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

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Abstract

Imperfect take-up of social transfer programs 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 an information shock. Using a nationwide reform of France's in-work transfer scheme and exhaustive social security admin data, I provide the first at-scale causal evidence of the role of money and untargeted information provision in shaping take-up behaviors of welfare programs. 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 less than 0.1. In contrast, and exploiting additional data sources, I show that the media coverage of the reform acted as an information supply shock, that improved awareness and knowledge of the program, causing an 18% permanent surge in take-up in the month following the reform. This specific type of information provision intervention was successful even among populations traditionally perceived as harder to reach (e.g. those with no past contact with the welfare administration). Finally, I develop a theoretical framework to estimate the welfare effects of these interventions and draw broader policy implications. The model highlights that, in the case of in-work transfers, broad information provision might be socially desirable not only for *equity* motives but also for *efficiency* ones by encouraging joint take-up and labor supply responses.

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1 Introduction

In-work transfers[1] have gradually become the main anti-poverty policy tool of modern welfare states. In the United States, one of the first country to implement such program, the Earned Income Tax Credit (EITC) lifts a total of 9.1 million individuals out of poverty, including 4.7 million children (Nichols and Rothstein 2015).[2] 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). 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).[3]

Despite the key role of in-work transfers, take-up of such programs 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.[4] 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 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) transfer increase. To do so, I leverage a recent nationwide reform of France's in-work benefit as a natural experiment. 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 broad 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. Interestingly, some of these studies find positive effects on take-up (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.

[1] Throughout this paper, I use the term "in-work transfer" or "in-work benefit" to refer to means-tested social transfer programs reserved for working individuals.

[2] 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).

[3] Some recent studies have questioned the consensus regarding the positive effects of in-work benefits. In particular, Kleven (2024) finds that most EITC reforms lead to no positive employment effects and Albertini et al. (2020) finds that the EITC has negative effects on high-school completion rates.

[4] 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.

2019; Castell et al. 2025). It has been speculated that this could be due to differences across studies in the targeted population of interest (e.g. tax filers versus non-tax-filers) or in the scale of the intervention tested.[5] By using a nationwide reform and exhaustive data, this paper provides new evidence about the heterogeneous effects of such interventions across populations and about whether they can be successfully scaled up or not.[6] Additionally, I develop 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.

In 2019, France implemented a large and salient reform of the in-work benefit program ("prime d'activité" or PA hereafter). 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 single 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 represents an interesting setting to study policy interventions aiming to improve welfare participation. First, it provides quasi-random *within-program* 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 that might have improved knowledge of the program among eligible individuals. Since the media coverage produced additional information about the in-work benefit program solely, it also provides *cross-program* variation in information provision that I will leverage for identification.

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 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

[5] To the best of my knowledge, in the extensive literature studying information outreach interventions, there have 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. Neither study an information shock through media coverage of the program like this paper.

[6] 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; DellaVigna and Linos 2022).

[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.

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 small in magnitude (a 2% surge in the take-up rate) and statistically not significant. This would imply a take-up elasticity with respect to the benefit size of about 0.05.[10]

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 leverage a wide range of additional data sources (survey data[11], media corpus, and Google Trends) and start by documenting four key descriptive facts. First, awareness and knowledge of the program were imperfect pre-reform, even among likely eligible individuals. Second, at the time of the reform, the program attracted a large media coverage which increased the quantity of information available.[12] Third, media coverage successfully shifted attention toward the program as seen by the large increase of Internet searches of the program's name. Finally, awareness and knowledge of the program significantly and permanently increased after the reform. Program awareness rose from 75% to 80% in the overall population in the year following the reform.[13] Importantly, these effects are similar among low-income individuals, a subset of the population that is likely eligible for the program. Because the media coverage of other programs did not change at the time of the reform, I 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) to identify the causal effect of the information shock.

[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] To the best of my knowledge, there haven't been other papers estimating such take-up benefit elasticity and it is thus hard to benchmark this result. In a recent study, Rosenqvist and Selin (2023) estimated a 0.25 take-up elasticity in the context of parental leave benefits in Sweden, which is arguably a very different type of program than in-work benefits.

[11] The General Population Survey I leverage asks a representative panel of the French population about their knowledge of several welfare programs.

[12] In both printed press and online media, the number of articles mentioning the program rose by almost 600% compared to previous months.

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

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 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.[14] 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(Nichols and Zeckhauser 1982; Kleven and Kopczuk 2011).[15] 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.[16]. A possible explanation is that this paper evaluates quite a different type of

[14]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.

[15]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.

[16]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

information intervention. This paper investigates the role of general and untargeted information provision, similar to a nationwide 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.

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"/"frictional" non take-up decision. 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 families 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 program 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.

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

11% increase in take-up (from 75 to 83%).

[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).

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 and on the sub-population of unemployed workers, Castell et al. (2025) investigates the role of personal assistance and information provision (via an online simulator tool) on take-up of several welfare programs, including the in-work benefit. Contrary to them, I focus on the role of information provision via media and study the entire population of eligible. Most of the rest of the literature has centered around the EITC in the U.S. (Kopczuk and Pop-Eleches 2007; Bhargava and Manoli 2015; Goldin, Homonoff, Javaid, 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 families identified as 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.

Finally, this paper aims 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 why imperfect take-up might be part of the second-best optimum. This can arise when governments can use ordeals as efficient self-targeting mechanisms (Besley and Coate 1992; Nichols and Zeckhauser 1982) or when using monitoring technologies (Kleven and Kopczuk 2011; Jacquet 2014). Yet, these papers often do not account for the role of endogenous labor supply in shaping optimal policies, even though take-up and labor supply are in fact joint decisions when transfers are means-tested.[19] The model I develop thus helps to intersect both frameworks and allows

[18] Very few papers have studied the effects of nationwide reform on aggregate take-up rates. To the best of my knowledge, this is the first paper to use a nationwide 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%.

[19] In the model developed by Jacquet (2014), the decision to participate to the labor market is endogenous. However, she focus on a disability benefit and a welfare benefit that are reserved for individuals not participating to the labor force.

me to derive new insights. In doing so, I relate in particular to two papers. First, Bastani et al. (2024) studies the question of the optimal design of redistributive policies but focusing exclusively on "rational" non take-up behaviors (i.e. driven by application costs) exclusively. In contrast, and in line with my empirical framework, I incorporate both application costs as well as information friction as drivers of take-up behaviors. Second, Finkelstein and Notowidigdo (2019) derives welfare formulas for the effects of interventions aiming to increase take-up. Contrary to their model, my model is more closely related to the optimal taxation framework with (i) a flexible non-linear transfer schedule and (ii) labor supply adjusting at both margins. This allows me to highlight the overlooked efficiency rationale for why broad information provision might be desirable. The tax perturbation (or variational) approach I use, following closely the recent advances in this literature made by Bierbrauer et al. (2023), provides me with welfare formulas expressed in terms of sufficient statistics that can be estimated empirically.

2 Institutional Context & 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 Setting: the French *prime d'activité* (PA) in-work benefit

The in-work benefit program ("*prime d'activité*") is a transfer program created in 2016 to provide financial support to low-income workers and foster work incentives.[20] It stands as the second largest means-tested cash transfer in France, representing a total annual spending of about 10 billion euros and reaching about 15% of the population. In 2022, the average monthly transfer was 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 *Earned Income Tax Credit* (EITC) in the U.S or the *Working Tax Credit* (WTC) in the U.K.

Eligibility. Eligibility status and the amount of the transfer depend on a household's total resources, individual labor income, and family composition.[21] The benefit is calculated based on labor income earned in the previous quarter. The program includes both a phase-in and a phase-out region (see Figure 2.1). Unlike many in-work transfer programs, it is not structured as a tax credit. Instead, it is administered by the national welfare agency, the Caisse Nationale des Allocations Familiales (CNAF). While it is formally a family benefit—means-tested on total household income—it also includes "individual bonuses" that are based solely on personal labor income. Further details on the benefit formula and schedule can be found in Appendix A.

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

[21] Self-employed individuals and public-sector employees are also eligible for the program and are subject to the same eligibility rules as private-sector employees.

Program's take-up & barriers to participation. Take-up of the program is incomplete and was estimated at 73% in 2016 (DREES 2017). Previous research has documented several factors that may explain why some eligible families do not claim a given benefit, including administrative burdens, stigma, and information frictions. Below, I discuss each of these aspects in the context of the in-work transfer program.

Overall, the in-work benefit can be considered a relatively "low-burden" program. The application process is conducted online and typically takes around twenty minutes.[22] Applicants must provide their social security number, bank account information, current family situation (e.g. number and age of children, marital status), past household income, and individual labor income over the past three months. Importantly, no documentation is required at the time of application.[23] While the initial claiming process is relatively straightforward, beneficiaries must re-certify their eligibility every three months in order to remain enrolled. This frequent re-certification allows the administration to adjust benefit amounts in response to changes in household circumstances, but it also imposes a recurring burden on claimants.

Stigma-related barriers are generally considered to be relatively limited in the context of in-work transfers, which are less likely to be associated with negative feelings of being "assisted" compared to traditional safety net programs. A qualitative study by Kula et al. (2020) found that stigma was infrequently cited as a reason for non-take-up in the context of in-work transfers, as opposed to other welfare programs, when individuals who had previously been eligible but did not claim the benefit were asked about reasons for non take-up. Furthermore, the online application process ensures anonymity, which may further reduce the potential for external stigma.

Finally, information or behavioral frictions may affect take-up. In 2018, 25% of the adult population reported having never heard of the program (DREES 2000). Low awareness levels are also observed among low-income individuals—those most likely to be eligible. Even among those who are aware of the program, understanding of the eligibility criteria and benefit formula is frequently self-reported as limited.

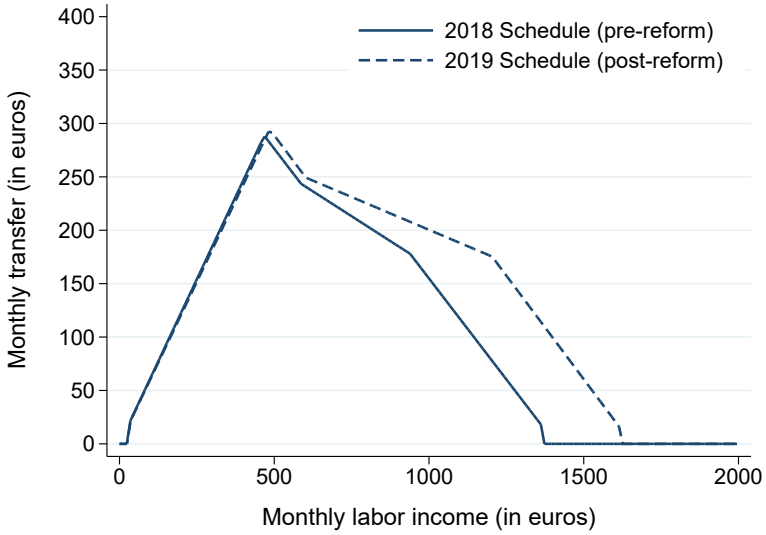
The 2019 welfare reform. Following the yellow vests protests that started in November 2018 in France, the government announced an unexpected set of emergency responses to the crisis on December 10th 2018. The leading measure was a reform of the in-work benefit.[24] The reform changed the transfer's schedule to increase its generosity in specific range of the schedule. Figure 2.1 represents the change in benefit schedule for a single person without kids and with only wage income. 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)

[22] Claimants can, if they wish, visit a local welfare agency office to receive assistance from a caseworker, or use a paper form submitted by regular mail.

[23] The welfare administration verifies parts of the declarations ex post, using data from the tax authority and conducting occasional audits.

[24] More details about the program and the political context leading to the reform can be found in Appendix A.

Figure 2.1: Changes in the in-work transfer's schedule (2018–2019)



Notes: The figure represents the in-work transfer (*prime d'activité*) 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.

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 household members' labor income and overall family composition.

The reform generated changes in several other dimensions, beyond the increase in the transfer's generosity. First, it led to an expansion in the number of families eligible for the program by pushing up the income eligibility threshold. Figure 2.1 shows that the eligibility expansion affects households in the upper range of income. In the case of a single wage-earner individual, those who become eligible are those with monthly earnings in the 1,400-1,600 euros range, roughly. Second, the reform also changed the work incentives faced by beneficiaries, by reducing the participation tax at most income levels[25] and by changing the marginal tax rate in some earnings range. Finally, we note that the reform was very salient as it attracted an unusually large media coverage due to the specific yellow vests context. The media coverage may have, indirectly, contributed to putting the program in the spotlight. In other words, the reform could have generated a salience or information shock. While this aspect is speculative at this stage, Section 3 will provide detailed descriptive evidence regarding this aspect of the reform.

In summary, the 2019 reform may have prompted individuals to take up the in-work transfer program through two main mechanisms: (i) an increase in the monetary incentives to claim (due to the increase in program's generosity) and (ii) a possible indirect positive information (or salience) shock caused by the media coverage of the reform. The empirical analysis will use this quasi-experimental framework to

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

assess the causal effect of these two types of interventions on take-up behaviors. The analysis will also account for the fact that the number of eligible families could have also increased due to the eligibility expansion and the change in work incentives.

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 cash transfer programs, namely family, housing and social benefits (including the in-work transfer 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.

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[26] 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 in-work benefit program at least once between January 1st, 2017 and January 1st, 2020. By definition, the data only sample families that are eligible for some transfers and made a claim. It has two important implications. First, my sample does not contain eligible families who never claimed the in-work benefit. Second, even for those who did take up at least once, there is uncertainty about their situation whenever they disappear 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. Subsection 4.1 explains how I deal with these empirical challenges of endogenous sample selection to study take-up behaviors. Importantly, I drop families who entered the program after the 2019 welfare reform due to the eligibility expansion from my sample.

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 in-work benefit as well as the amount of benefits received monthly by each family. I define the dummy variable of being a program 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 program for this month. I will also use the precise date of the claim in the empirical analysis. While the dataset contains monthly information, I have access to the day date (JJ/MM/YYYY) when a claim for the program was made.

Descriptive Statistics Every month, around 15 million families (i.e., half of all French families) receive some transfers from the social administration. Table 2.B1 describes the characteristics of these families, as well as the characteristics of the subsample of those enrolled in the in-work benefit program. Among

[26] 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.

the families benefiting 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.

Other data sources In Appendix C, I describe other data sources used throughout the analysis, especially to document the nature of the information shock created by the reform. The first extra source I use is a General Population Opinion Survey (*Baromètre d'Opinion de la DREES*) which measures self-reported knowledge of various programs in France for a representative sample of the population. The second source I use is the Europress database which provides me with a corpus of news articles from 905 French sources in France (national press, regional press, and several online media). The third source I use is the National Audiovisual Institute (INA) data which gathers all TV and radio programs' descriptions. It allows me to identify descriptions that mention the name of the welfare program and their date of broadcast. I these two sources to produce quantitative measures of the media coverage of the program throughout time. Last, I use the Google Trends database to produce a measure of a proxy for interest in the program throughout time.

3 Descriptive Evidence

Before turning to the causal analysis, this section documents several important facts using aggregate descriptive statistics. It first illustrates the sharp increase in participation in the French in-work benefit program following the 2019 welfare reform.

3.1 Evidence regarding the program's enrollment boost after the reform

Prior to the reform, approximately 2.7 million families were enrolled in the program.[27] One month after the reform, total enrollment rose to about 3.6 million families (Figure 2.2(a)). This surge continued throughout the year, with the number of enrolled families reaching nearly 4 million by the end of 2019. This sharp increase around the time of the reform stands in stark contrast to the relative stability observed before and after that period.[28] When looking at the flow of enrollment rather than the stock, we observe that roughly 1 million applications were submitted in January 2019 – five times the pre-reform monthly average of around 200,000 (Figure 2.2(b)).

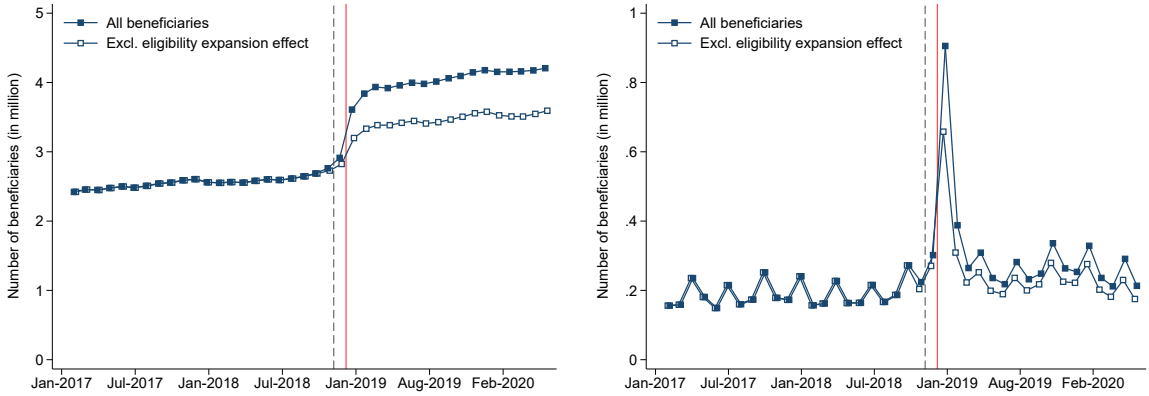
A natural explanation for this surge in enrollment is the eligibility expansion induced by the reform. To test this hypothesis, I use micro-simulation methods to identify families who became "newly eligible" solely due to the reform – i.e., those eligible under the post-reform rules but not under a counterfactual regime applying pre-reform rules.[29] In both panels of Figure 2.2, I isolate the enrollment dynamics after excluding these newly eligible families. Of the 900,000 new families enrolling in January 2019, less than

[27] This corresponds to over 5 million individuals residing in beneficiary households. For comparison, France's total population was estimated at 66.9 million in 2018 (Insee).

