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What works?

Evaluation of Three Public Policies in the Field of Labor and Public Economics

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Résumé

L'évaluation quasi-expérimentale des politiques publiques est un outil précieux pour l'analyse économique et l'avancement des politiques publiques. Cette thèse se concentre sur trois politiques, dans le domaine de l'emploi et des finances publiques, et plus précisément sur un programme pour l'emploi des jeunes, une subvention à la formation professionnelle, et une règle budgétaire. En évaluant chacune de ces trois politiques, je consacre une attention particulière à l'identification économétrique des effets causaux. J'adopte conjointement une approche innovante en essayant de lier des effets causaux, à validité locale, à des modèles structurels, de validité plus générale. Cela vise ainsi à mieux comprendre les mécanismes derrière les résultats, mais aussi à tester les implications des résultats pour la théorie économique dans son ensemble.

Le premier chapitre, intitulé "De quoi les jeunes ni en emploi, ni en études et ni en formation professionnelle (NEET) ont-ils besoin ? L'effet conjoint des politiques actives et passives pour l'emploi", évalue une politique française d'insertion, la Garantie Jeunes. Ce programme offre une année d'indemnité monétaire et un parcours d'activation intensif aux jeunes défavorisés NEET. En utilisant une méthodologie de différence dans les différences, les résultats mettent en évidence un fort effet positif conjoint des politiques actives et passives (+21 points d'emploi) après que les jeunes aient quitté le programme. Pendant la participation au programme, l'emploi à temps partiel diminue lorsque les jeunes sont occupés par des mesures d'activation et si le transfert monétaire n'est pas cumulable avec le revenu du travail. Cela suggère que recevoir une indemnité monétaire et être engagé dans une formation risque de réduire l'emploi des jeunes, compensé par l'effet positif des mesures d'activation.

Le deuxième chapitre, "Qui profite des subventions pour la formation générale ? Les effets du compte individuel de formation en France", porte sur le CPF français, un dispositif qui dote tous les travailleurs de crédits de formation. En etudiant une réforme en 2019, je montre que la participation à la formation n'est pas significativement affectée par le valeur du CPF parce que, en équilibre, plus de la moitié du bénéfice de la subvention est capturée par une augmentation des prix de formation de 53 centimes pour chaque Euro de subvention. De plus, la subvention affecte les profits des organismes de formation, sans changer ni leurs coûts ni leur nombre de salariés. Ces résultats peuvent être expliqués avec la présence d'une demande de formation inélastique et par des marchés de cours de formation peu compétitifs. Dans ces conditions, les subventions à la formation continue risquent d'être un simple transfert aux producteurs et aux bénéficiaires de la formation, sans une amélioration du bien-être générale.

Enfin, le troisième chapitre étudie l'impact des politiques d'austérité. Le coût de la réduction des déficits budgétaires est une question empirique ouverte : les estimations quasi-expérimentales des multiplicateurs fiscaux locaux varient entre 1,5 et 1,8, mais la plupart d'entre elles sont obtenues à partir de chocs augmentant les dépenses. Ce chapitre se focalise par ailleurs sur un choc de sense opposé: l'extension de règles budgétaires plus strictes en 2013 aux municipalités italiennes de moins de 5 000 habitants, ce qui génère une augmentation d'environ 100 euros par habitant de l'excédent budgétaire net municipal. Nous ne constatons aucune diminution du revenu des résidents suite à cette réduction budgétaire. Le multiplicateur fiscal local estimé n'est donc jamais significativement différent de zéro, et nous pouvons exclure qu'il soit supérieur à 1,5 avec 95% de confiance. Ces résultats suggèrent que le coût de la réduction du déficit budgétaire peut être, dans le cas de diminution de dépenses, inférieur à celui de 1,5 Euros de revenu pour chaque Euro de reduction du déficit.

Summary

This Thesis includes three empirical papers which common denominator is what I call a Policy Evaluation perspective. I intend this as a research method that joins three elements: a Design-Based approach aiming at identifying causal reduced-form effects, an attempt to derive implications and conditions for generalization to more structural models and parameters, and a focus on policy-relevant questions.

The first chaper is titled “What do NEETs need? The Joint Effect of Active and Passive Labor Market Policies”. Active and passive labor market policies are often used jointly, but the literature has only evaluated them one conditional on the other. This paper evaluates a flagship French program for disadvantaged youth Not in Employment Education or Training (NEETs) that provided a year of cash transfers and intensive activation measures. I exploit the staggered adoption of the program using a classical event study and a difference-in-differences methodology. The results highlight a strong positive joint effect of active and passive policies (+21 percentage points in employment, +63% with respect to control) after youths exit the program. During program enrollment, I show that part-time employment decreases in the first semester – when youths are busy in activation measures – while in the second semester the decrease is concentrated in income brackets where the cash transfer is phased-out with labor income. This suggests that cash transfers and lock-in from training reduce youth employment, but this is more than compensated by the positive effect of activation measures.

The second chapter, titled “Who Profits from Subsidizing General Training? Evidence from a French Individual Learning Account”, studies the effect of the French Compte Personnel de Formation (CPF), a scheme endowing all workers with generous training credits to be spent on the training market. Workers could under-invest in general training, hence governments often subsidize lifelong learning. We show that the total amount of training undertaken is not significantly affected by the subsidy. This happens because, in equilibrium, more than half of the benefit of the subsidy is captured by training producers through a significant change in prices. Moreover, a change in the subsidy eventually affects producers’ profits, with no effect on labor costs and employment of trainers. Our results can be rationalized by inelastic demand for training, and by either perfectly inelastic supply or imperfectly competitive training markets. Under such conditions, subsidies to lifelong learning are a simple transfer to training producers and consumers, with no effect on aggregate welfare.

Finally, the third chapter focuses on "The Impact of Austerity Policies on Local Income: Evidence from Italian Municipalities". Fiscal consolidation is often a necessity for local governments, but the cost of austerity for local economic activity is an open empirical question. Quasi-experimental estimates of local fiscal multipliers range between 1.5 and 1.8, but most of them are obtained from expansionary shocks. We study the extension of tighter budget rules in 2013 to Italian municipalities below 5,000 inhabitants, which generates a persistent increase of about 100 Euros per capita (0.5% of local income) in municipal net budget surplus, mostly driven by a cut in capital expenditures. We find no decrease in local income over a eight-year horizon. The estimated multiplier is always not significantly different from zero, and we can exclude it is above 1.5 with 95% confidence within 4 years from the shock. We find no evidence of spillovers to neighboring municipalities. These results suggest that the cost of fiscal consolidation can be lower than what currently prevailing estimates of local multipliers imply.

Contents

List of Figures

List of Tables

Introduction

This Thesis includes three empirical papers which common denominator is what I call a Policy Evaluation perspective. I intend this as a research method that joins three elements: a Design-Based approach aiming at identifying causal reduced-form effects, an attempt to derive implications and conditions for generalization to more structural models and parameters, and a focus on policy-relevant questions.

In modern Applied Microeconomics, policies have to generate experimental or quasi-experimental conditions, so that a causal link can be established between the variation in institutions and outcomes, delivering credible insights on the underlying economic structure. This approach is called Design-Based Economics (as opposed to Model-Based), and was pioneered thirty years ago by the work of, among others, 2021 Nobel-prize winners D. Card, J. Angrist and G. Imbens. Since then, even if Design-Based research seemed at the beginning to deliver very specific insights, credibility and Econometric rigor became the basis for progressing scientifically toward a more nuanced, comprehensive, and adaptive understanding of how the economy and institutions interact. This thesis is a contribution to this large and influential field of Economic research. More specifically, I focus on the fields of of Labor and Public Economics, as the three chapters of this thesis evaluate a program for disadvantaged job-seekers, an individual learning account, and local budget rules. In all three cases, the emphasis on causality has required the construction of novel administrative datasets, a careful study of the institutional context, and a strong emphasis on robust identification strategies.

Yet, Policy Evaluation should not be limited to reduced-form estimates: as policies ar rarely replicable in the same way and under the same conditions, a key challenge is to understand the Economic mechanisms behind them, and to predict policy effects out-of-sample. This requires some help from the other side of applied research: Model-Based economics. In this thesis, I try to bridge reduced-form estimates of policy effects with more general parameters by regularly trying to disentangle mechanisms and to derive insights on structural parameters using stronger assumptions and simple models. In all three papers, for example, I estimate parameters such as, respectively, job search efficacy, the elasticities of training supply and demand, and the local economy multiplier. This

approach allows us to learn more general economic lessons without stretching credibility too far, or at least being transparent about which assumptions map data into causal claims.

Finally, a goal of this thesis is to produce valuable insights for policy makers, by evaluating three important policies which try to answer big challenges of our times: how to promote social inclusion, how to invest in skills and human capital, how to sustain employment and income in communities. The results are sometimes encouraging, sometimes disappointing on the capability of policy to improve aggregate welfare: while in the first chapter intensive active and passive labor market policies seem to successfully rescue disadvantaged NEETs, the second chapter shows that training subsidies fail to affect training participation, and the third chapter that local governments budget doesn't seem to significantly relate to local income. This points out the crucial importance counterfactual evaluation policy evaluation, which can be costly but also highlight large merits and costly limits of public action, helping to understand where public resources can be most fruitfully allocated.

Chapter 1

What Do NEETs Need? The Joint Effect of Active and Passive Labor Market Policies

Abstract

Active and passive labor market policies are often used jointly, but the literature has only evaluated them one conditional on the other. This paper evaluates a flagship French program for disadvantaged youth Not in Employment Education or Training (NEETs) that provided a year of cash transfers and intensive activation measures. I exploit the staggered adoption of the program using a classical event study and a difference-in-differences methodology that extends De Chaisemartin and D'Haultfœuille (2020a) to a setting where individuals enter the population of interest in cohorts. The results highlight a strong positive joint effect of active and passive policies (+21 percentage points in employment, +63% with respect to control) after youths exit the program. During program enrollment, I show that part-time employment decreases in the first semester – when youths are busy in activation measures – while in the second semester the decrease is concentrated in income brackets where the cash transfer is phased-out with labor income. This suggests that cash transfers and lock-in from training reduce youth employment, but this is more than compensated by the positive effect of activation measures.

Keywords: active labor market policies, cash transfers, NEETs, job search, difference-in-difference

JEL Codes: J64, J68, C23

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1.1 Introduction

Youths who are neither in employment, education or training (NEETs) are a persisting problem in Europe[1]. To rescue NEETs, governments often resort to social protection, for example cash transfers and income support. Economists have long argued that such "passive" policies alone risk creating welfare dependence, without structurally changing individual behavior, or even decreasing job search (Moffitt, 1985; Card et al., 2007; Britto et al., 2020). For this reason, the combination of active and passive labor market policies is often advocated by international institutions (OECD, 2013; Pignatti and Van Belle, 2018), and governments are increasingly following this advice. Yet, what is the *joint* effect of active and passive policies? It is not guaranteed that active labor market policies will improve employability enough to compensate for a negative effect of passive policies, especially in the case of NEETs.

The economic literature has only evaluated the effect of active policies conditional on passive ones, or vice versa. A large literature summarized by Card et al. (2018) studied active labor market policies, finding that their effect depends a lot on the program type. Some of these estimates concern programs offered to receivers of passive policies, obtaining the effect of active policies conditional on a given level of passive ones. Vice versa, offering cash transfers to youth who would have anyway be exposed to activation measures might finance the opportunity cost of participation in the program (Heckman et al., 1999), but this doesn't always lead to more effective job search (Aeberhardt et al., 2020). Yet, to the best of my knowledge, no estimates exist of the effect of a program offering passive and active policies combined, rather than one on top of the other, making difficult to directly understand how much active policies can balance-out the effect of passive ones. Moreover, there are theoretical reasons to believe that the joint effect of active an passive polices might be larger than the sum of the effects of active and passive policies alone (Boone et al., 2007).

This paper fills the gap in the literature by evaluating the flagship French program for disadvantaged NEETs between 16 and 25 years old, *Garantie Jeunes*. The program combines a year of cash

of Economics, EEA-ESEM Congress, ADRES Doctoral conference, and EALE Conference for their constructive comments. This research has been possible thanks to technical support by the French Ministry of Labor and Social Affairs (*DARES*). The author acknowledges the financial support of the Norface Dynamics of Inequality Across the Life-course (DIAL) Joint Research Programme (file number 462-16-090), "Human Capital and Inequality During Adolescence and Working Life". The paper has been awarded the Best Poster prize at the 2022 AIEL Workshop on Labor Market Institutions.

[1]NEET rates in the last decade for youths aged 15-24 ranged between 12% and 22% in countries such as Spain or Italy, and were persistently above 10% in others, such as France. Higher levels were reported for women, less educated persons and foreign-born individuals. Economists have long wondered about the possible causes. Given that disadvantaged youths are more likely to become NEETs (Carcillo and Königs, 2015), some have posited that those who become NEETs face significantly higher job search frictions, lacking networks and soft-skills[2]. Moreover, NEET spells can become a poverty trap. In fact, unemployment has proven to be "scarring", in the sense that it can permanently harm one's employability (Oreopoulos et al., 2012; Schwandt and Von Wachter, 2019; Rothstein, 2019) as much as prematurely dropping out of formal education (Brunello and De Paola, 2014).

transfers equivalent to the French minimum income with intensive activation measures, namely soft-skills training for a month, regular counseling and short-term job experiences. My main results show that the combination of active and passive policies has a positive effect on employment and hours worked from the second year after exposure to the program, driven by youths who finished the program. In fact, I show that during enrollment in the program cash transfers and lock-in from training are associated to a reduction in youth employment, compensated by the positive effect of activation measures on search technology. When the program ends, only the positive effect of better search technology obtained through activation remains, driving the improvement in youth employability.

To identify the effects of the program, I exploit its staggered adoption by youth employment centers (YECs) in 2013-2017, and follow quarterly cohorts of youths over their time since registration with YECs. For estimation of the Intention-to-Treat (ITT) effect of *exposure* to *Garantie Jeunes*, I firstly use a simple fixed-effects model. Then, I develop a new "rolling" diff-in-diff methodology, which extends De Chaisemartin and D'Haultfœuille (2020a) to a setting where units enter the population of interest in cohorts, and it's robust to heterogeneous treatment effects. Finally, leveraging my new methodology, I regress ITT effects for a specific wave-cohort-time since YEC registration cell on the share of youths at specific stages of program enrollment, recovering dynamic LATEs since actual *enrollment* in the program.

The ITT estimates show that employment and hours worked for treated youth increase from the second year after they were exposed to the program. Instead, no significant effect is observed on wages. Crucially, the effect on employment and hours worked is entirely driven by youth who have completed the program, to which a 21 percentage points increase in employment is associated (+63% with respect to control) as well as an increase of 49 hours worked on a quarterly basis (+81% with respect to control). During program enrollment, the effect is instead zero or slightly negative on employment. It should be noted that the positive effect on employment after completion comes overwhelmingly from fixed-term contracts and agency jobs. Also, the program has considerable costs for public finances, almost as large as the benefits it generates, such that the Marginal Value of Public Funds (Hendren and Sprung-Keyser, 2020) is only slightly above one (1.19).

To disentangle the role of cash transfer and activation measures, I exploit the timing of the activation measures and the phase-out of the cash transfer. Time-consuming activities such as intensive training and job immersions are concentrated in the first semester of enrollment *Garantie Jeunes*. In addition, the cash transfer is cumulative with job earnings only up to €300, while it decreases by 0.55 cents for every euro earned between €300 and about €1100. During the first semester of enrollment, when youths are involved in intensive training and receive the cash transfer, I find a decrease in the probability of having job earnings below €300 or between €300 and €1100. In the second semester, when youths are out of the training but continue receiving the transfer, the decrease is concentrated in jobs earning €300-1100, where transfers are only partially cumulative with job earnings. I interpret this heterogeneity through the lens of a simple model of labor supply with discrete hours choice and search frictions. Under the assumptions of the model, cash transfers reduce employment mostly through implicit taxation, lock-in from training dents the probability of finding a job by about 40%, while activation compensates these negative effects by doubling the probability of finding the chosen job thanks to improved search technology.

The main contribution of this work is to offer evidence on the joint effect of active and passive labor market policies, while prior work mostly evaluated one component conditional on the other. For instance, Aeberhardt et al. (2020) evaluates a cash transfer of similar value and context as that of this paper, with no change in activation requirements, finding a non-significant effect on job search and a small negative effect on employment. In my program, where cash transfer are bundled with activation, I estimate a negative effect of cash transfers which is close to their finding. In turn, an extensive literature evaluates programs that increase activation measures, but not cash support, finding that in the medium term programs with a work-first approach improve employability, while more intensive forms of training risk a lock-in effect (Card et al., 2018). In the French context, some working papers indicate a large positive effect of job search assistance (Crépon et al., 2015) and of collective counseling (van den Berg et al., 2015) for youth. Compared to these papers, my results suggest a similar lock-in effect, but an even larger positive effect of better search technology, obtained through activation when jointly offered with cash transfers, persisting after program completion. Hence, when combined with cash transfers, activation not only compensates for lock-in and for the negative effect of passive policies, but also drives the positive effect of the program after completion. In the US context, a close setting to mine is the *Year-Up* sectoral training program, where youth didn't receive a cash transfer but were paid a stipend by partner employers to work after the training occurred. Katz et al. (2022) evaluate the program finding very similar lock-in from training and positive effects only after completion of the program.

Secondly, the results provide empirical insights on labor supply and job search of disadvantaged NEETs. Similarly to (Le Barbanchon, 2020; Saez et al., 2012), I highlight significant effects of implicit taxation, but the implied elasticity of earnings to net-of-tax rate which is very large, possibly due to larger reactions observed in sub-populations less attached to work (Card and Hyslop, 2005). Moreover, I empirically confirm the role of time constraints and search technology in activation policies (Gautier et al., 2018), finding that activation generates small lock-in but increases job finding. An implication of the large observed effect of activation search technology is that job finding is estimated low for untreated compliers. This speaks to several streams of the literature that show how lack of networks, geographical isolation and low soft skills can dramatically limit job search efforts on the part of disadvantaged youth[3].

My final contribution is methodological, as I extend De Chaisemartin and D'Haultfœuille (2020a) to a setting where individuals enter the population of interest in cohorts and are staggeredly exposed to treatment. An example of this circumstances can be staggered adoption of a restructuring program across schools, where students enter schools in cohorts corresponding to their class, as for example in Martorell et al. (2016). Similar instances can arise when programs affect different cohorts of workers entering firms, or cohorts of patients entering hospitals, with staggered exposure. In all these cases, the rolling diff-in-diffs approach I develop is useful when the researcher needs to apply heterogeneity-robust estimators.

Finally, as main policy implications the paper supports the importance of providing active and passive labor market policies jointly. Namely, cash transfers are shown to reduce employment, especially if the transfer is sharply phased-out with job earnings, but activation has a strong enough net effect to compensate for lock-in and for the negative effects of the cash transfers. While bearing in mind the limits in terms of external validity of my results, the insights I find are interesting also for policies using different combinations of the same ingredients, such as minimum income or unemployment insurance with activation measures. In the French context, the paper proves the effectiveness of a flagship labor market policy, promoting employability of disadvantaged NEETs. However, the gain is concentrated in precarious jobs, the costs of the program are also large, and given that the eligible population was selected on motivation it is not guaranteed that the program will remain cost-effective if its scope is extended, as it's currently happening.

The paper is constructed as follows. Section 2 provides the relevant institutional background and describes the program. Section 3 describes the data and sample selection process, and outlines the main identification strategy. Section 4 presents the results in terms of ITT and LATE, their heterogeneity according to contract type and youths characteristics, and the cost-benefit analysis. Section 5 exploits differences in timing of the program and the cash transfer phase-out to disentangle the mechanisms, namely the effect of better search technology obtained through activation, lock-in, disincentives from cash-on-hand and implicit taxation. Section 6 discusses the results in comparison with related studies. Section 7 draws policy implications and conclusions.

[3]See Ioannides and Datcher Loury (2004); Pellizzari (2010); Dustmann et al. (2016); Cingano and Rosolia (2012); Kramarz and Skans (2014); Marinescu and Rathelot (2018); Mendolia and Walker (2014); Schlosser and Shanan (2022)

1.2 Institutional Background

Garantie Jeunes was part of the European Union Youth Guarantee, which financed a number of national programs aimed at promoting youth employment, sharing the same name but having very different characteristics[4]. The French version of the program was launched in October 2013, co-financed by the French government, and targeted disadvantaged NEETs aged 16-25. The program lasts one year, and its outline is reported in Figure 1.1[5]. Upon enrollment, the participant is required to sign a contract of engagement, including penalties for not participating in the mandated activities. The early activation part consists of a six-weeks period of collective courses provided by 2 counselors, with 10-20 participants per class. The training is centered on job search and search frictions (*freins à l'emploi*) covering soft skills linked to job search (presentation skills, job search strategies, applications, CVs, motivation letters) but also personal habits and self confidence (learn to be timely, manage your health, plan your week, ...). There follows a ten-month period of job search assistance, with a personal counselor following the youth by phone, emails and interviews held once every 21 days on average. This second part is characterized by a "work-first" approach, i.e. frequent proposals of internships and short work experiences of at most a month, during which the youth works on small tasks in a partner firm with the aim of learning about the working environment and the industry.

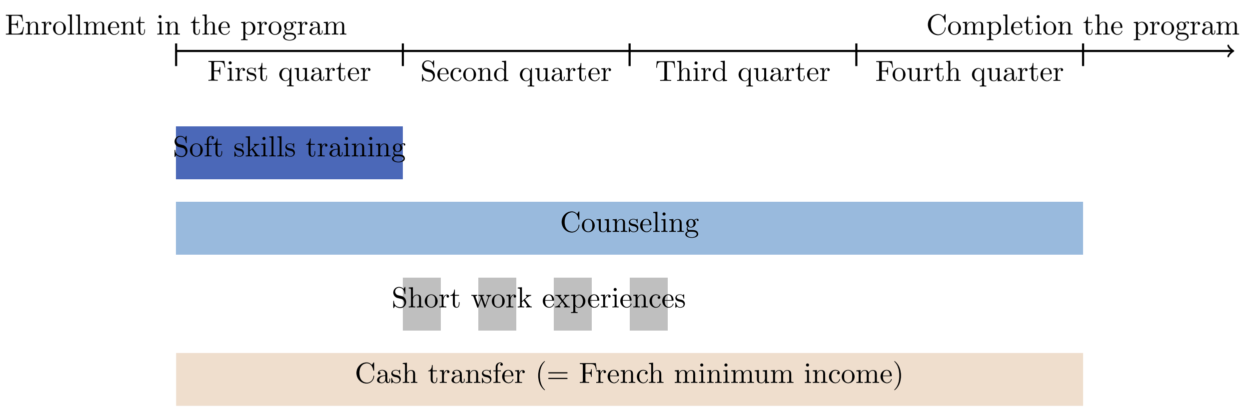

FIGURE 1.1: Outline of the program of *Garantie Jeunes*

[4]The concept of Youth Guarantee derives actually from a Nordic tradition of establishing a right to employment or training for youth entering the labor market. The EU channeled part of the European Social Fund toward financing nationally-defined implementation programs aiming at supporting employment of disadvantaged youth. There was quite some variability in focus and kind of the implementation programs at national level (Escudero and López, 2017; Escudero and Mourelo, 2018). Other counterfactual evaluations of country-specific initiatives include Bratti et al. (2017) and Pastore and Pompili (2019).

[5]While implementation details may vary in different youth centers (Gautié, 2018), the timeline of activities and income benefits observed in the data aligns quite well with the national guidelines (Figure A.2 in Appendix). It should be noted that according to Gautié (2018) the number of events reported in the administrative data of YECs under-estimates the number of effective events.

During the year of the program, youths receive a monthly cash transfer equal to the amount provided by the French minimum income scheme, which is annually updated. For example, it was €484.82 gross in April 2018. Importantly, if youth find a job before the end of the program, the cash transfers is not reduced until €300 of labor earnings. Above €300, cash transfers decrease proportionally with earnings until they reach zero at 80% of the French gross monthly Minimum wage (i.e. between €1,120 and €1,174 in the period considered). Most of the youths arrive until the end of the program, but 3% were expelled for not adhering to the terms of of the contract[6]. Such a combination of activation policies and generous cash transfers was considered quite innovative in the French context, and the design of *Garantie Jeunes* was done in light of evidence from previous experimental programs and evaluations of comparable policies (Gurgand and Wargon, 2013).

French local Youth Employment Centers (YECs)[7] are in charge of the administration of the program. These employment centers were introduced in the 1990s, and focus specifically on youths between 16 and 25, who are assigned to a specific YEC based on municipality of residence. A large number of youths registers to YECs, about half a million youths every year[8], for reasons independent from *Garantie Jeunes*. YEC registration is in fact often automatic for young unemployed and youth in the last year of professional schools, and it's required for several forms of subsidized training and employment, including the standard job search assistance program (*Contrat d'insertion dans la vie sociale*, CIVIS) which offers a modest number of required activities (Figure A.3 in Appendix). Importantly, YEC registration coincides with the beginning of job search for most of the youths, who are likely just out of school and are beginning to enter the labor market, so that their employment rate tends to rise from registration with YECs onward (Figure A.5 in the Appendix). Once youths register with a YEC, there is no formal de-registration, so youths can remain in contact with YECs for a variable amount of time and can come back if needed [9].

The introduction of *Garantie Jeunes* was staggered over time, which provides our source of identification. A pilot wave was launched in October 2013 in a number of areas selected as those with the highest reported NEETs rate among a set of volunteer territories. The program was then extended in six waves until it reached all volunteer territories in January 2016. Finally, after a preliminary evaluation, the program was extended to the whole French territory in January 2017. Figure 1.2

[6]Only 13% quits before the last quarter of the program. Of those who quit, roughly a third quits because they found a full-time jb or training, one-third quit for exogenous reasons (age, relocation), and the remainder split between unmotivated voluntary quit and sanctioned youth.

[7]*Missions Locales* in French

[8]Out of about 9 million youths aged 16-25 in France, this suggests that more than one-third of French youth register with YECs at some point.

[9]Figure A.4 in the Appendix indicates that 31.4% of youths are still considered active in a specific cohort of registration – meaning youths for whom the YEC records at least one action on their file during a quarter – 3 years from the time of registration. However, after 3 years since registration only 10.1% of the youth still records an action "youth toward YEC"[10], e.g. an email sent by the youth, an interview, or another activity with participation by the youth.

maps this process. Beside the seven official waves of extension,[11] some YECs delayed the introduction of the program, so that between 2013q3 and 2017q2 in every quarter except one there were some YECs adopting the program for the first time. Finally, it is important to note that YECs receive additional funding for administering *Garantie Jeunes* conditional on the number of youths enrolled (70% of the funding), on the number of youths who complete the program successfully (20%), and 10% conditional on the provision of complete data in their information system and proof of their correctness (e.g. enrollment documentation).

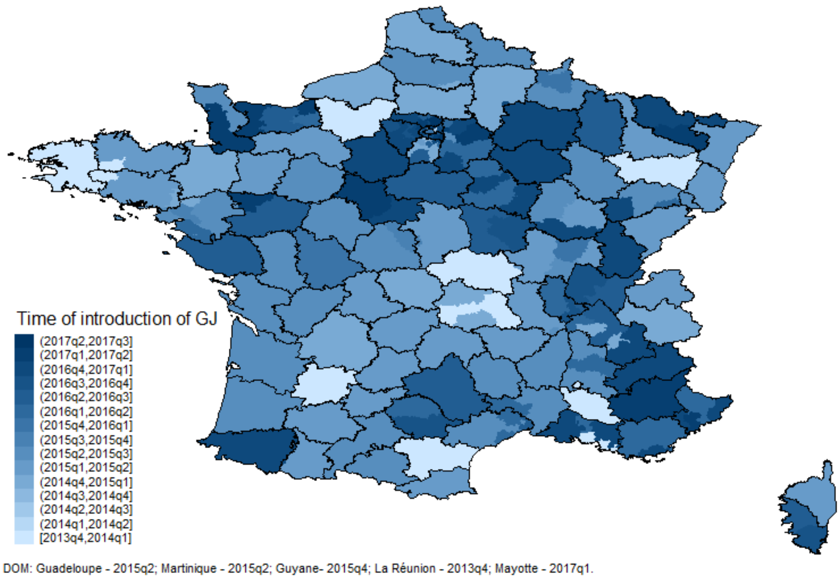

FIGURE 1.2: Progressive extension of *Garantie Jeunes*.

Notes. French municipalities (black borders correspond to *départments*) by quarter of first case of enrollment in *Garantie Jeunes* in their corresponding YEC. Overseas departments (DOM) are reported in the note.

Crucially, among youths registered at YECs, not all youths are eligible to apply, and not all who apply are selected for the program. Firstly, in order to be eligible, youths must either live in a household below the minimum income threshold (minimum income is not available for youths below 25 years old in France), have quit their parents or receive no support from them, have dropped out of school without a qualifying secondary school diploma, or be convicted[12]. Then, to enroll in *Garantie Jeunes* youths must demonstrate a condition of "fragility" and "motivation" through an application process. Qualitative reports describing this process argue that the first selection mechanism involved

[11]The pilot wave started in October 2013 q4, a second commenced in January 2015, a third in April 2015, a fourth in September 2013 q3, a fifth in March 2016, a sixth in September 2016, and, finally, the whole territory in January 2017.

[12]Young parents are not expected to be the target of *Garantie Jeunes*, since they are eligible to the French minimum income program RSA – guaranteed also to any individual in poverty from 25 years old onward – but are nonetheless not prevented to participate and 5% of *Garantie Jeunes* participants are reported to have kids.

selective targeting of youths by YECs, which often themselves organized information sessions and pitched the program to a selected group of registered youths. After an individual applies, the decision on the application is made by local independent commissions[13]. Eventually, youths who actually enrolled are roughly half of the eligible ones according to Gaini et al. (2018).

In the public debate, *Garantie Jeunes* is perceived as a successful program: since 2013 more than 500,000 youths have participated in the program, and the program got scaled-up as an answer to the Covid-19 pandemic, with the goal of doubling the number of enrolled youths by easing the up-front selection. In 2022, a new version of the program re-named *Contrat d'Engagement Jeunes* was hotly debated in the electoral campaign, and should cover all individuals earning below minimum income starting in March 2022.

1.3 Research Design

1.3.1 Data, Sample and Measurement

I build a novel dataset using two administrative sources. The first source is the administrative system of YECs, called I-Milo. This dataset reports details of programs and activities undertaken by the youth at the YEC or with partner firms, including the dates and duration of the events attended. In addition, the dataset includes socio-demographics of youth and additional information provided by youths when registering at YECs. For most individuals, I have information on housing difficulties, access to child-care services, mean of transportation used, and financial resources. I can also calculate the distance between youths' declared residency and the local YEC main office or satellite office[14]. The dataset covers all YECs from late 2010 until the present.

