Figure A.16: Effect of the reforms on behaviors

D.3 Proofs of Section 5.3

In this section, I provide the proofs for the results of the welfare analysis of the 2019 reform presented in Section 5.3.

Proof of Proposition 4. 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[68], $y(\omega,\theta)$ 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 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:

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

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

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

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: $dV_1(y) = (\delta^T(y) - \delta - T^P(y) - \gamma)$ and $dV_2(y) = (B(y) - \gamma)$ □

Proof of Proposition 5. 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.

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

D.4 Welfare effects of introducing an in-work benefit

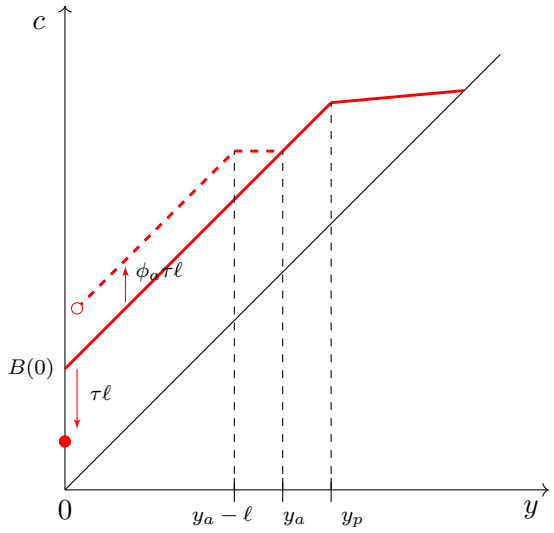
In this section, I revisit partially the question of the optimal transfer program at the bottom of the income distribution (Saez 2001). More precisely, I re-derive the necessary and sufficient conditions for the introduction of an in-work benefit (negative marginal tax rates and participation tax at 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.

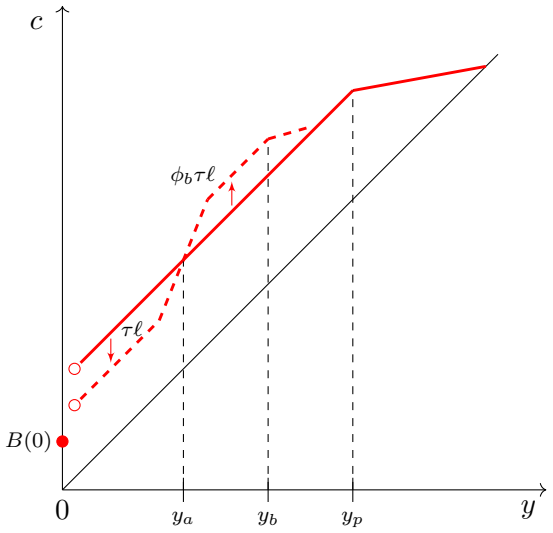
$$B \text{ is such that } T^P(y) = t_p \text{ for all } y \in (0, y_p). \tag{A4bis}$$

Figure A.17 represents the effect of such reform on the individual's budget constraint. The reform is introduced in two steps. Figure 17A.17.1 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 17A.17.2 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.17.1 Introduction of negative participation tax



A.17.2 Introduction of negative marginal tax rates

Figure A.17: 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*

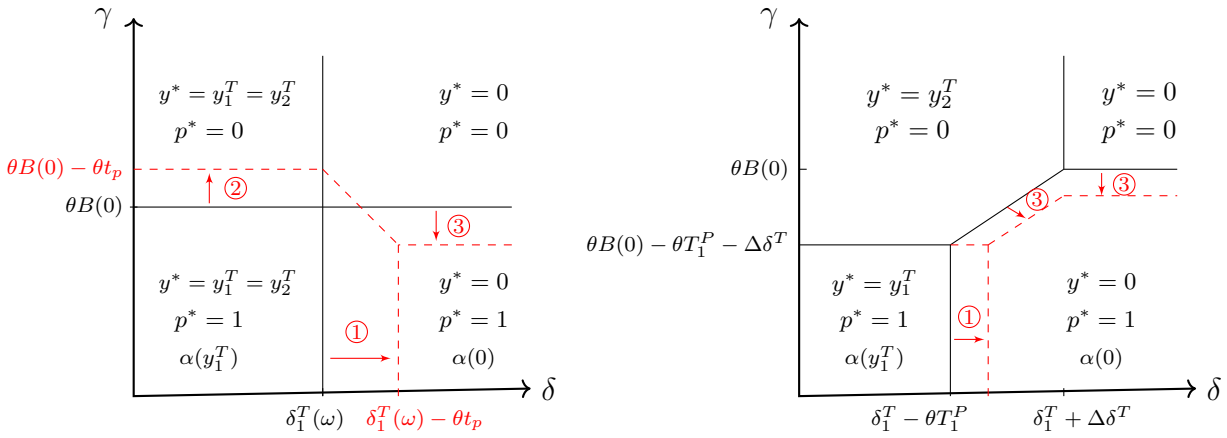
$$\int_{y_p}^{\bar{y}} f_y(y)\tilde{\eta}(y)T^P(y)dy + F_y(0)B(0)\left[\xi_0 - \hat{\xi}(\underline{y}, y_a)\right] > F_y(0)\left[\hat{\alpha}(0) - \hat{\alpha}(\underline{y}, y_a)\right].$$

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 $F_y(0)(\hat{\alpha}(0) - \hat{\alpha}(\underline{y}, y_a))$. 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.[69] 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?