[28] Although aggregate enrollment numbers tend to be stable month-to-month, underlying flows are sizable. In 2018, the average monthly entry rate was about 7%, and the exit rate was approximately 6%.

[29] The detailed methodology used to identify these households is described in Subsection 4.1.

Figure 2.2: In-work transfer's aggregate 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 ("prime d'activité" or 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 the reform's 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, 2017/02-2020/03.

one-third can be attributed to the mechanical effect of the eligibility expansion. After netting out this effect, enrollment rises from 2.7 to 3.2 million families in the month following the reform. The remainder of the empirical analysis will focus on the subsample of families unaffected by the eligibility expansion.

Other factors beyond the mechanical eligibility expansion may account for the observed increase in enrollment. First, holding the take-up rate constant, enrollment would rise if the number of eligible families increased. Examining the labor market status of new entrants prior to enrollment. I find that a large share of them had been stably employed for at least three consecutive months prior to joining the program. Since earning labor income is a core eligibility requirement, this pattern is inconsistent with the hypothesis that the surge in enrollment is primarily driven by individuals becoming newly eligible due to recent labor market entry. Instead, the evidence points toward a behavioral response in take-up, holding the number of families eligible constant. In other words, families who had long been eligible may have been encouraged to enroll following the reform.

Using daily claims data, I show that the enrollment surge started immediately after the December 10th announcement. Figure 2.B1a in Appendix B shows a spike in new claims the day after the announcement. After adjusting for usual seasonality effects, we observe even more clearly that daily claims were stable before the reform but jumped sharply right after the announcement, peaking at the end of January before returning to normal after three months (see Figure 2.B1b). This reveals an "announcement effect" not visible in the monthly data. The modest enrollment increase observed in December could be attributed to administrative delays which make new claimants only enroll in January.

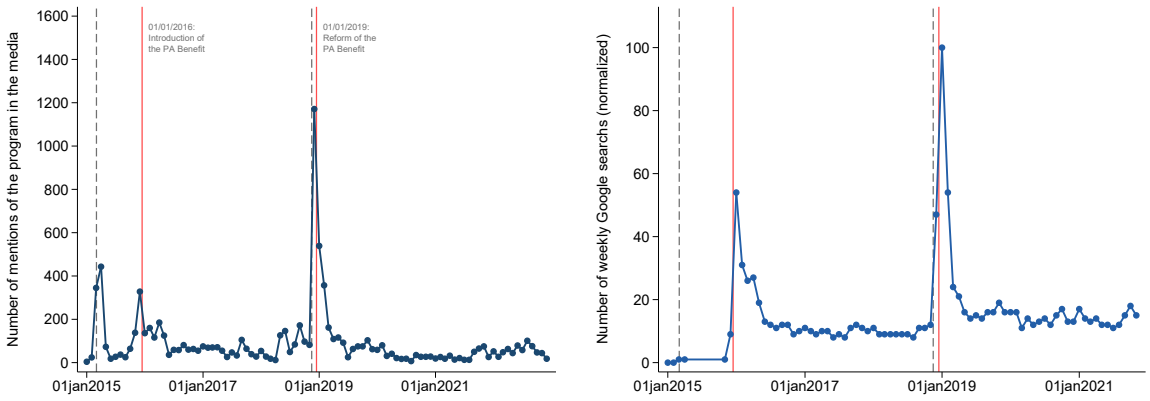
3.2 Evidence regarding the information shock

Using various extra data sources, in particular media corpus data and survey data, I document four stylized facts to help understand how the reform may have indirectly acted as an information shock, besides its direct effect on the generosity of the program.[30]

Fact 1: Media coverage of the program surged at the time of the reform

Media coverage of the in-work benefit program in France increased significantly after the reform's announcement. Figure 2.3a shows that mentions of the program in the press, radio, and TV peaked at the time of the reform. The magnitude of this media attention shift is substantial. To illustrate, we compare the media coverage of the 2019 welfare reform with that of the previous in-work transfer reform. In 2016, two schemes were merged to create the current in-work transfer, which also increased the overall generosity of the transfer. We find that the number of mentions around this earlier reform (announced in March 2015 and introduced in January 2016) was more than twice smaller than for the 2019 reform. Qualitative analysis of press coverage reveals that imperfect take-up was frequently mentioned, with narratives highlighting the large number of families potentially missing out and providing practical information on how to apply. This suggests that the media coverage was not only about the reform of the program but also help put the program in the spotlight.

Figure 2.3: Media coverage and Internet search requests (2015–2022)



(a) Media coverage of the program **(b)** Google searches of the program

Notes: Panel (a) represents the total number of monthly occurrences of the in-work benefit keyword in the entire press corpus covered by the Europresse and in the TV and radio shows description corpus compiled by the INA Lab. The outcome is a count variable of the number of keyword occurrences for the name of the program in the title or the introduction of the press articles or in the TV/radio show's descriptions.
Panel (b) represents the number of Google searches associated with the program's name. Search numbers are normalized to 100 at the date with the highest number of research for the topic in the region and period considered.
In each graph, the dashed gray lines represent reforms' announcement date and the solid red lines represent the reforms' implementation date.
Sources: Europresse database, French press, 2014–2022. INA Lab media data, 2014 – 2022. Monthly Google Trends.

[30] Details about the extra data sources used can be found in Appendix C.

Fact 2: Interest in the program surged at the time of the reform

To assess whether the significant media coverage of the program shifted individuals' attention, I use Google searches of the program's name as a proxy for public interest. Figure 2.3b shows that the highest level of search interest over the 2015–2022 period occurred at the time of the reform. Internet search patterns closely follow media coverage trends, though with a slight delay. In particular, mentions of the in-work benefit program peak in December 2018, while Google searches peak in January 2019. Similar delays between media attention and search behavior are observed around the previous reform. This pattern suggests that media coverage was effective in shifting individual attention.

Fact 3: Awareness of the program was limited pre-reform

Because of information frictions, some individuals may not be aware that a given welfare program exists or may fail to understand their eligibility. Using General Population Survey data, Figure 2.4a presents descriptive statistics on knowledge of various welfare programs in 2018. In the adult population, 76% reported being aware of the in-work benefit program. 65% had intermediate knowledge (i.e., were aware and had some idea of who can benefit), and 29% had advanced knowledge (i.e., were aware and had a good idea of who can benefit). The program displays a notably low awareness rate compared to other major programs such as family benefits, housing benefits, and the safety net (RSA). A possible explanation is that the program was relatively new at the time.[31] Notably, imperfect awareness is relatively homogeneous across the population.[32] Single parents and women show slightly higher awareness, while young adults (18–25) are much less aware than others. Awareness and knowledge are not higher among low-income individuals, even though they are more likely to be eligible.[33]

Fact 4: Program's awareness improved post-reform

Self-reported awareness and knowledge of the program significantly increased after the reform. Figure 2.4b plots coefficients from an event-study regression of program knowledge indicators on yearly dummies, using 2018 as the reference period. The share of individuals declaring awareness of the program increased by nearly 5 percentage points in 2020 relative to 2018. Similar lasting improvements are observed for other proxies of program knowledge (see Figure 2.B3 of Appendix C). Notably, these perception shifts occurred rapidly, within weeks of the reform. Using an alternative survey dataset[34], I show that awareness rates increased permanently as early as March 2019. Lastly, improved awareness and understanding appear widespread, affecting both groups likely eligible for the program and those

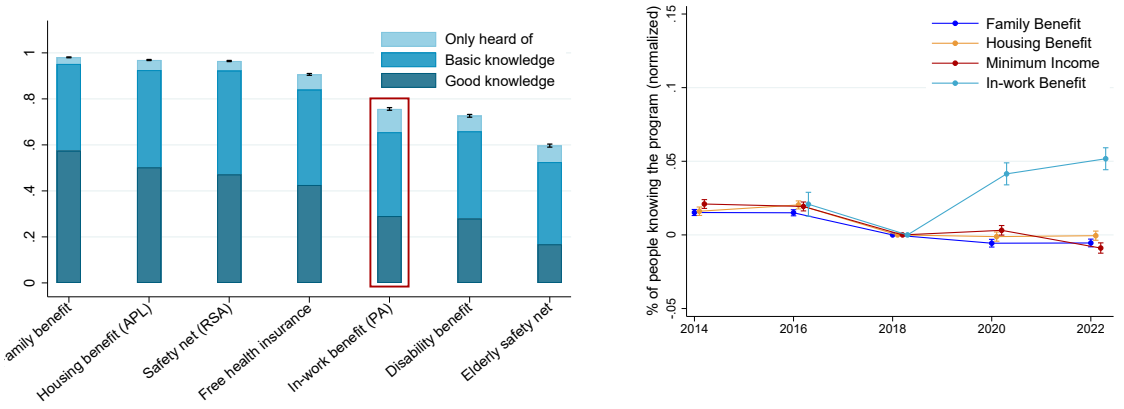
[31] It was introduced in 2016. By contrast, the safety net program dates to 2008 in its current version, and housing and family benefits have existed for decades.

[32] Results are from Table 2.B4 of Appendix B. Table 2.B5 and Table 2.B6 show similar statistics for intermediate and good knowledge measures.

[33] However, the survey lacks sufficient information to reliably determine respondents' eligibility status, so awareness among eligible individuals cannot be estimated.

[34] This dataset is the DJEPVA Youth Survey (*Baromètre DJEPVA sur la jeunesse*), which collects yearly data on self-reported awareness of transfer programs among young adults (18–30 years old), as opposed to every two years in the General Population Survey.

Figure 2.4: Self-reported knowledge level about welfare programs



(a) Knowledge levels (2018)

(b) Knowledge's evolution (2014 – 2022)

Notes: Panel (a) represents the average share of individuals with (i) awareness of the program, (ii) intermediate knowledge and (iii) advanced knowledge of the in-work benefit program in France. It also features the same statistics for other welfare programs: *Revenu de Solidarité Active* (safety net), *Aides au Logement* (housing benefits), *Prestations familiales* (family benefits), *Couverture maladie universelle complémentaire* (free health insurance), *Allocation de Solidarité aux Personnes Agées* (Old-age safety net) and *Allocation Adulte Handicapé* (Disability Benefit).
Panel (b) represents the event-studies coefficients of a regression of the share of individuals with self-reported knowledge of a given program on yearly dummies (excluding year 2018). The outcome variable is a dummy equal to one if the respondent answers "Yes" to the question "Have you ever heard about [program's name]?".
All figures are produced using the sampling weights provided by the data producers.
Sources: General Population Survey (*Baromètre d'opinion*), DREES, 2014–2022. Sample of 18-65 years old individuals.

who are not. Indeed, effects are homogeneous across income and family composition, key determinants of eligibility.[35]

We note that pre-reform, knowledge of the in-work transfer program was evolving in parallel way to other transfer programs' knowledge. After the reform, however, we observe a sharp improvement specific to this program. While this evidence is not causal, it suggests that the rise in knowledge is indeed linked to the 2019 welfare reform. Moreover, the magnitude is substantial: a 5 percentage point increase in awareness among the 18–65-year-old population implies that around 2 million individuals (or approximately 3 million households) learned about the program post-reform. This increase far exceeds the number of families newly eligible due to the eligibility expansion and thus cannot be attributed to this mechanical effect of the reform.

Summary. Using several sources, including survey and media corpus data, I have documented four key facts. First, the reform triggered substantial media coverage, which increased the supply of information about the program. Second, this media exposure appears to have generated interest in the program, as suggested by Google search trends. Third, knowledge of the program was limited prior to the reform. Fourth, a significant improvement in awareness and knowledge of the in-work transfer program followed the surge in media coverage. These findings suggest that, beyond its direct effect on program generosity, the reform also acted as an information shock. By alleviating pre-existing information frictions, this

[35] See Table 2.B7 of AppendixC.

shock may have triggered positive take-up responses. The empirical strategy will test this hypothesis.

4 Methodology

This section presents the empirical strategy used to estimate the causal effect of the 2019 welfare reform on take-up behaviors.

4.1 Empirical challenge: measuring program take-up

A longstanding challenge in empirical research on welfare program participation is the accurate measurement of program take-up (Goedemé and Janssens 2020), which cannot be directly observed. In this paper, I propose a novel approach that circumvents this measurement issue and allows for the identification of the treatment effect of an intervention on take-up outcomes.

Conceptual Framework To understand the empirical challenge, I start by outlining a simple framework that clarifies the definition of three key concepts - take-up, eligibility and enrollment - and the object of interest for our analysis. Consider a welfare program represented by a mapping $g(.)$ that links individual-level characteristics X (e.g., family composition, earnings, work history, place of residence) to a continuous benefit entitlement $\tilde{b}$. Agents must claim the benefit in order to receive it. The amount of benefit they receive is denoted by b. An agent is said to be *enrolled* into (or to benefit from) the program when $b > 0$, and to be *eligible* for the program when $\tilde{b} > 0$. Take-up is defined as a binary decision p made by the agent. An agent is said to *take up* the program if they are eligible for $(\tilde{b} > 0)$ and claimed the benefit $(b > 0)$. Conversely, an agent does *not take up* the program if they are eligible for $(\tilde{b} > 0)$ but did not claim the benefit $(b = 0)$. When agents are not eligible for the program, the take-up decision is undefined.[36] At the aggregate level, considering a population of size N, I will denote by B the total number of beneficiaries (or enrollees) and by E the total number of eligible. The take-up rate in the overall population is $\bar{p} = \mathbb{E}[\mathbb{1}_{b>0}|\mathbb{1}_{\tilde{b}>0}]$, i.e. the share of eligible individuals who do claim the benefit.

	Not enrolled $b = 0$	Enrolled $b > 0$
Not eligible $\tilde{b} = 0$	Not relevant $p = .$	Fraud/β-error $p = .$
Eligible $\tilde{b} > 0$	Non take-up $p = 0$	Take-up $p = 1$

Table 2.1: Characterization of Take-Up, Eligibility and Enrollment Status

The analysis is interested in studying the effect of a given factor T on the take-up of the program. To simplify, we can think of T as a policy intervention and the object of interest as the average treatment

[36] A non-eligible agent $(\tilde{b} = 0)$ can receive $b > 0$. This can reflect either an administrative error or a measurement error in $\tilde{b}$ or b made by the analyst. Measurement errors are often referred to as "β-errors" in the literature. Another type of error/fraud can occur when $\tilde{b} \neq b$ while both are positive.

effect of this intervention on take-up ($d\log(\bar{p})/dT$). While it is possible to observe who is enrolled in a program in data, take-up is an unobserved outcome.

Identification of take-up responses – Existing approaches A first type of studies relies on *survey data* to infer program take-up behaviors at the individual level and, in turn, estimate take-up responses. This approach relies on data where relevant characteristics X and benefit receipt b are observed. It requires to first simulate eligibility = using observed characteristics X ($\tilde{b} \equiv g(X)$) and then compare simulated eligibility $\tilde{b}$ to observed benefit receipt b to identify take-up status (see Table 2.1). The survey data approach allows to identify take-up responses as well as to estimate the aggregate take-up rate $\bar{p}$. However, survey data are prone to issues, such as measurement error[37], missing data[38] or sample selection bias, leading to frequent misclassification of take-up status and thus estimation bias.

More recently, a second type of studies has used experimental set-ups, mostly randomized controlled trials, where the analyst tests the effects of a treatment provided to a subpopulation of individuals that are *ex ante* assessed as likely eligible for the program. These studies use *administrative data* – data used to operate the program by the government – which typically do not suffer from measurement error or missing data (as X and b are perfectly observed). As the experiment takes place among individuals eligible for the program, enrollment and take-up outcomes are identical in this subpopulation (see Table 2.1). Using admin data, these studies can thus estimate the average treatment effect on enrollment b which is equivalent to the average treatment effect on take-up p.[39]

Identification of take-up responses with admin data and quasi-experimental set-up This paper aims to study the effect of the 2019 reform of the French in-work benefit on take-up behaviors. As it relies on a quasi-experimental setting rather than a controlled experiment, it is not possible to restrict the analysis to a subsample of eligible individuals where enrollment and take-up become identical outcomes (as the standard approach described above does). Still, I show that, under some additional assumptions, it is possible to identify take-up responses using administrative data and a quasi-experiemental set-up.

Proposition 1. *If* $p(b > 0 \mid \tilde{b} = 0) = 0$*, then:*

$$\frac{d\log(\bar{p})}{dT} = \frac{d\log(B)}{dT} - \frac{d\log(E)}{dT}$$

Corollary 1. *If* $p(b > 0 \mid \tilde{b} = 0) = 0$ *and* $d\log(E)/dT = 0$*, then* $\frac{d\log(\bar{p})}{dT} = \frac{d\log(B)}{dT}$

Proposition 1 shows that the effect of a treatment on the aggregate take-up rate is equal to the treatment

[37] Measurement error in survey data can occur regarding both the characteristics X (e.g. income, family status) and benefit receipt b, when such variables are self-reported. In particular, several studies have documented significant under-reporting of program enrollment in survey data (Meyer et al. 2015; Bitler et al. 2003)

[38] Eligibility for welfare programs is often assessed by program administrators using multiple characteristics X. Most general population survey, that have not been designed directly to measure take-up, face an issue of missing data, i.e. only a subset of X is present in these survey data. To simulate eligibility, studies thus often rely on imputation of X leading to errors in the measurement of eligibility $\tilde{b}$ and therefore of take-up.