To follow the employment path of youth also when they are not in contact with YEC, I use as second source an extraction of French social security records. The dataset, which was prepared by the French Agency for Social Security under an agreement with the French Labor Ministry, includes information on all contracts signed during the period 2013-2018 by all youths who registered in YECs between 2013 and 2017. The available information includes date of start and termination of the contract, type of contract, total earnings and hours worked.

I merge these two sources to obtain a dataset covering all youths who registered with YECs between January 2013 and December 2016, approximately 2 Million individuals, following their employment

[13] These commissions are composed by a president appointed by the local representative of central government (*Prefecture*), one representative of the government of the departement, presidents of local YECs, and other members named by the *Prefecture*.

[14] The dataset also contains information on French or foreign language proficiency, skills, and hobbies, but only for smaller samples.

history and YEC activities from the time of registration with YECs onwards. The percentage of youth in our sample who earned less than secondary vocational qualification is similar to that of the overall French population, but a larger share of youth in our sample has at most a secondary diploma (about 52%, against a national mean of 44%). With respect to all youths 16-25 in France, the population of YECs is not significantly different in terms of share of females and French nationals. However, the population is characterized by early experience of activities which are typical of adulthood. On average, 35% of youth in YECs have experienced in the quarter preceding registration (national mean 30%), and 37% live independently (national mean 23%), 8% have children of their own (national mean 4%). Youths who got selected for *Garantie Jeunes* are instead not easily distinguishable from the pool of youth at YECs in terms of these observable characteristics, except that they have a much lower employment rate in the quarter before registration.

TABLE 1.1: Characteristics of the overall population, of youth in YECs (sample observed), of youth registering in the standard program of YECs, and in *Garantie Jeunes*.

	All youth (Census)	Youth in YECs	Youth in standard pr.	Youth in *Gar. Jeunes*
Number of youths (stock)	9327476	1967000	444659	118606
Youths inflow (quarter)		125689	41471	14899
Lower than secondary educ.	0.394	0.373	0.424	0.469
Upp. secondary edu. diploma	0.434	0.519	0.541	0.506
Avg. age	20.3	20.1	19.7	18.8
Female	0.491	0.491	0.511	0.461
French nat.	0.915	0.912	0.919	0.930
Empl. last quarter	0.297	0.349	0.335	0.211
Lives independently	0.230	0.365	0.369	0.352
Has children	0.0390	0.0838	0.0878	0.0498

Notes. The table compares the characteristics of youths in different population. The first column concerns all youths aged 16-25 in France, as reported by the Census in years 2013-2016. The second column reports all youths in the sample, namely all youths who registered at YECs in the 2013-2016 period. The third and fourth columns report, respectively, information on youth enrolling in the standard program offered at YECs, CIVIS, and on those enrolling in *Garantie Jeunes* at some point of their stay at YECs. All information from second to fourth column is measured at the quarter of registration at YECs.

For simplicity, I will aggregate time variables by quarters. Concerning the measurement of my outcomes, I calculate quarterly earnings and hours based on the duration of the contract, and trim values at 99%. For employment, I define a dummy equal to one if the youth has at least one hour of work reported in the quarter. Then, I define the cohort of registration at YEC as the quarter in which the youth first checks in at her YEC, and the wave of introduction of *Garantie Jeunes* as the quarter in which the first enrollment in *Garantie Jeunes* occurs in the YEC. Tables A.3-A.5 in the Appendix provide some descriptive statistics of the cohorts entering our panel.

1.3.2 Identification of ITT

Figure 1.3 reports a simplified illustration of our setting, including only 12 youths, in 4 cohorts of registration with YECs, and 3 different YECs, j_1,j_2 and j_3 adopting the program staggeredly. Each youth in the population is denoted by i. Youths belong to a cohort of registration with the YECs, $c \in \{2013q1, ..., 2016q4\}$, and each YEC belongs to a wave of introduction of *Garantie Jeunes*, $w \in \{2013q4, ..., 2017q1\}$. In my data, I am then able to follow each individual over calendar time t or, equivalently, over time since registration at the YECs $h = t - c + 1, h \in \{1, ...\}$, with $h = 1$ at time of registration. Each line represents the period from when the youth registers with YEC, and then gets "exposed" to the program (the red snaky line) when his YEC adopts the program (the gray shaded area). Recall that, even when exposed to the program, not all youths will enroll in the program, and not all of them will enroll at the same time.

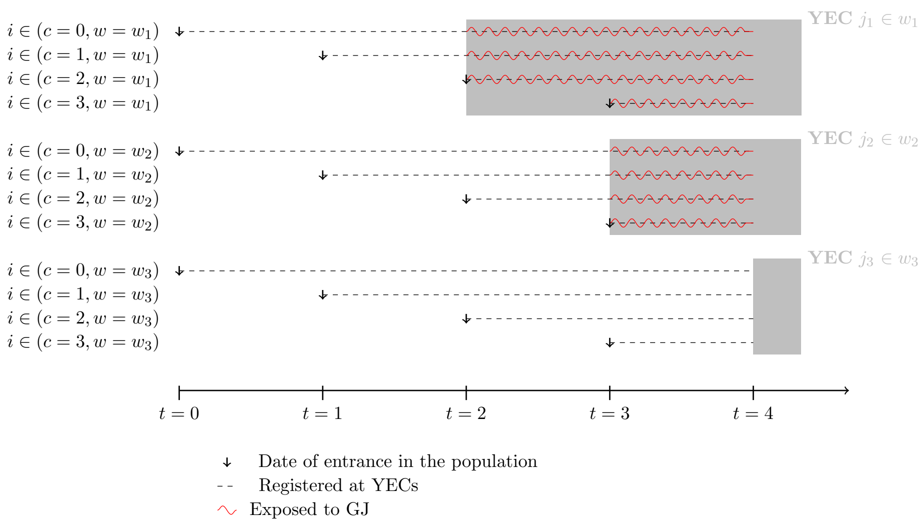

FIGURE 1.3: A simplified illustration of the setting.

The first parameter of interest is the intention-to-treat (ITT) effect of exposure to *Garantie Jeunes*, i.e. the average causal change in employment of a cohort as a function of the number of periods a youth could have applied to the program. Denote such number $G^h_{w,c} = g$, the periods of exposure to *Garantie Jeunes*, which is equal to either the time passed since adoption of the program or to the full time since registration with YEC, in case the youth registered with a YEC which was already offering the program: $g = \min(t-w, h)$. Denote $Y^h_{w,c} := \mathrm{E}(Y^h_{i,j,c} | j \in w, c, h)$, the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration. Our estimand corresponds to the expected value of the difference in outcomes when treatment exposure is g and when not exposed[15]:

$$\Delta^{ITT}(g) = \quad \mathrm{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0))$$

A common approach in the literature for identifying ITT of this kind is the event-study approach. This approach uses multiple-ways fixed-effects regressions to estimate dynamic treatment effects. Consider:

$$y_{i,t} = \sum_{g\neq 0} \beta^g \mathbb{1}(G^h_{w,c} = g) + \gamma_{c,h} + \mu_{j,h} + \epsilon_{i,t} \tag{1.1}$$

Where $\gamma_{c,h}$ is the cohort fixed effects, $\mu_{j,h}$ YEC fixed effects, all interacted with time-since-registration with YECs. By interacting all fixed effects with time-since-registration with YECs h, our model compares youths at the same time since registration with YECs, i.e. at the same point in their job search. Then, identification stems from comparing cohorts which have been exposed for g quarters to the program to cohorts not yet exposed. Standard errors are double-clustered at the YEC and time since registration level, following Cameron and Miller (2015)[16].

As an alternative, I propose a difference-in-differences estimator which has two advantages. First, it is *a-priori* robust to heterogeneous treatment effects over time and groups, unlike fixed effects estimators (De Chaisemartin and D'Haultfœuille, 2020a). Second, it allows us to flexibly obtain cell-specific ITTs and use them to study LATEs associated to individuals at a specific stage of enrollment in the program. Assumptions and propositions are detailed in Appendix A.2. In a nutshell, my estimator first estimates cell-specific $DID^h_{w,c}$, i.e. the expected outcomes of youths of a cohort who, h quarters after registration with YECs, have been exposed to the program, minus the earliest cohort from the same YEC not-yet exposed (first difference), minus the difference in outcomes in the same cohorts but in YECs where both cohorts are not-yet-exposed (second difference).

To get the intuition, Figure 1.4 reports in the left panel the observations used to estimate $DID^{h=1}_{w_1,c=2}$, the effect of one period of exposure to the program, which compares the evolution of the outcome

[15] An alternative option is to estimate what happens to the average employment rate in treated YECs. Yet, in Appendix A.1, I show that in settings like ours, where youth enter the population of interest in cohorts, classical difference-in-differences estimates of the effect *since adoption* of the program (the gray area in Figure 1.3) will be a mix of youths at different stages of exposure and enrollment in the program. If the program has dynamic effects or if youths self-select over time since registration with YECs, then the effect *since adoption* will be a mix which is difficult to interpret. Note also that the parameter of interest should not be confused with a variation in a survival rate, since we are looking at the probability of *being* employed at a specific point (reversible) in time and not at the probability of *having found* an employment by a specific time (irreversible), without requiring assumptions on the shape of the hazard function.

[16] I will also exclude from analysis cells where all waves are treated, and omit the coefficient with $g < -12$ to avoid rotations as suggested by Borusyak and Jaravel (2017)

across cohort 2 and baseline, for individuals 1 period after registration with YECs, in YECs where cohort 2 is treated and baseline is not vs. YECs where both cohort 2 and baseline are untreated. Accordingly, in the right panel of Figure 1.4, I show which observations will be used to construct $DID^{h=2}_{w_1,c=2}$, the effect of two periods of exposure to the program.

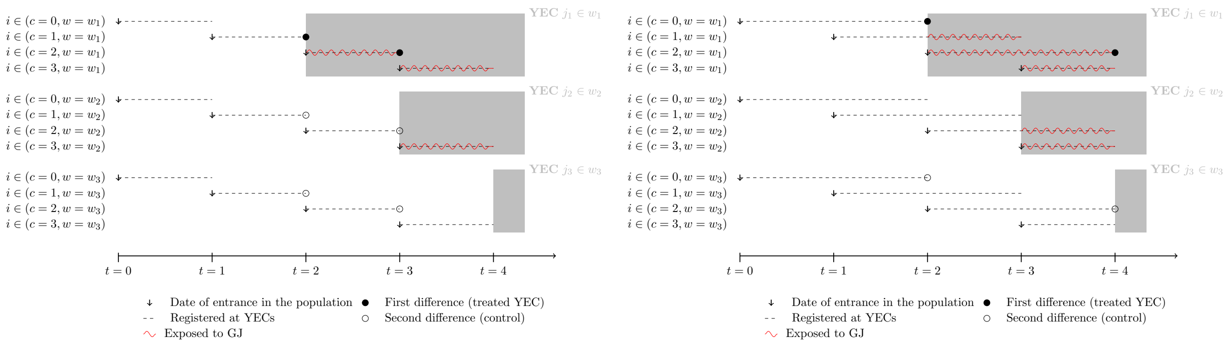

FIGURE 1.4: Examples of data used for estimation of the rolling difference-in-differences estimator. $DID^h_{w_1,c=2} = 1$ (left panel) and $DID^h_{w_1,c=2} = 2$ (right panel)

Then, I aggregate those $DID^h_{w,c}$ corresponding to the same level of exposure, obtaining an estimator of the ITT effect of being exposed for g quarters, DID^g. From a different angle, this methodology simply adapts the one by De Chaisemartin and D'Haultfœuille (2020a) to our cohort setting by comparing individuals at the same time since registration h, therefore "rolling" over h. Finally, I will obtain standard errors by bootstrapping, accounting for clustering at the level of treatment variation (YEC and time-since registration level), following the same algorithm of De Chaisemartin and D'Haultfœuille (2020b).

1.3.3 Identification of LATEs

As a second parameter of interest, I am interested in understanding the magnitude of the effect associated to being actually enrolled in *Garantie Jeunes*. First, I can estimate LATE on all compliers exposed for g quarters to the program:

$$\Delta^{LATE}(g) \quad = \quad \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(g)|D^h_{i,j,c} > 0)$$

Where $D^h_{i,j,c}$ is the number of quarter elapsed since a youth enrolled in the program, and, by construction, in w, c, h cells where $g > 0$ there is always at least one youth for which $D^h_{i,j,c} > 0$. Yet, $\Delta^{LATE}(g)$ tells the average program effect associated to any complier after a number of quarters they could have enrolled in the program (i.e. *exposure* to the program). In fact, we might be more interested in obtaining an estimate of the effect associated to compliers being actually at a specific stage of the program (i.e. by *enrollment* status in the program, for example if they are at the beginning, at the end, or after exiting the program):

$$\Delta^{LATE}(d) \quad = \quad \mathrm{E}(Y^h_{i,j,c}(d) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d)$$

My difference-in-difference methodology is particularly handy for recovering both LATE since exposure $\Delta^{LATE}(g)$ and LATE since enrollment $\Delta^{LATE}(d)$. Proposition 3 in Appendix A.2 points out that $\Delta^{LATE}(g)$ can be estimated by simple rescaling of ITT estimates by the share of compliers. This is not a novelty in IV estimation, but it is worth pointing out that the caveats highlighted by De Chaisemartin and d'Haultfoeuille (2018) don't apply because we always have at least one fully untreated wave and no defiers/always takers in the control group. Under more restrictive assumptions, and levaraging the definition of expectations, Proposition 4 in Appendix A.2 shows that we can recover $\Delta^{LATE}(d)$ using a Minimum Distance regression of cell-specific ITTs on the share of youths at different stages since enrollment in the program in that specific cell. Namely, I will recover LATE effects since actual enrollment in the program as the $\hat{\delta(.)}$ estimated from the regression:

$$\begin{aligned} DID^h_{w,c} = {} & \delta(0 < d \leq 2)Pr(0 < D^h_{i,j,c} \leq 2|h, w, c) \\ & + \delta(2 < d \leq 4)Pr(2 < D^h_{i,j,c} \leq 4|h, w, c) \\ & + \delta(d > 4)Pr(D^h_{i,j,c} > 4|h, w, c) + \varepsilon_{h,w,c} \end{aligned} \tag{1.2}$$

Where, to gain more power, I aggregated d into three classes: $0 < d \leq 2$, $2 < d \leq 4$ and $d > 4$, respectively the first semester of enrollment in the program, the second, and more than one year after enrollment. Regression 1.2 also clarifies the intuition behind this last step of my methodology: the $\hat{\delta(.)}$s are coefficients estimating how much the cell-specific ITT $DID^h_{w,c}$ changes following a change in the share of youths at a particular stage of the program in that cell.

1.4 Results

1.4.1 Balance Checks

An implication of the strong exogeneity assumption underlying our identification is that cohorts of youth entering YECs before and after the introduction of *Garantie Jeunes* are comparable. That is, the composition of youths registering to YECs doesn't change with the introduction of *Garantie* *jeunes*. In this section I exploit the wide range of information available in YECs administrative data to run a set of balance checks that test this hypothesis on a wide range of observable characteristics in YEC data. Table 1.2 reports a set of regressions of average characteristics of a cohort on a dummy for *Garantie Jeunes* adoption (Check 1), on a linear trend by quarter after adoption (Check 2), and on both the dummy and the linear trend together (Check 3). The results are reassuring: of the many variables evaluated, the only relevant concern is an increase in youths registering with housing problems, which increases by 0.6 percentage points over a mean of 10.5% before *Garantie Jeunes* introduction. It also appears that there was a mildly significant increase in the share of youth registering who have children, but the magnitude is again very small. All other characteristics of youths registering with YECs don't significantly change with *Garantie Jeunes* introduction, supporting the assumption that treatment status doesn't affect individuals' potential outcomes.

Table 1.2: Balance checks.

	(Check 1)	(Check 2)	(Check 3)		(Mean)
	GJ adopt.	GJ adopt.*q. adopt.	GJ adopt.	GJ adopt.*q. adopt.	
Share of female	-0.00111	-0.00031	-0.00145	-0.000371	0.491
	(0.00179)	(0.000392)	(0.00177)	(0.000388)	
Age at registration	0.0138	-0.000236	0.0135	0.000533	20.1
	(0.0121)	(0.00324)	(0.0127)	(0.00333)	
Lower secondary	0.00156	0.0000216	0.00157	0.000108	0.0744
	(0.00160)	(0.000375)	(0.00151)	(0.000354)	
Upper secondary education	0.0000462	0.000184	0.000258	0.000186	0.817
	(0.00239)	(0.000614)	(0.00236)	(0.000605)	
French nationality	-0.00212	0.000487	-0.00157	0.000368	0.912
	(0.00217)	(0.000511)	(0.00230)	(0.000538)	
Housing problems	0.00588***	0.000388	0.00633***	0.000716	0.0500
	(0.00157)	(0.000431)	(0.00175)	(0.00046)	
Resident in Urban Sensitive Area	0.000656	0.00298	0.0041	0.00301	0.105
	(0.00355)	(0.00211)	(0.0052)	(0.00220)	
Distance residency-YEC	-4.63	1.00	-3.44	0.752	715
	(3.48)	(1.43)	(3.75)	(1.43)	
Resources declared	1.11	0.400	1.56	0.461	155
	(2.26)	(0.780)	(2.59)	(0.815)	
Has a motor vehicle	-0.00386*	0.000128	-0.00371	-0.0000836	0.410
	(0.00233)	(0.000499)	(0.00239)	(0.000516)	
Lives alone	0.000523	0.000252	0.000814	0.000281	0.899
	(0.00217)	(0.000473)	(0.00223)	(0.000486)	
Has children	0.00154	0.000652*	0.00230*	0.000738*	0.0837
	(0.00119)	(0.000383)	(0.00125)	(0.000381)	
Problems with childcare	0.00621	-0.00122	0.00479	-0.000865	0.348
	(0.00620)	(0.00145)	(0.00609)	(0.00140)	

Notes. The table reports the coefficients of a separate regression of each characteristic of youths registering to YECs (listed in the first column) on a dummy for GJ introduction (Check 1), on a linear trend (Check 2), and on both (Check 3). The last column reports the mean of the variable before GJ introduction. The dependent variables used are cohort size (number of youths registering), share of females, average age of youths registering, share of registering youth with lower than vocational-secondary education, with at most vocational secondary, and with at most secondary education, share with French nationality, residency in disadvantaged zones, housing difficulties, average resources declared, and distance between residency and closest YEC office. I also exploit the abundant information in the administrative data of YECs to check balance for a dummy of whether the youth owns a motor vehicle, whether she lives independently, has kids, and, if so, if she has problems with childcare.

1.4.2 Main Results: ITT and LATE on Employment, Hours Worked and Earnings per Hour

I then proceed to estimate the ITT effect after being exposed g quarters to the program. Figure 1.5 reports the results obtained both using a fixed effect regression as in (1.1) and using our DID methodology . The first stage indicates that, while the coefficients before the introduction of the program are all omitted because nobody participates in *Garantie Jeunes* in YECs which are not yet treated (no defiers and no always takers), after exposure about 1% of youth enters the program each quarter, quite linearly over the first two years since exposure. This linear increase in first stage coefficients shows that compliers of a cohort are not entering the program all together as soon as they are exposed, but quite staggeredly over the time of exposure, with some youth entering the program much later, even 8 quarters after they have been exposed the first time. Turning to our outcomes of interest, coefficients on employment, hours worked and wages display a clear and long parallel trend in outcome variables between different waves before the introduction of the program, which reassures us on the validity of our identification strategy. After youth starts being exposed to *Garantie Jeunes*, there is still no significant differences in outcomes in the first 4 quarters of exposure. However a positive effect arises in employment and hours worked starting at the beginning of the second year after exposure. This dynamic of employment and hours worked can reveal a hint about the effect during the program vs. after completion, since four quarters after exposure of a cohort it's exactly the time when youths who entered *Garantie Jeunes* in the first quarters of exposure start completing the program. The effect also increases in the subsequent quarters, as more and more youth complete the program. Results using fixed-effects and our methodology are extremely close, suggesting that heterogeneous effects are not a concern and reassuring us about the validity of our methodology.

FIGURE 1.5: Intent to treat (ITT) estimates using the rolling diff-in-diff approach.

Notes. The figure reports results of the rolling diff-in-diff approach. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable is a dummy for exposure to *Garantie Jeunes*. The other three panels report the reduced-form coefficients: the dependent variables are employment, hours and wages (earnings per hour), while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation A.2. Cell-specific effects were obtained as in Equation A.1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

Table 1.3 reports the aggregated effects of the first semester, second semester and from second year of exposure. The average effect in the second year of exposure is +1.15 percentage points in employment probability, while hours worked increases by +3 hours on a quarterly basis. Wages (measured as average earnings per hour) are instead not significantly affected, remaining at a mean close to the minimum wage and with small standard errors. This suggests that the new jobs obtained by participants are mostly minimum-wage jobs.

Table 1.3: Intent to treat (ITT) estimates aggregated.

	Enrollment in GJ		Employment		Hours		Wage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	FE	DID	FE	DID	FE	DID	FE	DID
ITT 1st semester of exposure	0.000502*	0.0158***	0.00269**	-0.000452	1.239***	0.334	-0.0430	0.0292
	(0.000285)	(0.000634)	(0.00136)	(0.00131)	(0.356)	(0.529)	(0.0278)	(0.0797)
Total n.obs	4003538		4003538		3957848		1529554	
ITT 2nd semester of exposure	0.0171***	0.0401***	0.00126	-0.00328	0.643	-0.248	-0.0610	0.0162
	(0.000523)	(0.000699)	(0.00203)	(0.00217)	(0.537)	(0.604)	(0.0381)	(0.0564)
Total n.obs	3890678		3890678		3833155		1587769	
ITT 2nd year of exposure	0.0367***	0.0631***	0.00849***	0.0115**	2.365***	3**	-0.0179	0.0957
	(0.000694)	(0.000911)	(0.00277)	(0.00524)	(0.700)	(1.5)	(0.0484)	(0.0666)
Total n.obs	5574885		5574885		5472754		2373426	
Control mean 1st sem. in YEC			0.386		63.7		12.02	
Control mean 2nd sem. in YEC			0.468		99.3		11.85	
Control mean 2nd year in YEC			0.486		124.6		11.85	

Notes. The table reports the weighted averages of the $DID^h_{w,c}$ coefficients where exposure is between 1 and 2 quarters, between 2 and 4 quarters, or above 4 quarters. Columns (1), (3), (5) and (7) use a specification regressing the outcome on an indicator of exposure, YECs interacted with time-since-registration and cohort interacted with time-since-registration fixed effects. Standard errors are clustered at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level. Columns (2), (4), (6) and (6) use the rolling diff-in-diff approach outlined in Appendix A.2, where I estimate a full set of $DID^h_{w,c}$, for every $(w, c|h)$ cell, and then aggregate $DID^h_{w,c}$ corresponding to same levels of g. Standard errors are in parenthesis and obtained by bootstrap sampling with clustering at the YEC-time since registration level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Subsequently, in Table 1.4 I can estimate LATEs on all compliers, conditional on time of exposure to *Garantie Jeunes*, which allows to have a better sense of the magnitude of the effect. Compliers in the second year of exposure increase their probability of employment by 18 percentage points, and hours worked quarterly by 48. Then, using Equation 1.2 I can estimate how much of the effect is associated to compliers at different stages of program enrollment, which identifies dynamic LATEs since actual enrollment in the program. The estimates on employment and hours worked indicate that positive ITT effects in the second year of exposure are driven by the share of youth who has completed *Garantie Jeunes*. The LATE estimated on compliers in the second year after enrollment (LATE after completion) is +21 percentage points in employment and +49 hours worked. To get a better sense of the magnitude of effects, we can compare the estimated LATEs to average employment of compliers in the treatment group (as we have no way to identify compliers in the control group). Finally, by subtracting the estimated LATE from average employment of compliers in the treatment group, we can obtain the counterfactual employment rate for compliers were-they-not treated. The results imply roughly a 50% of employment probabilities and a 80% increase in hours worked after completing the program, compared to counterfactual expected outcome without treatment. Finally, also in terms of LATE there is no significant effect on wages.

TABLE 1.4: Local average treatment effects (LATEs) on all compliers at a particular point of exposure and by level of enrollment.

	Employment (1) DID	Hours (2) DID	Wage (3) DID
LATE 1st semester of exposure	-0.0282	20.8	1.83
	(0.0854)	(33.7)	(5.19)
LATE 2nd semester of exposure	-0.0819	-6.15	0.402
	(0.0538)	(14.9)	(1.41)
LATE 2nd year of exposure	0.183**	47.6**	1.51
	(0.0826)	(23.7)	(1.05)
LATE 1st semester of enrollm.	-0.0958*	5.44	-0.312
	(0.0559)	(14.4)	(2.73)
LATE 2nd semester of enrollm.	-0.0310	-1.66	0.315
	(0.0680)	(22.7)	(2.5)
LATE after completion	0.209**	48.6*	3.74
	(0.0962)	(26.7)	(2.32)
Compliers mean 1st semester in GJ	0.327	33.84	11.72
Compliers mean 2nd semester in GJ	0.408	59.75	11.85
Compliers mean after completing GJ	0.541	108.7	12.02

Notes. The upper panel reports reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages for compliers, obtained according to Proposition 3 a). The middle panel reports the LATE effect of being at different stages of *Garantie Jeunes*, obtained according to Equation 1.2. The lower panel reports average employment rates for compliers in the treatment group. Standard errors are bootstrapped and reported in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Although the LATE effects we find when youths finish the program are very high, some aspects reassure us on the credibility of the results. First, the results are extremely similar to the one found by the pilot evaluation by Gaini et al. (2018), who focused on the first wave and used a matched survey to estimate LATE of +22.2 in the probability of employment (over a control mean of 25%) on the fifth quarter after enrollment in the program[17]. Second, the results are driven by

[17]For the first quarter of exposure, our estimates are similar but not significant compared to Gaini et al. (2018). This can be linked to the fact that their design is different, and that I might lack power for estimating significant effects in the first quarter. Differently from them, I find estimates close to zero in the second and third quarter. This might be due to the fact that they use a survey question asking for "having worked at least one hour in the

very precarious forms of contracts, which can be more volatile. Table A.6 in the Appendix reports the ITT and LATE effect on employment in open-ended contracts, temporary contracts, agency jobs (quite frequent in this population) and apprenticeship. The effect on open-ended employment is very close to zero, while the overall employment effect mostly comes from temporary contracts (+.5 percentage points in ITT) and agency jobs (+.4 percentage points ITT). Apprenticeships also increase significantly, but they do so since the beginning of enrollment, suggesting that many youths are channeled into this type of contract also as a form of activation measure. Finally, I run heterogeneity by youth characteristics (Figure A.6-A.8 in the Appendix). The effect in ITT terms does not vary by gender, it's stronger for youth aged over 19 years-old, and it appears to be fully driven by youth with upper secondary education, as the ones with less than secondary education are likely channeled toward formal training rather than employment.

1.4.3 Cost-benefit Analysis

In this section, I compare the benefits to the costs of *Garantie Jeunes* by calculating the Marginal Value of Public Funds, following Hendren and Sprung-Keyser (2020):

$$MVPF = \frac{WTP}{NetCost}$$

Where WTP represents the aggregate willingness to pay for the program. By analogy with the work done by the same authors for estimating the MVPF for programs similar to *Garantie Jeunes*, such as the Job Corps program, I estimate WTP as the present value of the impact of the policy on after-tax income. This is given by the significant LATE effect on gross labor earnings the second year after enrollment in *Garantie Jeunes*, €828 quarterly in the year after completion, discounted by one year. Conservatively, I assume no effect from *Garantie Jeunes* at an horizon longer than one year after completion, since the literature suggests that job-search assistance has effects mostly in the short run (Card et al., 2018; Crépon et al., 2013a), and our heterogeneity analysis highlights the precarious nature of employment contracts obtained thanks to *Garantie Jeunes*. Concerning the costs associated with *Garantie Jeunes*, one should sum the direct cost of implementing the program for each youth and the opportunity cost of using YECs pre-existing infrastructure (classrooms, fixed admin personnel). I estimate the latter by calculating the average per-youth cost before *Garantie Jeunes* (Arambourou et al., 2016). The per-youth direct cost is instead covered by additional funding allocated to each YEC, consisting in €1120 per youth enrolling in the program, plus €320 after the youth completes the program or secures employment or formal training and €160 for data

quarter", while short work immersions (PMSMP) usually proposed to youths in the second and third quarter of *Garantie Jeunes* are not reported in our administrative data. Gaini et al. (2018) do not report results for hours of work and wages, so comparison with them is not possible.

reporting, hence a total of €1600 per youth. Given that only 17% of participants quit the program before the end for reasons not related to having found an employment or formal training (Gautié, 2018), I can estimate the net cost at €1964. The cumulated cash transfer received while in the program, calculated from the data at €4039 on average, is a simple transfer so it is added both to WTP and to net costs.

Under these assumptions[18], the MVPF of *Garantie Jeunes* is estimated at 1.19. In order to better benchmark this result, Figure 1.6 reports MVPF for all programs in the job training and cash transfer category analyzed in Hendren and Sprung-Keyser (2020) in the US. Compared to job training programs, the MVPF of *Garantie Jeunes* appears to be larger than the one for programs targeting youth, but quite in line with similar programs targeting the whole working-age population in financial difficulties (such as JTPA Adult). The MVPF of *Garantie Jeunes* appears also very similar to the one for cash transfer programs, although these programs usually target the whole working-age population and thus report an higher average age. Further comparison with other kind of programs such as adult education, which are less comparable to *Garantie Jeunes*, shows that *Garantie Jeunes* underperforms relative to the MVPF of policies supporting college attendance, which tend to have MVPF between 2 and infinity.

[18] First, to address potential substitution between programs (Kline and Walters, 2016) I assume that both the opportunity cost of the infrastructure and the cost-saving arising from substitution away from alternative programs is included in the extra funding guaranteed for each youth in *Garantie Jeunes*. Second, the estimated MVPF doesn't consider externalities. These can be both negative and positive. As an example of potential negative externalities, Crépon et al. (2013a) highlighted significant displacement effects in the French context for a population of young, educated, job-seekers. Positive externalities may instead arise from potential effects on social capital, health, or crime rates of target youth. Finally, time discounting is assumed exponential in the calculation of the present value of net earnings, with a discount rate of 3%, as in Hendren and Sprung-Keyser (2020). The MVPF falls to 1.13 when using a discount rate of 5% and to 1.09 when using a discount rate of 10%.