The fiscal externalities terms represent the extra tax revenue generated by the reform due to behavioral responses. They are represented in Figure A.18. 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 18A.18.2). 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

[69] 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).



A.18.1 Individuals with $y_1^T(\omega) < y_p$

A.18.2 Individuals with $y_1^T(\omega) > y_p$

Figure A.18: 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.

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 18A.18.1) 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 and taking up.[70] 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 18A.18.1 and Figure 18A.18.2) 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*

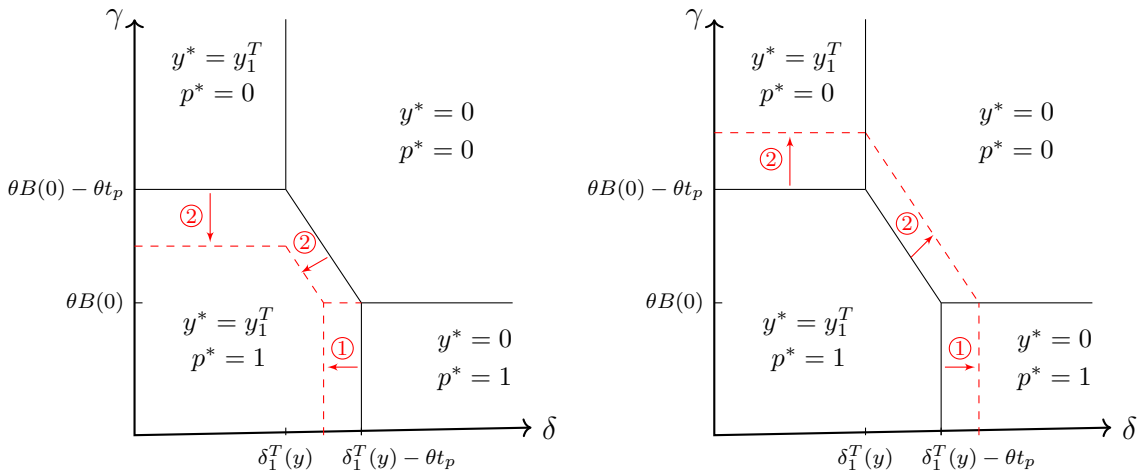
$$-t_p\left[\hat{\tilde{\eta}}(\underline{y}, y_a) - \hat{\tilde{\eta}}(y_a, y_b)\right] + \bar{B}\left[\hat{\xi}(\underline{y}, y_a) - \hat{\xi}(y_a, y_b)\right] > \left[\hat{\alpha}(\underline{y}, y_a) - \hat{\alpha}(y_a, y_b)\right].$$

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

[70] 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.

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.



A.19.1 Individuals with $y_1^T(\omega) < y_a$

A.19.2 Individuals with $y_1^T(\omega) \in (y_a, y_b)$

Figure A.19: 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.

The behavioral responses generating the fiscal externalities are represented in Figure A.19. 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 19A.19.1). 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 19A.19.2). 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 $(\hat{\bar{\eta}}(y_a, y_b) - \hat{\bar{\eta}}(\underline{y}, y_a))$ 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 19A.19.1). 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 19A.19.2). 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.

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.

D.4.1 Proofs for this section

Proof of Lemma 2 Let us define a tax reform (τ, h_a) depicted in Figure 17A.17.1 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[71], the marginal effect of the small reform on welfare is:

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

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

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:

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

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 17A.17.2 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:

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

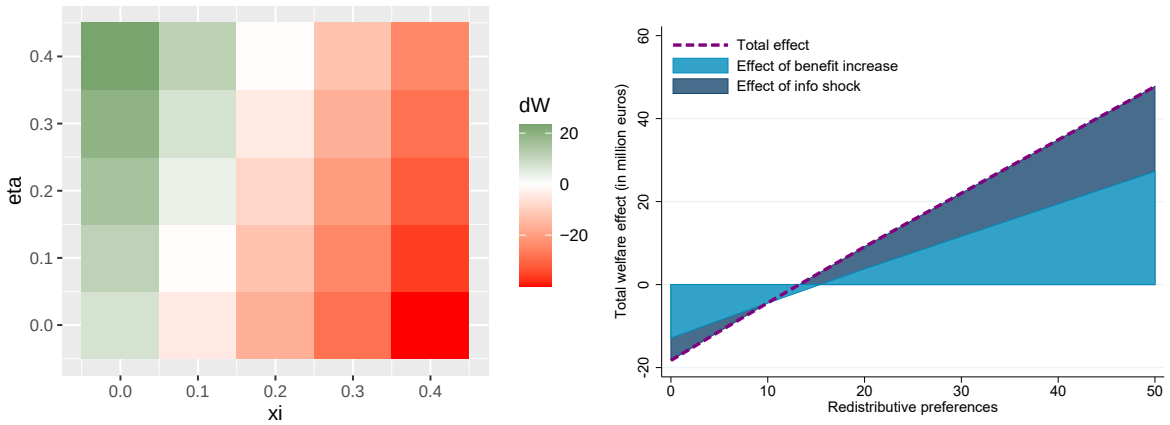
Hence we can express the second-order derivative of the welfare function as:

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

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. □

E Welfare Analysis



A.20.1 Welfare effects by elasticities

A.20.2 Welfare effects by welfare weights

Figure A.20: 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.