[39] However, experimental studies relying on administrative data cannot estimate aggregate take-up rates due to sample selection issue (those identified as eligible for the program are often not representative of the overall population of eligible).

effect on the number of individuals enrolled (B) minus the treatment effect on the number of individuals eligible for the program (E). This relationship holds true assuming away fraud or overpayment in the welfare program. Therefore, in a quasi-experimental setting and when using administrative data where enrollment is perfectly observed, identification of take-up treatment effect can be achieved, under the key identification assumption that the treatment has no effect on the number of individuals eligible for the program (see Corollary 1).

Identification Threat # 1: Eligibility Expansion The welfare reform changed the eligibility formula – i.e. $g(.)$ – leading to higher-income families becoming eligible for the program (see Figure 2.1 of Subsection 2.1). Enrollment may increase as a result of this increase in the number of families eligible, even if $\bar{p}$ remains unchanged. In other words, the eligibility expansion violates the identification assumption. For the whole analysis, I will thus exclude these newly eligible families from the sample of interest. To do so, I micro-simulate each family's eligibility under both the reformed and pre-reform rules. Among families enrolled, I remove families who are found to be eligible under the post-reform rules but not under the pre-reform rules.[40]

Identification Threat # 2: Eligibility Responses A second potential threat arises if families adjust their behavior to become eligible for the program – i.e. if the treatment has an effect on X. In particular, since the program generated a decrease in the participation tax at some income range, this could have encouraged extensive-margin labor supply responses. Individuals who were previously not working and not eligible for the program may respond by starting to work and thus becoming eligible for the program.[41] Yet, eligibility is determined based on *past* characteristics (e.g. eligibility to the program in a given month is based on the earnings of the last three months).[42] Since the reform was unanticipated and because of this delay in the eligibility criteria, any behavioral responses in terms of characteristics X will only affect eligibility $\tilde{b} = g(X)$ in the next period. Therefore, in the very short run, the identification assumption holds true. While the analysis will estimate the unfolding effects of the reform, I will thus focus on responses estimated in the first month after the reform (i.e. January 2019) when discussing the results.

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

[40] To be more precise, if a family is labelled as "newly eligible" in some period but not in other, I will keep it in the sample for the period where it is not.

[41] Similarly, for working indiviudals whose income are close to the eligibility threshold, the reform may have generated negative intensive-margin labor supply responses that also could increase the number of eligible.

[42] Labor market frictions further reduce the likelihood of immediate income adjustments. In contrast, take-up decisions can be almost instantaneous as they only involve undergoing a short online application process.

to have a balanced panel. Observations will be weighted by the total population in the local area.

4.2 Identification strategy

To identify the effect of the reform on program take-up behaviors, the empirical strategy will use a difference-in-differences approach. In this section, I present separately the strategies used to identify the effect of each of the reform's mechanisms; (i) the increase in monetary incentives to claim and (ii) the information shock.

4.2.1 Identification of the change in program generosity

Exposure to the increase in the program's generosity induced by the reform was heterogeneous across households. A natural strategy to identify its causal impact is to compare the evolution of program enrollment between families who experienced a change in benefit amount and those who did not. I define exposure as the difference between the benefit a family is eligible for under the *actual* system and under a *counterfactual* system without the reform, given their pre-reform characteristics. For each entrant, I micro-simulate both amounts using their declared characteristics. Treated families are those for whom the simulated amounts differ at the time of entry.[43]

Estimation strategy. A standard difference-in-differences strategy relies on the *parallel trends* assumption: in the absence of the reform, outcomes for treated and control families would evolve similarly over time. However, treated families exhibit different seasonal patterns than controls (see Figure 2.B6 of Appendix B). This is plausible, as treated families are on average richer (see Table2.B2) and may thus face different seasonal labor market dynamics. Notably, enrollment among control families drops more sharply each January (and July) compared to treated families. Since this differential pattern appears stable over time, I can explicitly control for it.

Using a de-trended difference-in-differences strategy, the analysis compares instead 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.[44] Therefore, it relies on a *relative parallel trend* assumption: the excess enrollment should trend the same across the two groups.

$$\log(Y_{jgkt}) = \sum_{\substack{d=-5 \\ d\neq -1}}^{d=6} \beta_d \times \mathbb{1}_{\{m=d\}} \times \mathbb{1}_{\{k=1\}} \times T_g + \lambda_{gk} + \lambda_{gt} + \lambda_{kt} + \mu_j + \varepsilon_{jgkt} \tag{2.1}$$

Equation 2.1 describes the estimated regression. Y_{jgkt} is the outcome variable (enrollment stock) in local area j, treatment group g, event time t and event k. Observations for the period 2017/07 – 2018/06 are considered as belonging to the event-time $t = 0$ (i.e. 12-month period around January 2018 when no

[43] In the preferred specification, treated families are those facing a benefit increase of more than 15 euros. Note that if total benefits are below 15 euros, they are not paid. Since the reform generates few "pure" control families, expanding the control group improves precision in estimation.

[44] 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.

reform took place). Observations for the period 2018/07 – 2019/06 are considered as belonging to the event-time $t = 1$ (i.e. 12-month period around January 2019 when the reform took place). 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 exposure to the benefit increase on take-up, under several identification assumptions. First, the relative parallel trends assumption must hold: excess enrollment would have followed similar trends across treated and control groups absent the reform. The event-study coefficients β_d are shown for all periods, including pre-reform, to better assess the likelihood that this assumption holds. Second, as discussed above, I must assume that take-up, not eligibility, is driving the change in claiming behavior. In the short run, due to eligibility rules, an eligibility response can be ruled out. Moreover, the difference-in-differences strategy controls for any exogenous change in eligibility unrelated to reform exposure. Third, to attribute the effect solely to the benefit increase, treated and control groups must be similarly affected by other mechanisms. Evidence presented in Subsubsection 4.2 show no correlation between income or family composition and self-reported changes in awareness after the reform. This suggests that indeed exposure to the increase in program generosity is orthogonal to exposure to the information shock.

4.2.2 Identification of the effects of the information shock

Exposure to the "information shock" described in Subsubsection 4.2 is not directly observable in our data.[45] Moreover, evidence suggests that the increase in awareness and knowledge was relatively uniform across the population. I thus define exposure to the information shock as follows: all families eligible for the in-work transfer program are considered exposed to the information shock and form the treatment group. Given that the media coverage and the associated alleviation of information frictions were specific to this program, I define families eligible for another welfare program (the safety net program or RSA) as not exposed. These families constitute the control group.

Estimation strategy. The analysis relies on the exact same de-trended difference-in-differences regression as described in Equation 2.1. The only difference is in the sample of interest and the definition of the groups g. The sample now includes all families ever enrolled in either the in-work transfer program or the safety net program. The same identification assumptions as before need to hold for the β_k coefficients to identify the causal effect of the information shock on take-up behavior. To ensure that the treatment effect captures the effect of an increase in the media coverage and not the direct effect of the reform which increased the program's generosity, I conduct the analysis by restricting the treated group to families eligible for the in-work transfer program but not affected by the increase in program's generosity (i.e. those that formed the control group in the previous diff-in-diffs).

[45] Unfortunately, we cannot link the General Population Survey data – which includes self-reported knowledge and awareness of the program – with our main data source, the administrative social records.

Table 2.2: Estimation strategy – summary

	Diff-in-diff # 1	**Diff-in-diff # 2**
Nature of treatment effect identified	Monetary incentives	Information supply
Variation exploited	Variation in transfer amount	Variation in media coverage of program
Treatment group	(G1) Eligible for in-work transfer program facing a benefit increase	(G2) Eligible for in-work transfer program facing no benefit increase
Control group	(G2) Eligible for in-work transfer program facing no benefit increase	(G3) Eligible for safety net program

Descriptive statistics. In summary, the analysis will compare enrollment behaviors across three groups; families eligible for the in-work transfer and affected by a benefit increase (G1), those eligible for the same program but not affected by the benefit increase (G3) and families eligible for the safety net program which was unaffected by the reform (G3). Table 2.B1 presents descriptive statistics for each group.

5 Empirical Results

This section presents the results of the empirical analysis estimating how take-up changed after the reform and identifying the mechanisms beyond this variation.

5.1 Take-up responses to variation in program's generosity

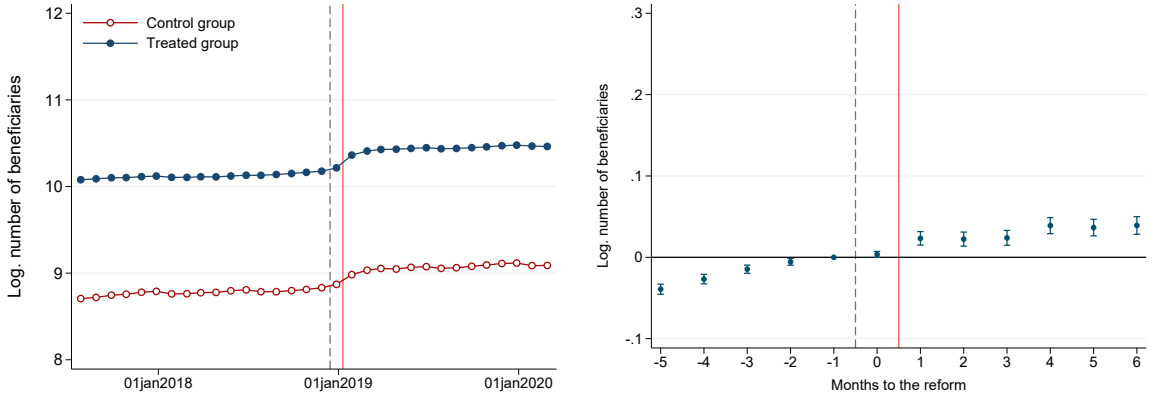
To identify the role of the transfer increase on take-up behavior, I use a difference-in-differences strategy comparing families exposed to the benefit increase to those not exposed. Figure 2.5(a) shows the evolution of the raw average of the outcome of interest – the log number of families enrolled in a given month. Enrollment among treated and control families followed parallel trends prior to the reform. Interestingly, the figure also shows that both groups experienced an increase in enrollment following the reform.

Turning to the estimation, Figure 2.5(b) presents the estimated coefficients from the de-trended difference-in-differences regression. The causal effect of the benefit increase on enrollment is not statistically significant. Pre-trends are mildly significant and suggest a linear upward trend in excess enrollment among the treated relative to the control group. If anything, this implies that the estimated effects likely constitute an upper bound of the true treatment effect. The effect is economically small: the estimated increase in enrollment is only 1.8% in January 2019. Scaling this up shows that—even if statistically significant—the effect would account for less than 10% of the aggregate enrollment surge. To derive an elasticity of take-up with respect to the benefit amount, I estimate the treatment effect on the log transfer amount.[46] On average, treated families experienced a 36% increase in the transfer amount. This yields an estimated elasticity of the take-up rate of about 0.05: a 1% increase in the benefit amount leads to a 0.05% increase in the number of families taking up the program.

[46] See Figure 2.B5 of Appendix B.

Given that the cost of claiming remained stable around the reform, the results indicate that increasing monetary incentives had no effect on take-up. In other words, for the marginal entrant, non take-up does not appear to reflect a rational cost-benefit decision.

Figure 2.5: 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 Subsection 4.2. 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.

5.2 Take-up responses to variation in media coverage of the program

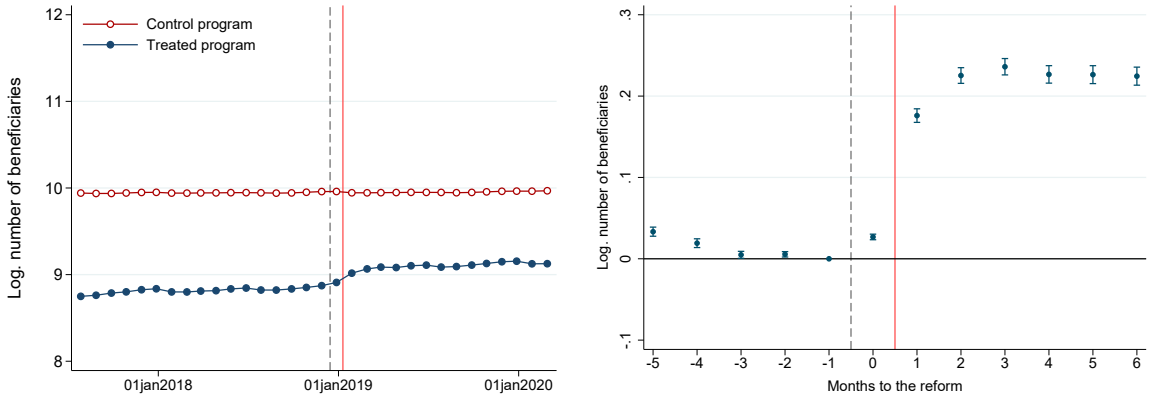
To identify the effect of the information shock generated by the large media coverage of the program at the time of the reform, I use a diff-in-diffs strategy comparing families enrolling into the in-work transfer program (treated program) and families enrolling into another program unaffected by the information shock (control program). I restrict the treated group to families enrolling into the in-work transfer program but facing no changes in transfer amount. Figure 2.6(a) represents the raw average evolution of enrollment in these groups. Enrollment in the safety net program is very constant over time, as there is, on average, less entry and less exit in a given month than for the in-work transfer program.

Figure 2.6b presents the coefficients of the de-trended difference-in-differences regression. While enrollment in both programs was trending in a relatively parallel manner prior to the reform, enrollment in the in-work transfer program surged by 18% in the month following the reform.[47] Examining the effects on entry flows rather than enrollment stocks reveals that the increase in take-up was much larger among first-time entrants than among past beneficiaries.[48] This is consistent with an information provision mechanism that alleviates frictions such as lack of awareness or misperceptions about eligibility.

[47] Importantly, the magnitude of the treatment effect is very similar when defining the treated group as families eligible for the in-work transfer *and* affected by the change in the program's generosity (see Figure 2.B7 of Appendix B). This provides additional evidence that the benefit increase mechanism did not generate significant take-up responses.
[48] See Figure 2.B10 of Appendix B.

It rejects the hypothesis that the reform merely acted as a salience shock reminding well-informed individuals to claim. We also observe a smaller, but visible, enrollment response in December, which is plausible given that the reform was announced on December 11th. This timing of the response suggest that individuals are responding the the media coverage of the reform, which started on December 11th, rather than to other aspects of the reform, which only occurred on January 1st.

Figure 2.6: Effect of the information supply 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 families eligible for the in-work transfer program and not affected by the benefit increase (treated program) and families eligible for the safety net (control program) separately. Panel (b) plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Subsection 4.2. 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 and who are not exposed to the change in program generosity (i.e. face a benefit increase of less than 15 euros). I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Other mechanisms than information supply could contribute to the observed treatment effect. One possibility is that the media coverage surrounding the program changed the public narratives, thereby reducing feelings of stigma. Figure 2.B9 replicates the difference-in-differences estimation on two subsamples: areas with a low share of beneficiaries in the population pre-reform, and areas with a high share. Arguably, the former are locations where external stigma (i.e., social image concerns) is more likely to exert a negative effect on take-up compared to the latter. Estimated take-up responses are slightly larger in high-stigma areas (+20%) than in low-stigma areas (+17%). However, the difference in magnitude is minimal, casting doubt on the hypothesis that the information supply shock substantially reduced stigma. The primary mechanism appears to be a pure information shock that increased the availability of information about the program without fundamentally altering the narrative surrounding it. Moreover, welfare stigma is typically considered a second-order barrier to take-up in the context of in-work transfer programs, which are targeted at individuals who are employed.

Overall, the results indicate that the information shock had a substantial effect on the aggregate number of beneficiaries. They suggest that nearly half a million families began claiming the in-work benefit in the

month following the reform, as a direct consequence of reduced information frictions. Assuming a pre-reform take-up rate of 73% (DREES 2017), a back-of-the-envelope calculation shows that the observed 18% increase in enrollment nearly corresponds to a cut by half of the non-take-up rate. [49]

5.3 Targeting effects

Finally, I investigate how the reform affected the characteristics of the marginal enrollee. To do so, I use a static before/after version of the difference-in-differences strategy and examine enrollees' average characteristics as outcomes. Table 2.B3 displays the coefficients of these regressions across several outcomes, estimated separately for the "treated" group (those experiencing a benefit increase) and the "control" group (those not affected by the benefit change).

On average, both groups exhibit broadly similar trends. On one hand, new entrants post-reform tend to have lower incomes than what would have been the case absent the reform, suggesting that the information shock encouraged take-up, especially among the most financially vulnerable households. On the other hand, these new enrollees are also more likely to belong to socio-demographic groups traditionally perceived as being in relatively more favorable economic positions, such as non-foreigners, single males, and those without prior exposure to the safety net. Interestingly, these groups often have limited interaction with welfare institutions, either because they are eligible for fewer programs (e.g., not eligible for family benefits) or because they are more socially or institutionally marginalized. Notably, nearly half of the new entrants had no prior contact with welfare agencies. This pattern contrasts with findings from the U.S. literature, where studies of the EITC generally report strong effects of information outreach among existing tax filers but little response from non-filers. In contrast, the large media coverage of the French in-work transfer appears to have successfully reached and mobilized even the harder-to-reach populations.

6 Welfare & Policy Implications

The causal empirical analysis has documented how an increase in media coverage, after the welfare reform led to a substantial take-up increase. What broader lessons can be learned from this event? In this section, I first discuss the policy implications of my empirical results, also reflecting on possible external validity concerns. Then, I develop a general theoretical framework to assess the welfare effects of interventions that encourage take-up behaviors. I use it to estimate the welfare effects of the French reform and discuss more general normative implications for such policies.