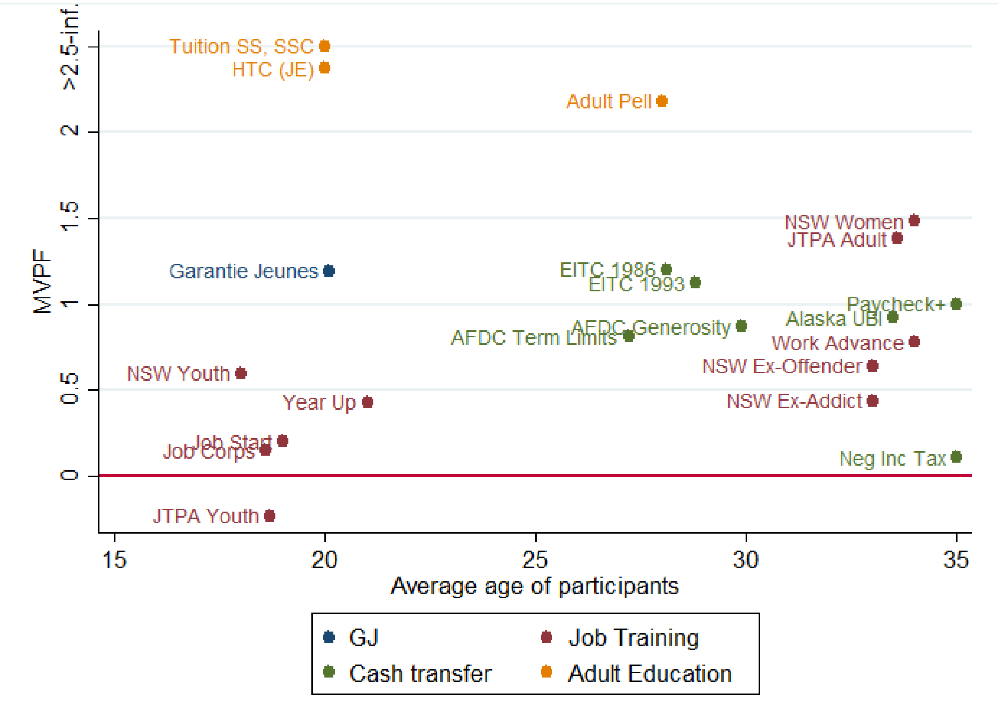

FIGURE 1.6: Marginal Value of Public Funds (MVPF Hendren and Sprung-Keyser, 2020) for *Garantie Jeunes* and for comparable programs, by average age of participants.

Notes. The figure reports the Marginal Value of Public Funds (MVPF) *Garantie Jeunes* and for programs in the"Job Training" and "Cash Transfer" categories analyzed by Hendren and Sprung-Keyser (2020) in the US context, plotted over average age of the participants in the program.

1.5 Disentangling the Role of Cash Transfers and Activation

1.5.1 Earnings at Different Stages of the Program

In this section, I interpret the mechanisms behind our reduced form by exploiting two dimensions of treatment variation: the schedule of activation measures and the cash transfer phase-out with job earnings, summarized in Figure 1.7. The left panel reports the number of working days with a scheduled training, interview or job immersion for participants in *Garantie Jeunes*, before and after enrollment in the program. In the first two quarters of the program, youths are busy 25 and 15 days in a quarter respectively, possibly lacking the time to actually look for a job ("lock-in" effect). This is due to the intensive collective training sessions held in the first quarter, and to job immersions that peak in the second quarter. The right panel reports instead the evolution of income with and without *Garantie Jeunes*. The cash transfer of *Garantie Jeunes* can be fully cumulated with job earnings up until €300 of net earnings. The transfer is then reduced quite steeply for every additional Euro of job earnings, until it disappears at 80% of the gross minimum wage (€1120 in 2013, €1159 on average in 2013-2016), where income with *Garantie Jeunes* equals income without. Hence, the phase-out of the cash transfer significantly flattens the schedule of monthly income with *Garantie Jeunes*: for every additional Euro earned the cash transfer is reduced by about 55 cents, implying 55% marginal tax rate and up to 40% average rate.

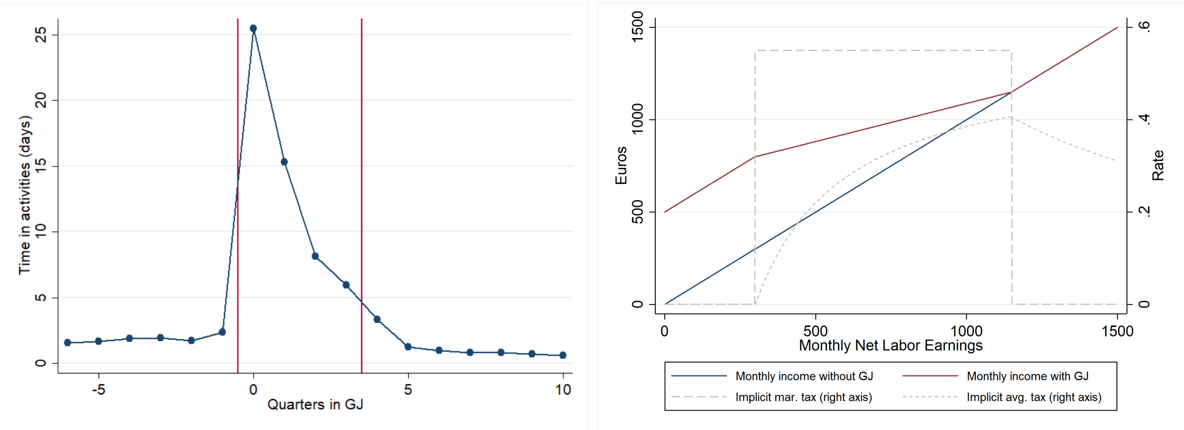

FIGURE 1.7: Working days with a scheduled activity as a function of time since enrollment in *Garantie Jeunes* (left panel) and cash transfer phase-out (right panel).

Notes. The left panel reports the estimated average working days with a scheduled activity as a function of time since enrollment in *Garantie Jeunes*. Source: I-Milo. The right panel shows the implicit marginal and average tax rate and the effect on the difference between monthly gross and net income. The figure is estimated from interpretation of the legislation.

Given these variations in the treatment, we aim at studying how the front-loading of time-consuming activation measures and the discontinuities in cash transfers cumulability are reflected in labor earnings of participants in *Garantie Jeunes*. Namely, I can use Proposition 3 to recover the LATE for individuals in the 1st semester, 2nd semester after enrollment, and after completion of *Garantie Jeunes*, using as outcome the probability of earning a monthly amount below €300, between €300 and €1100, or above €1100. Note that because €1100 roughly corresponds to monthly net earnings at a full-time minimum wage, earning a monthly amount below €300 or between €300 and €1100 corresponds respectively to very short part-time or agency jobs and to more consistent part-time jobs[19].

[19]For separating the second and third category, I will use a threshold of €1100 instead of €1159 (the precise average of 20% gross minimum wage in the period) since I want to avoid including in the previous class individuals bunching around the net minimum wage (which is slightly lower, especially at the beginning of the period). Note that an alternative option would be to look for bunching at €300. However, it is possibly difficult for youths to bunch sharply in terms of net earnings. Moreover, the resources are self-declared, so there might be a wedge between the actual earnings reported in our administrative data and those declared.

Table 1.5 reports the results. In the first semester after enrollment, when youths are busy in soft-skill training and activation policies, I find a significant decrease in the probabilities of part-time jobs, while no significant effect is found for the probability of earning over €1100. This could be interpreted interpreted as youths being as busy in activation measure as to reduce search effort and availability for less remunerative jobs, while still remaining open or targeting their search on full-time minimum-wage jobs. Then, once youths completed the most time-consuming part of the program, but are still eligible for the cash transfer, the estimated LATEs suggest an increase in the probability of earning below €300 and in the probability of earning above €1100, but also a strong decrease in the number of youths earning €300-€1100. This could be rationalized by a general increase in youth employability, and a negative reaction of youth to implicit marginal taxation on earnings in the €300-€1100 range. Finally, in the second year after enrollment, when youths completed the program and stop being eligible for the cash transfers, both the probability of earning in the €300-€1100 range and of earning above €1100 increase substantially. This corresponds to a generally positive effect of the program on employability and job quality after completion.

Table 1.5: Diff-in-diff estimates of the impact of *Garantie Jeunes* on the probability of declaring at least once in the quarter monthly job earnings in different income brackets.

Local Average Treatment Effect

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
	(1)	(2)	(3)
LATE 1st semester of enrollm.	-0.0674*	-0.0482*	0.0221
	(0.0359)	(0.0290)	(0.0361)
LATE 2nd semester of enrollm.	0.0846**	-0.146***	0.129**
	(0.0431)	(0.0544)	(0.0577)
LATE after completion	-0.0863	0.188***	0.197**
	(0.0618)	(0.0700)	(0.0793)

Average outcomes of takers in treatment group

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
1st semester of enrollm.	.068	.042	.119
2nd semester of enrollm.	.091	.085	.211
After completion	.101	.153	.336

Notes. The table reports estimates of LATE effects obtained using Proposition 3b and Equation 1.2, using as outcome the probability of earning in different income brackets. Standard error are reported in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The lower panel reports the average outcomes estimated for the compliers of the treatment group. Estimates are obtained using Equally Weighted Minimum Distance.

1.5.2 A Framework for Formally Disentangling the Mechanisms

Yet, to causally interpret the coefficients of Table 1.5 as the result of variations in cash transfers and activation measures in Figure 1.7, one needs a more formal set of restrictions on the channels through which cash transfers and activation measures can have an effect on labor earnings. In the literature, activation measures are mostly considered as affecting job search. Gautier et al. (2018) model the impact of activation measures on search effort, assuming that participation in activation programs improves the matching technology (i.e. increases the number of applications sent per unit of time) but requires limited time and effort and hence risks reducing the amount of job search. In turn, passive policies typically influence the amount of earnings through the elasticity of labor supply, by changing the relative utility of employment/unemployment (Card et al., 2007; Chetty, 2008; Le Barbanchon, 2020). Saez et al. (2012) reviews the literature on the topic and points out that compensated earning elasticities of labor supply are generally small (0.1-0.5), but larger effects are observed for workers less attached to labor force, including low income earners in welfare programs (Card and Hyslop, 2005).

In light of this, let us first model how cash transfers affect labor supply in the context of *Garantie Jeunes*. Suppose that wages are given and equal for all individuals so that, for each period, youth maximize utility from choosing gross working earnings $z^t \in \{z^0, z^1, z^2, z^3\}$. The last three brackets correspond to those of Table 1.5, i.e. working earning €1-300, €300-1100, >€1100 per month, while z^0 corresponds to unemployment. Since €1100 is roughly the minimum wage, and wages are assumed equal for all individuals, z^1 includes those who work by the hour for short time, discontinuous jobs or low-intensity part-time (e.g 5-10 hours per week), z^2 corresponds to less discontinuous jobs or normal part-time, and z^3 corresponds to full-time employment. The assumption of fixed wages is strong but plausible, because takers of *Garantie Jeunes* mostly work at the minimum wage, given that the estimated effect of the program on wages is non-significant (Table 1.4), and because participants are few with respect to the overall population of minimum-wage earners (hence general equilibrium effects are unlikely).

Let *cash* be a dummy for being enrolled in the year-long *Garantie Jeunes* program, thus having the right to receive the cash transfer, which is equal to one for compliers when they are enrolled in the program, and equal to zero otherwise. Assume that utility for individual i and choice j is linear:

$$\begin{aligned} &U_{ij} = u_j(cash) + \eta_i \\ &\text{where} \quad u_j(cash) = a_1(z_j + cash \cdot (b - \min[b, \max[0, (z_j - 300)\tau]]) + a_2 z_j / w \end{aligned} \tag{1.3}$$

In such expression a_1 is the marginal utility of consumption, a_2 is the marginal utility of leisure, b

is the amount of the cash transfer from *Garantie Jeunes* (€484.82 gross in April 2018), τ is implicit marginal taxation due to the phase-out (55%), and η_i is individual heterogeneity. The dummy variable *cash* indicates if the youth is enrolled in Garantie Jeunes, hence receiving the cash transfer if she earns less than the minimum wage each month. Denote $\alpha_j = a_1 z_j$, $\beta = a_1 b$, $\gamma_j = a_2 z_j / w$. Let consumers maximize the utility of their desired employment so that $U_{j^*i} > U_{ji} \quad \forall j \neq j^*$. If η_i is distributed as extreme values, then McFadden et al. (1973) shows that $Pr(z^{j*} = z^j) = \frac{e^{u_j}}{\sum_j e^{u_j}}$. Hence:

$$Pr(z^{j*} = z^j) = \Phi_j(cash)$$

$$\text{where} \begin{cases} \Phi_1(1) = \frac{e^{\alpha_1+\beta+\gamma_1}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_1} e^{\beta} \\ \Phi_1(0) = \frac{e^{\alpha_1+\gamma_1}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_1}}}{K_0} \\ \Phi_2(1) = \frac{e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}}{e^{\alpha_0+\beta}+e^{\alpha_1+\gamma_1+\beta}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_1} e^{\beta-(\alpha_2-300a_1)\tau} \\ \Phi_2(0) = \frac{e^{\alpha_2+\gamma_2}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_2}}}{K_0} \\ \Phi_3(1) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0+\beta}+e^{\alpha_1+\beta+\gamma_1}+e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_1} \\ \Phi_3(0) = \frac{e^{\alpha_3+\gamma_3}}{e^{\alpha_0}+e^{\alpha_1+\gamma_1}+e^{\alpha_2+\gamma_2}+e^{\alpha_3+\gamma_3}} & = \frac{e^{\hat{\alpha_3}}}{K_0} \end{cases} \tag{1.4}$$

Where $\hat{\alpha_j} = \alpha_j + \gamma_j$ is the net value of choice j when there are no cash transfers, $K_0 = e^{\alpha_0} + e^{\alpha_1+\gamma_1} + e^{\alpha_2+\gamma_2} + e^{\alpha_3+\gamma_3}$ and $K_1 = e^{\alpha_0+\beta} + e^{\alpha_1+\gamma_1+\beta} + e^{\alpha_2-(\alpha_2-300a_1)\tau+\gamma_2+\beta} + e^{\alpha_3+\gamma_3}$.

Then, I introduce job search with activation measures. Suppose that the probability of being employed in a bracket j is equal to the product of the share of youth who supply labor in that bracket times the probability of obtaining a job instead of remaining unemployed $P(.)$. Following Gautier et al. (2018), I impose $P(.)$ to depend on whether the youth has improved his job search technology thanks to activation measures (*tech*) and on time spent searching (*time*). I call the activation term "search technology" as it will be sort of a residual of the effect on employment when youths are activated, so that *tech* is equal to zero in the control group, and equal to one for treated youth, who receive soft-skills training, counseling and network opportunities with *Garantie Jeunes*[20]. Finally, the dummy for time availability *time* is equal to one as a default and equal to zero in the first semester of enrollment, when the youth must attend activities offered at the YECs risking the so-called lock-in effect. As Figure 1.7 suggests, this is the case in the first semester of

[20]The relationship between *tech* and $P(.)$ is ambiguous *ex-ante*: although I might expect that the knowledge derived from activities provided by *Garantie Jeunes* improves search efficacy, it could also disorient the youth (choice overload), or make him overconfident, or represent a stigma, decreasing the probability of finding employment. Note that *Garantie Jeunes* could also increases η_i. I tend to exclude the hypothesis that *Garantie Jeunes* leads to shocks to η_i since I find no effect on wage per-hour worked.

enrollment in *Garantie Jeunes*[21].

$$Pr(Y_{ji} = 1) = Pr(z^{j*} = z^j) \cdot P(tech, time) \tag{1.5}$$

At this point, we can plug Equation 1.4 into Equation 1.5, obtaining $Pr(Y_{ji} = 1)$ for compliers of *Garantie Jeunes*, as a function of different labor supply factors and of search frictions $P(tech, time)$. This expression will vary for every income bracket j, at different stages of the program, and according to individuals being in treatment or control group, as summarized in Table 1.6. For understanding our interpretation, let us start from the control group (lower panel of Table 1.6). In the control, compliers are not exposed to *Garantie Jeunes* and cannot enroll, hence $cash = 0$ for all youth and brackets. They also don't receive activation measures, so $tech = 0$, and they don't risk to not have enough time to look for a job due to lock-in, so $time = 1$. Thus, the $P(tech, time)$ term representing job search is $P(0, 1)$ in the control group.

Turning to treated compliers, in the upper panel of Table 1.6, during the first semester of enrollment $cash = 1$, as youths are receiving the cash transfer. Note also that $\Phi_1(1) = \Phi_1(0)\frac{K_0}{K_1}e^{\beta}$ in the upper left cell of the upper panel of Table 1.6: intuitively, labor supply for the €1-300 bracket with cash transfers is equal to labor supply without cash transfers times the effect of cash transfers e^{β} rescaled by $\frac{K_0}{K_1}$. Turning to the probability of finding a job $P(tech, time)$, in the first semester of enrollment youths are receiving activation measures, hence $tech = 1$, and risk lock-in as they might be too busy to effectively look for a job, so $time = 0$. Now, moving to the second column in the first line of Table 1.6, labor supply in the €300-€1100 income bracket reports an additional term, $e^{-(\alpha_2 - 300a_1)\tau}$, which is the effect of implicit taxation on earnings arising from the phase-out of the cash transfer. Finally, in the third column, labor supply differs with respect to control group only due to the term $\frac{K_0}{K_1}$. This term can be interpreted as the option value or the spillover effect of cash transfers, as it multiplies labor supply in all brackets, independently from whether youth are actually receiving cash transfers or not. In fact, cash transfers are zero for jobs above €1100, but youths might still reduce labor supply in this bracket as the other options become relatively more attractive.

Turning to the second semester of enrollment, in the second line of the upper panel, all terms remain the same as in the first line except that $time = 1$, because youth in the second semester of enrollment have completed activation measures and have time to dedicate to job search. Finally,

[21] Note that Equation 1.4 derives from consumers maximizing their utility as-if search frictions did not exist. That is, they choose the optimal employment they will look for only as a function of *cash* and η_i, without considering that they could have more/less probabilities of obtaining the job. This corresponds to fully separate the channel of the cash transfer (labor supply) and of activation measures (search frictions). Although this structure might appear simplistic, it is useful as an extreme case. Also, in the context of inexperienced youth this hypothesis might be realistic that youth only care about their direct incentives to supply labor, failing to incorporate the risk of not being hired

after completion of *Garantie Jeunes* youth stop receiving cash transfers, and labor supply of treated compliers is the same as in the control group. The term representing the probability of finding a job $P(.)$, however, has still $active = 1$, reflecting the fact that youth have now a better search technology thanks to activation measures.

TABLE 1.6: Structural interpretation of the probability of employment in different income brackets, $Pr(Y_{ji} = 1)$, for compliers in treatment and control groups, at different stages of the program.

$Pr(Y_{ji} = 1)$ in treatment group

	Monthly income €1-€300	Monthly income€300-€1100	Monthly income over €1100
1st semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta} \cdot P(1,0)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300a_1)\tau} \cdot P(1,0)$	$\Phi_3(0)\frac{K_0}{K_1} \cdot P(1,0)$
2nd semester of enrollm.	$\Phi_1(0)\frac{K_0}{K_1}e^{\beta} \cdot P(1,1)$	$\Phi_2(0)\frac{K_0}{K_1}e^{\beta-(\alpha_2-300a_1)\tau} \cdot P(1,1)$	$\Phi_3(0)\frac{K_0}{K_1} \cdot P(1,1)$
After completion	$\Phi_1(0) \cdot P(1,1)$	$\Phi_2(0) \cdot P(1,1)$	$\Phi_3(0) \cdot P(1,1)$

$Pr(Y_{ji} = 1)$ in control group

	Monthly income €1-€300	Monthly income €300-€1100	Monthly income over €1100
No program	$\Phi_1(0) \cdot P(0,1)$	$\Phi_2(0) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$

Notes. The table reports structural interpretation of $Pr(Y_{ji} = 1)$ the probability of being actually employed in bracket j conditional on enrollment status in *Garantie Jeunes*. It is obtained from Equation 1.4 and Equation 1.5.

Now, the goal is to fit the structural interpretation to our results and estimate the role of each component. Notice in fact that our estimates in the lowest panel of Table 1.5 provide an empirical counterpart to every moment in the upper panel of Table 1.6. Moreover, I can subtract estimates of the LATEs to average outcomes for compliers in the treatment group in Table 1.5 to recover estimates of average outcomes for control group compliers (Imbens and Rubin, 1997), corresponding to moments in the lower panel of Table 1.6. For example, having an estimate of $\mathrm{E}(Y_{ji}(D^h_{i,j,c})|0 < D^h_{i,j,c} \leq 2)$ and of $\mathrm{E}(Y_{ji}(D^h_{i,j,c})|0 < D^h_{i,j,c} \leq 2) - \mathrm{E}(Y_{ji}(0)|0 < D^h_{i,j,c} \leq 2)$, I can recover $\mathrm{E}(Y_{ji}(0)|0 < D^h_{i,j,c} \leq 2)$. In sum, by equating each of the estimated average outcomes for compliers in treatment and control to their structural interpretation in Table 1.6, I obtain a system of 18 equations, which I can use to solve for our effects of interest $P(1,0)/P(1,1)$,$P(1,1)/P(0,1)$, β, $(\alpha_2 - 300a_1)\tau$,and K_0/K_1. $P(1,0)/P(1,1)$ and $P(1,1)/P(0,1)$ are respectively the lock-in effect (having $time = 0$ w.r.t. $time = 1$, keeping $tech$ constant) and the effect of activation (having $tech = 1$). Then, e^{β} represents the effect of receiving cash-on-hand (moral hazard/liquidity effect, as we cannot distinguish the two), while $e^{-(\alpha_2-300a_1)\tau}$ represents the effect of implicit taxation, and K_0/K_1 captures the spillovers of cash transfers. Note that all these estimates are to be interpreted as multiplicative factors of the probability of employment.

Results are reported Table 1.7. Because the system is over-identified, I either aggregate the different estimates of the parameters by averaging them (the detailed procedure is reported in the Appendix), or I estimate the results by nonlinear least squares. Column (1) shows the results when different estimates of the parameters are simply averaged. Alternatively, one might want to take into consideration the different levels of significance of the underlying LATEs, so Column (2) of the table reports the estimates using a weighted average, weighting by the average of the inverse of the standard errors squared of the LATEs used to derive the components of the effect. Finally, the estimates obtained with weighted Nonlinear Least Squares are reported in Column (3).

Table 1.7: Estimated net effects of cash (implicit tax, cash-on-hand, and spillovers) and activation measures (lock-in and search tech.) – multiplicative effect on $E(Y_{ji})$.

Effect (interpretation)	(1)	(2)	(3)
$e^{-(\alpha_2-300a_1)\tau}$ (implicit tax)	.226	.146	.523
e^{β} (cash-on-hand)	.967	.989	1.100
$\frac{K_0}{K_1}$ (cash tr. spillovers)	.628	.627	.992
$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.601	.600	.565
$\frac{P(1,1)}{P(0,1)}$ (search tech.)	2.053	2.162	2.125
Method	Avg. of estimates	Weighted avg. of estim.	Solve system by wNLS

Notes. The table reports the estimated structural parameters obtained by equating the structural interpretation in Table 1.6 to the average outcomes of compliers in treatment (estimated from the data) and of compliers in the control group (obtained by subtracting the effect in Table 1.5 to average outcomes of compliers in treatment). In column (1) and (2) the effects are obtained by solving for the effects and averaging the different estimates, with or without weights for inverse standard errors of LATE terms involved, as detailed in the Appendix. In column (3) normalizing α_0 provides 8 linearly independent equations and 8 unknowns (leftmost column) which can be estimated and used to recovered the distribution of $Pr(z_{j*} = z_j)$ and effets of different components of *Garantie Jeunes*. The effects in the last column are multiplicative.

The results concerning the effect of implicit taxation show that implicit taxation drives away enrolled youths from the implicitly-taxed brackets, reducing employment by 48%-85% depending on the estimation method. In fact, the first row of Table 1.7 suggests that the presence of implicit taxation multiplies expected employment by a factor ranging between .146 and .523. Concerning instead the effect of cash-on-hand, the estimated multiplicative effect is very close to one, signaling an insignificant role of this aspect. The large effect of implicit taxation and the relatively smaller reaction to cash transfers availability can be seen as suggestive evidence that cash-on-hand effects are contained in *Garantie Jeunes*.

Turning to the effect of activation measure on search technology, the results point at a negative lock-in effect, reducing expected employment by about 40%. This shows that youths participating in the program face significant time constraints. Finally, the positive effect of activation on youths search technology is highly positive, corresponding, on average, to more than doubling employment in cells where youths have been activated. An implication of this large effect is that the probability of finding a job for compliers were-they-not treated is very low. This points out that disadvantaged NEETs who are the target of *Garantie Jeunes* have very low matching probability without the program, either because they face high search frictions, or because they would exert low effort in absence of the program.

1.6 Discussion

Section 4 estimated the reduced form effect of jointly providing a year of cash transfers and activation measures in the context of *Garantie Jeunes*. The estimated coefficients suggest a null effect during enrollment in the program and a significantly positive effect after completion. Subsequently, Section 5 disentangled the role of cash transfers and activation measures by exploiting variations in the timing of activities and in the phase-out of cash transfers with job earnings. Under the assumptions of a discrete-choice model, I estimated a negative effect of cash transfers and a compensating positive effect of activation. In this section I discuss these results in comparison with closely related studies in the same context of *Garantie Jeunes*, i.e. French YECs and disadvantaged youth.

On the one hand, a work closely related to mine is Aeberhardt et al. (2020). This working paper studies the effect of an increase in cash transfers but keeps activation measures constant, in the same context as that of this paper. The authors consider an experimental program introduced in a small set of French YECs in 2011, which offered a similar cash transfer to youths in the standard YECs program, but no extra activities. The cash transfer was equivalent to that of *Garantie Jeunes* in terms of cumulative amount, but was spread over two years rather than one. Moreover, the transfer was reduced proportionally to job earnings since the first euro earned, not since 300 Euros as in *Garantie Jeunes*. Hence, the monthly amount of the transfer and the rate of implicit taxation were roughly half than in *Garantie Jeunes*. Crucially, in the setting of Aeberhardt et al. (2020) youths are only required to attend the standard program at YECs, that was available also for control group youths[22] Aeberhardt et al. (2020) find that the program they evaluated increased the amount of

[22] It should be noted that there are additional sources of difference with their study. A first one might be selection of the compliers, since in *Garantie Jeunes* eligible youths are selected on motivation and fragility, requiring a sunk cost of application, while in the setting of Aeberhardt et al. (2020) all youths in randomly selected YECs and cohorts are offered the cash transfer with no anticipation by them. Or, the commitment by YECs in implementing *Garantie Jeunes*, which was for them a structural change and a political spotlight, might have played a role, while for the experiment of Aeberhardt et al. (2020) YECs were mostly running business as usual. For instance, Aeberhardt

time youth stayed at YECs, and increased attendance at compulsory activities. Yet, the effect on search effort was null, while employment decreased between 7% and 13% in the first year of the program. Interestingly, their estimated effect on employment can be very closely replicated using the model I estimated in Section 5.3, as proved in the Appendix. This result corroborates the validity of my simple model, and the estimated negative effect of cash transfers that results from it.

On the other hand, there exist two working papers which study a shock to activation measures but not to cash transfers in the French context. First, Crépon et al. (2015) study the effect of job search assistance targeting disadvantaged youth aiming at entering apprenticeship. They exploit an experimental increase in the number of invitations that the youth would receive from YECs to counseling meetings, generating an increase of about three times of the number of meetings (up to one 1.25 every month). Counselors at YECs are also instructed to re-focus job search assistance on apprenticeship contracts. The result of this activation program, which is relatively modest compared to activation measures in *Garantie Jeunes*, is an increases in the probability of signing a contract of around 20%, almost totally driven by apprenticeships contracts. The effect is caused by an increase in the returns from applications (i.e. an increase in the search technology), since the number of applications is actually unchanged. Second, (van den Berg et al., 2015) study a switch from individual intensive counseling to collective job clubs, similar to the ones occurring in the early phase of *Garantie Jeunes*. They show that, in the case of disadvantaged young jobseekers in France, switching from individual to collective counseling further increases employment by 10% (and permanent employment by 28%).

The result of a negative effect of cash transfers alone found by Aeberhardt et al. (2020) is consistent with the results of this paper. In their setting, the model estimated in Section 5 would predict a mild negative effect of cash-on-hand on employment, and no positive effect of activation and lock-in. An additional negative effect from implicit taxation (which is less than half in their case) could be occurring, but it's hard to detect it in their setting where no kink is present. The effect of activation measures at YECs without additional cash transfers, such as job search assistance or job clubs, estimated by Crépon et al. (2015) and van den Berg et al. (2015), is also consistent with our results, since both studies signal positive and large effects of collective and individual activation measures. However, the magnitude of the effect we find is larger, concentrated after completion, and increased further when netting-out the effect of cash transfers and lock-in in Section 5. Thus, there seems to be an additional wedge between the joint effect of activation measures and cash transfers,

et al. (2020) report a large drop in take-up after the first year of enrollment, when youth employment centers have to actively renovate the contract with the youth, checking the respect of activation conditions. For comparison, in *Garantie Jeunes* counselors are required to check monthly, and to provide detailed proof to central government (e.g. work contracts of the youth, proof of attendance).

estimated in this paper, and the sum of the two conditional effects of either an increase in activation or in cash transfers or in activation measures, estimated in the literature. This wedge could arise from potential complementarities between active and passive labor market policies, arising only when the two are jointly provided. For example, Boone et al. (2007) suggest that activation can function as a monitoring device, and conditional cash transfers represent the potential loss in case of sanctions for not respecting conditionality. Alternatively, complementarities might arise from sources other than monitoring, for instance if cash transfers enable youths to exert effort in activities, e.g. if they are credit constrained and need to work when not attending the training sessions.

1.7 Conclusions

In this paper I studied the effects of a labor market policy combining an intense activation program and generous monthly cash transfers to young disadvantaged NEETs. The results point in the direction of a strong positive effect of the program on employment and hours worked of participants, starting the year after completion, and no effect during enrollment in the program. The increase in employment is however driven by precarious contracts such as fixed-term contracts and agency jobs. I show that the results can be explained by a negative effect of cash transfers, particularly through implicit taxation, and a positive one of activation. The positive effect of activation compensates for lock-in and for the negative effect of cash transfers during enrollment in the program, and drives the positive effect after completion.