[49] Before the reform, there were approximately 2.7 million beneficiaries. With a 27% non-take-up rate, this implies about 3.7 million individuals were eligible, of whom 1 million were not claiming. An additional 490,000 entrants due to the reform would thus reduce the pool of non-claimants by nearly half.

6.1 Discussion & Policy Implications

Two key results emerged from the empirical analysis. First, the increase in the transfer amount had very little effects on families' decision to claim the transfer. This result is at odds with the neoclassical view which considers take-up as a rational decision driven by the costs and benefits of claiming. It suggests that, in this context, standard interventions aiming to reduce the cost of claiming (e.g administrative burden reductions, simplification of the application procedure, personal assistance) may have failed to improve program participation as well. However, this result is likely context-specific. The costs associated with claiming the in-work benefit are small compared to other existing policies[50] and therefore claiming may have already led to positive net returns for eligible families. In set-ups with higher transaction costs, existing evidence show that interventions that reduce such costs have been effective in encouraging take-up (Finkelstein and Notowidigdo 2019; Deshpande and Li 2019; Castell et al. 2025).[51]

The second main result of the analysis is that the heighten media coverage of the program led to a significant increase in awareness and, in turn, higher take-up. This suggests that policy instruments like national information campaigns (e.g. with TV and radio spots) can be useful tools for governements seeking to improve take-up. While these tools are routinely used to raise awareness about risks and encourage positive behaviors (e.g., smoking quit, vaccination, road safety), they have been much less utilized to encourage policy take-up behaviors specifically.[52] Instead, targeted information provision, such as letters or texts sent to individuals identified as likely eligible and informing them about their entitlement, are more commonly used. This type of policy tool has also drawn much more attention from the economic literature, with evidence being quite mixed about how effective they are (positive effects for Bhargava and Manoli (2015); Guyton et al. (2017); Manoli and Turner (2014); Matikka and Paukkeri (2022), null effects for Bettinger et al. (2012); Linos et al. (2022); Cranor et al. (2019); Castell et al. (2025)).

Why could untargeted information campaigns be more effective than targeted information provision? First, targeted interventions may be, perhaps paradoxically, poorly targeted. Identifying individuals

[50] Filling a claim can be done online, is relatively rapid and does not involve the need to collect and send any documentation. The program also involves almost no monitoring of the beneficiaries, which can represent a significant disutility cost in other contexts.

[51] In a randomized experiment on French job seekers, Castell et al. (2025) find that a meeting with a caseworker significantly increased take-up of the in-work benefit, while providing access to an online eligibility simulator did not. They interpret these results as evidence that reducing application costs works while providing pure information does not. Our results can be reconciled when considering the difference of population of interest. Their analysis focuses on previous job seekers who are "*entitled to in-work (PA) benefits, often temporarily, with thus limited incentive to claim it*". Instead, my analysis focus on a broader population (only 47% of the new enrollees in Jan. 2019 were registered job-seekers in the year pre-reform). For the marginal entrants, take-up is quite stable over time in my sample, suggesting that these are not individuals with temporary eligibility. In other words, while transaction costs are identical, the benefit of claiming may be smaller for the population in Castell et al. (2025) than for my population of interest, explaining why they are more likely to respond to a change in transaction costs. Additionally, meetings with caseworkers are a bundled treatment, a mix of application cost reduction and information provision. Their results could still be consistent with the fact that information friction was a key barrier to take-up but only the information provided by the caseworkers led to an increase in take-up, not the information provided by the online simulator.

[52] Still, we can note the existence in the U.S. since 2007 of a large information campaign, the EITC Awareness Day organized by the IRS. The explicit aim is to generate extensive media coverage during the tax filing season in the hope of increasing awareness of the program and thus policy's take-up. To the best of my knowledge, there has been no causal study evaluating the impact of this campaign on take-up behaviors.

who are likely eligible individual for a program in existing administrative data is notoriously hard (and also explains, in part, why so few programs operate under automatic enrollment). Those who can be identified as eligible are a selected subpopulation among all eligible individuals, one in which information frictions may not be the main barrier to take-up. In my empirical analysis, I find that the marginal entrant responding to the information shock is less likely to be in the administrative data of the welfare agency as they are first-time entrants. They would thus be quite hard to identify and target with standard information letters interventions. Second, by focusing on increasing awareness on those who actually could benefit from the program, targeted information interventions can miss on possible positive spillover effects. When non-eligible individuals benefit from an information intervention, they can share this information with other individuals in their network (e.g. friends, relatives, colleagues or neighbors) who they view as possibly eligible.[53] Spillover may exist with targeted interventions but are much more limited.

A natural concern when discussing the policy implications of empirical results obtained in a quasi-experimental setting is the concern of external validity. Would an information campaign work as well at other periods, and for other welfare programs? The empirical analysis exploits an information shock that occurred during a quite specific political context in France, the "yellow vests" protests. Protests were widely documented and discussed in the media, attracting a lot of attention from the general population. One may be concerned that media consumption was unusually high at the time, explaining the large observed effects of the information shock happening through the media. Yet, total TV consumption was stable around the time of the reform (Figure 2.B2).[54] Take-up responses may also have been particularly large if they were driven by the "yellow vests" themselves, a group particularly attentive to the government's response to their protests. In Figure 2.B8, I conduct the difference-in-differences analysis, comparing areas where yellow vests protests organized roundabout blockades to areas with no such protests, where living in a blockade area is a proxy, though imperfect, of "yellow vests" belonging. Take-up responses are similar across areas, if anything, families living in non-protest areas exhibited slightly larger responses than those in protest areas. Such evidence is in line with the reality of the context; an increase in the in-work transfer was never part of the yellow vests protesters' demands. Protesters were unhappy with the policy tool chose by the government, as they would have favored an increase in the minimum wage instead.

Overall, while the quasi-experimental set-up exploited in the empirical analysis is specific in some aspects, evidence still points towards the fact that broad information provision, through policy tools like national information campaigns, can be effective in improving program participation. The effects of these policies are likely to be higher in context with lower initial awareness of the program, lower claiming

[53] A recent qualitative study based on in-person surveys finds that these networks play an important role in generating program awareness and alleviating possible doubts about eligibility status (Kula et al. 2020).

[54] There was a noticeable shift from regular channels to continuous news channels, especially on Saturdays when yellow vests protests occurred (+54% viewers during the Saturdays from November 15th to January 31st). Under the polar assumption that information supply only works when transmitted via continuous news channels, the information shock would have still resulted in an 11% (0.18/1.54) increase in enrollment under a "regular" context with standard audience patterns.

costs and when non take-up is especially driven by population with less contact with the administration (and thus harder to reach with standard targeted information provision interventions). Of course, whether governments want to implement such policies or not, and whether they should prefer it to other alternative policies, ultimately depends on the overall welfare implications.

6.2 Welfare implications

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 2001). This framework assumes that the tax and benefit system can be represented by a single net tax function, which includes both taxes and transfers – where negative taxes reflect means-tested benefits. It also assumes that individuals fully comply with both taxes and benefits, which rules out incomplete take-up of benefits. These assumptions are starkly at odds with the design and functioning of real-world welfare systems. In practice, tax collection and benefit administration are governed by distinct institutions, often with limited coordination. More importantly, empirical evidence consistently shows that a non-negligible share of eligible households do not take up the benefits they qualify for.

In Appendix D, I develop a theoretical model that extends the standard public finance framework to incorporate endogenous take-up behavior in welfare programs. The model closely follows the structure and methodology of Hansen (2021) but introduces endogenous participation in the welfare program.[55] To evaluate the welfare effects of policy changes, I use the tax perturbation method introduced by Saez (2001) and extended by Bierbrauer et al. (2023) in the context of two-bracket tax reforms. It allows me to derive closed-form welfare formulas that rely on a set of sufficient statistics. I calibrate these formulas using the empirical findings from the previous sections. I consider a simple redistributive social preference structure, where the social planner values consumption of (low-income) families eligible for the transfer 10% more than consumption of (high-income) families who are not eligible.

Welfare implications of the French reform. Overall, using my preferred calibration, I find that the 2019 welfare reform produced a positive welfare gain of approximately 3.55 million euros for the month of January 2019.[56] This monthly gain corresponds to about 0.8% of the pre-reform monthly public spending allocated to the program. Decomposing the welfare effects, the bulk of the gain – around 3.5 million euros – is attributable to the increase in the generosity of the transfer. In contrast, the information shock, despite leading to a substantial behavioral response in take-up, contributes a relatively modest 0.05 million euros to the total welfare gain, which I explain in detail below. Figure 2.B11 reports the

[55] Importantly, this version of the model does not nest the framework of Nichols and Zeckhauser (1982), which incorporates "ordeals" as self-targeting mechanisms to improve targeting efficiency. While my model includes claiming costs and information frictions, it assumes that these costs are uncorrelated with individuals' skill levels and that preferences are quasi-linear. As a result, the model currently excludes self-targeting behavior through differential claiming costs. This does not imply that optimal take-up must be 100%, since policies that reduce friction or claiming costs may still involve implementation costs. Future work will relax the orthogonality assumption to allow for the modeling of ordeals and targeting trade-offs.

[56] Full derivations and parameter calibration details are provided in Appendix D. All effects and costs are expressed for the month of January 2019. The total annual cost of the reform is substantially larger; however, because the empirical strategy yields causal estimates for the month immediately following implementation, I restrict the welfare analysis to this short-run horizon.

results of several robustness checks that vary key parameters in the calibration. These sensitivity tests confirm that the welfare conclusions are stable under reasonable alternative assumptions.

Decomposing the effects of the first component of the reform – the increase in benefit generosity – I estimate that its mechanical monthly cost amounted to 98 million euros. It also generates an additional fiscal externality cost estimated to be of about 5 million euros. This fiscal externality cost reflects several offsetting labor market responses. On the one hand, the reform induced positive extensive-margin labor supply responses, which reduce program costs by 8 million euros.[57] On the other hand, intensive-margin labor supply responses led to an additional cost of 1 million euros. These behavioral effects are quantified based on calibrated labor supply elasticities drawn from the empirical literature. Finally, the increase in take-up driven by higher benefits generated an additional public spending cost of 12 million euros. This estimate uses the empirically identified elasticity of take-up with respect to benefit levels. Because of the standard envelope theorem argument, such take-up responses yield only second-order welfare gains to new beneficiaries. This is because they necessarily stem from individuals who were optimizing with respect to their take-up decision pre-reform. In contrast, the reform produces a first-order welfare gain by delivering increased transfers to inframarginal beneficiaries. Using my baseline calibration of redistributive preferences, I estimate this redistributive welfare gain to be of 106 million euros. Combining these components yields a total net welfare gain from the benefit increase of 3.5 million euros (106 - 98 - 5).[58]

Regarding the effects of the information shock, I estimate that the resulting increase in take-up led to an additional 68.58 million euros in public spending in the month following the reform. These new beneficiaries experienced positive first-order welfare gains[59], which I estimate at 68.63 million euros. As a result, the net welfare gain from the information shock is modest – about 0.05 million euros in January 2019. This near equivalence between public spending and welfare gains reflects two opposing forces. On one hand, the social planner places a higher marginal value on transfers to eligible (low-income) households, implying that each euro transferred yields more than one euro in social welfare gains. On the other hand, newly enrolled beneficiaries incur application costs, which reduce the net welfare benefit they experience. These claiming costs create a wedge between the fiscal cost and the associated welfare gain.Figure 2.B11 shows that the welfare-improving nature of the information shock is sensitive to redistributive preferences. The shock yields positive welfare effects when the social planner values consumption of eligible households at least 8% more than that of ineligible households. As expected, the welfare gain increases with stronger redistributive preferences. For instance, if the planner values consumption of low-type households 50% more than that of high-type households, the information provision reform generates a welfare gain of 20.5 million euros – nearly 5% of total monthly spending on the program.

[57] These extensive-margin labor supply responses come from previously unemployed individuals, who are assumed to be taking up the safety net when out of the labor force. Therefore, when they start working, they enroll in the in-work transfer program but dis-enroll from the safety net, which, in net, saves money for the government.

[58] This calculation abstracts from the effects of eligibility expansion and does not incorporate potential welfare impacts stemming from improved targeting efficiency. Under the current model assumptions, such targeting effects are assumed to be zero. Moreover, this effect is zero if policy-makers value the consumption of all eligible families the same.

[59] Indeed, responses to the information shock imply that beneficiaries were making an optimization mistake pre-reform.

Policy implications. In this section, I have examined the welfare implications of two policy interventions aimed at increasing take-up: a transfer amount increase and a positive information shock. While information provision is increasingly recognized as a key tool to reduce non-take-up, raising benefit generosity is less commonly considered in this context. This is largely because such increases come with substantial mechanical costs, as it implies increasing the transfer amount for all infra-marginal households who were already enrolled. 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 in Appendix D). I now discuss and compare the welfare implications of administrative burden reduction policies and information provision policies, distinguishing also between broad information provision and targeted information outreach.

Information provision generates welfare gains that are (i) increasing in social redistributive preferences, (ii) increasing in the magnitude of the optimization error due to unawareness[60] 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). In my setup, costs are likely to be small on average, as the program tends to be easier to navigate than most welfare programs. 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. An important insight from the theoretical model is that information provision can also generate welfare gains by raising jointly employment and take-up.[61] Indeed, information frictions regarding in-work transfers might lead unemployed individuals to overestimate their participation tax, thus causing a negative distortion of employment. Reducing these frictions can thus boost both employment and benefit take-up, leading to first-order welfare gains by lowering public spending – regardless of redistributive preferences. This mechanism has been largely overlooked in the literature but may be especially relevant for in-work benefits, as prior evidence shows labor supply responds to information (Kostøl and Myhre 2021). Notably, this possible positive effect would arise only from broad information efforts that reach non-working, potentially ineligible individuals. To the contrary, targeted information outreach efforts cannot, by design, generate these efficiency gains.

While I have not derived welfare formulas for interventions that reduce administrative burdens, it is easy to see that it has very similar properties to the benefit increase.[62] 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 the same fiscal externalities effects as the change in benefit amount. It also generates a positive welfare gain for infra-marginal beneficiaries who face lower costs of claiming. Note that contrary

[60] The optimization error is equal to the current transfer amount minus any application costs/administrative burden faced by enrollees.

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

[62] 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.

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) and are not costly to decrease.[63]

To conclude, the model highlights that different take-up policies have distinct welfare implications that needs to be carefully assessed when choosing which one to implement. First, administrative burden reductions mainly help infra-marginal individuals—those already taking up – by targeting "rational" non take-up behaviors. In contrast, information provision addresses "frictional" non take-up and thus concentrates the welfare gains among marginal entrants. Second, when designing information policies, it is crucial to distinguish between targeted outreach and broad campaigns. While targeted outreach is often seen as more effective, my context shows that broad media-based information provision had large effects too. Moreover, broad interventions can generate additional welfare gains, beyond redistribution, by changing perceived incentives to work. While I did not derive the full optimal policy, an optimal broad information intervention may need to trade off the possible positive efficiency gains stemming from extensive-margin labor supply responses and the possible efficiency losses stemming from intensive-margin labor supply responses. This would call for information intervention aiming to target all low-income households, including those out of the labor force, but excluding higher-income households.

7 Conclusion

This paper studies the issue of imperfect take-up to welfare programs by focusing empirically on the case of the French in-work benefit. According to a pre-reform estimation made by the French Ministry of Social Affairs, 27% of eligible families were not claiming the program. Using exhaustive administrative data, I document the very large and unexpected take-up response triggered by the 2019 reform of the program. The empirical analysis investigates what drove the take-up responses and who were the families responding. 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, at least for the marginal entrants. Indeed, exploiting the heterogeneous change in benefit amount created by the reform, I find a small and non-significant elasticity of the take-up rate with respect to the monetary amount of the benefit. Instead, I show evidence suggesting that the reform acted as an information supply shock which successfully improved awareness and knowledge about the program leading to an increase in take-up behaviors. Survey data show that pre-reform one person out four had never heard about the program, even among low-income individuals. In a short period of time, media produced a large body of information about the program (e.g. its existence, who is eligible, how to claim). I estimate an 18% take-up increase caused by this information supply shock, which suggests that non take-up rate was almost cut by half.

[63] 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.

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 a broad 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. Importantly, such positive efficiency gains do not arise with targeted information outreach intervention.

Targeted information outreach interventions (e.g., personalized letters sent to individuals identified as likely eligible) have garnered significant attention from researchers as a potentially cost-effective way to increase program participation. This paper, however, focuses on broad and untargeted information provision interventions disseminated through media channels (e.g., intensive national information campaigns). The results suggest that such interventions could be both more efficient and more effective. First, the theoretical framework shows that, by alleviating information frictions globally, untargeted information interventions can simultaneously encourage labor supply and the take-up of in-work benefits, potentially reducing government expenditures on social assistance. Second, the findings demonstrate that non-targeted interventions succeeded in increasing take-up even among populations, such as those with no prior contact with governmental agencies, who typically do not respond to light-touch information outreach (Linos et al. 2022). One possible explanation for this success could be a social multiplier effect, where information spreads across networks of eligible and non-eligible individuals, amplifying the overall impact. Another possibility is that marginalized populations may place greater trust in information received from people within their social circles. Future research exploring these mechanisms would help shed light on the optimal design of information interventions.

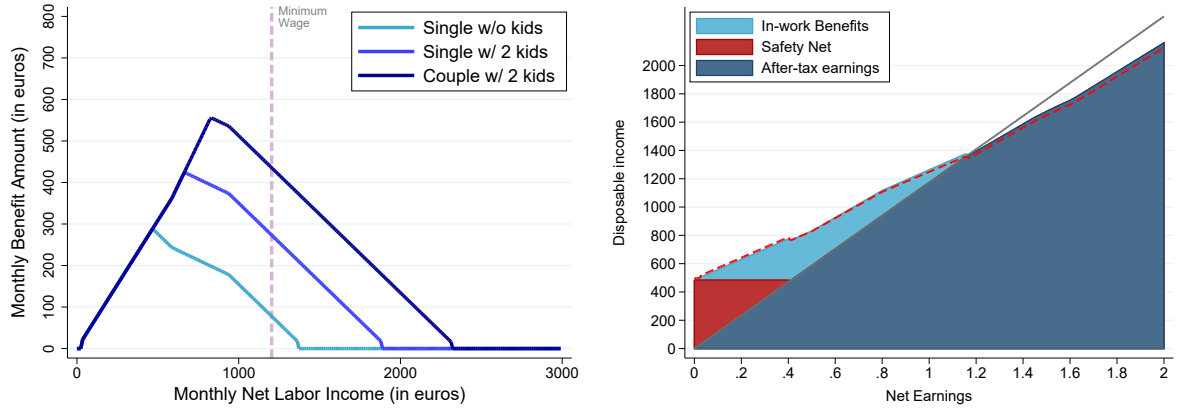
Appendices to Chapter 2

Appendix A: Details on the Institutional Setting

This section provides additional details about the French in-work benefit (*prime d'activité*).

Benefit schedule. Figure 2.A1 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. 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. 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.

Figure 2.A1: In-work transfer's schedule (2018)



(a) Transfer's schedule by family composition (2018) **(b)** Disposable income of PA beneficiaries (2018)

Notes: Panel (a) represents the theoretical amount of PA transfer that a family can receive, depending on their labor income (x-axis) and their family composition. In these test cases, 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 "lone 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. The dashed pink line represents the level of the national minimum wage in France in 2018.
Panel (b) represents the overall theoretical disposable income received by a single home-owner individual with only wage earnings as resources. It is decomposed between three resources; their after-tax earnings (i.e. labor income); their safety net transfer (i.e. *Revenu de Solidarité Active* or RSA) and their in-work benefit transfer (i.e. *Prime d'activité* or PA).
Sources: Maquette Edifis 2018, DREES, Ministry of Health and Social Affairs.

Benefit formula. Equation 2.2 provides the exact formula for the transfer's amount in a given month. Table 2.A1 and Table 2.A2 detail the legislative parameters used to compute this formula over the 2016–2020 period. The base amount depends solely on the family composition of the household captured by the parameter δ^f. The benefit is augmented by a fraction τ_t of the total household's labor income R^a at month t. The means-testing part consists in subtracting from the amount of the benefit the total household's resources R including labor income R^a as well as any other types of income (e.g. family/housing/unemployment benefits, pensions, capital income). If these resources are lower than the base amount, they are assumed to be equal to the base amount. Hence, when the family's resources R are lower than the base amount, the benefit phases in at rate τ_t. When the resources are higher than

the base amount, the benefit phases out at rate $1 - \tau_t$. Finally, the benefit is augmented by individual bonuses granted for each working family member as a function of their labor earnings R_i^a. The detailed formula for the bonus of an individual i is provided by Equation 2.3.[64]

$$B_t = \underbrace{MB_t \left(1 + \delta_t^f\right)}_{\text{Base amount}} + \tau_t R_t^a - \underbrace{\max\left(MB_t \left(1 + \delta_t^f\right), R_t\right)}_{\text{Means-Testing}} + \underbrace{\sum_i Bonus\left(R_{it}^a\right)}_{\text{Individual bonuses}} \tag{2.2}$$

$$Bonus\left(R_{it}^a\right) = \min\left(\tau_b MB_t, \max\left(0, \tau_b MB_t \times \frac{R_{it}^a - s_{min}\bar{w}}{s_{max}\bar{w} - s_{min}\bar{w}}\right)\right) \tag{2.3}$$

	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 2.A1: In-work transfer program's formula – Legislative parameters (2016–2022)

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

Table 2.A2: PA Welfare Program – Amount by family composition (2018)

Political Background. Following the "yellow vests" protests in France in late 2018, it was announced that the benefit would be 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 in-work transfer for a global cost of about 2.5 billion euros of additional public spending.[65] Note that the President in his December 10th speech did not mention the in-work transfer

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

[65] 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).

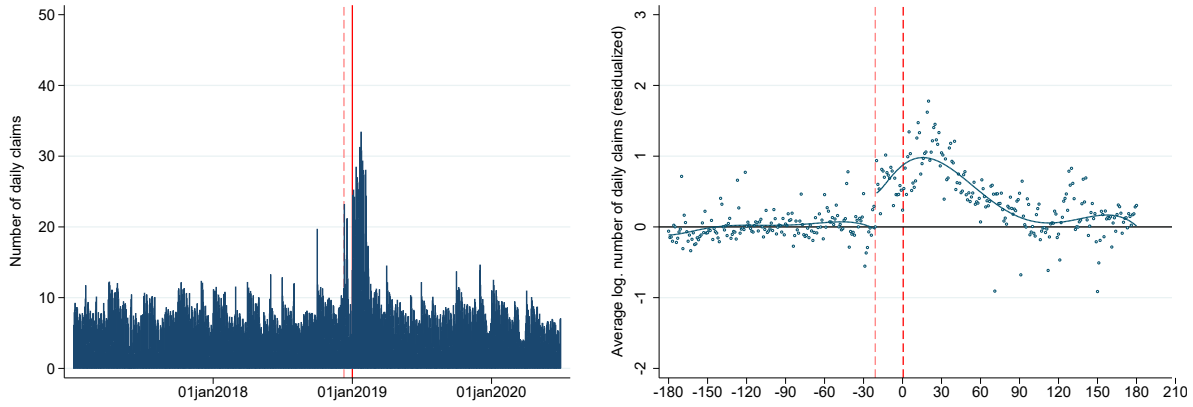
explicitly. It was the following day that the government announced that the increase was going to take place through this specific program. Before the announcement of the reform on Dec. 10th, no specific focus was put by the media on the PA program and no policy-makers had mentioned a possible reform 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]

[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.

Appendix B: Additional figures and tables

This section provides additional figures and tables that complement the main analysis.

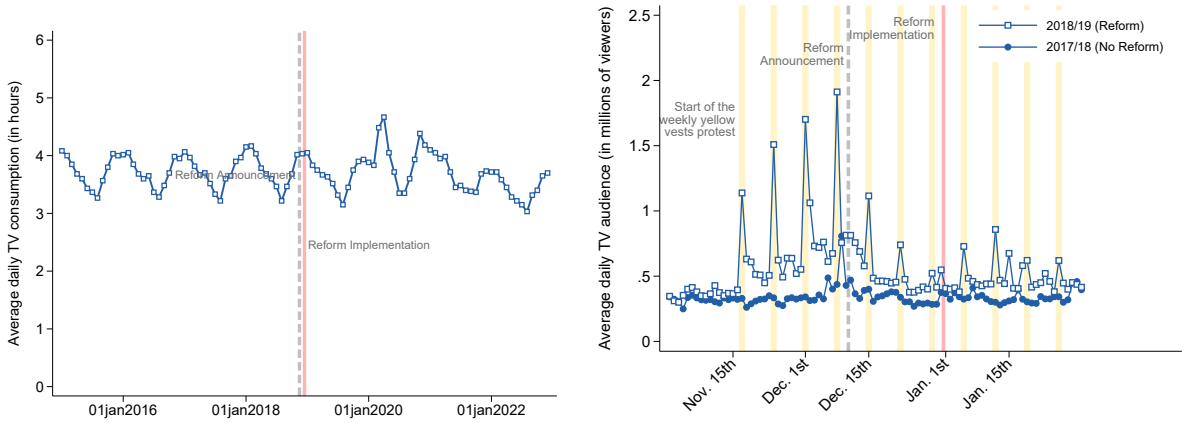
Figure 2.B1: Evolution of daily claims of the program (2017-2020)



(a) Daily number of claims (in thousand) (2017–2020) **(b)** Log. number of daily claims (2018/07–2019/06)

Notes: This figure plots the evolution of the daily number of new families claiming the in-work benefit program. The sample excludes families entering the program because of the eligibility expansion (see Subsection 4.1). Panel (a) plots the evolution of the raw number of daily new claims (i.e. the number of applications to the program sent to the welfare agency). Panel (b) plots the log. number of daily claims after removing seasonality effects (more precisely, after removing a day-of-the-week and a first-day-of-the-month x quarter fixed effect). The x-axis represents the number of day relative to the reform (i.e. to Jan. 1st 2019). The sample is restricted to claims made between July 1st 2018 and July 1st 2019. In each panel, the vertical dashed red line represents the reform's announcement date (Dec. 10th 2018) and the vertical red line represents the reform's implementation date (Jan. 1st 2019).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

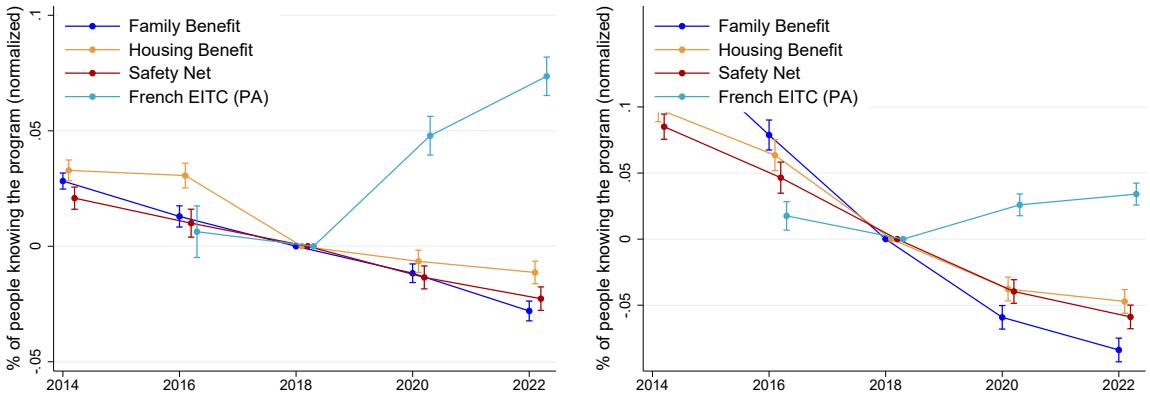
Figure 2.B2: Television Consumption



(a) All channels **(b)** Continuous news channels

Notes: Panel (a) plots the evolution of the average number of hours in the day spent watching TV (all channels). Panel (b) plots the average daily number of viewers of continuous news channels for the period November 1st-January 31st, in the year of the reform and in the year before. The yellow vertical line denotes the period of the yellow vests protests.
Source: Mediametrie and Data.gouv.fr.

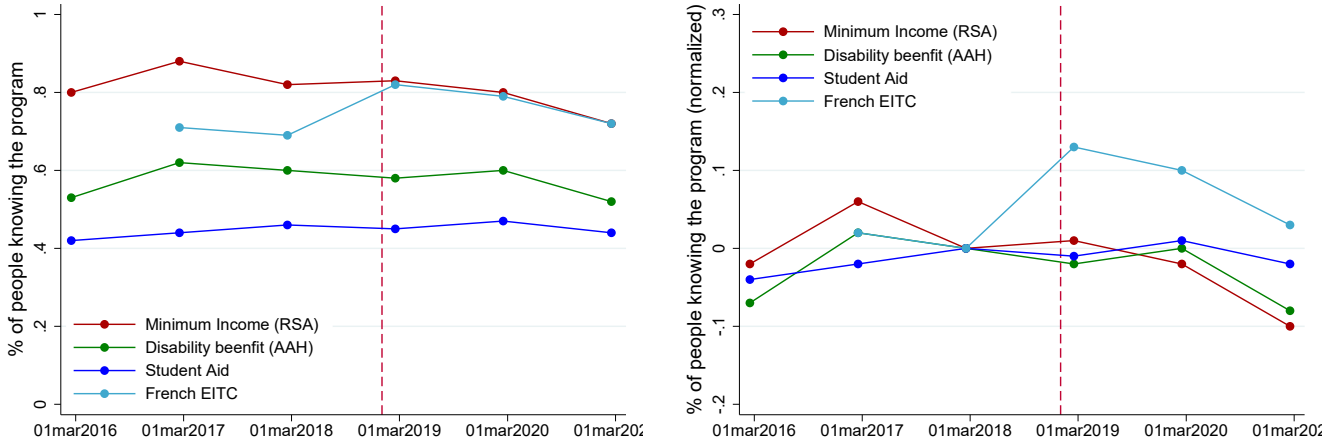
Figure 2.B3: Evolution of population knowledge of welfare programs



(a) Intermediate Knowledge **(b)** Advanced Knowledge

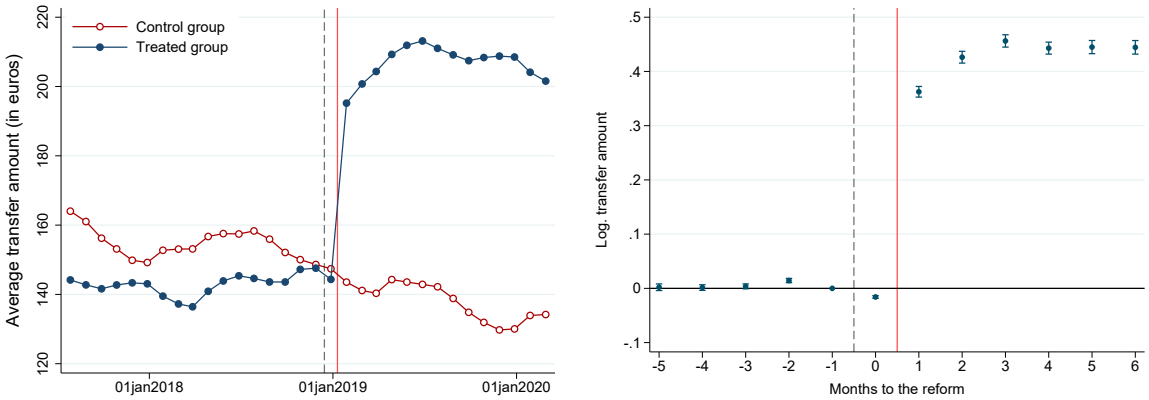
Sources: General Population Survey (*Baromètre d'opinion*), DREES, 2014-2022. Sample of 18-65 years old individuals.
Notes: The figures represent the event-studies coefficients of a regression of the share of individuals with self-reported knowledge of a given program on yearly dummies (excluding year 2018). Panel (a) represents the regression coefficients using the "intermediate knowledge" outcome, i.e. a dummy equal to one if one answers "Yes, approximately" or "Yes, quite precisely" to the question "Do you know who can benefit from the program [program's name]?" and declares being aware of the program's existence. Panel (b) represents the regression coefficients using the "advanced knowledge" outcome, i.e. a dummy equal to one if one answers "Yes, quite precisely" to the question "Do you know who can benefit from the program [program's name]?" and declares being aware of the program's existence. All figures are produced using the sampling weights provided by the data producers.

Figure 2.B4: Evolution of program's awareness (18–30 years old) – alternative survey data



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

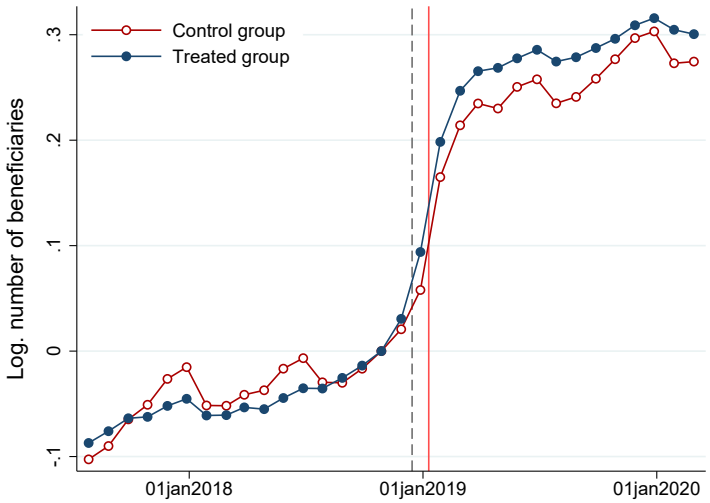
Figure 2.B5: Effect of the increase in monetary incentives – First stage



(a) Transfer amount (raw data) **(b)** Low-skilled occupation workers

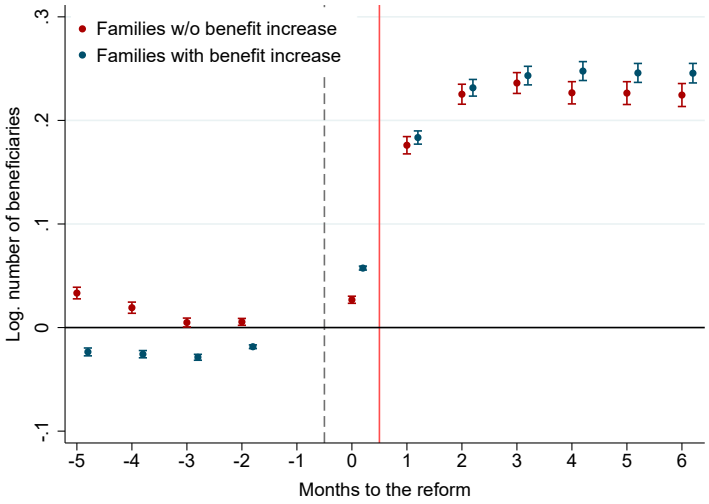
Notes: Panel (a) plots the evolution of the raw average transfer amount received by families in local welfare agencies for the group of treated and control families separately. A family is labeled as "treated" if it faces a difference of more than 15 euros between its current benefit amount and the counterfactual 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 4.2. 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.