This work speaks chiefly to the literature on employment policies. Prior research has mostly evaluated active policies, such as activation measures, conditional on a given level of passive policies, such as cash transfers, and vice versa. This paper provides the first evidence of the joint effect of cash transfers and activation measures. The results suggest a large positive joint effect of active and passive policies after completion of the program. The effect is determined by activation compensating for a negative effect of cash transfers. The large magnitude of the effect of activation signals a significant role of search frictions for this population, with control youths facing very low matching probabilities. Secondly, the results provide empirical insights for the literature on labor supply and job search behavior. I estimate a 52% reduction in employment as a reaction to a 55% increase in implicit taxation from benefits phase-out, implying an elasticity to net-of-tax rate between .4 and .8 for this very specific population. I also confirm the role of time and activation in determining job search efficacy, as in Gautier et al. (2018). As a final methodological contribution, my rolling diff-in-diff methodology is relevant for studies where units enter the population of interest in group-cohort cells, and are exposed to treatment at different tenures. When a treatment is adopted by these groups in a staggered fashion, so that units are exposed to treatment at different tenures, the diff-in-diff methodology proposed is flexible for estimating dynamic ITT and LATE, is robust to selection into treatment over tenure, as well as to heterogeneous treatment effects. Although tailored for our setting, this setting is not uncommon in applied work. For example, we can imagine a similar setting for a school restructuring program, where cohorts are age cohorts, tenure is their school grade, and the program is staggeredly adopted by schools (Martorell et al., 2016).

I suggest three main avenues for future research. First, this paper is not able to disentangle the exact magnitude and nature of complementarities between cash transfers and activation measures. Future studies should look for shocks and direct measures of the possible sources of complementarities, namely monitoring, motivation, and job search technology components of activation measures. The question is extremely relevant for understanding the extent to which social policy should worry about moral hazard vs. abating obstacles to employment and break poverty traps. Second, externalities represent a challenge for policy evaluators, whether positive or negative. In the period of this evaluation, *Garantie Jeunes* concerned a very small population, but displacement effects on other disadvantaged job seekers will become more likely when the program is extended (Crépon et al., 2013a). Alternatively, given the extremely disadvantaged population targeted by *Garantie Jeunes*, positive externalities might arise from a reduction in the crime rates of participants (Britto et al., 2020). Qualitative research by Loison-Leruste et al. (2016) reports abundant anecdotal evidence of youths in *Garantie Jeunes* grown up in high-delinquency environments.

Nonetheless, this work already offers relevant policy implications. The simplest one is that a combination of active and passive policies seems indeed desirable to improve employability of disadvantaged NEETs, as argued by comparative analysis such as OECD (2020b); Pignatti and Van Belle (2018). The mix of services and cash transfers provided by *Garantie Jeunes* is effective, in line with pilot evidence by Gaini et al. (2018) and qualitative results by Gautié (2018). Second, my estimates show that youths reduce employment due to welfare benefits, so that their elasticity of labor supply is large. A possible solution would be allowing youths to fully cumulate benefits and job earning, but this could clearly be costly. Activation is shown to be a viable alternative, as its effect is estimated strong enough to compensate for lock-in and distortive effects of the cash transfers. Finally, my insights can be used to study other policies that combine cash transfers and activation policies, like many minimum income schemes or unemployment insurance with activation requirements. However, external validity should be handled with care. *Garantie Jeunes* concerned only a very selected population, and the costs of the program are only 19 points lower than total benefits after 2 years. In the ongoing extension of the program, it might not be easy to maintain cost-effectiveness, as marginal returns from the program can be decreasing the broader the target population.

Chapter 2

Who Profits from Subsidizing General Training? Evidence from a French Individual Learning Account

This chapter is based on a joint work with Éloïse Corazza (Ministry of Labour, DARES)

Abstract

Workers could under-invest in general training, hence governments often subsidize lifelong learning. This paper studies the effect of the French Individual Learning Account (CPF), a scheme endowing all workers with generous training credits to be spent on the training market. We show that the total amount of training undertaken is not significantly affected by the subsidy. This happens because, in equilibrium, more than half of the benefit of the subsidy is captured by training producers through a significant change in prices. Moreover, a change in the subsidy eventually affects producers' profits, with no effect on labor costs and employment of trainers. Our results can be rationalized by inelastic demand for training, and by either perfectly inelastic supply or imperfectly competitive training markets. Under such conditions, subsidies to lifelong learning are a simple transfer to training producers and consumers, with no effect on aggregate welfare.

Keywords: training, subsidies incidence, imperfect competition

JEL Codes: M53, H22, J24, J28, L13

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2.1 Introduction

Since Becker (1964) famously distinguished between training in general or firm-specific skills, it is generally thought that investment in general skills training will be financed mostly by the worker himself.[1] Yet, training was shown to have high non-monetary costs for workers, widely uncertain private returns, and potential externalities, so that workers might under-invest in general training, especially if they are credit-constrained (Bassanini et al., 2005). Thus, governments often engage in subsidization of lifelong learning (OECD, 2020a). However, in most instances professional training is undertook via a market, where workers buy courses offered by profit-maximizing training producers. When mediated by such market interactions, it is not a given that subsidizing a good will determine an increase in the quantity consumed, both if the market is competitive (Fullerton and Metcalf, 2002) or if it is imperfectly competitive (Weyl and Fabinger, 2013).

How much can subsidies to general training increase training consumption? Scholars studying the effect of small training voucher programs (Hidalgo et al., 2014; Van den Berg et al., 2020; Görlitz, 2016; Schwerdt et al., 2012) found that they have often insignificant and always very small effects on training participation. However, experimental studies of training vouchers might not be informative of the effect of national subsidies: vouchers are typically small, and concentrated on a small population, so that general equilibrium effects are unlikely. Conversely, tax deduction of firms' training costs are found to stimulate training, for example in the Netherlands (Leuven and Oosterbeek, 2004). Yet, deductions to firms are more likely to be effective for training in firm-specific skills, not general ones. When tax deductions were granted to workers, they were more likely to end up financing general training, but had a much more limited effect on overall training participation (van den Berge et al., 2022).

This paper studies the effect of a large national training subsidy, the French *Compte Personnel de Formation* (CPF). The CPF is a national "Individual Learning Account" in which each worker accumulates training credits proportionally to its years of social security contributions, a kind of scheme which is increasingly popular in Europe[2]. Credits can then be used only at the will of the worker – hence, they are likely going to be spent in general-skills training – and only for training by certified providers. The subsidy was quite generous until 2018, amounting up to 120 hours, covering

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[1]Nonetheless, it was also shown that general training might in part be financed by firms (Acemoglu and Pischke, 1999, 2000).

[2]Examples include the *Opleidingscheques* in Flanders (Belgium), the *Bildungsprämie* in Germany, the Cheque formação in Portugal, the *Individual Training Accounts* in Scotland, the *Chèque annuel de formation* in Geneva Canton (Switzerland), and the Individual Training Accounts in the United States. Other examples, with some slight deviation from the standard case, are The *Bildungskonto* in Upper Austria, the *SkillsFuture* Credit in Singapore, and *Carta ILA* in Tuscany (Italy).

the full cost of the training in 70% of the training episodes where CPF was requested. Yet, the maximum per-hour training subsidy differed substantially across industries, with richer industries allowing larger caps to the per-hour subsidy. CPF was dramatically reformed in 2019: the value of the subsidy was made uniform across industries, with most industries undergoing a cut in the amount available, some larger than than others, providing an unexpected differential change in the value of the subsidy. The cut was substantial, and the share of training episodes fully covered by the subsidy fell to 45% of the training episodes.

The results point out that, despite the reform, the total hours of training in an industry-training kind pair was not significantly affected. At the same time, we show that a €1 change in the subsidy triggers a €0.53 change in prices, so that more than half of the subsidy is captured by producers. Consistently with this, producers' revenues change by 1.3% for every euro change in the average hourly subsidy used by consumers, concentrated in training producers more heavily relying on CPF-subsidized training. Instead, no effect is detected on total costs, labor costs and on the number of trainees, so that profits also change by 0,8% for every euro change in the average hourly subsidy used by consumers.

What do these results suggest about the training market? Microeconomic theory dating back to Harberger (1962) points out that if a per-unit subsidy in a perfectly competitive market generates a) an insignificant increase in quantity, and b) a significant, but less than 1-to-1, increase in prices, then both supply and demand for training are quite inelastic (Fullerton and Metcalf, 2002). However, researchers recently highlighted that low elasticity of quantity consumed to subsidy changes can derive not only from inelastic supply and demand but also from imperfect competition (Weyl and Fabinger, 2013). In fact, we show that our results can be rationalized either by inelastic demand for training and perfectly inelastic supply under perfect competition, or by imperfectly competitive training markets. We argue that the latter mechanism is more plausible, as the training market is likely less than perfectly competitive. For example, asymmetric information on training quality risks making the market for training courses a market for "lemons", with low-quality training providers pushing high-quality ones out of the market. Hence, reputation, repeated interaction, as well as policies such as mandatory certifications play an important role, but could in turn build entry barriers and jeopardize competition.

Our results contribute to the literature studying on-the-job training by studying the effect of training subsidies when training is acquired in a training market. We are the first, to our knowledge, to consider the general equilibrium effect of training subsidies on both training participation and prices. A large literature has focused on the question of whether or not human capital accumulation is under-financed (Bassanini et al., 2005; Acemoglu and Pischke, 1999, 2000), so as to justify (or not) subsidization policies. Yet, scholars under-considered the fact that subsidies to general training risk failing to increase training participation in general equilibrium. Impact evaluation of training vouchers (Hidalgo et al., 2014; Van den Berg et al., 2020; Görlitz, 2016; Schwerdt et al., 2012) were unfit to study potential effects of subsidies on prices, as vouchers are typically concentrated on a small target population. In turn, tax deductions studied by Leuven and Oosterbeek (2004) target firm-specific skills, rather than general training. More in line with our focus on general skills, van den Berge et al. (2022) study tax deductions for workers lifelong learning expenditures, but while our null results on training participation are consistent with theirs, their setting doesn't allow them to study the effect of deductions on training prices.

Our study also enriches the literature the effect of subsidies in Industrial Organization and Public Economics by studying subsidies incidence in the case of training subsidies. Classical incidence studies found that, consistently with models which assume perfect competition, subsidies mostly generate an increase in prices where marginal costs are high and supply elasticities low (e.g. in the housing market, Gibbons and Manning, 2006; Fack, 2006). Instead, when markets are imperfectly competitive, pass-through of subsidies to consumers depends on how market power interacts with the shape of the demand function (Weyl and Fabinger, 2013). Exploiting quasi-experimental variation in subsidies, studies such as Kirwan (2009); Cabral et al. (2018); Pless and van Benthem (2019) test the degree of imperfect competition in the market for agricultural land, health insurance, and solar energy systems. In this paper, we study a market for investment in skills which is characterized by high asymmetric information and regulation, similarly to Turner (2012). We find that the incidence of training subsidies partially falls on suppliers, and that the subsidy is directly related to suppliers' profits. We argue that the most plausible mechanism behind our findings is the presence of market power by suppliers in the training market.

In terms of policy implications, we offer an evaluation of an important Active Labor Market Policy in France. The policy was costly, financed through a contribution by firms of 0.2% of the wage bill, and was relatively welcomed by social parties. Nonetheless, some scholars had voiced concerns about effectiveness and equity of this kind of subsidies (Cahuc and Zylberberg, 2006). We show that CPF failed to increase training, ending up being a transfer, mostly to producers. The silver lining is that, studied through the lenses of a sufficient statistics framework, the dead weight loss arising from CPF is also close to zero. As a general implication, the paper offers insights on the effectiveness of subsidization strategies relying on a secondary market for the subsidized good, which often occurs whenever consumer choice is considered more efficient than central planning e.g. in building subsidies, in upper education, in health insurance. In case of inelastic supply/demand or imperfect competition in the market for the subsidized good, a subsidy could fail to increase the consumed quantity. In our example example, policy makers who want to support human capital investment should – before subsidizing it – take measures to ensure that supply is sufficiently elastic and the market competitive, for example easing market entry. Interestingly, in a market with large asymmetric information, such as the one for training, regulators might face a tradeoff between the need to guarantee training quality and the risk of reducing competition.

The rest of the article is structured as follows. Section 2 presents our empirical setting: the institutional context, the data, and some descriptives of the policy shock. Section 3 explains our identification strategy. Section 4 presents the results. Section 5 discusses the mechanisms. Section 6 draws implications in terms of welfare and policy. Section 7 summarizes and concludes.

2.2 Empirical setting

2.2.1 The French CPF

Introduced in 2015, the *Compte Personnel de Formation* (CPF) provides workers with credits to be spent for training, guaranteeing a fixed amount of additional credits for every year of social security contributions, depending on personal characteristics. Credits are accumulated in a "personal" account, in the sense that only workers can access it and decide how to use it, which is also "portable", in the sense that workers maintain the credits even when changing employer[3]. Initially, the scheme covered only employees of the private sector, while workers of the public sector were added to the program from 2017, and self-employed workers from 2019 (in this study, we are anyway going to focus on private sector workers). Importantly, CPF credits can be used to finance only training courses from a list of eligible providers. As of 2018, the annual cost of CPF was estimated at about €650 Millions, roughly 10% of French public expenditures in professional training as calculated by the OECD.

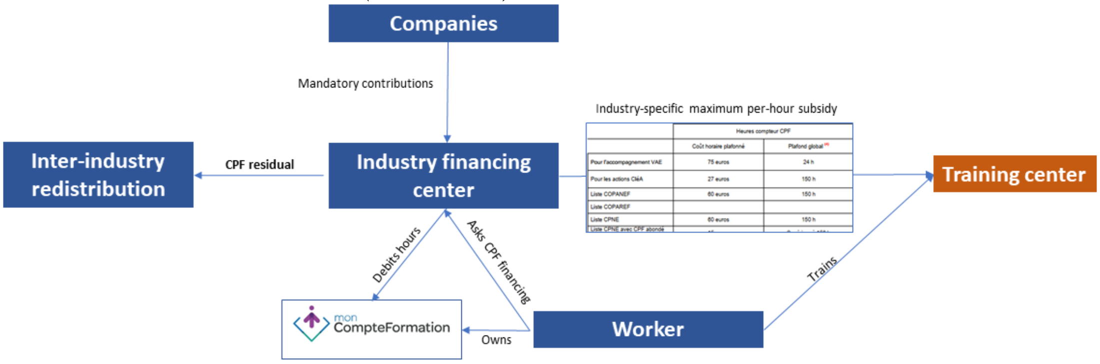
CPF underwent a significant reform in January 2019. Before the reform, between 2015 and 2018, CPF credits were accounted in hours. Workers gained 24 hours of training each year up to 120 (then 12 per year up to 150) if working full time, with the exception of low qualified workers, who obtained 48 hours yearly up to 400. To use their credits, workers had to select any training among the ones available on an online internet platform (*"Mon Compte Formation"*). Then, they had to submit a request to industry-specific training agencies to approve the financing of the training with their CPF credits. Finally, the training agency would pay the training provider and reduce the amount of hours credits in the worker CPF account of an amount equal to the duration of the training. This pre-reform institutional context is summarized in Panel A of Figure 2.1.

[3]By contrast, the previous device (*Droit Individuel de Formation – DIF*) replaced by CPF, was instead attached to each working contract: the employer could see the amount of training guaranteed to each employee on the payslip, the credits disappeared if the contract terminated, and the worker could not transfer training credits from one employer to the other.

Importantly, industry-specific training agencies would not be willing to finance a CPF-subsidized hour of training at any rate, but they were fixing different caps to per-hour subsidy payable for each hour of training. These caps were reported in official tables communicated to the government (an example of these table is reported in Figure B.2 in the Appendix). For instance, suppose that in 2018 a worker has 120 CPF hours in his account, and wanted to undertake a training which costs €80 per-hour for 50 hours of training duration, so €4,000 of total cost of the training. Assume that for that specific kind of training his training agency fixed a maximum subsidy up to €60 per hour. Hence, €3,000 of the training cost will be covered by 50 hours of CPF credits, 70 hours will remain available on the worker CPF account, while €1000 of training costs will remain uncovered. For covering these costs uncovered by CPF credits, discretionary additions (*Abondements complémentaires*) could be offered by the training agency, consisting in an extra lump-sum amount of financing. These additions have to be actively requested by workers, have very complex rules that depend on workers' characteristics, and are often assigned with a degree of discretion by industry financing centers. In case there would still be leftover costs to pay, the worker would finance them by himself.

Before 2019, industry-specific training agencies had strong incentives to be generous in financing CPF. In fact, the CPF subsidy was financed through large mandatory contributions by companies, 0,2% of the wage bill. Contributions by companies were collected by industry training financing centers to be used only for CPF training within the industry. If contributions exceeded the cost of all CPF used by workers in the industry during the year, leftover resources would be redistributed across industries. Industry training agencies had thus incentives to avoid leaving money from CPF contributions "on the table", allowing high caps to per-hour subsidy, in order to keep the money within the industry. Several French regulators confirmed this mechanism. We quote a regulator from the Minister of Labor in charge of supervision of CPF: "The system pushed industry training financing agencies to fix whatever high per-hour subsidy cap, just to consume the CPF financing line, and avoid giving up the money". Finally, it is worth noting that despite its generosity CPF was underused in 2018: individuals tended to accumulate credits without using them (Figure B.3 in the Appendix), so that most individuals actually reached the maximum amount of hours which could be accumulated in the account.

Panel A: the pre-reform setting (2015-2018)



Panel B: the post-reform transition period (2019)

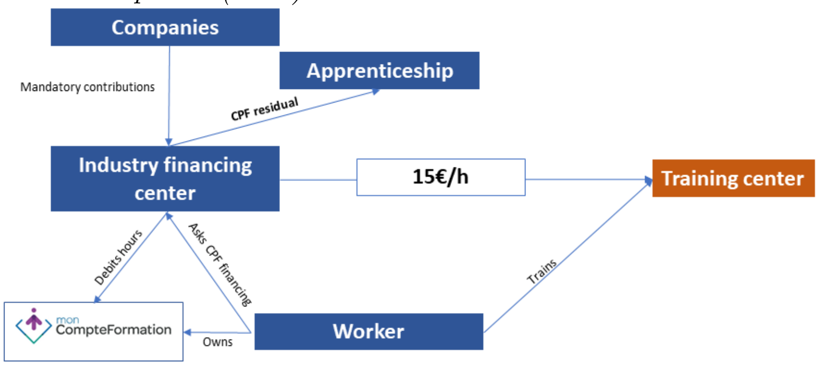


FIGURE 2.1: Pre and post-reform organisationof the CPF

Notes. Panel A reports the functioning of CPF before the reform of 2019. Workers own an amount of CPF credits, which can be used to pay for training up to industry-specific caps to the per-hour subsidy. Industry financing centers collect mandatory contributions from companies, decide per-hour subsidy caps, and are forced to give the unused funds to inter-industry redistribution. Panel B reports the functioning of CPF in 2019 (transition year after the reform). Workers own an amount of CPF credits, which can be used to pay for training up to €15 per-hour subsidy. Industry financing centers collect mandatory contributions from companies, finance training at uniform €15 per-hour subsidy, and can use the unused funds for subsidizing apprenticeship within the industry.

The CPF was reformed in January 2019[4], and the main change was the so-called "monetization" of the credits: for all private workers, the account would be denominated in Euros rather than hours. As a consequence, industry-specific per-hour subsidy caps were abolished: an hour of CPF, once having different values in different industries according to per-hour subsidy caps defined by training agencies, became after 2019 uniformly worth 15 Euros[5]. Although the final goal of the reform was

[4] *Loi pour la liberté de choisir son avenir professionnel* of September the 5th 2018.

[5] Although the reform was expected, the exact magnitude of the uniform rate was not clear until the very end. The discussions about the CPF reform started in January 2018, but a reform of the CPF system in the sense of a monetization was already in the electoral program of the Macron government, elected in 2017. After a year of discussion and several changes due to harsh bargaining between the government and industry training agencies, the 15 Euros uniform rate was decided by Decree in December 2018, after the approval of the law, to be applied from

to allow workers to use CPF directly in euros, directly paying training providers through a mobile app and bypassing industry financing centers, between January and November 2019 a transition period was enacted, which in practice affected almost all trainings of 2019[6]. Our analysis will focus on this transition period, during which CPF worked in an extremely similar way to the pre-reform years, but the per-hour subsidy was harmonized at 15 Euros per hour across industries (Panel B of Figure 2.1). Specifically, workers still submitted requests to the training agency of their industry to pay training providers and debit their CPF account, but the value of the CPF subsidy was determined as the amount of hours available on the CPF account multiplied by the uniform 15 Euros rate. Because industry-specific caps were mostly higher than €15 before the reform, the reform determined a huge drop in the CPF subsidy. Discretionary additions were still possible from the training agency, if the CPF subsidy was not enough to cover the cost of training. In fact, some industry financing centers started using discretionary additions to increase the total value of their workers' CPF in 2019, attenuating the reform. Training agencies were nonetheless not incentivized to do so, since the reform allowed training agencies to keep the unused CPF contributions for financing apprenticeship in their industry. We can thus expect that the cut in CPF will not be fully compensated by an increase in discretionary additions.

2.2.2 Data sources, sample selection, and cleaning

For the purpose of this study, our main source of data is the SI-CPF (*Système d'information du CPF*). This database is an unexploited administrative source, which registers all CPF training episodes since 2015. It is built by the French public investment bank in charge of monitoring the CPF and it's used by French authorities to build official statistics on the device. Between 2015 and 2019, the SI-CPF recorded information sent by employers on employment of workers, to calculate CPF credits, and from financing centers to calculate CPF consumption, determine redistribution requirements and from 2019 to actually reimburse training agencies. The dataset contains: personal characteristics of beneficiaries (identifier, sex, age, working status, diploma, CPF stock, etc.); data on the training (duration, title of the training, name, training provider, etc.); and financial data (cost, financing center, amount financed through CPF, amount financed through discretionary additions,...). Training provider is reported basing on the firm fiscal identifier, and local labor markets where the training occurs are defined basing on reported municipality and postal code of the training establishment. SI-CPF was never used for academic purposes before, and a selected sub-sample was extracted in collaboration with the French Ministry of Labor for the purpose of this

January 2019. Consequently, large anticipation was not likely. Figure B.4 in the Appendix suggests only a small bunching of CPF-subsidized trainings at the end of 2018.

[6] As Figure B.4 in the Appendix shows, the value of trainings undertaken through the unique mobile up in December 2019 is negligible, and most trainings are still the result of previous validation by industry financing centers. The December 2019 period is also a particular one in France, due to historically harsh strikes of public transportation.

study. We selected private sector workers, excluding training episodes concerning other training devices, draft training episodes, and CPF trainings by unemployed workers, as described in Table B.1 in the Appendix. After the first selection, outliers were eliminated[7]. Finally, we drop CPF training episodes financed by other institutions than industry-specific financing centers (1.2% of the observations)[8].

Our second data source is the official documentation on the caps to the per-hour value of the CPF subsidy allowed by training agencies. We construct a small database digitalizing publicly available documentation from the inter-industry training organization, the national training council, and from the training agencies themselves[9]. Through the French Labor Ministry, we also sent requests of additional information to training agencies to complete the dataset and ensure a better understanding of the process. The final dataset records 224 different per-hour subsidy caps according to the industry financing agency and the year (2017 to 2019), and for different kinds of training[10]. We merge this new dataset with SI-CPF, sucessfully assigning a subsidy cap to more than 90% of the training episodes[11].

Our final source is called BPF (*bilans pédagogiques et financiers*), which reports balance-sheet information for training providers, e.g. public and private firms such as language schools, vocational schools, driving licence agencies, chambers of commerce... . This source is an administrative dataset coming from mandatory declarations by any training provider which uses public subsidies (not only CPF). It's collected by the Ministry of Labor, and it's used for official statistics as well as supervision by the French government. The advantage of these data is that they are more quickly updated than balance-sheet administrative data from tax declarations, and include more detailed

[7]In the pre-2019 period, the Ministry suggested that some operators inserted the total cost for the whole session instead of that for the individual: we drop all training episodes with average training cost both above Q3+3 IQR and above 95% for each training kind (1.4% of the observations are dropped). This selection is consistent with practices adopted by the French administration when using SI-CPF. Extreme values (inferior to 1% or superior to 99%) for program duration or prices were replaced as missing (3.1% of observations).

[8]These are exceptional cases financed by employers, regions, and by the unemployment agency *Pôle emploi*

[9]Named FPSPP, CNEFOP and OPCA respectively

[10]In practice, the subsidy cap is the same for groups of training kinds. We identify 10 of them: Skills balance (*Bilan de compétences*), certification for conduction of industrial machines (*CACES*), Certification of professional general and specific skills (*VAE, CléA, CQP*), certification of enterpreneurial skills (*Création d'entreprise*), IT and accounting certificates (*Informatique et bureautique*), language certificates (*Langues*), base vehicle driving licence (Permis B), others (Autres). They have been constituted according to the classifications by training agencies.

[11]In some cases (4.1% training episodes in 2018 and 10.1% training episodes in 2019) the financing center does not fix a cap to per-hour value of the subsidy, but a cap to the total subsidy for the training episode. This happens almost always when the training is aimed at obtaining very common and standardized certificates, so that trainings have specific durations (for example, a professional skill qualification called VAE, which always lasts 24 hours). In these few cases, we define the per-hour subsidy cap by dividing the cap on total subsidy by the mode duration of the training. Moreover, two industry financing agencies (FAFSEA and OPCA 3+) did not establish any subsidy cap for the pre-reform period, as they were in theory willing to cover any per-hour cost of training. A third one (OPCA Transport) did not define a conversion rate for all trainings but only for two quite popular types (VAE and common cars driving licence). All these training-financing center pairs, not linkable to a specific per-hour subsidy cap, were then excluded from the analysis (6.2% of the sample).

information. BPF provides financial data (revenues, costs, subsidies received), breakdown of costs paid by the training providers (employees wages, teacher wages, external consultant wages) and information on the staff (number of teachers, external consultants). This information doesn't only concern CPF-subsidized trainings but all trainings undertaken at the training provider, including unsubsidized trainings or trainings subsidized by other devices. We use a version of BPF as of the beginning of 2021, which reliably covers training providers activities until fiscal year 2019. The data report outliers, so that we trim our variables of interest – revenues, costs, profits, and revenues from CPF – to the 1-99th percentile. We merge BPF with SI-CPF basing on firm fiscal identifier. The merge is quite satisfying: 93,3% in 2018 and 95,1% in 2019 of SI-CPF training episodes found a match in the BPF dataset.

2.2.3 Descriptives of the shock

Before digging into identification, we present 3 descriptives of the shock to per-hour subsidies generated by the reform of January 2019. First, Figure 2.2 displays the maximum cap to the per-hour subsidy applied in 2018 as reported in official documentation and from interviews with industry financing centers, for the 9 most common groups of training kind. The graph also reports mean, mode and IQR of the average actual amount of CPF subsidy used to cover the training, in per hour terms, observed in the data. This set of figures points out two considerations. First, our data gathering of the different per-hour subsidy caps across industry looks accurate: with few exceptions, the per-hour value of the CPF subsidy actually seen in the data is never above the per-hour subsidy cap. Interestingly, although the per-hour value of the CPF subsidy is often bunched at the value of the cap, suggesting that the per-hour subsidy cap is binding, the subsidy used by consumers is sometimes below the cap, possibly as prices themself are lower than the total subsidy[12]. This happens especially when the subsidy cap is higher. We will return to this in Section 3.2. As a second consideration, caps to per-hour subsidy are quite variable, and almost always above the new per-hour conversion set by the 2019 reform (15 Euros per hour). Looking at the ranking of financing centers on the horizontal axis, one can see how richer financing centers (see Figure B.5 in the appendix for the correspondence between the agency name and the industry they represent) tend to be more generous, although there is quite a variability across different kinds of training.

A second interesting descriptive is reported in Figure 2.3, which plots the distribution of the share of the total training cost which is covered by workers CPF credits, without discretionary additions, in 2017, 2018 and 2019. Clearly, the reform of 2019 represents a dramatic cut in the capability of CPF to cover the cost of training: while before the reform CPF was fully covering the cost of

[12] In Appendix B.2, we show how this can happen for example in the case of high non-monetary costs of training and large subsidies, making demand for training inelastic and concave.

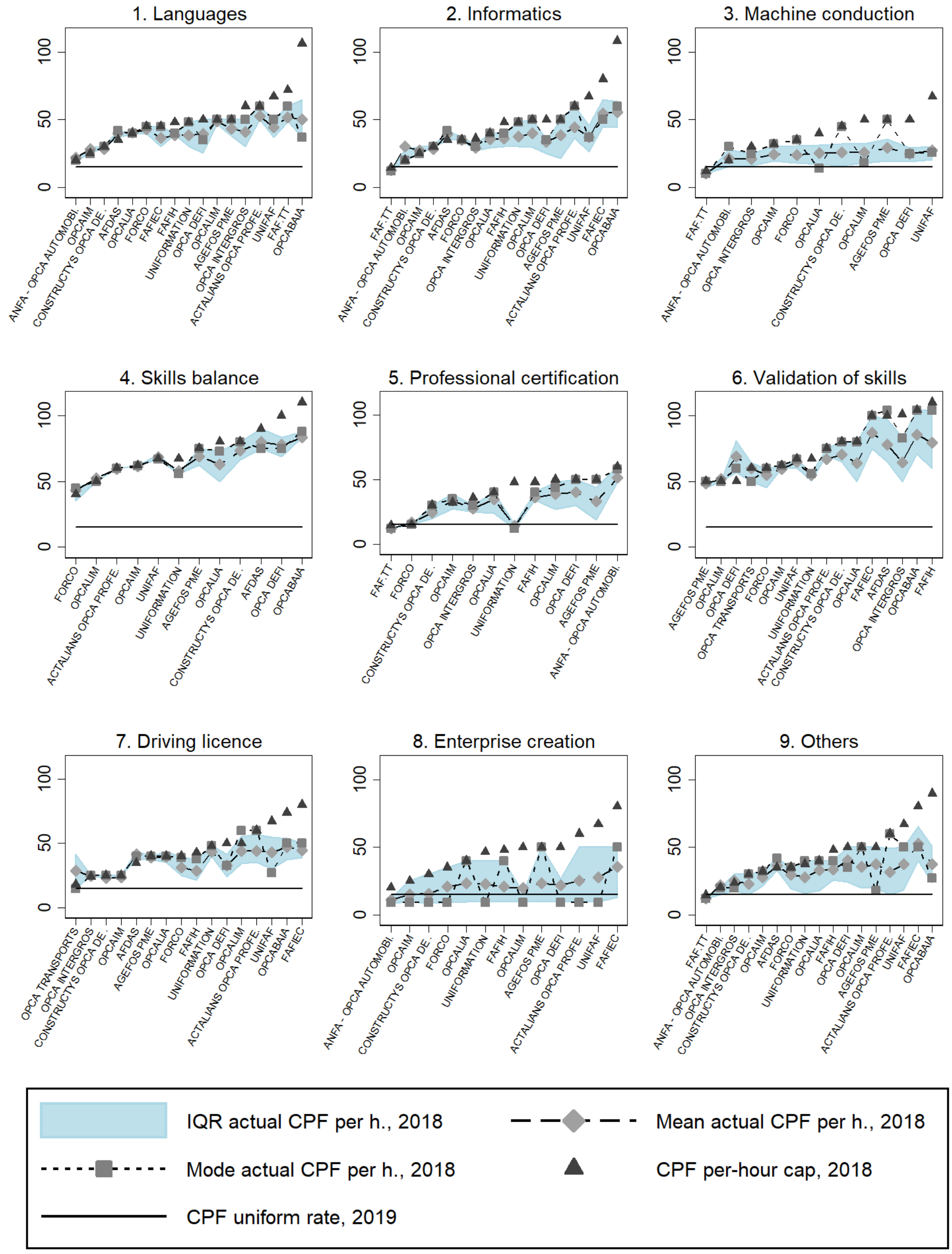


FIGURE 2.2: Differences in per-hour training subsidy across industries

Notes. The figure reports the per-hour subsidy caps, and average actual amount of CPF subsidy used per-hour (average, mode, and IQR), for different industry financing centers and according to training type. The per-hour subsidy caps are determined by industry financing centers until the reform of 2019, which harmonizes the subsidy at 15 Euros per hour. The actual amount of CPF subsidy used per-hour is calculated, for every training episode in SI-CPF, as the ratio of the total value of CPF used C_i over the total hours of CPF debited to the worker x_i^{CPF}.