Figure 2.B6: Enrollment evolution – treated vs control families



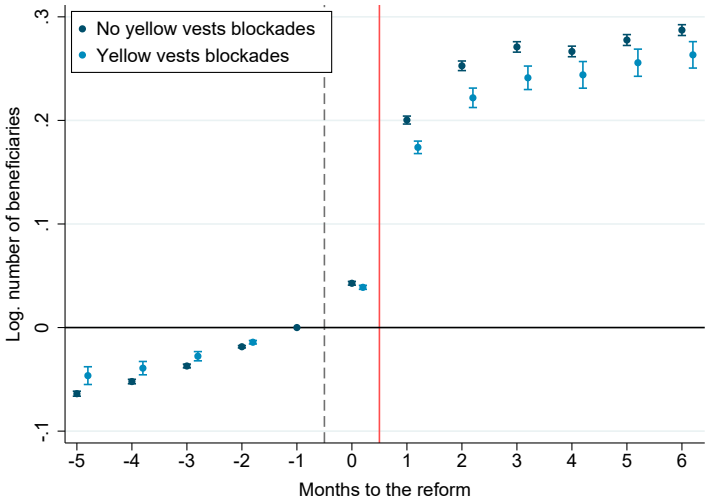
Notes: The figure plots the evolution of the raw average log. number of families enrolled in local welfare agencies (normalized as of October 2018) for the group of treated and control families separately. A family is labeled as "treated" if it faces a difference of more than 15 euros between its current benefit amount and the counterfactual benefit amount it could get under a system with no reform in place. The sample is restricted to beneficiaries who are not in the eligibility expansion range.
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Figure 2.B7: Effect of the information supply shock (by subgroups)



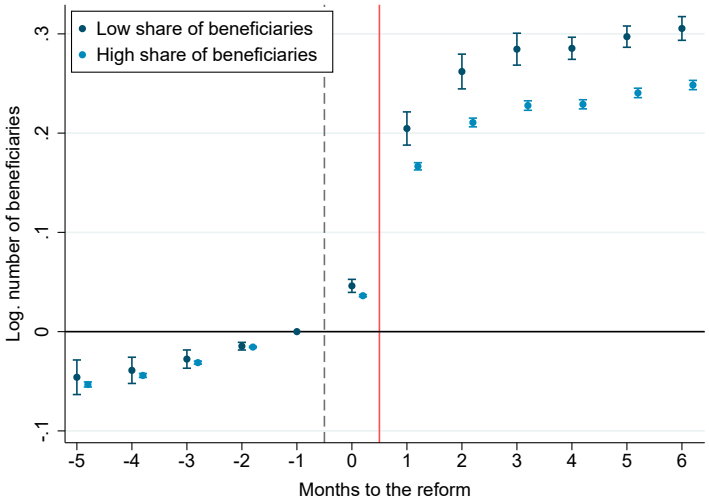
Notes: The figure plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Subsection 4.2 splitting the treated sample (families eligible to the in-work transfer program). The blue coefficients represent the results for the sample of families belonging to the treated group (i.e. exposed to the transfer increase). The red coefficients represent the results for the sample of families belonging to the control group (i.e. not exposed to the transfer 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. I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Figure 2.B8: Diff-in-diffs regression - Effect of the information shock (by yellow vests areas)



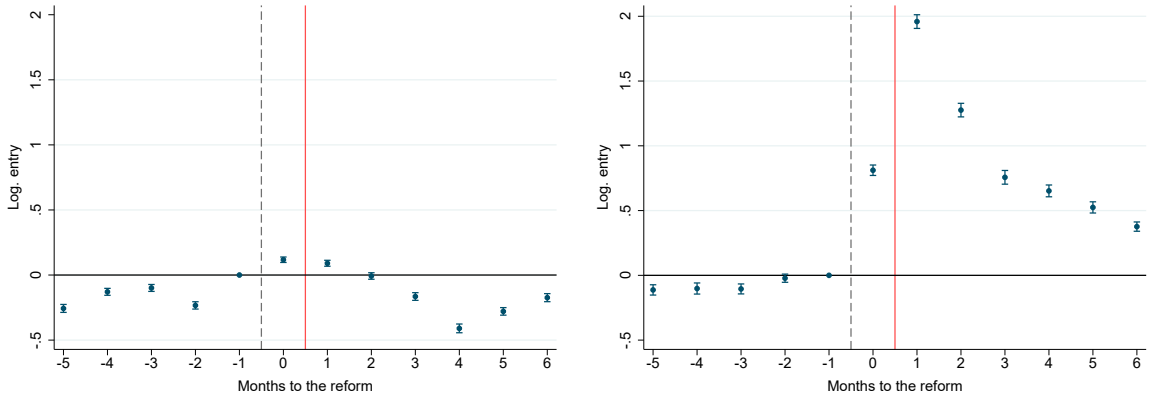
Notes: The figure plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Subsection 4.2 including all beneficiaries of the in-work transfer (regardless of their exposition to the benefit increase). The treated group is composed of families enrolled in the in-work transfer program (i.e. exposed to the information supply shock) and the control group of families enrolled in the minimum income scheme (i.e. not exposed to the information supply shock). The regression is performed separately on the sample of families living in local living zones ("bassin de vie") where a yellow vests blockade occurred and on the sample of families living in a zone with no blockade. 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. I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Figure 2.B9: Diff-in-diffs regression - Effect of the information shock (by stigma areas)



Notes: The figure plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Subsection 4.2 including all beneficiaries of the in-work transfer (regardless of their exposition to the benefit increase). The treated group is composed of families enrolled in the in-work transfer program (i.e. exposed to the information supply shock) and the control group of families enrolled in the minimum income scheme (i.e. not exposed to the information supply shock). The regression is performed separately on the sample of families living in local living zones ("bassin de vie") with a high share (above the median) of in-work transfer beneficiaries in 2017 (low stigma areas) and on the sample of families living in zones with a low (below median) share of beneficiaries (high stigma areas). 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. I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

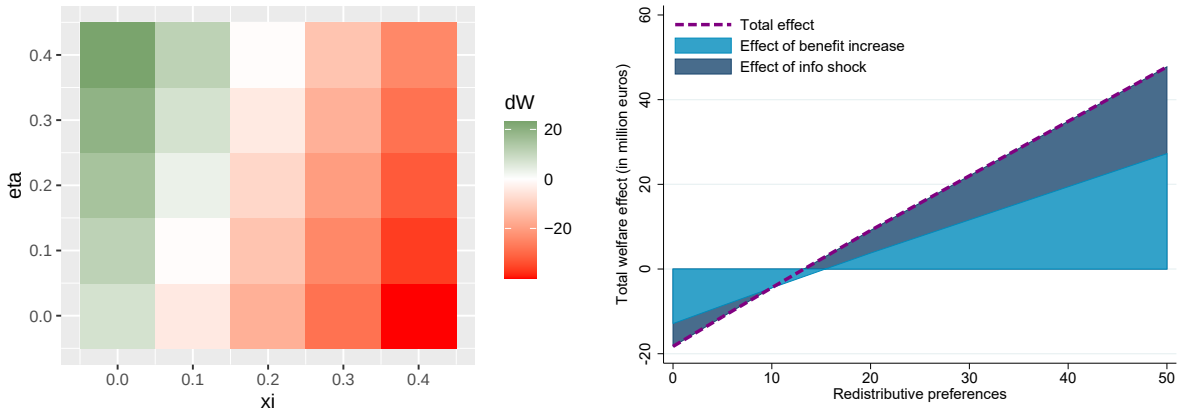
Figure 2.B10: Effect of the information supply shock - Flow outcome



(a) DiD coefficients - Re-entry **(b)** DiD coefficients - First-time entry

Notes: The figure plots the estimated coefficients of the de-trended difference-in-differences regression detailed in Subsection 4.2 but using a *flow* variable (the log. number of entrants in the program) rather than a *stock*. The analysis is performed on two separate outcomes: (i) the number of re-entry in the program by families who have ever been enrolled in the past, (ii) the number of first-time entry in the program. 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. I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).

Figure 2.B11: Sensitivity analysis



(a) Welfare effects by elasticities **(b)** Welfare effects by welfare weights

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.

Table 2.B1: Descriptive Statistics

	Any Program (1)	In-work transfer 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 as 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.

Table 2.B2: Descriptive Statistics - Estimation Samples

	In-work Benefit Program		Safety Net Program
	Control Group (G2)	Treated group (G1)	(G3)
Single Adults	0.59	0.50	
Single Parent	0.22	0.22	
Couples	0.19	0.28	
Number of children	0.68	0.74	0.89
Age of the family head	39.41	36.89	40.57
Non-EU foreigners	0.07	0.05	
Labor income	6,177.27	9,993.69	1,541.48
Pensions	338.94	176.04	194.12
Self-employed income	361.60	141.69	124.78
UI Benefits	1,136.32	919.79	878.72
Total social benefits received	5,603.05	3,554.89	9,426.26
Average nb. of families	5,446	20,831	17,506
Number of observations	94	94	94

Notes: This table presents descriptive statistics for the three key groups used for the estimation, as of November 2018. Among the in-work benefit program, the treated group are families enrolled into the program that are exposed to the benefit increase (transfer increase by more than 15 euros) and the control group are those not exposed to the benefit increase (transfer increase less or equal than 15 euros). Incomes are the annual total income per adult in the family.
Source: Administrative social data, ALLSTAT-FR6, 2018/11.

Table 2.B3: Effects of the reform on average characteristics – Targeting effects

	Baseline average	All Entrants	Entrants (w/ benefit increase)	Entrants (w/o benefit increase)
	(1)	(2)	(3)	(4)
Annual log. taxable income	8,358	−0.11***	−0.10***	−0.12***
		(0.01)	(0.01)	(0.01)
Single female w/o kids	0.28	0.013***	0.028***	0.011***
		(0.00)	(0.01)	(0.00)
Single male w/o kids	0.27	0.040***	0.027***	0.042***
		(0.00)	(0.01)	(0.00)
Single parent	0.16	−0.031***	−0.037***	−0.027***
		(0.00)	(0.00)	(0.00)
Couple	0.29	−0.021***	−0.017***	−0.021***
		(0.01)	(0.02)	(0.01)
Non-EU foreigners	0.09	−0.022***	−0.024***	−0.020***
		(0.00)	(0.00)	(0.00)
Past safety net beneficiaries	0.29	−0.09***	−0.12***	−0.08***
		(0.01)	(0.02)	(0.01)
No past contact with welfare agency	0.13	0.25***	0.18***	0.29***
		(0.01)	(0.01)	(0.01)

Notes: This table presents the estimated coefficients of a static version of the de-trended difference-in-differences regression detailed in Subsection 4.2, i.e. when pooling all pre-reform periods and all post-reform periods together. The results are presented for various outcome variables, which are the average characteristic of enrollees. For all results presented, I use families eligible for the safety net program as the control group. Column (2) presents the results of the diff-in-diff when using all families eligible for the in-work transfer program as the treated group (G1 + G2). Column (3) presents the results when using families eligible for the in-work transfer program and affected by the benefit increase (G1). Column (4) presents the results when using families eligible for the in-work transfer program and not affected by the benefit increase (G2). The sample is restricted to beneficiaries who are not in the eligibility expansion range. I also remove families that are enrolled in both the in-work benefit and the safety net program so that the two groups are separate. Standard errors are clustered at the geographical unit (*département*).
Source: Administrative social data, ALLSTAT-FR6, 2017/01-2020/03.

Table 2.B4: Awareness rates of various welfare programs

	In-work benefit	**Other welfare programs**				
		Safety Net	Housing Benefit	Family Benefit	Old-Age Safety Net	Disability Benefit
Full population	0.76	0.96	0.97	0.98	0.60	0.73
	(0.43)	(0.18)	(0.17)	(0.14)	(0.49)	(0.45)
By gender						
Men	0.75	0.96	0.96	0.97	0.54	0.66
	(0.44)	(0.19)	(0.19)	(0.17)	(0.50)	(0.47)
Women	0.77	0.97	0.97	0.99	0.65	0.79
	(0.42)	(0.18)	(0.16)	(0.10)	(0.48)	(0.41)
By income per capita						
< 600 euros	0.78	0.98	0.98	1.00	0.61	0.83
	(0.41)	(0.14)	(0.12)	(0.00)	(0.49)	(0.38)
600-1200 euros	0.73	0.96	0.97	0.97	0.55	0.73
	(0.45)	(0.21)	(0.18)	(0.18)	(0.50)	(0.44)
1200-2000 euros	0.77	0.97	0.96	0.98	0.59	0.72
	(0.42)	(0.18)	(0.19)	(0.13)	(0.49)	(0.45)
> 2000 euros	0.76	0.97	0.98	0.99	0.66	0.73
	(0.42)	(0.17)	(0.15)	(0.11)	(0.47)	(0.45)
By family category						
Single w/o kids	0.72	0.96	0.96	0.98	0.57	0.69
	(0.45)	(0.19)	(0.18)	(0.16)	(0.50)	(0.46)
Couple	0.77	0.96	0.97	0.98	0.62	0.74
	(0.42)	(0.19)	(0.17)	(0.13)	(0.49)	(0.44)
Single parent	0.81	1.00	0.99	0.99	0.61	0.79
	(0.39)	(0.00)	(0.08)	(0.08)	(0.49)	(0.41)
By age						
[18-25yo]	0.63	0.96	0.98	0.96	0.38	0.53
	(0.48)	(0.20)	(0.13)	(0.20)	(0.49)	(0.50)
[25-30yo]	0.76	0.97	0.97	0.99	0.45	0.60
	(0.43)	(0.18)	(0.16)	(0.11)	(0.50)	(0.49)
[30-50yo]	0.78	0.97	0.97	0.98	0.59	0.73
	(0.42)	(0.18)	(0.16)	(0.12)	(0.49)	(0.45)
[50-65yo]	0.75	0.96	0.96	0.98	0.69	0.80
	(0.43)	(0.19)	(0.20)	(0.15)	(0.46)	(0.40)
By education						
Brevet or less	0.76	0.97	0.97	0.99	0.59	0.75
	(0.43)	(0.18)	(0.17)	(0.11)	(0.49)	(0.44)
CAP or BEP diploma	0.76	0.95	0.97	0.98	0.56	0.70
	(0.43)	(0.22)	(0.18)	(0.15)	(0.50)	(0.46)
High-school diploma	0.75	0.97	0.96	0.97	0.56	0.69
	(0.43)	(0.18)	(0.19)	(0.17)	(0.50)	(0.46)
University diploma	0.76	0.97	0.98	0.98	0.65	0.75
	(0.43)	(0.16)	(0.15)	(0.12)	(0.48)	(0.43)
By living area						
Rural area	0.80	0.97	0.97	0.99	0.63	0.79
	(0.40)	(0.16)	(0.16)	(0.10)	(0.48)	(0.41)
Small city	0.76	0.95	0.96	0.97	0.61	0.75
	(0.42)	(0.22)	(0.20)	(0.16)	(0.49)	(0.43)
Large city	0.73	0.97	0.97	0.98	0.57	0.68
	(0.45)	(0.17)	(0.16)	(0.14)	(0.49)	(0.47)
By employment status						
Employed	0.77	0.97	0.97	0.98	0.58	0.71
	(0.42)	(0.18)	(0.16)	(0.13)	(0.49)	(0.45)
Self-employed	0.80	0.95	0.96	0.98	0.71	0.73
	(0.40)	(0.21)	(0.21)	(0.12)	(0.45)	(0.44)
Unemployed	0.80	0.97	0.99	0.98	0.49	0.77
	(0.40)	(0.17)	(0.12)	(0.15)	(0.50)	(0.42)
Inactive and other	0.68	0.97	0.95	0.97	0.64	0.77
	(0.47)	(0.18)	(0.21)	(0.16)	(0.48)	(0.42)
N	2,104	2,104	2,104	2,104	2,104	2,104

Source: General Population Survey (*Baromètre d'opinion*), DREES, 2018. Sample of 18-65 years old individuals.
Notes: The table presents average rates of awareness about various welfare programs in France among different groups. Awareness is defined as a dummy variable equal to one if the individual declares having heard about a given program. Rural areas are cities of less than 20,000 inhabitants. Small cities are cities with a total population between 20,000 and 100,000 inhabitants. Large cities are cities with more than 100,000 inhabitants or cities in the Parisian region. The other welfare programs are the following: *Revenu de Solidarité Active* (safety net), *Aides au Logement* (housing benefits), *Prestations familiales* (family benefits), *Allocation de Solidarité aux Personnes Agées* (Old-age safety net) and *Allocation Adulte Handicapé* (Disability Benefit). All figures are produced using the sampling weights provided by the data producers.

Table 2.B5: Intermediary knowledge rates of various welfare programs

	In-work benefit	**Other welfare programs**				
		Safety Net	Housing Benefit	Family Benefit	Old-Age Safety Net	Disability Benefit
Full population	0.65	0.92	0.92	0.95	0.52	0.66
	(0.48)	(0.27)	(0.26)	(0.21)	(0.50)	(0.47)
By gender						
Men	0.64	0.91	0.91	0.93	0.47	0.60
	(0.48)	(0.29)	(0.28)	(0.25)	(0.50)	(0.49)
Women	0.67	0.94	0.93	0.97	0.58	0.71
	(0.47)	(0.24)	(0.25)	(0.17)	(0.49)	(0.45)
By income per capita						
$<$ 600 euros	0.66	0.93	0.92	0.94	0.54	0.78
	(0.47)	(0.25)	(0.28)	(0.25)	(0.50)	(0.41)
600-1200 euros	0.62	0.92	0.94	0.93	0.48	0.67
	(0.48)	(0.27)	(0.24)	(0.25)	(0.50)	(0.47)
1200-2000 euros	0.67	0.93	0.91	0.96	0.51	0.64
	(0.47)	(0.26)	(0.28)	(0.20)	(0.50)	(0.48)
$>$ 2000 euros	0.66	0.92	0.93	0.96	0.59	0.66
	(0.47)	(0.27)	(0.25)	(0.19)	(0.49)	(0.47)
By family category						
Single w/o kids	0.62	0.91	0.92	0.93	0.50	0.63
	(0.49)	(0.28)	(0.27)	(0.25)	(0.50)	(0.48)
Couple	0.67	0.92	0.92	0.96	0.54	0.66
	(0.47)	(0.27)	(0.27)	(0.19)	(0.50)	(0.47)
Single parent	0.73	0.99	0.97	0.99	0.54	0.74
	(0.45)	(0.12)	(0.17)	(0.12)	(0.50)	(0.44)
By age						
[18-25yo]	0.52	0.91	0.95	0.90	0.33	0.45
	(0.50)	(0.29)	(0.21)	(0.30)	(0.47)	(0.50)
[25-30yo]	0.61	0.92	0.94	0.93	0.38	0.54
	(0.49)	(0.27)	(0.23)	(0.26)	(0.48)	(0.50)
[30-50yo]	0.69	0.93	0.92	0.96	0.51	0.66
	(0.46)	(0.26)	(0.27)	(0.20)	(0.50)	(0.48)
[50-65yo]	0.65	0.92	0.92	0.96	0.62	0.73
	(0.48)	(0.28)	(0.27)	(0.20)	(0.48)	(0.44)
By education						
Brevet or less	0.64	0.92	0.93	0.96	0.51	0.67
	(0.48)	(0.27)	(0.26)	(0.20)	(0.50)	(0.47)
CAP or BEP diploma	0.65	0.91	0.93	0.94	0.50	0.63
	(0.48)	(0.29)	(0.26)	(0.23)	(0.50)	(0.48)
High-school diploma	0.67	0.94	0.91	0.96	0.49	0.64
	(0.47)	(0.25)	(0.28)	(0.20)	(0.50)	(0.48)
University diploma	0.66	0.93	0.93	0.95	0.57	0.69
	(0.47)	(0.26)	(0.25)	(0.21)	(0.50)	(0.46)
By living area						
Rural area	0.68	0.92	0.91	0.95	0.54	0.71
	(0.47)	(0.27)	(0.29)	(0.21)	(0.50)	(0.45)
Small city	0.66	0.91	0.92	0.95	0.54	0.70
	(0.48)	(0.29)	(0.28)	(0.23)	(0.50)	(0.46)
Large city	0.64	0.94	0.94	0.95	0.51	0.61
	(0.48)	(0.25)	(0.24)	(0.21)	(0.50)	(0.49)
By employment status						
Employed	0.67	0.93	0.93	0.96	0.51	0.64
	(0.47)	(0.26)	(0.26)	(0.21)	(0.50)	(0.48)
Self-employed	0.70	0.92	0.89	0.94	0.63	0.66
	(0.46)	(0.28)	(0.31)	(0.24)	(0.48)	(0.47)
Unemployed	0.67	0.91	0.95	0.94	0.40	0.71
	(0.47)	(0.28)	(0.21)	(0.23)	(0.49)	(0.46)
Inactive and other	0.57	0.92	0.92	0.95	0.57	0.70
	(0.49)	(0.27)	(0.28)	(0.22)	(0.49)	(0.46)
N	2,104	2,104	2,104	2,104	2,104	2,104

Source: General Population Survey (*Baromètre d'opinion*), DREES, 2018. Sample of 18-65 years old individuals.
Notes: The table presents average rates of intermediate knowledge about various welfare programs in France among different groups. Intermediate knowledge is defined as a dummy variable equal to one if the individual declares having heard about a given program and having at least an approximate knowledge of who is eligible for this program. Rural areas are cities of less than 20,000 inhabitants. Small cities are cities with a total population between 20,000 and 100,000 inhabitants. Large cities are cities with more than 100,000 inhabitants or cities in the Parisian region. The other welfare programs are the following: *Revenu de Solidarité Active* (safety net), *Aides au Logement* (housing benefits), *Prestations familiales* (family benefits), *Allocation de Solidarité aux Personnes Agées* (Old-age safety net) and *Allocation Adulte Handicapé* (Disability Benefit). All figures are produced using the sampling weights provided by the data producers.

Table 2.B6: Advanced knowledge rates of various welfare programs

	In-work benefit	**Other welfare programs**				
		Safety Net	Housing Benefit	Family Benefit	Old-Age Safety Net	Disability Benefit
Full population	0.29	0.47	0.50	0.57	0.17	0.28
	(0.45)	(0.50)	(0.50)	(0.49)	(0.37)	(0.45)
By gender						
Men	0.28	0.43	0.46	0.51	0.14	0.24
	(0.45)	(0.50)	(0.50)	(0.50)	(0.35)	(0.43)
Women	0.30	0.50	0.54	0.63	0.19	0.31
	(0.46)	(0.50)	(0.50)	(0.48)	(0.39)	(0.46)
By income per capita						
$<$ 600 euros	0.30	0.69	0.66	0.62	0.24	0.45
	(0.46)	(0.46)	(0.48)	(0.49)	(0.43)	(0.50)
600-1200 euros	0.29	0.50	0.54	0.55	0.16	0.30
	(0.46)	(0.50)	(0.50)	(0.50)	(0.36)	(0.46)
1200-2000 euros	0.31	0.48	0.51	0.57	0.17	0.28
	(0.46)	(0.50)	(0.50)	(0.50)	(0.37)	(0.45)
$>$ 2000 euros	0.25	0.41	0.44	0.60	0.17	0.25
	(0.43)	(0.49)	(0.50)	(0.49)	(0.38)	(0.43)
By family category						
Single w/o kids	0.29	0.48	0.50	0.49	0.16	0.29
	(0.46)	(0.50)	(0.50)	(0.50)	(0.37)	(0.45)
Couple	0.28	0.45	0.47	0.61	0.17	0.26
	(0.45)	(0.50)	(0.50)	(0.49)	(0.37)	(0.44)
Single parent	0.37	0.58	0.71	0.77	0.18	0.39
	(0.48)	(0.49)	(0.45)	(0.42)	(0.38)	(0.49)
By age						
[18-25yo]	0.25	0.39	0.57	0.37	0.06	0.13
	(0.43)	(0.49)	(0.50)	(0.48)	(0.24)	(0.34)
[25-30yo]	0.27	0.46	0.47	0.50	0.12	0.24
	(0.44)	(0.50)	(0.50)	(0.50)	(0.33)	(0.43)
[30-50yo]	0.30	0.48	0.50	0.60	0.14	0.27
	(0.46)	(0.50)	(0.50)	(0.49)	(0.35)	(0.44)
[50-65yo]	0.29	0.48	0.50	0.59	0.23	0.33
	(0.45)	(0.50)	(0.50)	(0.49)	(0.42)	(0.47)
By education						
Brevet or less	0.33	0.52	0.52	0.59	0.20	0.32
	(0.47)	(0.50)	(0.50)	(0.49)	(0.40)	(0.47)
CAP or BEP diploma	0.29	0.51	0.54	0.57	0.17	0.27
	(0.45)	(0.50)	(0.50)	(0.50)	(0.38)	(0.44)
High-school diploma	0.29	0.45	0.52	0.59	0.12	0.28
	(0.46)	(0.50)	(0.50)	(0.49)	(0.32)	(0.45)
University diploma	0.27	0.43	0.46	0.57	0.18	0.28
	(0.45)	(0.50)	(0.50)	(0.50)	(0.38)	(0.45)
By living area						
Rural area	0.29	0.44	0.46	0.55	0.17	0.29
	(0.46)	(0.50)	(0.50)	(0.50)	(0.38)	(0.45)
Small city	0.32	0.50	0.53	0.63	0.17	0.33
	(0.47)	(0.50)	(0.50)	(0.48)	(0.38)	(0.47)
Large city	0.27	0.47	0.51	0.55	0.16	0.24
	(0.44)	(0.50)	(0.50)	(0.50)	(0.37)	(0.42)
By employment status						
Employed	0.30	0.47	0.50	0.57	0.15	0.27
	(0.46)	(0.50)	(0.50)	(0.49)	(0.36)	(0.44)
Self-employed	0.27	0.44	0.40	0.52	0.24	0.26
	(0.44)	(0.50)	(0.49)	(0.50)	(0.43)	(0.44)
Unemployed	0.30	0.55	0.60	0.59	0.13	0.33
	(0.46)	(0.50)	(0.49)	(0.49)	(0.34)	(0.47)
Inactive and other	0.26	0.47	0.54	0.60	0.20	0.32
	(0.44)	(0.50)	(0.50)	(0.49)	(0.40)	(0.46)
N	2,104	2,104	2,104	2,104	2,104	2,104

Source: General Population Survey (*Baromètre d'opinion*), DREES, 2018. Sample of 18-65 years old individuals.
Notes: The table presents average rates of advanced knowledge about various welfare programs in France among different groups. Advanced knowledge is defined as a dummy variable equal to one if the individual declares having heard about a given program and having a quite precise knowledge of who is eligible for this program. Rural areas are cities of less than 20,000 inhabitants. Small cities are cities with a total population between 20,000 and 100,000 inhabitants. Large cities are cities with more than 100,000 inhabitants or cities in the Parisian region. The other welfare programs are the following: *Revenu de Solidarité Active* (safety net), *Aides au Logement* (housing benefits), *Prestations familiales* (family benefits), *Allocation de Solidarité aux Personnes Agées* (Old-age safety net) and *Allocation Adulte Handicapé* (Disability Benefit). All figures are produced using the sampling weights provided by the data producers.

Table 2.B7: 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 in-work transfer program.

Appendix C: Details on the data sources

To better characterize the "information shock", I exploit several sources beyond the main data source which are the administrative social security records (ALLSTAT). These sources allow to measure outcomes typically unobserved in administrative data, such as media coverage of the program and intermediate outcomes like self-reported knowledge about the program by beneficiaries and non-beneficiaries or internet searches of the program.

The General Population Opinion Survey. This source allows to observe measures of self-reported knowledge about the program's existence and eligibility criteria. The *Baromètre d'Opinion* survey is a representative annual survey conducted by an agency linked to the French Ministry of Social Affairs (*Direction de la Recherche, des Études, de l'Évaluation et des Statistiques* or DREES). The data source is a repeated cross-section sample of between 3,000 and 4,000 individuals representative of the adult population in mainland France every year. The survey asks respondents several hundreds of questions about people's perceptions and opinions about various topics such as inequalities, poverty risks, housing situation, public policies, and some current social debates. It also provides a rich set of socio-demographic information about the respondent. Of special interest for this paper, the survey asks two questions about the interviewee's knowledge of different welfare programs, including the in-work benefit. The first question asks "*Have you heard of [program's name]?*". If the answer is positive, the second question asks "*Do you know who can benefit from it?*" and allows for the following possible answers; "*Yes, quite precisely*", "*Yes, but approximately*" or "*No*". I use these questions to construct a proxy of information with three levels of knowledge: awareness of the program's existence, limited knowledge (awareness and limited understanding of eligibility criteria) and advanced knowledge (awareness and good understanding of eligibility criteria). Unfortunately, these questions are only asked once every two years (2014, 2016, 2018 and 2020) as the survey rotates some questions every year.[67]

Europresse database. This source allows to observe the media coverage of the program in media. This data source is produced by the private company *Cision*. It aggregates articles from about 3,500 sources in the world. For the French press, it covers the main nationwide generalist newspapers as well as local press newspapers and online media for a total of 905 sources. By searching for the (exact) name of the in-work benefit, *prime d'activité*, in the whole corpus for the period 2014 to 2022, I obtain a measure of the intensity of the media coverage of the program throughout time.[68] For smaller windows of time, I also obtain information about the content of the media coverage. In particular, the Europress source allows the download of all articles. I manually produce a measure of the articles' qualitative content, such as if they mention details about (i) general eligibility criteria, (ii) benefit formula, (iii) claiming process or (iv) the 2019 reform.

INA database.

Google Trends. This source allows me to observe the evolution of Google searches regarding several welfare programs in France. This provides a proxy for the interest that people have in a given program at a given period. Google Trends produces data based on a random sample of Google searches. It contains information about the relative interest of various topics ranked on a scale of 1 to 100, where 100 denotes maximal interest over the period considered for a given topic. In other words, it produces an ordinal but not a cardinal measure of interests and can only be used to study trends rather than inform about the total number of searches.

[67] The survey takes place in October and November of a given year. Since the reform was only announced in December 2018, I will consider that the answers to the 2018 wave of the survey are not affected by any potential informational treatment implemented by the reform.

[68] To produce this measure, I restrict the sample to articles featuring the program's name in their title or introduction.

Appendix D: Theoretical model

In this Appendix, I expand the standard model of the optimal taxation literature (Mirrlees 1971; Saez 2001) to account for endogenous program take-up decisions. The theoretical framework and the methodology used follow the work from Hansen (2021). The key difference with this model is the introduction of endogenous participation in the welfare program. It also makes use of the "tax perturbation approach" introduced by Saez (2001) and recently revisited by Bierbrauer et al. (2023) in the context of two-bracket tax reforms, such as EITC reforms.

Section 1 starts by outlining the general conceptual framework of the model. Section 2 describes how imperfect take-up may arise in such a model and what implications it has for the individual problem's solution. Section 3 outlines a simpler version of the model described in Section 1, which matches key aspects of the French institutional context. Within this simpler model, Section 4 derives closed-form formulas that express the welfare effects of two policy changes: an increase in the program's generosity and an information shock that improves the program's awareness in the population.

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 Figure 7. An individual i thus has type $\{\omega; \delta; \gamma; \theta\} \in \Omega \times \Delta \times \Gamma \times \Theta \subseteq \mathbb{R}^4_+$. For convenience, I assume that the distributions of types are bounded, i.e. $\Delta = [\underline{\delta}; \overline{\delta}]$, $\Omega = [\underline{\omega}; \overline{\omega}]$ and $\Gamma = [\underline{\gamma}; \overline{\gamma}]$. For simplicity, I also assume that θ is a dummy and therefore $\Theta = \{0, 1\}$. The joint distribution of type is denoted by $F: \Omega \times \Delta \times \Gamma \times \Theta \mapsto [0; 1]$ and the density is denoted by f. For now, I assume that type distributions are not correlated. This assumption will be relaxed in future work.[69]

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 Figure 7, 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[70], meaning that an individual who chooses not to take up benefits is actually facing the *laissez-faire* situation.

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

[70] 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.

The benefit schedule is assumed to be a smooth, continuously differentiable and concave function of income y[71] 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.

$$\varepsilon(\omega) = \frac{\partial dy(\omega)}{\partial(1 - \tau(y(\omega)))} \frac{1 - \tau(y(\omega))}{y(\omega)} \tag{2.4}$$

$$\eta(\omega) = \frac{\partial r(\omega)}{\partial[\bar{y}(\omega) - T^P(y(\omega))]} \frac{1}{r(\omega)} \tag{2.5}$$

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

$$W = \int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \alpha(\omega) \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) V(\omega, \delta, \gamma, \theta = j | B) \; d\gamma \; d\delta \; d\omega. \tag{2.6}$$

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:

$$\int_{\underline{\omega}}^{\overline{\omega}} \int_{\underline{\delta}}^{\overline{\delta}} \int_{\underline{\gamma}}^{\overline{\gamma}} \sum_{j=0}^{1} F(\omega, \delta, \gamma, \theta = j) B(y^*(\omega, \delta, \gamma, \theta = j)) \; d\gamma \; d\delta \; d\omega \leq E. \tag{2.7}$$

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.

2. Model with imperfect take-up – General case

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.[72] 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.[73] 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.

[71] 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.

[72] 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.

[73] 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.

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 $\int_{\underline{\omega}}^{\bar{\omega}} q(\omega)d\omega$.[74] I define two semi-elasticities of the take-up rate, one with respect to the benefit amount one faced whenever working and the other one with respect to the demogrant $B(0)$:

$$\xi(\omega) = \frac{\partial q(\omega)}{\partial B(\bar{y}(\omega))} \frac{1}{q(\omega)} \tag{2.8}$$

$$\xi_0(\omega) = \frac{\partial q(\omega)}{\partial B(0)} \frac{1}{q(\omega)}. \tag{2.9}$$

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:[75]

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

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:

$$1 + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{2.10}$$

$$1 = h_y(y_2^T(\omega), \omega). \tag{2.11}$$

The overall solution of the individual's problem is:

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* $\Delta\delta^T = \delta^T(y_2^T) - \delta^T(y_1^T)$

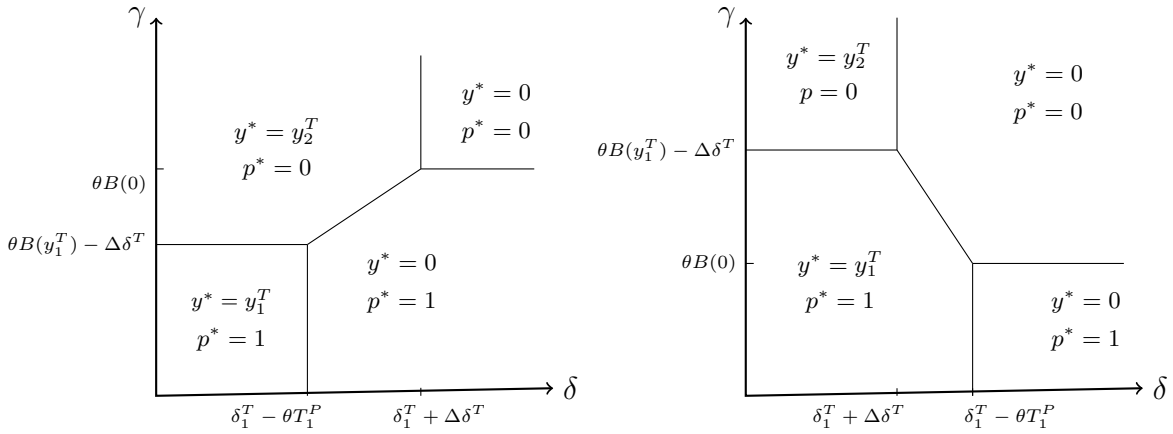
The solution $\{y^*, p^*\}$ is represented in the (δ, γ) space by Figure 2.D1. For individuals with sufficiently large or low costs of working δ and of taking up γ, take-up and labor supply decisions are in fact disjoint. However, for some individuals with intermediate values of δ and γ, a new trade-off 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

[74] 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.