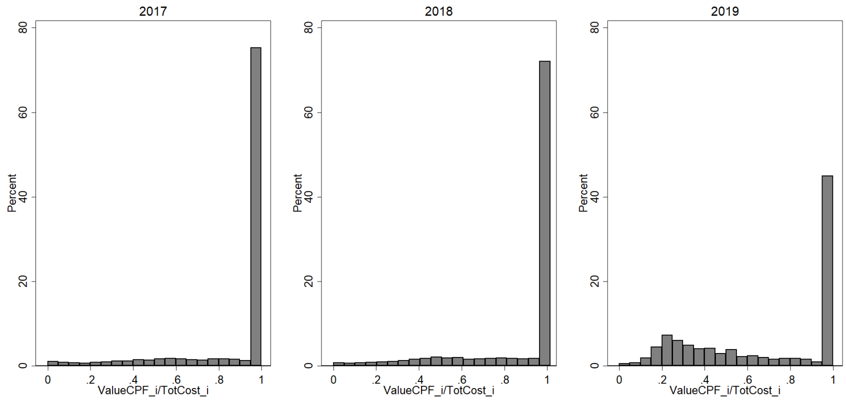


FIGURE 2.3: Percentage of total training cost covered by CPF subsidy

Notes. The figure reports the distribution of the ratio of the total value of CPF C_i to the total cost of the training P_i, by year, for every training episode observed in SI-CPF.

training for almost 80% of the training episodes reported, after the reform this share almost halves. While pre-reform the distribution is almost fully bunched at 1, with a slight left tail, in 2019 is bimodal, with one fourth of the training episodes having between 20% and 40% of the cost covered by CPF subsidy.

Finally, Figure 2.4 gives an example of the effect of the reform on training prices for two among the 10 most popular kind of training, the BULATS language certificate and the lifeguard certificate. In the former, the subsidy cap falls to 15 euros in every industry in 2019, but the average price remains widely heterogeneous for workers coming from different training agencies. This suggests different financing centers should be sometimes seen as different markets, with different prices and potentially different marginal costs (due to e.g. different areas, different level of qualification, ...). Conversely, in the case of lifeguard certificate prices converge to a much more similar level after equalization of the subsidy.

2.3 Identification

2.3.1 Specification at the Training Course Level

In our data, for every training episode i we directly observe the episode duration x_i, the total cost P_i, the euro amount of the subsidy from a worker CPF credits C_i , and discretionary additions by the industry financing center A_i. Instead, our dataset doesn't provide directly information on the hourly price p_i and on the effective per-hour subsidy used c_i. Concerning hourly prices, we can recover them as $p_i = \frac{P_i}{x_i}$. Concerning effective per-hour subsidy, this includes both direct subsidy

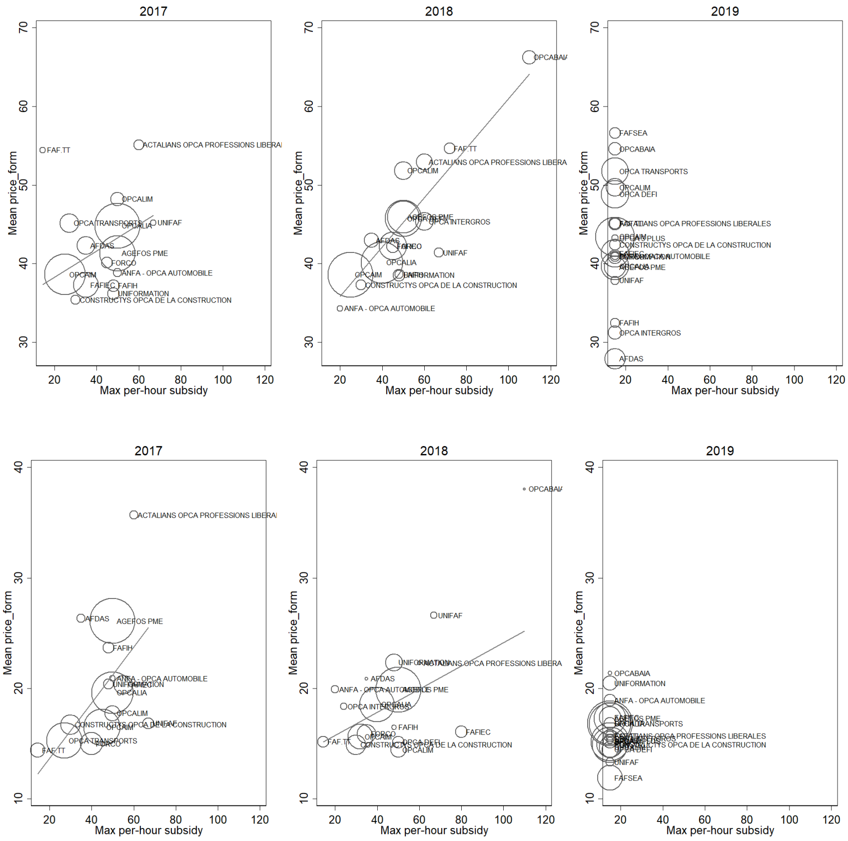


FIGURE 2.4: Effect of the reform on training prices for two among the 10 most popular kind of training, the BULATS language certificate (top) and the lifeguard certificate (bottom)

Notes. The plots the mean price and the per-hour subsidy cap $c_{q,\tilde{f},t}$ for two of the ten most popular trainings, the BULATS language certificate and the lifeguard certificate, for different industry financing centers. The size of the bubble for each observation is proportional to the number of training episodes from that center.

and discretionary additions, so we can recover them as $c_i = \frac{C_i + A_i}{x_i}$. Finally, we denote as $c_{q,\tilde{f},t}$ the industry-specific per-hour subsidy cap, directly observed in the ad-hoc dataset built using training agencies documentation.

To study the effect of the subsidy on the quantity and prices of training, our unit of analysis will be the "training course": a cell defined by different training typologies q (which are obtained as the combination of the title of the training and whether it is run online or in person), industry financing center f, and training establishment j (obtained as combination of enterprise and local labor market). Our baseline model to be estimated is:

$$y_{q,j,f,t} = \beta_y c_{q,f,t} + \gamma_{q,j,f} + \tau_t + \varepsilon_{q,j,f,t} \tag{2.1}$$

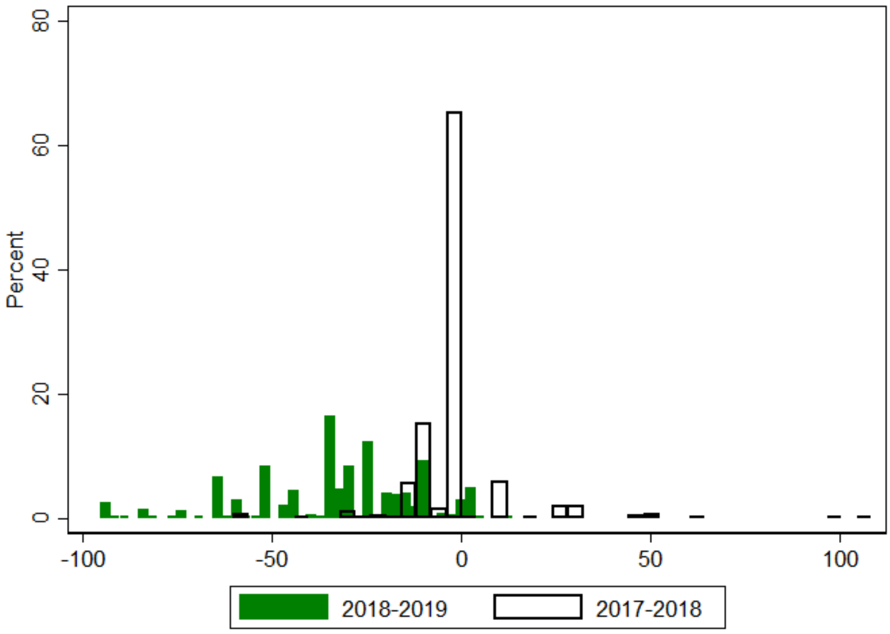


FIGURE 2.5: Distribution of $\Delta c_{q,\tilde{f},t}$

Notes. The figure reports the distribution of the change in the maximum subsidy rate allowed by different industry financing centers for different training kinds. The changes in 2018-2019 are the result of the reform of 2019, while those of 2017-2018 are decided by industry training financing centers.

Where $y_{q,j,f,t}$ is the outcome and $c_{q,f,t}$ is the averaged effective subsidy c_i for training kind q and industry financing center of the worker f. $\gamma_{q,j,f}$ are fixed effects for training kind interacted with industry financing center and establishment, and τ_t is the fixed effect for the year when the training occurs. Our estimand, β_y, is the average unit effect of a euro change in the per hour subsidy on outcome y.

Note that the model in 2.1 is not identified simply by using a fixed-effects model (FE), as changes in $c_{q,f,t}$ don't satisfy strong exogeneity *a-priori*, for at least two reasons. First, before 2019 the subsidy rate can be endogenously set by industry financing centers. To tackle this, we will exploit the unexpected exogenous change in subsidy rates generated by the reform of 2019, and focus only on 2018-2019. Second, even in 2018-2019 discretionary additions A_i can arise endogenously from decisions by financing centers. As they are often guaranteed only if CPF doesn't cover the whole amount of the training costs, discretionary additions can attenuate changes in the subsidy rate generated by the reform, especially for richer industries. To assess this concern, we will use an Instrumental Variable strategy, instrumenting $c_{q,f,2018}$ with $c_{q,\tilde{f},2018}$, the subsidy cap fixed by each industry financing center. Figure 2.5 reports the variation in our instrument for the pre-reform period 2017-2018 and the period of the reform 2018-2019.

Then, the first stage will be the linear projection of the endogenous variable, the effective subsidy rate gross of discretionary additions, on our instrument, the subsidy caps, and covariates (Wooldridge, 2010), before and after the 2019 reform:

$$c_{q,j,f,t} = \beta^{FS} \tilde{c_{q,f,t}} + \gamma_{q,j,f} + \tau_t + \varepsilon_{q,j,f,t} \qquad \text{if} \quad t = 2018, 2019 \tag{2.2}$$

And the reduced form is obtained by replacing our instrument in the structural equation of interest:

$$y_{q,j,f,t} = \beta_y^{RF} \tilde{c_{q,f,t}} + \gamma_{q,j,f} + \tau_t + \varepsilon_i \qquad \text{if} \quad t = 2018, 2019 \tag{2.3}$$

We will estimate both the first stage and the reduced form using simple fixed-effects estimators and obtain our structural equation in regression 2.1 using IV. For inference, Standard errors are clustered at the level where variation in the instrument occurs, which is the interaction between industry financing centers and the training kind category.

Finally, our identification strategy relies on the assumption that industries that will experience larger changes in $\tilde{c_{q,f,t}}$ in 2018-2019 did not report significantly different evolution in the outcome variable in previous years. To test the implications of this assumption, it is not straightforward to run a placebo test on pre-shock dates, because even between 2017 and 2018, the year before the exogenous change of the reform, several changes in the per-hour subsidy cap were enacted by industry financing centers. These changes are potentially endogenous, for example if industry financing centers change the 2018 subsidy cap following higher prices.

To circumvent this issue, I will partial-out the effect of endogenous pre-shock changes in the subsidy under the null of our reduced-form estimate, before running the placebo. Namely, I first estimate $\hat{\hat{y}}_{q,j,f,t} = y_{q,j,f,t} - \hat{\beta}_y^{RF} \tilde{c}_{q,f,t}$ where $\hat{\beta}_y^{RF}$ is the estimated reduced-form coefficient for the relationship between the instrument and the outcome. Then, I will test that the future change implied by the reform is unrelated to the outcome in the pre-shock years, partialled out of $\hat{\hat{y}}_{q,j,f,t}$:

$$y_{q,j,f,t} - \hat{\hat{y}}_{q,j,f,t} = \beta_y^{PL} \tilde{c}_{q,f,t+1} + \gamma_{q,j,f} + \tau_t + \varepsilon_{q,j,f,t} \quad \text{if} \quad t = 2017, 2018 \tag{2.4}$$

To not reject our identifying assumption, under the null that it's valid, the placebo coefficient β_y^{PL} should not be significantly different from zero[13].

[13] The same test is also run using IV, instrumenting a lead of the effective subsidy with the lead of the subsidy cap

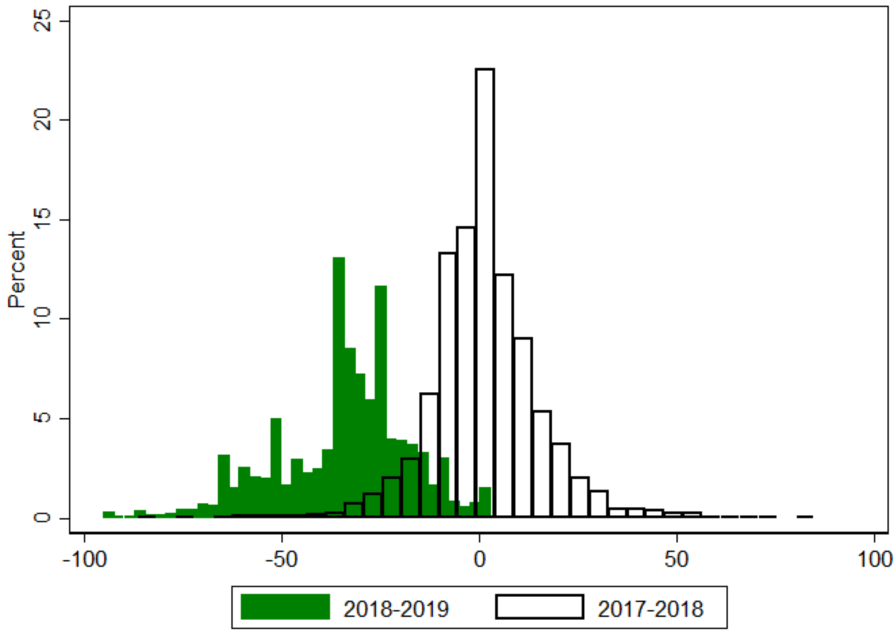


FIGURE 2.6: Distribution of the average subsidy cap for trainings offered by training suppliers

Notes. The figure reports the distribution of the change in the average subsidy rate that customers of training providers face (as they come from different industry financing centers). The changes in 2018-2019 are the result of the reform of 2019 harmonizing all subsidy rates to €15, while the changes in 2017-2018 are arising from decisions of industry training financing centers.

2.3.2 Specification at the Training Provider Level

For detecting the effect of the subsidy on training suppliers' revenues, costs and profits, we will use use as unit of analysis the training provider company J. This means that information at the training episode level from SI-CPF will be aggrgated for all training episodes from the same provider. Hence, our instrument will be $\tilde{c}_{Jt} = \sum_{i \in J} \frac{x_i p_{i,t_0}}{\sum_{i \in J} x_{i,t_0} p_{i,t_0}} \tilde{c}_{i \in (q,f),t}$, the average subsidy cap allowed for a supplier's customers, weighted by the share of CPF revenues that each training accounts for, and the endogenous variable will be $c_{Jt} = \sum_{i \in J} \frac{x_i p_{i,t_0}}{\sum_{i \in J} x_{i,t_0} p_{i,t_0}} \tilde{c}_{i \in (q,f),t}$. Note also that $\sum_{i \in J} x_{i,t_0} p_{i,t_0}$ includes only CPF training, while training centers might of course provide trainings also outside of CPF scope, which we don't see in our data. The variation in $\tilde{c}_{Jt}$ in the years of interest is reported in Figure 2.6.

Our structural equation at the training provider level thus is:

$$y_{J,t} = \beta_y c_{Jt} + \gamma_J + \tau_t + \varepsilon_{J,t} \qquad \text{if} \quad t = 2018, 2019 \tag{2.5}$$

Where $y_{J,t}$ are different producer-level outcomes. As usual, the reduced form will be identical with

$\tilde{c}_{J,t}$ instead of c_{Jt}, and the first stage will have c_{Jt} as outcome and $\tilde{c}_{J,t}$ on the right hand side. Standard errors are clustered at the training provider level.

2.4 Results

2.4.1 Changes in CPF Subsidy Don't Affect Training Participation

In Table 2.1 we report the estimates of the effect of the changes in CPF subsidy entailed by the 2019 reform on the quantity of training undertaken for training kind q, training supplier j, and industry f. Column (1) reports our first stage, signaling that a Euro decrease in CPF maximum per-hour subsidy leads to a significant .18 Euro decrease in the effective average per hour subsidy, gross of discretionary additions. Columns (2) to (7) report the reduced form and Instrumental Variable estimates on the effect of CPF subsidy on total amount of training in terms of hours $X_{q,j,f,t}$, the average duration of a training episode $x_{q,j,f,t}$, and the number of training episodes $N_{q,j,f,t}$. Since we take the natural logarithm of the outcome variables, the estimates should be interpreted as the percentage change in the outcome following a euro change in the subsidy rate. All estimates indicate a quite precisely estimated zero effect of changes in the subsidy cap on the quantity of training taken up. In fact, the coefficient of the regressions mean that for a one euro change in the subsidy, the total amount of hours decreases by 0.4% and it's not significant with a standard error of 1.5%. Since the reduced form coefficient in Column (2) is -0.07% (standard error of 0.3%), and the mean decrease in the subsidy cap is €33, we can exclude with 95% confidence that the effect on total quantity of training following the reform of 2019 will exceed 10% in absolute values. Given that subsidy caps were on average reduced by 104% by the reform between 2018 and 2019, this is a remarkably precise zero effect of the reform. Subsequently, one can check the identification with a placebo as in Equation 2.4. In column (1)-(4) of Table B.2 in the Appendix we find that no significant pre-trend or anticipation exists in the setting.

Table 2.1: Impact on Average Quantities of Training of the CPF Subsidy

VARIABLES	(1) c_{qjft}	(2) $\ln X_{qjft}$	(3) $\ln X_{qjft}$	(4) $\ln x_{qjft}$	(5) $\ln x_{qjft}$	(6) $\ln N_{qjft}$	(7) $\ln N_{qjft}$
$c^{\sim}_{qft}$	0.180***	-0.000676		-0.000521		-0.000186	
	(0.0172)	(0.00283)		(0.00129)		(0.00199)	
c_{qft}			-0.00375		-0.00289		-0.00103
			(0.0147)		(0.00629)		(0.0108)
Observations	49,038	49,038	49,038	49,038	49,038	49,038	49,038
R-squared	0.819	0.836		0.914		0.840	
Years	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	FE	FE	IV	FE	IV	FE	IV

Notes. Column (1) reports the first-stage relationship between the instrument - subsidy caps - and the effective subsidy rate. Columns (2), (4) and (6) report reduced form estimates of the relationship between subsidy caps and log total training quantity, log average training episode duration, and log total number of training episodes respectively. Columns (3), (5) and (7) report the IV estimates of the effect of a change in the effective subsidy on log total training quantity, log average training episode duration, and log total number of training episodes respectively. All regressions include fixed effects for year and for training course (an interaction of the training title, online/offline, and training establishment). Standard errors are reported in parentheses and clustered at the interaction between industry financing centers and the training kind category. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.4.2 Changes in CPF Subsidy Are Partially Captured by Training Producers Through Changes in Prices

We then turn to study the effect of the CPF subsidy on prices. Column (1) in Table 2.2 reports again our first stage, while column (2) and (3) report respectively the reduced form and IV estimates of the effect of the subsidy rate on prices. The coefficients in Columns (1) and (2) suggest that a .18 Euro decrease in the subsidy leads to a 9% decrease in the average price. The positive sign of the coefficient is consistent with our expectations that a reduction (resp. increase) in the per-hour subsidy leads to a decrease (resp. increase) in the price. In Column (3), the IV estimate implies that for every Euro of effective change in the subsidy following the reform of 2018, prices changed by .53 Euros, so that the pass-through rate of the subsidy to consumers (i.e. the reduction in the net price of training) is 47%. For prices as well, placebo estimates in column (5) of Table B.2 in the Appendix report no significant pre-trend or anticipation.

TABLE 2.2: Impact on Average Prices of Training of the CPF Subsidy

	(1)	(2)	(3)
VARIABLES	c_{qjft}	p_{qjft}	p_{qjft}
$\tilde{c_{qft}}$	0.180***	0.0956***	
	(0.0172)	(0.0172)	
c_{qft}			0.530***
			(0.132)
Observations	49,038	49,038	49,038
R-squared	0.819	0.846	
Years	2018-2019	2018-2019	2018-2019
Estimation	FE	FE	IV

Notes. Column (1) reports the first-stage relationship between the instrument - subsidy caps - and the effective subsidy rate. Columns (2) reports reduced form estimates of the relationship between subsidy caps and per-hour training price. Column (3) reports the IV estimates of the effect of a change in the effective subsidy on the training price. All regressions include fixed effects for year and for training course (an interaction of the training title, online/offline, and training establishment). Standard errors are reported in parentheses and clustered at the interaction between industry financing centers and the training kind category. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.4.3 Changes in CPF Subsidy Affect Producer Revenues and Profits, Not Costs

Finally, we use our training producer-level specification to study the effect of the CPF subsidy on log revenues ($\ln REV_{J,t}$), log costs ($\ln COST_{J,t}$), log profits ($\ln \pi_{J,t}$), log labor costs ($\ln L_{J,t}$), and the log of the number of workers employed in training centers ($\ln E_{J,t}$). The coefficients should thus be interpreted as the percentage changes in the provider-level outcome following a unitary change in the average subsidy guaranteed to a supplier's customers. Table 2.3 reports the results, with IV estimates in the upper panel and reduced form in the lower panel. The magnitude of the coefficients suggest that we observe a 0.6% decrease in revenues for a supplier for each 1 Euro decrease in the effective subsidy (i.e., in the lower panel, a 1 Euro reduction of the subsidy cap leads to a .21 Euros reduction in the effective subsidy and to a 0.13% decline in revenues). Conversely, the effect on costs is smaller and not significant, although still positive. Accordingly, we also find a small significant positive effect on profits: when subsidies decrease, profits decrease by a magnitude that roughly corresponds to the difference in reactions of costs and revenues. The zero effect on costs corresponds to a zero effect on labor costs and number of employees of the training center. Hence, the part of incidence of the subsidy which falls on producers seems to be eventually shifted to owners of capital invested in training centers. Table B.3 report placebos for this producer-level specification, again finding no significant effect. Finally, in Table B.4 in the Appendix we show that the relationship is unchanged using a log transformation to correct for the skewness of $\tilde{c}_{Jt}$ and c_{Jt}.

TABLE 2.3: Impact of changes in CPF subsidy on producers' revenues, costs, profits, labor costs and number of teachers

	(1)	(2)	(3)	(4)	(5)
VARIABLES	$\ln REV_{Jt}$	$\ln COST_{Jt}$	$\ln \pi_{Jt}$	$\ln L_{Jt}$	$\ln E_{Jt}$
c_{Jt}	0.00597***	0.00214	0.00367*	-0.00205	0.000739
	(0.00201)	(0.00265)	(0.00188)	(0.00281)	(0.00234)
Observations	9,604	8,900	8,816	8,472	8,792
Years	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	IV	IV	IV	IV	IV

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	c_{Jt}	$\ln REV_{Jt}$	$\ln COST_{Jt}$	$\ln \pi_{Jt}$	$\ln L_{Jt}$	$\ln E_{Jt}$
$\tilde{c}_{Jt}$	0.218***	0.00130***	0.000464	0.000800**	-0.000444	0.000160
	(0.0142)	(0.000435)	(0.000577)	(0.000406)	(0.000606)	(0.000507)
Observations	9,604	9,604	8,900	8,816	8,472	8,792
R-squared	0.853	0.970	0.964	0.822	0.954	0.957
Years	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	FE	FE	FE	FE	FE	FE

Notes. The upper panel reports the IV estimate of the effect of an increase in the average effective training subsidy for a firms' customers on log revenues, costs, profits, labor costs and employment in the training firm. The lower panel of the table reports the reduced-form estimate of the effect of an increase in the average subsidy cap for a firms' customers on log revenues, costs, profits, labor costs and employment in the training firm. All regressions include year and training firm fixed effects and are weighted by the total value of the firm revenues. Standard errors are reported in parentheses and clustered at the traning firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Intuitively, the relationship between log revenues and the average subsidy cap is mediated by how much of a supplier's revenues is coming from CPF trainings. The share of revenues from CPF can vary across suppliers, as different training centers may target individuals with no CPF (like youth or self-employed at the time of the reform) or as some trainings are simply not eligible for CPF. Table 2.4 reports estimates the effect of the 2019 cut in CPF on suppliers with different share of revenues coming from CPF. The estimates signal that, as expected, the effect of the decrease of the subsidy on profits increases with the share of revenues due to CPF. This is reassuring on the fact that the effect on revenues is actually due to policy changes in CPF, since significant effects materialize only for top two quartiles (which correspond to when CPF revenues are >20% of total revenues). A similar pattern emerges from regressions using profits as outcome.

TABLE 2.4: Impact of changes in CPF subsidy on producers' revenues: heterogeneity by importance of CPF revenues over total revenues

VARIABLES	(1) c_{Jt}	(2) $\ln REV_{Jt}$	(3) $\ln \pi_{Jt}$
$\tilde{c}_t * \mathbb{1}(\frac{RevCPF}{TotRev_{jt_0}} < p20)$	0.00605***	-0.000373	0.000250
	(0.000953)	(0.000482)	(0.000578)
$\tilde{c}_t * \mathbb{1}(p20 < \frac{RevCPF}{TotRev_{jt_0}} \leq p40)$	0.00514***	0.000529	0.000231
	(0.000764)	(0.000554)	(0.000602)
$\tilde{c}_t * \mathbb{1}(p40 < \frac{RevCPF}{TotRev_{jt_0}} \leq p60)$	0.00550***	0.000995**	0.000478
	(0.000683)	(0.000495)	(0.000648)
$\tilde{c}_t * \mathbb{1}(p60 < \frac{RevCPF}{TotRev_{jt_0}} \leq p80)$	0.00595***	0.00137***	0.00139**
	(0.000717)	(0.000490)	(0.000708)
Observations	11,470	11,470	10,756
R-squared	0.860	0.979	0.864
Years	2018-2019	2018-2019	2018-2019

Notes. Column (1) reports the first-stage effect of an increase in the average subsidy cap for a firms' customers on the average effective per-hour subsidy. Columns (2) and (3) report reduced-form estimates of the effect of an increase in the average subsidy cap for a firms' customers on log revenues and profits by quartile of the share of revenues of a firm coming from CPF. All regressions include year and training firm fixed effects and are weighted by the total value of the firm revenues. Standard errors are reported in parentheses and clustered at the training firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

2.5 Mechanisms: Inelastic Demand and Imperfect Competition Can Rationalize Low Pass-Through

In this section we discuss the implications of our results for the economic structure of the training market. A vast literature pioneered by Harberger (1962) studies subsidies incidence under perfect competition. Consider $\frac{dp}{dc}$, the change in the hourly price p paid to the training supplier, gross of the CPF subsidy, following a change in the effective subsidy c. Given our model in Equation 2.1, this parameter is exactly equal to the estimand β_p. The pass-through of the subsidy to consumers will then be $\rho = -\frac{d(p-c)}{dc} = 1 - \beta_p$, which is the change in the price paid by consumers net of the CPF subsidy, when the subsidy changes, with a minus in front, as pass-through are traditionally defined in terms of changes in taxes. Under perfect competition, pass through will be a function of demand and supply elasticities $\epsilon_d > 0, \epsilon_s > 0$, as in Fullerton and Metcalf (2002):

$$\rho = \frac{\epsilon_s}{\epsilon_s + \epsilon_d} \tag{2.6}$$

Note that by the definition of elasticity:

$$\begin{aligned} \epsilon_d = -\frac{dX/X}{d(p-c)/(p-c)} &= -\beta_X/\beta_{\ln(p-c)} \\ &= \frac{\beta_X}{1-\beta_p}(p-c) \end{aligned} \tag{2.7}$$

where the $\beta_.$ terms are all estimand studied in the paper. So we can recover the elasticity of demand in two ways: either using estimated $\hat{\beta_X}$ in Table 2.1 over the estimated percentage change in net prices following a unitary change in the per-hour effective subsidy, $\beta_{\ln(\hat{p-c})}$, estimated in Table B.6 in the Appendix; or following the last equality and using $\hat{\beta_p}$ in Table 2.2 and the average net price. The results obtained are very close and both signal an elasticity of demand close to zero. If ϵ_d is close to zero, then Equation 2.6 suggests that ϵ_s should also be very close to zero to have less than 1-to-1 pass through, if one assumes perfect competition.