[75] 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.

$\Pi(\omega|y>0) \equiv B(0) - T_1^P(y_1^T(\omega)) - \gamma - \Delta\delta^T(\omega)$ 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 2.D1a). 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 2.D1b). 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.

Figure 2.D1: Individual optimal earnings and take-up decision (y^*, p^*)



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

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

3. Model with imperfect take-up – Specific case

In this section, I outline the more specific version of the model I use to accurately model the French system and its 2019 reform. I begin 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 2.A1b in Appendix A represents the initial budget constraint implemented by this benefit schedule (solid red line). This initial system is close to the benefit system in place in France in late 2018, before the implementation of the welfare reform.

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

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).[76]

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

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

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

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 indicates that this program is well-known (see Section 3). 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.[77] In this section, p denotes the decision to take-up the in-work benefit B conditional on being eligible.

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

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:

$$1 + B_0'(y_1^T(\omega)) + B'(y_1^T(\omega)) = h_y(y_1^T(\omega), \omega) \tag{2.12}$$

$$1 + B_0'(y_2^T(\omega)) = h_y(y_2^T(\omega), \omega). \tag{2.13}$$

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

Proposition 3. *Under Assumptions (A4)-(A6), the solution to the individual problem is:*

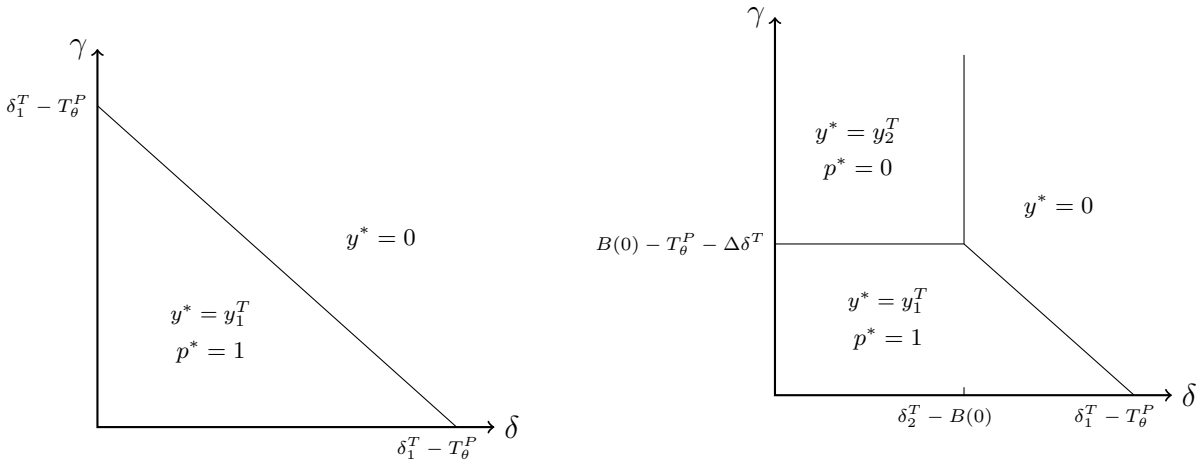
$$\{y^*, p^*\} = \begin{cases} \{y_1^T(\omega),\ 1\} & \text{if} \quad \gamma < \min\{B(0)\ ;\ \delta_2^T(\omega) - \delta\} - \Delta\delta^T(\omega) - T_\theta^P(y_1^T(\omega)) \\ \{y_2^T(\omega),\ 0\} & \text{if} \quad \gamma > B(0) - T_\theta^P(y_1^T) - \Delta\delta^T \text{ and } \delta < \delta_2^T(\omega)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

$$\text{with } \delta^T(y) = y - h(y) \quad \text{and} \quad \Delta\delta^T = \delta^T(y_2^T) - \delta^T(y_1^T)$$

Figure 2.D2 represents the individual's solution $\{y^*, p^*\}$ in the (δ, γ) space. Two cases arise depending on the skills parameter ω. Figure 2.D2(a) represents the solution for low-skills individuals whose optimal earning decision is $y_2^T(\omega) < B(0)$, conditional on not taking up the in-work benefit. Because they face a 100% marginal tax rate for $y < B(0)$ due to the perception of the safety net B_0, their optimal earning choice is $y_2^T = 0$. In other words, working and not taking up the in-work benefit is a dominated choice for these individuals. The figure shows that their optimal choice is either not to work ($y^* = 0$) or to work and take-up ($y^* = y_1^T$ and $p^* = 1$). This choice is driven by the relative cost of working δ versus claiming the in-work benefit γ. Figure 2.D2(b) represents the solution for higher-skills individuals whose optimal earning decision is $y_2^T(\omega) > B(0)$, conditional on not taking up the in-work benefit. Compared to the former case, an additional solution arises for individuals with a 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.

[77] 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 underestimates the true fiscal externality imposed by individuals who start working and taking up.

Figure 2.D2: Individual optimal earnings and take-up decision (y^*, p^*) – Simpler model



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

4. Welfare implications of policy reforms

Using the theoretical framework outlined in the previous section, I derive sufficient statistics welfare formulas for the effects of two policy reforms: a change in the amount of the benefit (dB) and a change in the program's awareness ($d\theta$).

4.1. Welfare effect of the positive information shock

Given a transfer system that satisfies Assumptions (A4)-(A6), Figure 2.D3 represents the solution of the individual's problem for agents that are unaware of the in-work benefit program ($\theta = 0$). Figure 2.D3(a) represents the case of individuals with low skills such that $y_2^T(\omega) = 0$. Figure 2.D3(b) represents the case of individuals with intermediate skills such that $y_2^T(\omega) > B(0)$.[78] 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 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 p_θ and that after the reform it is $p_\theta + \Delta\theta$.[79] 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 Figure 2.D3(a) and Figure 2.D3(b)). Some individuals were previously working but not taking up because they were also unaware of the program. If their claiming cost is sufficiently small, they display a positive take-up response and a negative intensive margin labor supply response (effect ② in Figure 2.D3(b)). Overall, the model predicts that after a positive information shock $\Delta\theta$, the take-up rate and the labor force participation rate increase.

Proposition 4. *The marginal welfare effect of an information shock $\delta\theta$ is:*

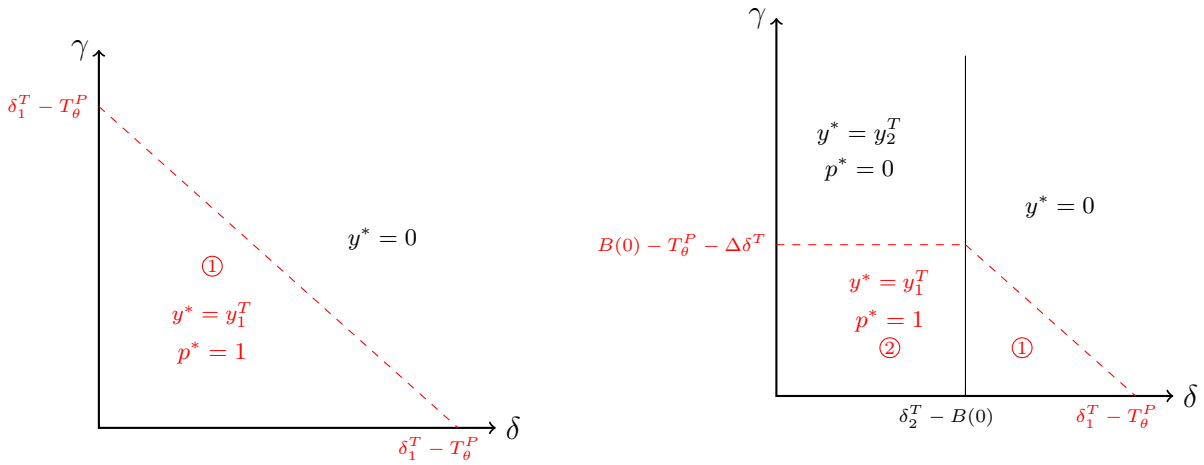
$$W_\theta(\theta) = \bar{q}(\theta) \int_{\underline{y}}^{\bar{y}} f_y(y) \left[\tilde{\eta}^\theta(y)(\alpha(\omega^T(y))dV_1(y) + T_\theta^P(y)) + \xi^\theta(y)(\alpha(\omega^T(y))dV_2(y) - B(y)) \right] dy$$

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

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

[79] 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.

of these responses on individual private welfare.[80] 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.



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

Figure 2.D3: Effects of an information shock on individual's optimal earnings and take-up decision

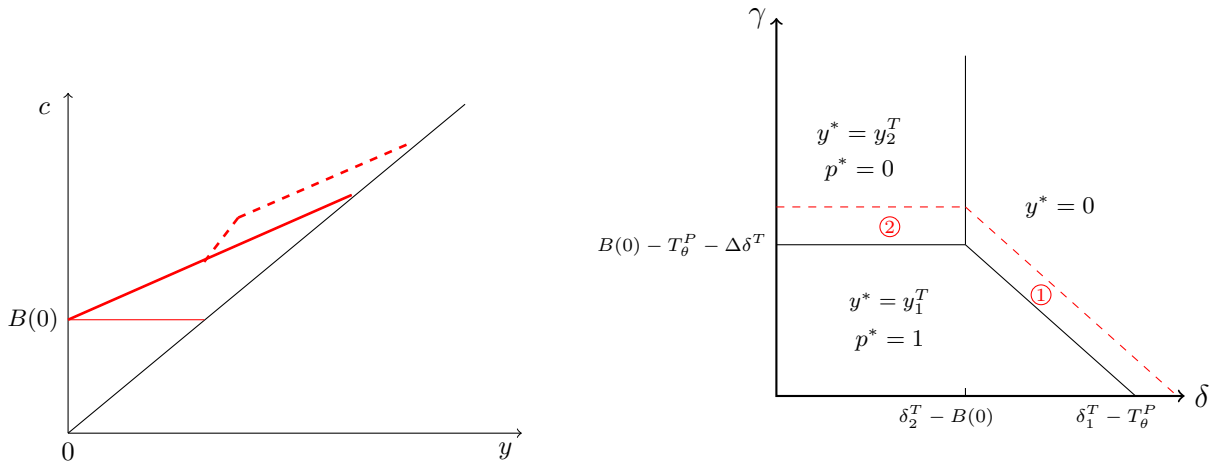
Lecture: The solid black lines represent the initial solution (when $\theta = 0$) and the dashed red lines represent the solution after the information shock (when $\theta = 1$).

4.2. 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 2.D4(a). Formally, a tax reform is defined as an object (τ, h) that replaces the initial transfer $B(y)$ by $B(y) + \tau h(y)$. In this section, I restrict the analysis to reforms where $h(0) = 0$ as is the case of the 2019 reform.

After a benefit increase such as the one described above, two types of behavioral responses can arise. First, because the participation tax T_θ^P has decreased, there can be a positive take-up and extensive labor supply response among individuals previously not working but who were aware of the program and had a relatively low cost of claiming compared to their cost of working (effect ① in Figure 2.D4(b)). Second, because the benefit amount that can be claimed has increased, *conditional* on labor supply choices, there can be a positive take-up response among individuals previously working but not claiming the benefit who are aware of the benefit and have a low enough cost of claiming (effect ② in Figure 2.D4(b)). This response can also imply an intensive margin labor supply response whose sign depends on the sign of $B'(y_2^T)$.

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



(a) Description of the reform **(b)** Effect of the reform

Figure 2.D4: Effects of the benefit increase on individual optimal earnings and take-up decision

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.

Proposition 5. *The marginal welfare effect of the reform* (τ, h) *around* $\tau = \bar{\tau}$ *is:*

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

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

Proposition 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.

4.3. 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 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:*

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

4.4 Calibration of the model

This section details how I calibrate the parameters of the welfare formulas detailed in Proposition 6 and Proposition 7.

- $B(y), T^P(y), B'(y)$ (the tax-benefit parameters). For each observation, I micro-simulate their benefit amount, participation tax, and 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 in November 2018, to avoid capturing the effect of responses that started in December 2018. The post-reform distribution is taken from the observed January 2019 distribution.
- $\tilde{\eta}(y)$ (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.[81] I calibrate the elasticity of the labor force participation conditional on take-up to be 0.2. In a recent paper, 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 a 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 a sensitivity analysis by making this parameter vary between 0 and 0.4.

[81] 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.

- $\xi(y)$ (the take-up semi-elasticity). Note that what I label as a take-up rate semi-elasticity is instead an enrollment rate semi-elasticity (i.e. the percentage increase in the share of the population enrolled in the program following a one euro increase in the benefit amount). Therefore, it fits with what I have estimated empirically and the welfare analysis doesn't require to make any assumption about the baseline take-up rate in the population. In Section 5, I have estimated that the average take-up elasticity with respect to the benefit amount was around 0.1.[82] Again, I perform a slight modification of the welfare formula to make the take-up elasticity appear rather than semi-elasticity. I perform a 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.

- $\xi^{theta}(y)$ (the take-up semi-elasticities with respect to a change in awareness). In Section 3, I have shown that following the information shock, which 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 includes the disutility of doing paperwork or a possible stigma feeling.

- $\alpha_0(y)$ and $\alpha_1(y)$ (the welfare weights sequences). These welfare weights represent the social value of transferring one euro from the overall population[83] 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).[84] 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 $\alpha_0 = \alpha_1 = 1.086$.[85]

[82] 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.

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

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

[85] In France, there was a total of 28.9 million 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.

Appendix E: Proofs

This section provides the proofs' derivations related to the theoretical model results outlined in Appendix D.

1. Preliminary Proofs

I start by stating and proving a lemma that provides an expression for the effects of a small reform of the benefit schedule in terms of sufficient statistics. A reform is an object (τ, h) that replaces the benefit schedule $B(y)$ by $B(y) + \tau h(y)$ where $h(.)$ is a flexible function. Lemma 1 characterizes the marginal effect of a reform (τ, h) on public spending. Corollary 2 characterizes the marginal effects of a reform (τ, h) around $\tau = 0$ on public spending, which is a specific case of the previous Lemma.[86]

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

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

Corollary 2. *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 7, the derivatives of $y(\omega, \tau)$, $q(\omega, \tau)$ and $\tilde{r}(\omega, \tau)$ can be expressed in the following way:

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

[86] The proofs in this subsection rely on the general framework described in Sections 1 and 2 of Appendix D. They do not require the additional simplifying assumptions made in Section 3.

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 7, the fiscal externalities can be represented graphically. Figure 2.E1 represents how the optimal earning and take-up decisions move with a reform (τ,h).

Proofs of Section 4 of Appendix D

In this section, I provide the proofs for the results of the welfare analysis of the 2019 reform presented in Section 4 of Appendix D.

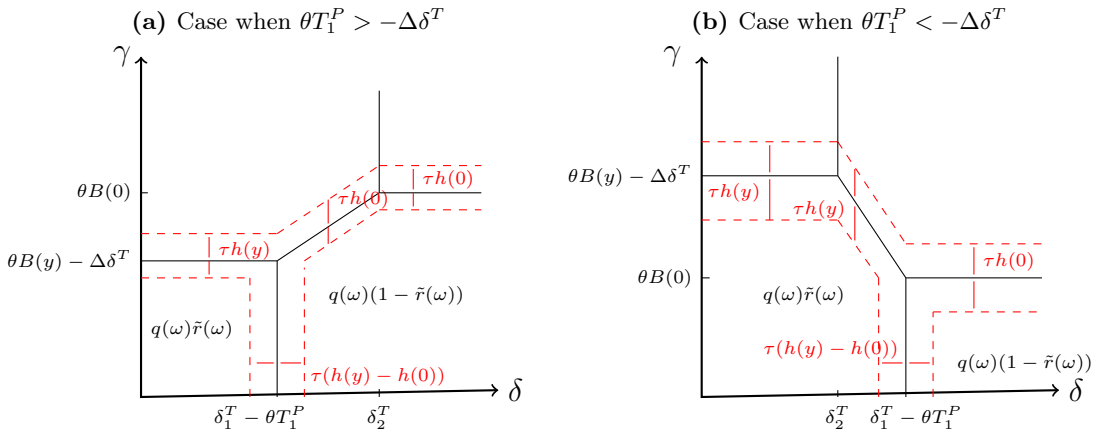


Figure 2.E1: Effect of the reforms on behaviors

2. 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[87], $y(\omega,\theta)$ 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 are necessarily 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:

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

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

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

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