On the other hand, the assumption of perfect competition is a strong one in the case of the training market, and we might be interested in studying what rationalizes our results under different degrees of competition. Weyl and Fabinger (2013) show that with suppliers' market power, measured by a parameter θ ranging from zero (perfect competition) to one (monopoly), producers optimization requires that the pass-through of the subsidy results from a mix of elasticities and market power:

$$\rho = \frac{1}{1 + \frac{\theta}{\epsilon_\theta} + \frac{\epsilon_d - \theta}{\epsilon_s} + \frac{\theta}{\epsilon_{ms}}}$$

where $\epsilon_{ms} \in [-\infty, +\infty]$ is the elasticity of the inverse marginal consumers' surplus, and can be interpreted as the degree of convexity of the demand function, with $\epsilon_{ms} = 1$ when demand is linear, $\epsilon_{ms} > 1$ when it's concave, $\epsilon_{ms} < 1$ when it's convex and $\epsilon_{ms} < 0$ when it's log-convex. Instead, ϵ_θ is the elasticity of the competition parameter with respect to quantity.

We can assume $\theta/\epsilon_\theta = 0$, as in Pless and van Benthem (2019), and as implied by Cournot and Dixit-Stiglitz models of oligopoly. Then, we are able to derive an expression for the elasticity of supply ϵ_s which rationalizes our estimates, where the only unknowns are the degree of competition in the market θ and the parameter ϵ_{ms} measuring the concavity of the demand function.

$$\epsilon_s = \frac{\theta - \epsilon_d}{1 - \frac{1}{\rho} + \frac{\theta}{\epsilon_{ms}}} \simeq \frac{\theta \epsilon_{ms}}{\theta - \epsilon_{ms}} \tag{2.8}$$

Where the last passage uses the fact that we estimate ϵ_d close to zero and $1 - \beta_p \simeq 0.5$. The last equation points out that imperfect competition ($\theta > 0$) can rationalize our results allowing for a strictly positive elasticity of training supply, with a concave demand for training (since if $\epsilon_s > 0$ and $0 < \theta < 1$, then $0 < \epsilon_{ms} < \theta < 1$). Figure 2.7 reports the values of ϵ_s estimated.

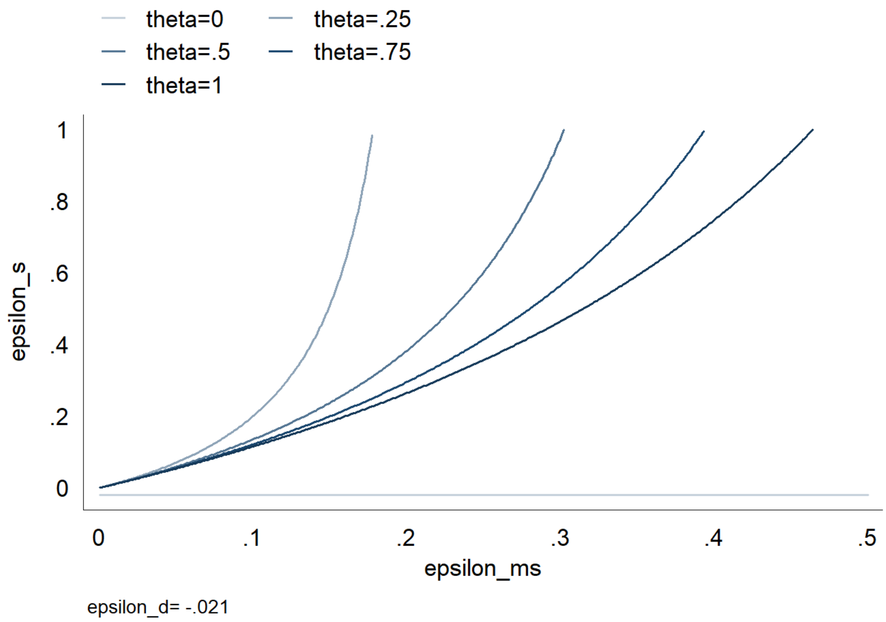


FIGURE 2.7: Values of the estimated elasticity of the supply of training ϵ_s as a function of market power θ and the elasticity of consumers' marginal surplus ϵ_{ms}

Notes. The Figure reports the values of the elasticity of supply (epsilon_s) in terms of market power theta and elasticity of consumers' marginal surplus, epsilon_ms (which measures the concavity of the demand function), obtained by substituting the estimates of the elasticity of demand and of pass-through from our data, in the equation of pass-through with market power derived by Weyl and Fabinger (2013).

In practice, we argue that a perfectly inelastic supply of training is unlikely for both quantitative and qualitative. Quantitatively, we showed how a reduction in the CPF subsidy leads to a substantial reduction in training prices gross of the subsidy. Although formally an industry can still be competitive and report profits as a form of remuneration for capital invested, such a significant fall in profits should lead to a substantial exit of marginal producers from the training market, which Table B.5 in the Appendix seems to exclude. Qualitatively, a near-perfectly inelastic supply of training is implausible. Marginal costs of training are simply made of labor costs and the rental cost of classrooms or training locations and equipment, and none of them seems to be sufficiently inelastic. Indeed, Table 2.3 shows that workers capture no rents from the subsidy, hence are likely an elastic factor, and although there is some evidence of inelastic housing supply in France (Fack, 2006), no increase in the number of students per-course was observed.

If supply is not perfectly inelastic, then competition in the training market has to be less than perfect to rationalize our results. This explanation is also a more credible one, as there are several reasons why the market for training can be imperfectly competitive. For example, market power can arise from asymmetric information in the training market. DARES (2018) highlights that poor quality of a training course was one of the most significant concerns of CPF users. Consumers might thus face high switching costs to ascertain the quality of a competitor. A second hypothesis is linked to regulatory barriers: with imperfect information, and precisely to avoid that the training market becomes a market for "lemons", French regulators required public certification for training centers in order to be eligible for public subsidies. Although reducing the problem of asymmetric information, this can make entry in the market more difficult. Finally, when the subsidy is so generous to cover the full cost of training, as often the case in CPF, consumers might be less responsive to price signals.

2.6 Welfare Effects

As a final step of our analysis we study the implication of our results for welfare. We start from the simpler case of perfect competition. The sufficient statistics approach (Chetty, 2009; Kleven, 2020) suggests that welfare consequences of policies can be derived as a function of high-level reaction of quantities to subsidy changes rather than deep primitives, maintaining validity under a wide array of assumptions about such primitives. This approach is not new in the study of taxes and subsidies: Harberger (1964) famously showed that the efficiency cost of small tax changes can be estimated using a simple elasticity-based formula. As reported in Chetty (2009), the Harberger model implies that the welfare of a representative consumer with utility $u(\boldsymbol{x})$ and monetary endowment ω given a subsidy ν on good x_1 is:

$$w(\nu) = \max_{\boldsymbol{x}}[u(\boldsymbol{x}) + \omega - \nu x_1 - \boldsymbol{p}(\nu)\boldsymbol{x}] + \max_{\boldsymbol{x}}[\boldsymbol{p}(\nu)\boldsymbol{x} - COST(\boldsymbol{x})] - \nu x_1$$

As a first step, we can adapt Harberger's approach to CPF subsidies. This requires taking the above expression for individual welfare, and allow the effective subsidy to be the maximum between the hourly price and the cap to per-hour subsidy. Assuming that welfare weights are equal across individuals and normalized to one, total welfare can be calculated as the sum of individuals' welfare. Finally, we assume that $u(\boldsymbol{x}) = \phi(x_i) + m_i$ where $\phi(x_i)$ is the utility from training episode i and

m_i is the value of leftover money:

$$W(\boldsymbol{c}) = \sum_i \max_{x_i}[\phi(x_i) + m_i + \min(p_{q,f,t}, c_{\tilde{q,f,t}})x_i - p_{q,f,t}x_i)] + \max_{x_i}[p_{q,f,t}x_i - COST(x_i)] - \min(p_{q,f,t}, c_{\tilde{q,f,t}})x_i$$

$$= \begin{cases} \sum_i \max_{x_i}[\phi(x_i) + m_i + c_{\tilde{q,f,t}}x_i - COST(x)] - c_{\tilde{q,f,t}}x_i & \text{if} \quad p_{q,f,t} \geq c_{\tilde{q,f,t}} \\ \sum_i \max_{x_i}[\phi(x_i) + m_i + p_{q,f,t}x_i - COST(x)] - p_{q,f,t}x_i & \text{if} \quad p_{q,f,t} < c_{\tilde{q,f,t}} \end{cases}$$

$$\frac{dW(\boldsymbol{c})}{d\boldsymbol{c}} = \begin{cases} \sum_{q,f} \sum_{i \in q,f,t} -x_i + x_i - c_{\tilde{q,f,t}} \cdot \frac{dx_i}{dc_{\tilde{q,f,t}}} & \text{if} \quad p_{q,f,t} \geq c_{\tilde{q,f,t}} \\ \sum_{q,f} \sum_{i \in q,f,t} -\frac{dp_{q,f,t}}{dc_{\tilde{q,f,t}}}x_i + \frac{dp_{q,f,t}}{dc_{\tilde{q,f,t}}}x_i - p_{q,f,t} \cdot \frac{dx_i}{dc_{\tilde{q,f,t}}} & \text{if} \quad p_{q,f,t} < c_{\tilde{q,f,t}} \end{cases}$$

$$= \sum_{q,f} -\frac{d \ln X_{q,f,t}}{dc_{\tilde{q,f,t}}} X_{q,f,t} \min(p_{q,f,t}, c_{\tilde{q,f,t}})$$

The last two lines write down how to recover the change in aggregate welfare for one extra euro of CPF subsidy for each eligible training hour in the sample, which we can recover using estimates of the reaction of total quantities to changes in the maximum cap of the subsidy, as in Table 2.1, and the actual subsidy used by each individual and in each industry/financing center. Then, one can divide by the number of hours of training to to obtain $\frac{dW(c)}{dc} = \frac{dW(\boldsymbol{c})}{d\boldsymbol{c}} / \sum_{q,f} X_{q,f,t}$, the average change in aggregate welfare from an additional Euro spent in CPF. Such estimates of are thus reported in Table 2.1. Not surprisingly, as the reaction of quantities is close to zero, the estimated impact on welfare of an additional Euro of CPF subsidy is also low.

Chetty (2009) shows that also in the presence of heterogeneity of preferences and discrete choice models the elasticity of the equilibrium quantity of the taxed/subsidized good with respect to the tax/subsidy is a sufficient statistic for estimating the change in welfare due to a marginal change in the tax/subsidy. However, this approach fails for large changes in a tax/subsidy, since behavioral responses $\frac{dx_i}{dc}$ in the consumer problem might not be ignored anymore. Kleven (2020) starts from the consideration that one can write a discrete welfare change, if welfare is a function of a policy variable, as the integral of the marginal welfare changes between initial and final values of the policy. This allows to derive a formula for changes in welfare following a change in the policy, with corrections for changes in tax wedges and elasticities. Assuming iso-elastic preferences, Kleven (2020)'s formula adapted to our case is:

$$\frac{\Delta W(\boldsymbol{c})}{\Delta \boldsymbol{c}} \approx \sum_{q,f} \frac{d \ln X_{q,f,t}}{dc_{\tilde{q,f,t}}} X_{q,f,t} \Big\{ \frac{c_{q,f,t-1}}{p_{q,f,t-1}} + \frac{1}{2} \Big[\min(1, \frac{c_{\tilde{q,f,t}}}{p_{q,f,t}}) - \min(1, \frac{c_{q,f,t-1}}{p_{q,f,t-1}}) \Big] \Big\}$$
$$\cdot [\min(p_{q,f,t}, c_{\tilde{q,f,t}}) - \min(p_{q,f,t-1}, c_{q,f,t-1})] \tag{2.9}$$

Since this quantities can all be estimated, in the last line of Table 2.1 we can report $\frac{\Delta W(c)}{\Delta c} = \frac{\Delta W(\boldsymbol{c})}{\Delta \boldsymbol{c}} / \sum_{q,f} X_{q,f,t}$, the average change in aggregate welfare from one euro more invested in CPF, which remains close to zero.

Finally, we can study the welfare effects of CPF in the case of imperfect competition. Adachi and Fabinger (2022) study welfare effects under imperfect competition of an increase in taxes, showing that market power amplifies the deadweight loss arising from a per-unit tax. Conversely, in the case of subsidies market power can reduce the deadweight loss, as it increases the quantity consumed closer to the efficient level (up to making a subsidy welfare-improving, as noted already by Auerbach and Hines, 2001). Nonetheless, in our results we find a null reaction of quantities to subsidies, and an elasticity of demand close to zero. This corresponds to a corner solution in Adachi and Fabinger (2022), so that even with imperfect competition the subsidy should have no effect on aggregate welfare.

2.7 Conclusions

In this paper we studied the effect of training subsidies on training participation, their incidence and welfare effects. To summarize, our empirical analysis delivers five results. First, the change (mostly a decrease) in the per-hour CPF subsidy occurred in 2019 did not significantly affect training participation. Second, this happened as the change in the subsidy was partially absorbed by prices, and the subsidy was passed-through to consumers only by 47%. Hence, we can infer that more than half of the incidence of the CPF training subsidy falls on training producers. Third, we show that producers suffer a reduction of revenues and profits, with no effect on costs, including labor costs and employment of trainers. Fourth, this can be rationalized by inelastic demand for training, and by either inelastic supply or imperfectly competitive training markets, which we argue is a more likely scenario. Fifth, in the case of CPF, training subsidies were a simple transfer to training producers and consumers, with no effect on aggregate welfare.

Our paper is an important insight for the literature studying on-the-job training and training policy. Scholars never considered until now that training subsidies are often implemented through a training market, and that general equilibrium effects of training subsidies on prices might attenuate the effect of the subsidy on training participation. Concerning future paths for research, one implication of our results is that demand for training is quite inelastic. This seems at odds with the claim that there is a large potential demand for training which is under-financed, and suggests the importance of understanding what are the reasons for which demand for training is inelastic. Another implication is that either supply of training is inelastic or the market for training is imperfectly competitive. Again, the determinants behind both of these two explanations are also left to future research.

Finally, this paper has relevant implications for training policy. We show how subsidies like CPF risk ending up in a transfer, mostly to producers, if supply is relatively inelastic or the market is less than competitive. Policy makers who want to support human capital investment must – before subsidizing it – ensure that supply is sufficiently elastic and the market competitive. Interestingly, regulators might face a tradeoff between the need to guarantee training quality (Acemoglu and Pischke, 1999) and the risk that certifications become an entry barrier, reducing competition. To sustain lifelong learning, it might not be sufficient to simply assign enough resources to general-skills training, but an effective human capital policy should take into comprehensive consideration the design of the training market.

Chapter 3

The Impact of Austerity Policies on Local Income: Evidence from Italian Municipalities

This chapter is based on a joint work with Andrea Cerrato (University of California, Berkeley)

Abstract

Fiscal consolidation is often a necessity for local governments, but the cost of austerity for local economic activity is an open empirical question. Quasi-experimental estimates of local fiscal multipliers range between 1.5 and 1.8, but most of them are obtained from expansionary shocks. We study the extension of tighter budget rules in 2013 to Italian municipalities below 5,000 inhabitants, which generates a persistent increase of about 100 Euros per capita (0.5% of local income) in municipal net budget surplus, mostly driven by a cut in capital expenditures. We find no decrease in local income over a eight-year horizon. The estimated multiplier is always not significantly different from zero, and we can exclude it is above 1.5 with 95% confidence within 4 years from the shock. We find no evidence of spillovers to neighboring municipalities. These results suggest that the cost of fiscal consolidation can be lower than what currently prevailing estimates of local multipliers imply.

Keywords: fiscal consolidation, fiscal policy, budget deficit, local economy multiplier

JEL Codes: E62, H71, H72

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3.1 Introduction

Fiscal consolidation is often a necessity for central and local governments, but the impact of austerity policies on local economic activity is an open empirical question. Most estimates of local fiscal multipliers range between 1.5 and 1.8. Such estimates are usually obtained from expansionary shocks (e.g., increased military spending, countercyclical government spending shocks, etc.). However, it is reasonable to hypothesize that central and local governments endogenously seek to minimize their impact on the economy when implementing fiscal consolidation and to maximize it when implementing a fiscal expansion. Moreover, significant differences might be present between local fiscal multipliers generated by windfall spending vs. policies that maintain intertemporal budget balance. As a consequence, fiscal multipliers could be asymmetric in times of fiscal consolidation vs. fiscal expansion or in windfall vs. intertemporal budget balance scenarios. How do estimated multipliers from the imposition of contractionary fiscal rules compare to the ones previously estimated in the literature? How do local austerity policies impact local income and through which channels?

This paper seeks to answer these questions, studying the extension of a tight budget rule to Italian municipalities below 5,000 residents in 2013. Figure 3.1 compares mean municipal surplus per capita for treated municiaplities (i.e., with 2011 population between 1,000 and 5,000 inhabitants) and control municipalities (i.e., with 2011 population between 5,001 and 9,000 inhabitants). The red line indicates the year in which tight fiscal rules are extended to municipalities with less than 5,000 inhabitants. As the figure shows, the regulation permanently increases budget surplus per capita in treated municipalities, relative to the pre-shock period.

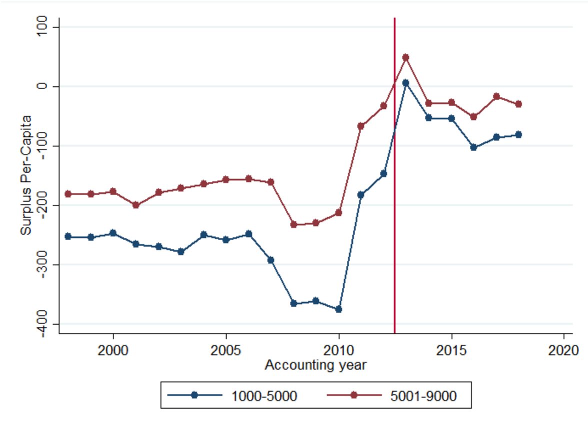

FIGURE 3.1: Per capita surplus and surplus net of central government transfers by municipality population

Notes. The graph reports budget surplus per-capita defined as fiscal, non-fiscal and capital revenues minus current and capital expenditures. The two series correspond to the average for municipalities having population between 1000 and 5000 and between 5001 and 9000 in 2011.

We provide three main results. First, treated municipalities comply to the newly introduced fiscal rule increasing municipal budget surplus by 100 Euros per capita (i.e., about 0.5% of municipal income). To reach this objective, treated municipalities mostly decrease municipal capital expenditures. Second, municipal fiscal consolidation has no impact on municipal income, over a eight-year horizon. The estimated local fiscal multiplier is always not significantly different from zero, and we can exclude it is above 1.5 with 95% , within 4 years from the shock. Finally, we find no evidence of spillovers to neighboring municipalities.

This paper relates to three main streams of literature. First, the literature on local fiscal multipliers. This literature has mostly focused on the impact of expansionary fiscal shocks, and has reached a wide consensus on estimates ranging between 1.5 and 1.8 for local fiscal multipliers (Chodorow-Reich, 2019). Such consensus is based on a number of studies that estimated the impact of the American Recovery and Reinvestment Act (i.e., ARRA) after the Great Recession exploiting heterogeneity of Federal spending across locations. A comprehensive list of these studies include Chodorow-Reich et al. (2012), Conley and Dupor (2013), Dube et al. (2018), Dupor and McCrory (2018), Dupor and Mehkari (2016), Feyrer and Sacerdote (2012), and Wilson (2012). Other studies exploiting non-ARRA induced geographical variation in spending find overall similar estimates. For instance, Nakamura and Steinsson (2014) exploit state-level variation in US military spending, estimating a local fiscal multiplier of 1.5. Other important contributions in this literature include Acconcia et al. (2014), Adelino et al. (2017), Corbi et al. (2019), Shoag (2013), Leduc and Wilson (2013), and Serrato and Wingender (2016). Our baseline 3-year-horizon point estimates for the local austerity multiplier range between -0.31 and 0.06, consistent to what Clemens and Miran (2012) find. The difference between our estimates and the ones prevailing in the literature can be rationalized by asymmetric optimization of government spending between contractions and expansions, as well as by the presence of "Ricardian" effects which differentiate persistent local budget shocks from transitory windfalls induced by central government fiscal policy. An important takeout from the literature on local fiscal multipliers is that researchers should be cautious when comparing estimates of the cross-sectional multiplier to estimates of the aggregate multiplier. Indeed, the presence of labor market or goods market spillovers across regions could make the local fiscal multiplier larger or smaller than its aggregate counterpart. This concern motivates our analysis of spatial spillover effects of austerity policies.

Second, the paper relates to the literature on local public finance shocks in Italy, exploiting discontinuities induced by changing fiscal rules. Our exercise is similar to Grembi et al. (2016), Coviello et al. (2017), and Daniele and Giommoni (2021). Grembi et al. (2016) study a relaxation of fiscal rules for Italian municipalities below 5,000 inhabitants, finding that treated municipalities run higher budget deficits and decrease taxes as a result of the shock. Coviello et al. (2017) exploit a tightening of the Domestic Stability Pact in Italy occurring in 2008, when only muncipalities with population above 5,000 inhabitants were subject to the Pact. They find that budget tightening resulted in lower infrastructure spending and unchanged current expenditures. They also find that affected firms in the upstream sector react to the negative demand shock by cutting capital rather than labor. Daniele and Giommoni (2021) use the same discontinuity we exploit to show that tightening municipal budgets results in lower capital expenditures, which in turn reduced corruption cases at the local level without significantly affecting local amenities. Our results are consistent with Daniele and Giommoni (2021) and document that tightening municipal budgets induce a different response than relaxing municipal budgets. We expand this literature providing an estimate of the dynamic effect of austerity policies on local income and testing spatial spillovers.

Finally, this paper relates to the literature on the impact of austerity policies. Alesina et al. (2019) provides an extensive review of this literature. One important caveat to keep in mind when relating our results to the ones in this literature is that we estimate an income multiplier, while macroeconomics literature usually estimates output multipliers. To the extent that fiscal consolidation induces a decrease in corporate profits, the income multiplier will be downward biased relative to the output multiplier. However, many papers exploiting cross-sectional variation to properly identify the parameter of interest use income or employment as proxies for value added (see Chodorow-Reich (2019)).

The reminder of the the paper is structured as follows. In the first part, we describe the institutional setting in which our quasi-experimental study takes place. Specifically, we discuss the role of municipalities in Italy and the historical evolution of the fiscal rules to which Italian municipalities have been subject since 1999, including the discontinuity we exploit for our study. In the second part of the paper, we discuss the data sources and the identification strategy we use to estimate the parameter of interest. The extension of fiscal rules around the 5,000 residents population threshold makes a difference-in-discountinuity approach (Grembi et al. (2016)) appealing in our setting. Then, we present our main findings on the direct impact of austerity policies on the municipalities forced to implement them, as well as the spillover effects on neighboring municipalities. Finally, we conclude discussing the implications of our results.

3.2 Empirical Strategy

3.2.1 Institutional Setting

In Italy, municipalities are the lowest level of subnational government. There are roughly 8,000 municipalities, with a median population around 2,500 and mean around 7,400 in 2011. Each municipality is administered by an elected mayor, an executive body appointed by the major, and an elected council. The total amount of municipalities' budgets was around 75 billions in 2004 (5.2 % of GDP) and went down to 57 billions in 2018 (3.2 % of GDP). Municipalities use such budget to cover services of their responsibility, which include local administration, utilities and waste management, municipal roads and transportation, schools building, social housing and services, and small services for tourism and economic development. Revenues come in large part from own fiscal revenues (32%), i.e., property tax and a surcharge on the income tax, and from non-fiscal revenues (21%), such as fees from building permits, traffic fines, parking and utilities fees. The upper levels of administration – regions and the central state – contribute to the financing of municipalities by covering on average 37% of municipal revenues with current and capital transfers. Finally, municipalities are also allowed to borrow, as 10% of the budget on average is raised through loans (historically from the Italian Public Investment Fund, but increasingly also by private banks) or issuing bonds[1].

Since 1999, Italian municipalities were subject to the so-called Domestic Stability Pact (DSP), with the purpose of containing municipal budget deficits. In fact, European treaties considered local government deficits as part of general government ones, which are in turn subject to common limits at the European level. Hence, not only Italy but several European countries tried to regulate

[1]The remaining revenues are accounted by clearing entries and transactions on behalf of others such as retained social security contributions from employees.

local administrations' deficits[2]. Yet, in Italy the need for a deficit reduction was particularly salient in the second half of 1990s – when the country struggled to comply with requirements of the European Monetary Union and the DSP was introduced – and after the crisis of Sovereign Debts, when the country debt-to-GDP ratio reached 135%. Beside debt reduction and compliance with European rules, the central government also aimed at preventing moral hazard from lower levels of government (Alesina and Tabellini, 1990). Bail-out or default of lower administrations is in fact not uncommon in Italy[3], and the risk is worsened by the low salience of municipal finances (Murtinu et al., 2021) or even by criminal infiltration (Acconcia et al., 2014).

The precise rules of the DSP varied over time, as summarized by Table C.1 in the Appendix. Initially, between 1999 and 2004 included, the DSP targeted deficit growth, either imposing zero or minimal growth with respect to two years before. A notable exception are 2005 and 2006, in which a stricter joint threshold on current and capital expenditure was implemented, just to go back to the zero-growth in budget deficit in 2007[4]. Importantly, from 2011 onward, the DSP became more and more restrictive, requiring a structural zero-deficit level goal, to be pursued through a yearly budget deficit proportional to a moving average of previous ones. Municipalities which did not comply with DSP were subject to mandatory measures, including a cap on the growth of current expenditures, ban on new hires and on borrowing to finance investment, a cut in administrators' bonus and wages, and a reduction of central government transfers. Crucially for our identification, while municipalities below 5000 inhabitants were exempted from the DSP since 2001, the DSP was extended to all municipalities above 1,000 inhabitants in 2013[5]. Finally, starting in 2016 the DSP was "abolished", although this meant that it was simply replaced by a zero-deficit requirement on an accrual basis.

3.2.2 Data, Sample Selection and Variables of Interest

We use two administrative sources. The first one are balance sheets from Italian municipalities collected by the Italian Ministry of Interior, which contain detailed information of all revenues and expenditures for Italian municipalities from 1998 to 2018. From the dataset, we extract the

[2]For example, similar mechanisms are in place in Germany, Spain, Austria and Belgium. In some countries the introduction of the rules is relatively recent, such as the *objectif d'évolution des dépenses locales (ODEDEL)* adopted in 2018 in France.

[3]For example, in the case of Rome (Law 122/2010), and recently during the Covid pandemic (Law Decree 73/2021). In 2013, the European Court for Human Rights has even imposed to the Italian state remarkable liability for credits of defaulted municipalities (De Luca vs. Italy, 2013).

[4]Note that from 2008 to 2015, the deficit considered for assessing the respect of DSP rules started being calculated on a "Mixed basis", meaning that current revenues and expenditures were accounted for on an accrual basis while capital revenues and expenditure were accounted on a cash basis.

[5]Municipalities between 3000 and 5000 inhabitants were initially foreseen to be subject to the DSP in 2005 and 2006, but their inclusion was suspended and never reconsidered in mid 2005.

total capital and current revenues and expenditures on an accrual basis, the breakdown of revenues into fiscal vs. non-fiscal revenues, borrowing and transfers, and the breakdown of expenditures by functional destination. The format of the balance sheet used by Italian municipalities underwent a change in 2015, which modified the way some of our variables of interest are reported. We provide a correspondence between variables from the old and new format in Table C.2 in the Appendix, and in Figure C.1 in the Appendix we plot the average value for all our variables of interest across the 2015 discontinuity. No clear discontinuity appears in the relevant variables.

The second source are data on income tax declarations at the municipality level elaborated by the Italian Ministry of Finance. This source covers all income subject to the standard income tax in Italy declared yearly by individuals, hence it fails to cover individuals with only income from capital invested in firms with more than one employee, capital income from housing rents, or the informal sector. On average, income reported in income tax declarations corresponds to roughly half of Italian GDP. The information in the dataset includes the total number of declarations, total income declared, income tax due, income from different sources (labor, self-entrepreneur, rents, pensions) and from declarations belonging to different tax brackets.

Table C.3 in the Appendix reports descriptive statistics of the dataset obtained by merging our sources. We split descriptives for the 2007-2012, which is our pre-shock period; for 2013-2015, i.e. three years after the shock; and for 2016-2018, i.e. after the format change in balance sheet data, including also 2019 and 2020 for income data. For balance sheet data, we report information on all municipalities for which it was possible to recover their fiscal code. In fact, the correct association of balance sheets to municipalities requires using correspondence tables between municipality balance sheet code and fiscal code, provided by the Italian Ministry of Interior, which fail to cover older municipalities and determines a substantial loss of municipalities in earlier periods. Table C.4 in the Appendix reports instead the same descriptive statistics, this time restricting the sample to the one we use for our analysis. This sample is the result of three restrictions. First, we keep only the 15 ordinary regions who applied a uniform version of the DSP. Second, we drop municipalities that were merged, and restrict to municipalities with no missing information in either balance sheet or income data between 2007 and 2018, so as to obtain a balanced panel. Finally, we keep municipalities having a number of inhabitants between 2,000 and 8,000 in the 2011 census, which comprise all the municipalities in the different bandwidths around the threshold of 5,000 we are going to use.

Our main outcomes of interest are total income in the municipality and net surplus. We observe income in current Euros, including in the sample few observations reporting negative declared income (due to tax credits). We measure net surplus as the difference between fiscal and non-fiscal current revenues net of current transfers from other branches of government, plus capital and financial revenues net of capital transfers from other branches of government, minus current and capital expenditures. Transfers are netted out from revenues because these entries are not raised in the municipality, and thus do not constitute a direct loss of income or resources for taxpayers of the municipality. Before running regressions, we winsorize outliers in per-capita income and net surplus at the 1% level. All monetary values are also corrected for inflation and expressed in 2012 Euros.

3.2.3 Identification

To study the effect of budget restrictions on local economies, we use a difference-in-discontinuities approach identification strategy (Grembi et al., 2016). The DSP was in fact sharply applying to municipalities above 5,000 inhabitants between 2001 and 2012, and was then extended to municipalities with population between 1,000 and 5,000 inhabitants from 2013 onward. Our treatment group (resp. control group) is made of municipalities just below (resp. above) the 5,000 inhabitants cutoff. Treatment group municipalities are, before 2013, comparable in all fundamental characteristics to municipalities above the treshold but differ sharply in the DSP and its correlated aspects (Daniele and Giommoni, 2021). However, administrative rules on the composition and election of municipal councils vary around the 5,000 threshold (Gagliarducci and Nannicini, 2013), making the assumptions of a traditional RD design fail. Hence, we exploit the longitudinal shock provided by the extension of DSP to net-out these pre-differences and identify the effect of the DSP.

Formally, let individuals be indexed by i, being resident in municipality j, with net surplus $NET\_SUR_{j,t}$, population $POP_{j,t}$, and total number of declarations $n_{j,t}$. Our diff-in-disc first-stage and reduced-form regressions are as follows:

$$\begin{aligned}
\frac{NET_SUR_{j,t}}{POP_{j,2011}} =& \beta_{FS}\mathbb{1}(t > 2012, POP_{j,2011} < 5000) \\
&+ \sum_{h \neq 0} [\pi^h POP_{j,t}\mathbb{1}(t = 2012 + h) + \delta^h POP_{j,t}\mathbb{1}(t = 2012 + h, POP_{j,2011} < t)] \\
&+ \gamma_j + \tau_t + \varepsilon_{j,t} \qquad \text{(First stage)} \\
\frac{\sum_{i \in j} Y_{i,t}}{POP_{j,2011}} =& \beta_{RF}\mathbb{1}(t > 2012, POP_{j,2011} < 5000) \\
&+ \sum_{h \neq 0} [\pi^h POP_{j,t}\mathbb{1}(t = 2012 + h) + \delta^h POP_{j,t}\mathbb{1}(t = 2012 + h, POP_{j,2011} < t)] \\
&+ \gamma_j + \tau_t + \varepsilon_{j,t} \qquad \text{(Reduced form)}
\end{aligned}$$

Where $Y_{i,t}$ is declared income for individual i at time t, γ_j and τ are municipality and year FEs respectively. We can also define a fully-dynamic specification in which $\beta_{...}\mathbb{1}(t \geq 2013, POP_{j,2011} <$

5000) is substituted by $\sum_{h\neq 0}[\beta^{h}_{...}\mathbb{1}(t = 2012 + h, POP_{j,2011} < 5000)]$, which is the dynamic difference-in-discontinuity change in the outcome at an h years horizon with respect to the baseline year 2012.

Our difference-in-discontinuities approach is equivalent, in terms of point estimates, to a parametric RDD applied to long differences with respect to baseline, with polynomial fit of order one. Hence, we should be careful when choosing the relevant bandwidth on which we estimate our model around the threshold of 5,000 inhabitants Calonico et al. (2014). In Table C.5 in the Appendix we report the optimal bandwidth values estimated following Calonico et al. (2020) on the RDD of long differences in per-capita surplus (equivalent to our static first stage) and income (equivalent to our static reduced form). The values for the reduced form vary between 1000 and 2500, according to different criteria for the estimator and bias-correction. To adapt this insight to our difference-in-discontinuities, we present our baseline local linear regression estimates for round values covering the whole range of optimal bandwidths, namely ± 1000, ± 1500, ± 2000, and ± 2500. We also cluster standard errors at the municipality level following Bertrand et al. (2004); Abadie et al. (2017), and account for this in choosing the optimal bandwidth.

The key assumption of our model is what we call the Common Trend in Discontinuities (CTD) assumption, i.e. that there is no pre-trend in the difference of outcomes of municipalities just above and below the 5,000 inhabitants discontinuity. Namely, $\forall t < 2012$:

$$
\begin{aligned}
&\mathrm{E}(\frac{NET\_SUR_{j,t}(0) - NET\_SUR_{j,2012}(0)}{POP_{j,2011}}|POP_{j,2011} \in [5000-\epsilon, 5000]) = \\
&= \mathrm{E}(\frac{NET\_SUR_{j,t}(0) - NET\_SUR_{j,2012}(0)}{POP_{j,2011}}|POP_{j,2011}[5000, 5000+\epsilon]) \qquad \text{(CTD for first stage)} \\
&\mathrm{E}(\frac{\sum_{i\in j} Y_{i,t}(0) - \sum_{i\in j} Y_{i,2012}(0)}{POP_{j,2011}}|POP_{j,2011} \in [5000-\epsilon, 5000]) = \\
&= \mathrm{E}(\frac{\sum_{i\in j} Y_{i,t}(0) - \sum_{i\in j} Y_{i,2012}(0)}{POP_{j,2011}}|POP_{j,2011}[5000, 5000+\epsilon]) \qquad \text{(CTD for reduced form)}
\end{aligned}
$$

Where ϵ is arbitrarily small. Note that a threat to our CTD assumption requires not only a sharp change at the threshold of 5,000 inhabitants, for example mayor's salary (Gagliarducci and Nannicini, 2013), but also that these sharp discontinuities vary over time or have a significantly time-varying impact on our outcomes. While no rules change regarding the 5,000 threshold occurs in the period of our analysis, we can test an implication of the CTD assumption, namely that $\beta^{\hat{h}}_{FS}$ and $\beta^{\hat{h}}_{RF}$ are not significantly different from zero for all years preceding the shock, when $h < 0$.

Under CTD, the estimated coefficient $\beta^{\hat{h}}_{FS}$ is an estimator of the change in net surplus per-capita at an h-years horizon relative to the baseline year, 2012. In turn, the estimated coefficient $\beta^{\hat{h}}_{RF}$ is an estimator of the change in income per-capita with relative to 2012. By dividing the two estimators we obtain the multiplier at an h-years horizon of an extra euro of net fiscal deficit:

$$\frac{\beta^{\hat{h}}_{RF}}{\beta^{\hat{h}}_{FS}} = -\frac{\mathrm{E}(Y_{j,2012+\hat{h}} - Y_{j,2012})}{\mathrm{E}(NET\_SUR_{j,2012+h} - NET\_SUR_{j,2012})} \tag{3.1}$$

3.3 Results

3.3.1 Budget Surplus and Local Income

Table 3.1 reports our difference-in-discontinuities estimates of the effect of DSP on per-capita surplus and local income. The upper panel reports the results considering the period up to 2015 included, i.e. excluding the period in which the new format of municipalities balance-sheets are introduced. The results point out a strong and significant effect of the extension of DSP on the net per-capita surplus run by municipalities below 5,000 inhabitants, which increases between €135 (1.1% of average income) and €65 (0.5% of average income) depending on the bandwidth used. In spite of this significant increase in municipalities' budget surplus, per-capita income declared do not react significantly. The estimated coefficients in columns 2, 4, 6, and 8 are either negative or slightly positive, but all not significantly different from zero. Consequently, the estimated multiplier is small and not significantly different from zero. Furthermore, standard errors indicate that we can exclude at 95% confidence that the multiplier is 1.5 or larger.

TABLE 3.1: Effect of DSP on Per-Capita Surplus and Local Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Surplus pC	Income pC	Surplus pC	Income pC	Surplus pC	Income pC	Surplus pC	Income pC
DSP	128.9***	-7.403	83.06***	11.50	62.02***	17.67	60.71***	19.26
	(29.42)	(97.23)	(23.83)	(79.35)	(20.68)	(68.46)	(19.87)	(61.46)
Observations	5,580	5,580	8,433	8,433	11,574	11,574	15,093	15,093
R-squared	0.513	0.983	0.500	0.983	0.506	0.982	0.500	0.982
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Bandwidth	1000	1000	1500	1500	2000	2000	2500	2500
Mean in 2012	-276.3	12664	-278.7	12650	-284.7	12640	-300	12584
Multiplier		.057		-.138		-.284		-.317
		[.757]		[.949]		[1.08]		[.998]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Surplus pC	Income pC	Surplus pC	Income pC	Surplus pC	Income pC	Surplus pC	Income pC
DSP	104.0***	-30.58	68.99***	4.140	61.36***	7.737	65.07***	12.92
	(24.03)	(131.0)	(19.14)	(105.1)	(16.37)	(91.37)	(15.61)	(81.70)
Observations	7,440	7,440	11,244	11,244	15,432	15,432	20,124	20,124
R-squared	0.538	0.975	0.529	0.975	0.531	0.974	0.523	0.974
Years	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018	2007-2018
Bandwidth	1000	1000	1500	1500	2000	2000	2500	2500
Mean in 2012	-275.4	12677	-278.3	12663	-284.3	12654	-299.5	12599
Multiplier		.294		-.06		-.126		-.198
		[1.27]		[1.52]		[1.48]		[1.25]

Notes. The Table reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities between 1,000 and 5,000 inhabitants, from 2013 onward on their net per-capita surplus and income. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors in the last row are calculated with an IV regression of per-capita income on net surplus, instrumented by the DSP dummy.

The lower panel of Table 3.1 reports estimates for the full sample, including the last three years, using our correspondance between balance-sheet items before and after 2015. The results are similar, although the reduced-form on income is noisier due to the auto-regressive nature of per-capita income, which makes long-difference estimators noisier at longer horizons. A more complete picture is provided by Figure C.2, which reports the results of the fully dynamic specification.

The left column reports the estimated coefficients for fully dynamic first stage and reduced form. The series on the effect on surplus per-capita provide striking evidence of how, after a five-year parallel trend, a sharp and permanent increase in surplus per-capita occurs. Conversely, income remains unaffected, except for a small insignificant dip 5-7 years after the fiscal contraction. In the right column, we compute the implied multipliers at different horizons after the shock, which are consistently around zero, although the estimate becomes noisier, as the estimated effect on income per-capita becomes noisier at longer horizons.

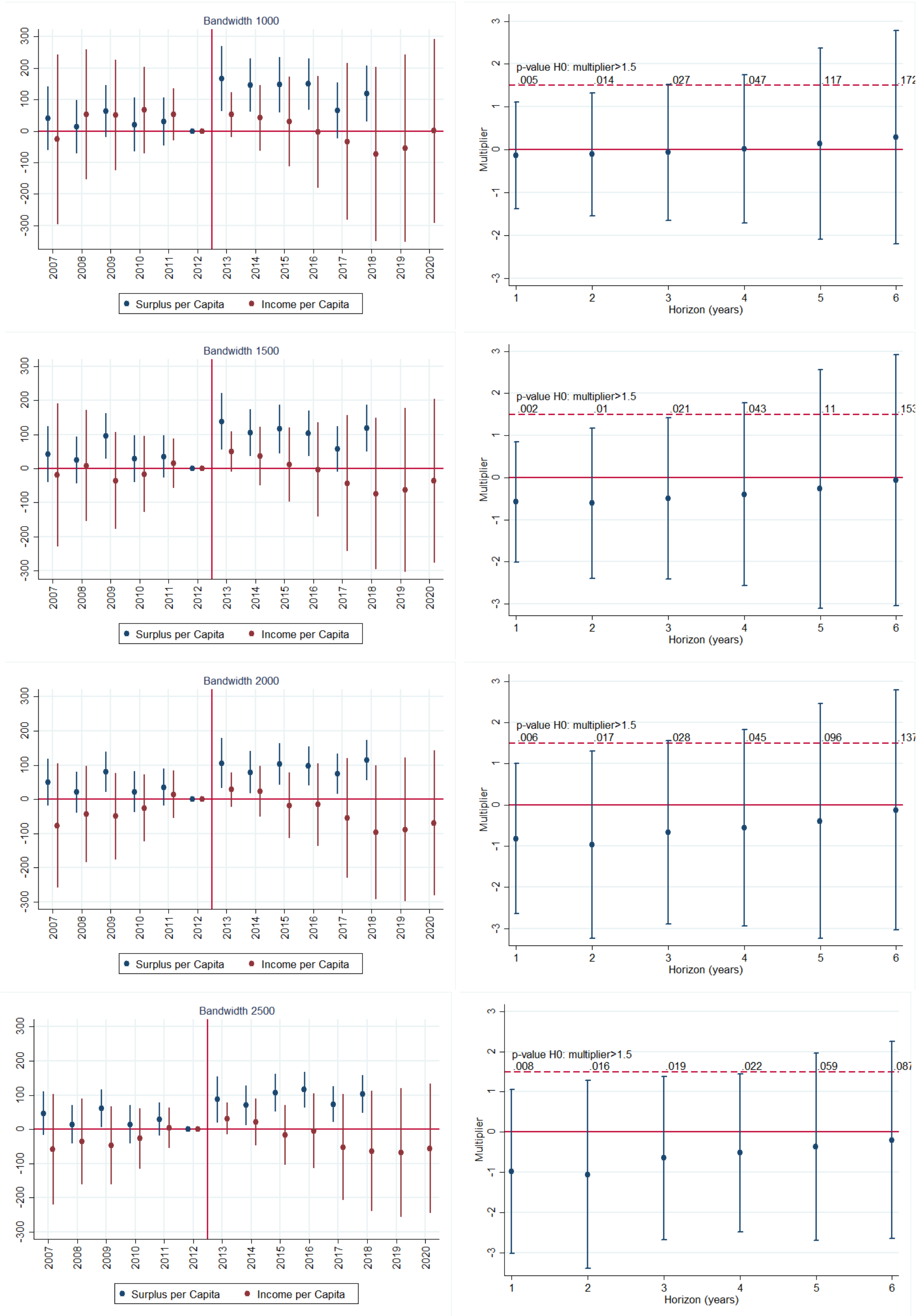

FIGURE 3.2: Dynamic Effect of DSP on Per-Capita Surplus and Local Income

Notes. The figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors on the right are calculated with an IV regression of per-capita income on net surplus, instrumented by the DSP dummy, keeping observations only up to a specific horizon after the shock. The p-value in the right figures refer to a one-sided test for the multiplier being below 1.5.

An alternative empirical approach is to calculate, for each municipality, a set of long differences on the relevant outcomes with respect to 2012, for the years before the extension of DSP in 2013 and afterwards, and plot them against the municipality's population in 2011 census. In fact, our difference-in-discontinuities can be seen as regression discontinuity in the long differences with respect to the baseline year. This allows us to visualize potential non-linearities around the discontinuity. Figure 3.3 reports this result in binned scatter plots, with 95% confidence intervals for each population bin. While in 2007-2013 there is no significant difference in the change in per-capita net surplus for between municipalities just below/above the 5,000 threshold, municipalities below the threshold clearly increase their surplus more in the 2013-2018 period. In Figure 3.3, we fit a second order polynomium to the binned scatterplot. The fit in surplus is convex below the threshold ad concave above. This explains why in Table 3.1 we observe a larger discontinuity at narrower bandwidths, where the linear fit we impose in the baseline specification results in two upward-sloped lines above and before the threshold. Conversely, as the bandwidth increases the lines flatten. We tend to prefer estimates at narrow bandwidths, which are consistent with optimal bandwidth selection. Anyway, the clear discontinuity differential across the 5,000 threshold in fiscal contraction survives with all polynomial specifications up to 4th degree, while no significant difference around the threshold emerges in the evolution of per-capita income.

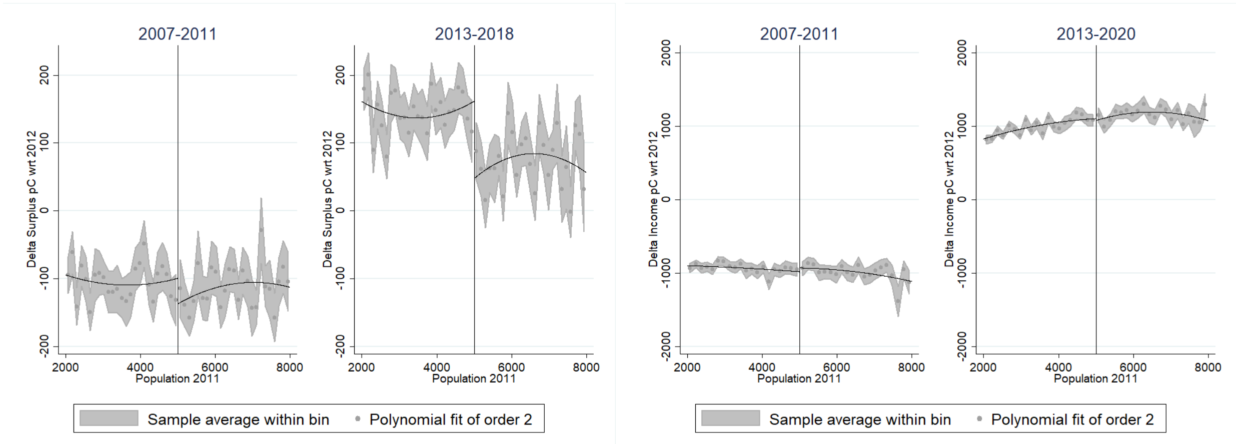

FIGURE 3.3: Regression Discontinuity in Long Differences

Notes. The Figure reports Robust Regression Discontinuity plots following Calonico et al. (2020) with as dependent variable the evolution of net per-capits surplus (resp. per-capita income) with respect to 2012 in each municipality. Quadratic trends and pre-determined 50 bins are imposed. Confidence intervals are at the 95% confidence level and clustered at municipality level.

A possible threat could be that municipalities just below the threshold are compensated for the fiscal contraction (which, as we will see in the next session, is mostly made of a cut to municipalities' investment) by higher direct investment by the central government or Regions on their territory.

This would potentially attenuate our first-stage and bias our multiplier estimates downward. To check the robustness of our identification to this threat, we run a placebo exercise using data from OpenCoesione, which contains all the capital and current expenditures related to the European Union cohesion policy, and OpenCUP, which contains all direct public investments (capital expenditures and related current expenditures) and fiscal incentives[6]. These two sources cover the two channels through which Regions, the central state and the EU Cohesion policy could have compensated for the reduction of investment by municipalities below 5,000 inhabitants. We re-run our baseline reduced-form using as outcome the total resources spent on accrual basis by subjects other than municipalities, thus not included in municipalities net surplus. The results are reported in Figure C.4 and show no significant effect in difference-in-discontinuities of investments from other public and EU sources.

3.3.2 Mechanisms

We then explore the mechanisms behind the insignificant effect of the fiscal contraction in municipalities below 5,000 inhabitants. First, we focus on the composition of the shock, i.e. the effect of DSP on different balance sheet changes underlying the increase in per-capita net surplus. Table 3.2 reports the results of our first stage regression using as outcome the different components of per-capita net surplus. Column 1-2 evaluate the change of per-capita net current and capital surplus. It appears that the fiscal consolidation is totally accounted for by an increse in the capital surplus. The result is confirmed by columns 3-6, which report the breakdown by total current per-capita revenues and expenditures, and total capital per-capita revenues and expenditures. The estimates on revenues are positive, but insignificant and close to zero, while the one of capital expenditures is large and extremely significant. A final piece of corroborating evidence is reported in Column 7, where we use as outcome the total borrowing by the municipality. The coefficient is negative, significant, and of a magnitude corresponding to 7/8th of the increase in per-capita surplus. Moreover, the dynamic specification in Figure C.6 in the Appendix highlights that the reduction in per-capita borrowing is very stable, similarly to the increase in surplus. This suggests that the shock to surplus we observe after 2013 corresponds to an actual reduction of municipality borrowing.

[6]It excludes however financial transfers to publicly-owned companies

Table 3.2: Composition of the shock of DSP extension

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Curr. Surpl. pC	Cap. Surpl. pC	Cur. Rev. pC	Cur. Exp. pC	Cap. Rev. pC	Cap. Exp. pC	Borrow. pC
DSP	8.557	67.01***	13.61	1.184	3.736	-69.70***	-53.34**
	(14.34)	(17.99)	(18.60)	(12.82)	(7.268)	(20.16)	(22.57)
Observations	8,433	8,433	8,433	8,433	8,433	8,433	8,433
R-squared	0.576	0.400	0.831	0.912	0.391	0.410	0.577
Years	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015	2007-2015
Bandwidth	1500	1500	1500	1500	1500	1500	1500

Notes. The Table reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities between 1,000 and 5,000 inhabitants, from 2013 onward on their net per-capita surplus and income. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

Thanks to the detailed information contained on our dataset, in Table 3.3 we can further breakdown the effect of DSP on expenditures by destination of municipalities' expenditures, using the thematic groups contained in the municipalities balance sheet. These groups are defined based on standardized criteria fixed by the central government for accountability purposes. The table reveals that the cut in expenditures is concentrated in the Administration and in the Roads and Transportation expenditure voices, each one being significantly cut by roughly 40%, i.e. 20 Euros per capita. Other items report a decrease in expeditures which is large but not statistically significant: School, Sport and Social Services are all cut by roughly 8-9 Euros per capita. In all these sectors, municipalities are in charge of the maintenance of buildings and accessory infrastructure (utilities, school bus), and in the case of schools this consists in a reduction of roughly one third of municipalities' annual investment in schools.

Table 3.3: Composition of the change in expenditures

	(1) Cur. Exp. pC	(2) Log Cur. Exp.	(3) Cap. Exp. pC	(4) Log Cap. Exp.
Administration	0.156	0.005	-19.162**	-0.369***
	(6.935)	(0.016)	(7.828)	(0.134)
Culture	-0.739	-0.035	-2.251	-0.120
	(0.728)	(0.061)	(2.859)	(0.300)
Justice	-0.132	-0.214	-0.164	-0.883
	(0.108)	(0.187)	(0.358)	(0.615)
School	0.348	-0.017	-7.400	-0.290
	(1.494)	(0.026)	(5.694)	(0.187)
Police	-1.844	-0.084	-0.228	-0.488*
	(1.160)	(0.051)	(0.309)	(0.293)
Utilities	-1.679	0.284	-2.783	1.111
	(2.201)	(0.237)	(4.316)	(0.726)
Social services	-7.417	-0.067*	-8.846	0.166
	(6.465)	(0.040)	(10.603)	(0.192)
Sport	-0.090	0.021	-9.809***	-0.284
	(0.706)	(0.051)	(3.256)	(0.213)
Economic Development	0.341	-0.015	1.568	0.011
	(0.448)	(0.116)	(2.254)	(0.563)
Environment	-0.766	-0.076	-3.795	-0.102
	(6.945)	(0.068)	(12.135)	(0.140)
Tourism	-1.620*	0.096	-1.510	-0.487
	(0.889)	(0.130)	(2.508)	(0.455)
Roads and Transp.	0.868	0.031	-20.219***	-0.360***
	(2.190)	(0.024)	(6.660)	(0.130)

Notes. The Table reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities between 1,000 and 5,000 inhabitants, from 2013 onward on their net per-capita surplus and income. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level. The years considered are 2007-2013, and the bandwidth is ±1500 around the 5,000 inhabitants threshold.

Turning to the composition of the reduced form effect of DSP extension, Table 3.4 suggests that the negative sign of the insignificant effect on total income is driven by a negative effect on labor income, still insignificant. No effect is also observed on distribution of income as captured by the number of income declarations per capita with total income in three brackets: "low" (≤€15,000), "middle" (€15,000-€26,000), and "high" (>€26,000). Finally, Figures C.5 and C.7 in the appendix report the estimated of first stage and reduced form composition from the fully dynamic specification. The graphs highlight that the CTD assumption is not violated even in every single breakdown of balance sheet and income item considered.

TABLE 3.4: Composition of the reduced form effect of DSP extension

VARIABLES	(1) Income per decl.	(2) Declar. pC	(3) Labor Inc. pC	(4) Pension Inc. pC	(5) Capital Inc. pC
DSP	81.31	-0.00554	-40.32	31.65	6.341
	(103.8)	(0.00406)	(75.52)	(30.95)	(15.36)
Observations	13,118	13,118	12,181	12,181	12,181
R-squared	0.983	0.878	0.971	0.978	0.960
Years	2007-2020	2007-2020	2008-2020	2008-2020	2008-2020
Bandwidth	1500	1500	1500	1500	1500
Mean in 2012	18062	0.696	6859	3707	870.5

Notes. The Table reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities between 1,000 and 5,000 inhabitants, from 2013 onward on their net per-capita surplus and income. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

Finally in Appendix Table C.7 and Figure C.3 we run an heterogeneity by macroregion, both using only most developed Italian Northern regions vs. the rest (which roughly splits the sample of muniipalities a the threshold in two) and by separating also the Center from the South. There are in fact deep socio-economic differences between these areas, with the south being more exposed to corruption and low efficiency of public spending. We find that the deficit reduction following the DSP extension seems to be larger in the south, although this seems to be driven partially be some pre-trend in 2007-2009. No significant differences appear in the effect on local income and in the estimated multiplier, which remains not significantly different from zero.

3.3.3 Spillovers

A potential explanation of the insignificant effect on income we find, alternative to the one of a local economy multiplier close to zero, is that while the increase in budget surplus is associated to a specific municipality, its effect on income is simply spread over neighboring municipalities. Hence, in this section we investigate the presence of significant spillovers of the DSP extension to neighboring municipalities. To do so, our main strategy consists in focusing only on control group municipalities, having between 5,000 and 10,000 inhabitants, and evaluating the effect of having neighboring municipalities becoming subject to the DSP rule. Specifically, we first define rings around municipalities of all other municipalities reachable in 15 minutes by car, according to the 2011 census, which we call "neighboring" municipalities. Second, we calculate the share of population in neighboring municipalities which becomes subject to the DSP in 2012 (i.e. that belongs to our baseline treatment group). Then, our first stage consists in a regression of the average surplus per capita in neighboring municipalities on the share of population in neighboring municipalities which becomes subject to the DSP in 2012. Accordingly, our reduced form is a regression of the income per capita in municipalities at the center of the ring on the share of population in neighboring municipalities adopting DSP in 2012.

Yet, neighboring municipalities considered in our ring can be also very far from our identifying discontinuity at 5,000 inhabitants, and can include big cities or v part of bigger urban areas. We thus restrict our sample to those rings which are made only of municipalities close to our identifying threshold, i.e. in the $\pm$2500 bandwidth around 5,000 inhabitants. These municipalities, reported in the left panel of Figure C.8 in the Appendix, are forcefully few (only 38). As an alternative, we define as "tolerance" the share of population in the ring of neighboring municipalities belonging to municipalities out of the $\pm$2500 bandwidth around 5,000 inhabitants. We then enlarge our sample by increasing the tolerance from 0 (only municipalities within the $\pm$2500 bandwidth) to 50%, including those municipalities surrounded by rings in which at most half of the population is resident in municipalities in the $\pm$2500 bandwidth, as in the right panel of Figure C.8 in the Appendix.

Table 3.5 reports the results. The estimates in Columns 1 and 2 consider only rings of neighboring municipalities fully made of municipalities in the bandwidth, and are extremely noisy, suggesting that although our strategy seems to work, with a positive and significant first stage, the reduced form might be under-powered. By increasing the tolerance of our selection and including rings that report at most 50% of population from municipalities outside the $\pm$2500 bandwidth, the reduced form becomes more precise and not significantly different from zero. The implied multiplier is very similar to the one of our baseline estimates in Table 3.1, confirming that the fiscal contraction following DSP extension seems not to have a significant effect on income, even when considering municipalities neighboring treated ones. This evidence suggests that including spillovers would not push the multiplier significantly higher.

TABLE 3.5: Spillovers of DSP

VARIABLES	(1) Surplus pC in Neighb. Municip.	(2) Income pC	(3) Surplus pC in Neighb. Municip.	(4) Income pC
%Pop. under DSP Neighb. M.	261.1***	448.4	259.6***	-0.971
	(88.00)	(294.6)	(42.87)	(159.0)
Observations	456	456	3,264	3,264
R-squared	0.576	0.983	0.558	0.981
Years	2007-2018	2007-2018	2007-2018	2007-2018
Bandwidth	2500	2500	2500	2500
Tolerance outside BW	Zero	Zero	50 pct	50 pct
Mean in 2012	-227.3	11002	-224.1	11828
Multiplier		-1.717		.003
		[1.16]		[.612]

Notes. The Table reports difference-in-difference estimates of the effect of the extension of the Domestic Stability Pact to neighboring municipalities. All regressions include FEs for municipality and year. Columns 1 and 2 include only municipalities in the ± 2500 bandwidth with respect to 5000, while Columns 3 and 4 allow a percentage of population to be from municipalities outside the bandwidth, namely up to 60% from municipalities above the bandwidth or 3% from municipalities from below (which are the mean % of population from municipalities above and below the bandwidth). Standard errors are clustered at the municipality level.

As an alternative, we can instead focus on the whole sample, and see if the effect of DSP is larger for those municipalities neighbouring treated ones. We run such heterogeneity of our baseline reduced form in Table C.6 in the Appendix, but the estimates are extremely noisy.

3.4 Conclusions

In this paper, we study the impact of fiscal consolidation implemented by municipal governments on local income. To do so, we exploit the extension of tight fiscal rules to municipalities below 5,000 inhabitants enacted in Italy, in 2013. We implement a difference-in-discountinuity approach to isolate the effect of budget tightening on local income, thus controlling for confounders correlated to the running variable in our setting (i.e., population). We find that tighter budget rule result in presistently higher surplus per capita net of government transfers (i.e., 100 Euros per capita, 0.5% of local income), mostly driven by cuts in capital expenditures. Such cuts are concentrated in administrative functions and local infrastructures. We estimate a null effect of austerity policies on local income, with an estimated multiplier never significantly different from zero and lower than

1.5 with 95% confidence over a 4-year horizon. We also test for the presence of spatial spillovers in neighbor municipalities, finding similar results. Our findings indicate that the local fiscal multiplier estimated from fiscal consolidation is lower than the estimates prevailing in the literature on local fiscal multipliers. Such differences may be induced by a variety of factors.

First, local governments behave differently when they are forced to consolidate the budget relative to when they are allowed to relax it. Grembi et al. (2016) document that relaxing local budgets results in higher deficits and lower taxes, while we find that budget tightening results in lower deficits driven by cuts in capital expenditures. This asymmetry could be driven by economic motives (if lowering taxes is more expansionary than capital spending) or by strategic motives (if taxes are more electorally salient than capital expenditures). We find this question very relevant and potentially interesting for future reasearch.

Second, differently from most studies in the literature, our shock is not a windfall from the central government, but rather a budgetary shock, which may induce local Ricardian effects. If lower expenditures today result in lower taxes tomorrow, the negative impact of a permanent decrease in expenditures can at least partially be counterbalanced by higher private spending, thus compressing the multiplier. Our results do not exclude other types of adverse effects for the local population, such as lower amenities, although Daniele and Giommoni (2021) exclude a negative effect of budget tightening on publicly provided goods and services. Overall, our results indicate that government-imposed and effectively enforced fiscal consolidation of local governments may be a viable tool to reduce fiscal deficit at the national level and increase debt sustainability without harming the real economy in the short to medium run.

Appendices

Appendix A

Appendix to Chapter 1

A.1 Why I Need a Rolling Diff-in-Diff?

De Chaisemartin and D'Haultfœuille (2020b) show that, in staggered adoption designs, event studies using two-way fixed effects or first-difference estimators heavily rely on homogeneous treatment effects, and are otherwise biased due to negative weighting of the effect in some groups. They propose a version of the diff-in-diff approach as a solution, and in De Chaisemartin and D'Haultfœuille (2020a) adapt their methodology to the staggered adoption case[1]. The building block for this kind of diff-in-diffs is basically a cell-specific estimator of the effect for youths in treatment wave w at time t, which in the notation of my setting would be

$$DID_{w,t}^{DCDH} = Y_{w,t} - Y_{w,t'} - \sum_{w' \in \Omega_w} \frac{n_{w',t}}{N_{\Omega_{w,t}}} (Y_{w',t} - Y_{w',t'})$$

Where $Y_{w,t}$ is the empirical average of the outcome of interest in cell w, t, t' is the period before w gets treated, $n_{w',t}$ is the number of units in cell w', t and $N_{\Omega_{w,t}}$ is the number of youths in all cells w, t such that treatment at t is still zero. If the program has been adopted at time T_w, units in cell w, t are $t - T_w$ periods away from adoption of the program, so that $DID_{w,t}$ identifies the treatment effect *since adoption*. In the context of *Garantie Jeunes*, such estimator would yield e.g. how much average employment improved in treated YECs after adoption of the program.

The need to roll over time since registration with YECs, and to estimate the effect since cohort *exposure* or individual *enrollment* arises first of all if there are dynamic effects of the program. Define G_i as the number of quarters a youth has been *exposed* to the program, meaning that he was registered at YEC who was offering *Garantie Jeunes*, and as D_i the time since actual enrollment

[1]A similar estimator is suggested by Callaway and Sant'Anna (2018)

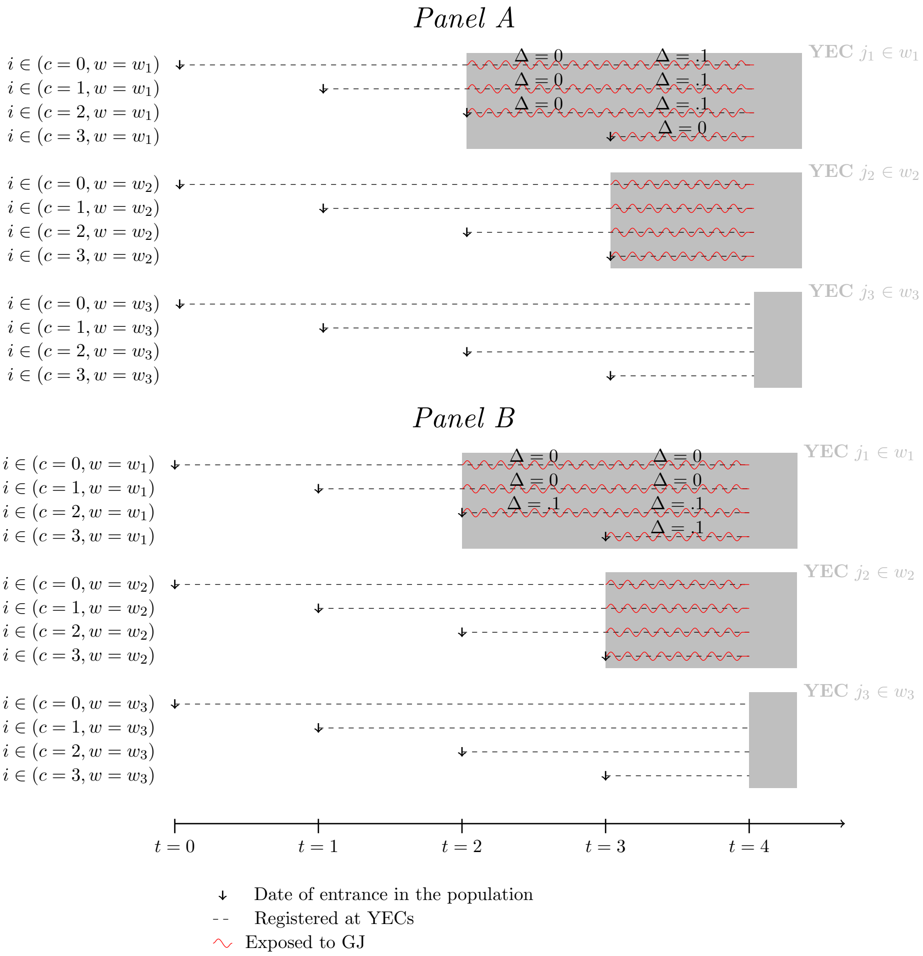

FIGURE A.1: When the effect since adoption is different than the average effect since exposure.

in the program. For simplicity, assume by now that all youths who can enroll in *Garantie Jeunes* do so as soon as it is offered in their YEC (full and immediate take-up), so that $G_i = D_i$ and the dynamic over *exposure* of a cohort coincides with the actual dynamic effect over *enrollment*. Panel A of Figure A.1 exemplifies this case. Now, suppose the program has dynamic effects, for example if $\Delta = 0$ when $G^h_{w,c} = 1$ and $\Delta = .1$ when $G^h_{w,c} = 2$, i.e. program effect is increasing with exposure/enrollment, and the average effect after two periods of *exposure*, when $G^h_{w,c} = 2$, is 0.1. Because some cohorts of youths register with YECs after that YEC has already adopted the program, $DID^{DCDH}_{w,t}$ will be an average of cohorts with different exposure $G^h_{w,c}$. For example, the average effect after 2 periods of *adoption* is estimated as $DID^{DCDH}_{w_1,t=4} = 0.075$, which is not informative about the relevant dynamic of treatment.

A second problematic case arises if time since registration is a source of selection into treatment, hence of potential heterogeneity. In my setting, this cannot be excluded *a priori*. In fact, youths remain in contact with YEC for long after their registration, so that when *Garantie Jeunes* is introduced in a particular YEC, both youths who just registered and youths who registered long time before will be able to take-up the program. These two groups might not be comparable, since the latter will be composed only of those youths who have not found a job or a formal training during the time they have been in contact with the YEC. Hence, treatment effect might be heterogeneous across these groups. For instance, in Panel B of Figure A.1 the true treatment effect is $\Delta = 0$ if $G^h_{w,c} > 0$, $h > G$, i.e. there is no treatment effect for youths who registered before treatment introduction and are exposed later. Instead, $\Delta = .1$ if $G^h_{w,c} > 0$, $h = G$, i.e. there is a positive 0.1 treatment effect for youths who registered at the moment of introduction of the program or later. The average effect when $G^h_{w,c} = 2$ is 0.03, but the effect two periods since adoption $DID^{dCDC}_{w_1,t=2} = 0.05$. My methodology solves this problem by comparing youths at the same time after registration with YECs, as explained in Section 3.

A.2 Assumptions, Propositions and Proof of Identification of ITT and LATEs

A.2.1 ITT

Denote $Y^h_{w,c} := \mathrm{E}(Y^h_{i,j,c}|j \in w, c, h)$, the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration. Let $G^h_{w,c}$ denote the number of periods that youths are exposed to *Garantie Jeunes* in that cell. Note that each h, w, c cell is associated to only one value of exoposure, $h, w, c \to G^h_{w,c} = g$.

Consider a set of assumptions typical of diff-in-diff settings.

Assumptions 1-4.

1. *(Independent groups)* Treatment status (i.e. exposure to *Garantie Jeunes*) of one wave doesn't influence the evolution of potential outcomes of others, i.e. $\mathrm{E}(Y^h_{w,c} - Y^h_{w,c'}|G^h_{w,c}, G^h_{w',c}) = \mathrm{E}(Y^h_{w,c} - Y^h_{w,c'}|G^h_{w,c})$ for each wave w and $w' \neq w$, given YEC-tenure h;
2. *(No anticipation)* Mean potential outcomes $Y^h_{w,c}$ in a cohort at a specific point in time are independent from treatment status in the next period $G^h_{w,c+1}$ (or $G^{h+1}_{w,c}$);
3. *(Strong exogeneity)* Treatment is independent from the evolution of mean potential outcomes when non-treated: $\mathbb{E}(Y^h_{w,c} - Y^h_{w,c'}|G^h_{w,c}) = \mathbb{E}(Y^h_{w,c} - Y^h_{w,c'}|G^h_{w,c'}) = \mathbb{E}(Y^h_{w,c} - Y^h_{w,c'}), \forall c, c'$ s.t. $G^h_{w,c} = G^h_{w,c'} = 0$, given wave w and time since registration with YEC h;
4. *(Common trends)* Expected variation in potential outcomes when non-treated doesn't vary across waves, given YEC-tenure h: $\mathbb{E}(Y^h_{w,c} - Y^h_{w,c'}) = \mathbb{E}(Y^h_{w',c} - Y^h_{w',c'})$, $\forall h, w, c$ s.t. $G^h_{w,c} = 0$.

The first two assumptions exclude that mean potential outcomes depend from treatment in other waves, in the next cohort or in the next period, but only depends from current cumulated treatment status, conditional on h, w, c. This allows us to write $Y^h_{w,c}(g)$ to represent the expected outcome in cell h, w, c when being treated for g quarters.

Analogously to De Chaisemartin and D'Haultfœuille (2020a), I first target cell-specific $\Delta^{ITT}(h, w, c)$, which will be the building block for identification of more aggregate parameters. Denote $Y^h_{w,c} := \mathrm{E}(Y^h_{i,j,c}|j \in w, c, h)$, the conditional expectation for all youths in YECs j belonging to treatment wave w, in cohort c, and observed h quarters after registration. For each h, w, c such that $G^h_{w,c} = g > 0$, define the cell-specific ITT estimand:

$$\Delta^{ITT}(h, w, c) = Y^h_{w,c}(g) - Y^h_{w,c}(0) \qquad \forall\, given\, (w, c, h) : G^h_{w,c} = g > 0$$

Consider:

$$DID^h_{w,c} := Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} (Y^h_{w',c} - Y^h_{w',c'}) \qquad \forall\, given\, (w, c, h) : G^h_{w,c} = g > 0 \quad \text{(A.1)}$$

Where $G^h_{w,c'} = 0$ but $G^h_{w,c'+1} = 1$, and Ω_w is the set of waves such that $G^h_{w',c} = G^h_{w',c'} = 0$, for each $w' \neq w$ and $c' \neq c$. $n_{w'}$ is the number of individuals of cohort c in wave w' while $N_{\Omega_w,c}$ is the total number of individuals of cohort c in all waves $w' \in \Omega_w$.

Proposition A.1. *Under Assumptions 1-4, $DID^h_{w,c}$ is an unbiased estimator of $\Delta^{ITT}(h, w, c)$.*

Proof.

$$\begin{aligned}
&\mathbb{E}[DID^h_{w,c}|G^h_{w,c}] = \\
&= \mathrm{E}\left[Y^h_{w,c} - Y^h_{w,c'} - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} (Y^h_{w',c} - Y^h_{w',c'}) \middle| G^h_{w,c} \right] \\
&= \mathrm{E}\left[Y^h_{w,c}(g) - Y^h_{w,c'}(0) - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} (Y^h_{w',c}(0) - Y^h_{w',c'}(0)) \middle| G^h_{w,c} \right] \\
&= \mathrm{E}[Y^h_{w,c}(g) - Y^h_{w,c}(0)|G^h_{w,c}] + \mathrm{E}[Y^h_{w,c}(0) - Y^h_{w,c'}(0)\,] - \sum_{w' \in \Omega_w} \frac{n_{w',c}}{N_{\Omega_w,c}} \mathrm{E}[Y^h_{w'c}(0) - Y^h_{w'c'}(0)] \\
&= \mathrm{E}[\Delta^{ITT}(h, w, c)|G^h_{w,c}]
\end{aligned}$$

The first equality applies the definition in (A.1), the second follows from no anticipation and independent groups, the third is obtained by adding and subtracting $Y^h_{w,c}(0)$ plus strong exogeneity to get rid of conditional expectation, while the last follows from common trends. $E[DID^h_{w,c}] = \mathrm{E}[\Delta^{ITT}(h, w, c)]$ follows by the law of iterated expectations.

I am then then interested in meaningfully aggregate cell-specific ITT estimator $DID^h_{w,c}$ into an unbiased estimators of $\Delta^{ITT}(g)$, the average effect of being exposed for g quarters to the program. Consider:

$$DID^g := \sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}} DID^h_{w,c} \tag{A.2}$$

Proposition A.2. *Given a set of $DID^h_{w,c}$, for all $(w,c|h) : G^h_{w,c} = g$, unbiased estimators of $\Delta^{ITT}(h,w,c)$, DID^g is an unbiased estimator of $\Delta^{ITT}(g)$.*

Proof.

$$\begin{aligned}
\mathrm{E}[DID^g] &= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathbb{E}[DID^h_{w,c}] \\
&= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathrm{E}(\Delta^{ITT}(h,w,c)) \\
&= \sum_{(w,c|h):G=g} \frac{n_{w,c}}{\sum_{(w,c|h):G=g} n_{w,c}} \mathrm{E}\left[Y^h_{w,c}(g) - Y^h_{w,c}(0)\right] \\
&= \mathrm{E}\{\mathrm{E}[Y^h_{w,c}(g)|G^h_{w,c} = g] - \mathrm{E}[Y^h_{w,c}(0)|G^h_{w,c} = g]\} \\
&= \mathrm{E}[\Delta^{ITT}(g)]
\end{aligned}$$

Where the first equality is the definition of DID^g in Proposition 2, the second relies on the proof of Proposition 1, the third is the definition of $\Delta^{ITT}(h,w,c)$, the fourth uses the definition of expectation and the last relies on the Law of Iterated Expectations.

Intuitively, Proposition 2 aggregates cell-specific ITT into a weighted average of effects from different waves, cohorts and tenures, sharing the same level g of treatment exposure.

Similarly, I can also define a placebo test for predictions implied by strong exogeneity and common trends:

$$Y^h_{w,c} - Y^h_{w,c'} - \sum_{w'\in\Omega_w} \frac{n_{w',c}}{N_{\Omega_w},c}[Y^h_{w',c} - Y^h_{w',c'}] = 0 \qquad \forall\, given\, (w,c,h) : G^h_{w,c} = 0 \tag{A.3}$$

And aggregate placebos sharing the same distance from treatment introduction $w - c$.

A.2.2 LATE

As explained in the institutional context, once youth are exposed to *Garantie Jeunes* it's not guaranteed that they actually enroll in the program. In fact, only some youths are eligible, and only some of the eligibles eventually applies and gets selected for the program. Once they start being exposed, youths from a specific cohort can apply and be selected to enroll in *Garantie Jeunes* immediately, later, or never enroll.

We then study what can be said about the effect of the program since enrollment, which requires studying potential outcomes at the individual level. Let potential outcomes for youths i, registering in cohort c to YEC j, h quarters after registration, be $Y^h_{i,j,c}(\mathbf{D_{i,j}})$, where $\mathbf{D_{i,j}} = \{d^p_{i,j,c}\}_{p=1}^{\infty}$ is a vector of dummies representing treatment status of individual i in YEC j and cohort c, from his registration with YECs onward. Define as $D^h_{i,j,c} = \sum_1^h d^p_{i,j,c}$ the cumulated treatment of individuals in a cell. Note that potential outcome in YEC j and cohort c is independent from enrollment status in other cohorts and YECs following assumptions 1 and 2.

A first parameter of interests is then the LATE on compliers:

$$\Delta^{LATE}(g) \quad = \quad \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} > 0)$$

Consider

Assumption 5. *(No spillovers on non-compliers)* $\mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} = 0) = 0$

Then

Proposition A.3. *Consider a set of $DID^h_{w,c}$, unbiased estimators of $\Delta^{ITT}(h, w, c)$, the cell-specific ITT treatment effect. Under assumptions 1-5, if $Pr(D^h_{i,j,c} > 0|h, w, c) = 0$ whenever $G^h_{w,c}$=0 (no defiers and no always-takers), then $\sum_{(w,c|h):G^h_{w,c}=g} \frac{n_{w,c}}{\sum_{(w,c|h):G^h_{w,c}=g} n_{w,c}} [DID^h_{w,c}/Pr(D^h_{i,j,c} > 0|h, w, c)]$ is an unbiased estimator of $\Delta^{LATE}(g)$*

Proof. It follows straightforwardly from the definition of expectations that

$$\begin{aligned}
\mathrm{E}(DID^h_{w,c}) =& \mathrm{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \\
=& \mathrm{E}\Big[\mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 0, h, w, c) \cdot Pr(D^h_{i,j,c} > 0|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} = 0, h, w, c) \cdot Pr(D^h_{i,j,c} = 0|h, w, c)\Big] \\
\mathrm{E}(DID^h_{w,c}/Pr(D^h_{i,j,c} > 0|h, w, c)) =& \mathrm{E}\Big[\mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 0)|h, w, c\Big] \\
=& \Delta^{LATE}(g)
\end{aligned}$$

Where the third passage holds since the second term is zero due to the assumption of no-spillovers on non-compliers, and the final equality is based on the law of iterated expectations. Yet, we might be interested in obtaining an estimate of LATE for some specific values of $D^h_{i,j,c}$, not only for $D^h_{i,j,c} > 0$.

$$\Delta^{LATE}(d) \quad = \quad \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d)$$

Consider:

Assumption 6. *(Exogeneity of enrollment).* Potential outcomes when actually treated, conditional on h, j, c, depend only on cumulated past treatment take-up, so that $Y^h_{i,j,c}(\mathbf{D_{i,j}}) = Y^h_{i,j,c}(D^h_{i,j,c})$, and the expected effect of being enrolled since d quarters in the program is homogeneous across cohorts, waves, and time since registration: $\mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d, h, w, c) = \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)|D^h_{i,j,c} = d)$

Assumption 6 is strong, given that it imposes for example an equal treatment effect on youth who take up the program early in exposure and youth who take up the program later (as $h, w, c \to g$), but allows us to express the cell-specific ITTs in terms of unknown LATE effect of being treated for d quarters, $\delta(d)$, and the known share of youths at different stages since enrollment in the program, i.e. $DID^h_{w,c} = \sum_{d=1}^{g} \delta(d) Pr(D^h_{i,j,c} = d|h, w, c)$.

Proposition A.4. *Under assumptions 1-6, $\delta(0 < d \leq 2), \delta(2 < d \leq 4), \delta(d > 4)$ in regression 1.2 are estimators of LATEs in the first semester of program enrollment, in the second semester, and after completion.*

Proof.

$$\begin{aligned}
\mathrm{E}(DID^h_{w,c}) =& \mathrm{E}(Y^h_{w,c}(g) - Y^h_{w,c}(0)) \\
=& \mathrm{E}\Big[\mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|0 < D^h_{i,j,c} \leq 2, h, w, c) \cdot Pr(0 < D^h_{i,j,c} \leq 2|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|2 < D^h_{i,j,c} \leq 4, h, w, c) \cdot Pr(2 < D^h_{i,j,c} \leq 4|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 4, h, w, c) \cdot Pr(D^h_{i,j,c} > 4|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} = 0, h, w, c) \cdot Pr(D^h_{i,j,c} = 0|h, w, c)\Big] \\
=& \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|0 < D^h_{i,j,c} \leq 2) \cdot Pr(0 < D^h_{i,j,c} \leq 2|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|2 < D^h_{i,j,c} \leq 4) \cdot Pr(2 < D^h_{i,j,c} \leq 4|h, w, c) \\
&+ \mathrm{E}(Y^h_{i,j,c}(g) - Y^h_{i,j,c}(0)\Big|D^h_{i,j,c} > 4) \cdot Pr(D^h_{i,j,c} > 4|h, w, c)
\end{aligned}$$

The first equality uses the definition of expectations and Assumption 5, while the last equality is obtained using Assumption 6 and the Law of Iterated Exectations. Which can be estimated using Equally-Weighted Minimum Distance (Altonji and Segal, 1996; Card and Lemieux, 2001).

A.3 Estimation of structural parameters

By equating each of the estimated average outcomes in treatment and control to their structural interpretation I obtain the following system:

$$\begin{cases}
\mathrm{E}(Y_{1i}(D_i)|0 < D_i \leq 2) & = \Phi_1(1) \cdot P(1,0) \\
\mathrm{E}(Y_{2i}(D_i)|0 < D_i \leq 2) & = \Phi_2(1) \cdot P(1,0) \\
\mathrm{E}(Y_{3i}(D_i)|0 < D_i \leq 2) & = \Phi_3(1) \cdot P(1,0) \\
\mathrm{E}(Y_{1i}(D_i)|2 < D_i \leq 4) & = \Phi_1(1) \cdot P(1,1) \\
\mathrm{E}(Y_{2i}(D_i)|2 < D_i \leq 4) & = \Phi_2(1) \cdot P(1,1) \\
\mathrm{E}(Y_{3i}(D_i)|2 < D_i \leq 4) & = \Phi_3(1) \cdot P(1,1) \\
\mathrm{E}(Y_{1i}(D_i)|D_i > 4) & = \Phi_1(0) \cdot P(1,1) \\
\mathrm{E}(Y_{2i}(D_i)|D_i > 4) & = \Phi_2(0) \cdot P(1,1) \\
\mathrm{E}(Y_{3i}(D_i)|D_i > 4) & = \Phi_3(0) \cdot P(1,1) \\
\mathrm{E}(Y_{1i}(0)|0 < D_i \leq 2) & = \Phi_1(0) \cdot P(0,1) \\
\mathrm{E}(Y_{2i}(0)|0 < D_i \leq 2) & = \Phi_2(0) \cdot P(0,1) \\
\mathrm{E}(Y_{3i}(0)|0 < D_i \leq 2) & = \Phi_3(0) \cdot P(0,1) \\
\mathrm{E}(Y_{1i}(0)|2 < D_i \leq 4) & = \Phi_1(0) \cdot P(0,1) \\
\mathrm{E}(Y_{2i}(0)|2 < D_i \leq 4) & = \Phi_2(0) \cdot P(0,1) \\
\mathrm{E}(Y_{3i}(0)|2 < D_i \leq 4) & = \Phi_3(0) \cdot P(0,1) \\
\mathrm{E}(Y_{1i}(0)|D_i > 4) & = \Phi_1(0) \cdot P(0,1) \\
\mathrm{E}(Y_{2i}(0)|D_i > 4) & = \Phi_2(0) \cdot P(0,1) \\
\mathrm{E}(Y_{3i}(0)|D_i > 4) & = \Phi_3(0) \cdot P(0,1)
\end{cases}$$

For simpler notation, denote $\ln(\mathrm{E}(Y_{ji}(treated)|D_i) = y_{j,\bar{d}}(treated)$, where $\bar{d} = 1$ if $0 < D_i \leq 2$, $\bar{d} = 2$ if $2 < D_i \leq 4$, $\bar{d} = 3$ if $D_i > 4$. Then taking logs from both sides:

$$
\begin{cases}
y_{1,1}(1) & = \hat{\alpha}_1 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,0) \\
y_{2,1}(1) & = \hat{\alpha}_2 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300a_1)\tau + p(1,0) \\
y_{3,1}(1) & = \hat{\alpha}_3 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,0) \\
y_{1,2}(1) & = \hat{\alpha}_1 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta + p(1,1) \\
y_{2,2}(1) & = \hat{\alpha}_2 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + \beta - (\alpha_2 - 300a_1)\tau + p(1,1) \\
y_{3,2}(1) & = \hat{\alpha}_3 - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha_1}+\beta} + e^{\hat{\alpha_2}-(\alpha_2-300a_1)\tau+\beta} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{1,3}(1) & = \hat{\alpha}_1 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{2,3}(1) & = \hat{\alpha}_2 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{3,3}(1) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(1,1) \\
y_{1,1}(0) & = \hat{\alpha}_1 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,1}(0) & = \hat{\alpha}_2 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,1}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{1,2}(0) & = \hat{\alpha}_1 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,2}(0) & = \hat{\alpha}_2 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,2}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{1,3}(0) & = \hat{\alpha}_1 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{2,3}(0) & = \hat{\alpha}_2 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1) \\
y_{3,3}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha_1}} + e^{\hat{\alpha_2}} + e^{\hat{\alpha_3}}) + p(0,1)
\end{cases}
\tag{A.4}
$$

Note that it is not possible to recover all parameters separately. For example, suppose the left-hand side of the system is a noisy estimate, there are actually only 8 different equations on the right-hand side and 9 unknowns, as showed below.

$$\begin{cases} y_{1,1}(1) - y_{2,1}(1) - (y_{1,1}(0) - y_{2,1}(0)) & = \alpha_2 \tau \\ y_{1,1}(1) - y_{3,1}(1) - (y_{1,1}(0) - y_{3,1}(0)) & = \beta \\ y_{3,1}(1) - y_{3,1}(0) & = \ln(e^{\alpha_0} + e^{\hat{\alpha}_1} + e^{\hat{\alpha}_2} + e^{\hat{\alpha}_3}) - \ln(e^{\alpha_0+\beta} + e^{\hat{\alpha}_1+\beta} + e^{\hat{\alpha}_2-\alpha_2\tau+\beta} + e^{\hat{\alpha}_3}) + p(1, \\ y_{1,2}(1) - y_{1,2}(1) & = p(1,1) - p(1,0) \\ y_{2,2}(1) - y_{2,2}(1) & = p(1,1) - p(1,0) \\ y_{3,2}(1) - y_{3,2}(1) & = p(1,1) - p(1,0) \\ y_{1,3}(1) - y_{1,3}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,3}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,3}(0) & = p(1,1) - p(0,1) \\ y_{1,1}(0) - y_{2,1}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \\ y_{1,1}(0) - y_{3,1}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \\ y_{3,1}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha}_1} + e^{\hat{\alpha}_2} + e^{\hat{\alpha}_3}) + p(0,1) \\ y_{1,2}(0) - y_{2,2}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \\ y_{1,2}(0) - y_{3,2}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \\ y_{3,2}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha}_1} + e^{\hat{\alpha}_2} + e^{\hat{\alpha}_3}) + p(0,1) \\ y_{1,3}(0) - y_{2,3}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \\ y_{1,3}(0) - y_{3,3}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \\ y_{3,3}(0) & = \hat{\alpha}_3 - \ln(e^{\alpha_0} + e^{\hat{\alpha}_1} + e^{\hat{\alpha}_2} + e^{\hat{\alpha}_3}) + p(0,1) \end{cases}$$

Let us instead try to recover $p(1,1) - p(0,1)$, $p(1,1) - p(1,0)$,$(\alpha_2 - 300a_1)\tau$, β. Note that there are multiple configurations of the system, including different combinations of different lines, that one can use to recover each parameter. These alternative configurations deliver different estimates of the parameters. To avoid cherry picking, I will estimate each parameter as an average of all possible ways to recover it. This means:

$$\begin{cases} y_{1,1}(0) - y_{2,1}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \\ y_{1,2}(0) - y_{2,2}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \\ y_{1,3}(0) - y_{2,3}(0) & = \hat{\alpha}_1 - \hat{\alpha}_2 \end{cases} \Rightarrow \widehat{\hat{\alpha}_1 - \hat{\alpha}_2} = \frac{y_{1,1}(0) - y_{2,1}(0) + y_{1,2}(0) - y_{2,2}(0) + y_{1,3}(0) - y_{2,3}(0)}{3}$$

$$\begin{cases} y_{1,1}(0) - y_{3,1}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \\ y_{1,2}(0) - y_{3,2}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \\ y_{1,3}(0) - y_{3,3}(0) & = \hat{\alpha}_1 - \hat{\alpha}_3 \end{cases} \Rightarrow \widehat{\hat{\alpha}_1 - \hat{\alpha}_3} = \frac{y_{1,1}(0) - y_{3,1}(0) + y_{1,2}(0) - y_{3,2}(0) + y_{1,3}(0) - y_{3,3}(0)}{3}$$

$$\begin{cases} y_{1,1}(1) - y_{2,1}(1) - \widehat{\alpha_1 - \alpha_2} & = (\alpha_2 - 300a_1)\tau \\ y_{1,2}(1) - y_{2,2}(1) - \widehat{\alpha_1 - \alpha_2} & = (\alpha_2 - 300a_1)\tau \end{cases} \Rightarrow (\alpha_2 \widehat{- 300}a_1)\tau = \frac{y_{1,1}(1) - y_{2,1}(1) - \widehat{\alpha_1 - \alpha_3} + y_{1,2}(1) - y_{2,2}(1) - \widehat{\alpha_1 - \alpha_2}}{2}$$

$$\begin{cases} y_{1,1}(1) - y_{3,1}(1) - \widehat{\alpha_1 - \alpha_3} & = \beta \\ y_{1,2}(1) - y_{3,2}(1) - \widehat{\alpha_1 - \alpha_3} & = \beta \end{cases} \Rightarrow \widehat{\beta} = \frac{y_{1,1}(1) - y_{3,1}(1) - \widehat{\alpha_1 - \alpha_3} + y_{1,2}(1) - y_{3,2}(1) - \widehat{\alpha_1 - \alpha_3}}{2}$$

$$\begin{cases} y_{1,2}(1) - y_{1,1}(1) & = p(1,1) - p(1,0) \\ y_{2,2}(1) - y_{2,1}(1) & = p(1,1) - p(1,0) \\ y_{3,2}(1) - y_{3,1}(1) & = p(1,1) - p(1,0) \end{cases} \Rightarrow p(1, \widehat{1) - p}(1,0) = \frac{y_{1,2}(1) - y_{1,1}(1) + y_{2,2}(1) - y_{2,1}(1) + y_{3,2}(1) - y_{3,1}(1)}{3}$$

$$\begin{cases} y_{1,3}(1) - y_{1,1}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,1}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,1}(0) & = p(1,1) - p(0,1) \\ y_{1,3}(1) - y_{1,2}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,2}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,2}(0) & = p(1,1) - p(0,1) \\ y_{1,3}(1) - y_{1,3}(0) & = p(1,1) - p(0,1) \\ y_{2,3}(1) - y_{2,3}(0) & = p(1,1) - p(0,1) \\ y_{3,3}(1) - y_{3,3}(0) & = p(1,1) - p(0,1) \end{cases}$$

$$\Rightarrow p(1, \widehat{1) - p}(0,1) = [y_{1,3}(1) - y_{1,1}(0) + y_{2,3}(1) - y_{2,1}(0) + y_{3,3}(1) - y_{3,1}(0) +$$
$$y_{1,3}(1) - y_{1,2}(0) + y_{2,3}(1) - y_{2,2}(0) + y_{3,3}(1) - y_{3,2}(0) +$$
$$y_{1,3}(1) - y_{1,3}(0) + y_{2,3}(1) - y_{2,3}(0) + y_{3,3}(1) - y_{3,3}(0)] \cdot 1/9$$

$$\widehat{k_0 - k_1} = y_{3,2}(1) - y_{3,3}(1)$$

Alternatively, one can normalize one parameter and directly estimate the following regression:

$$y_{j,\bar{d}}(treated) = F(\hat{\alpha}, \beta, (\alpha_2 - 300a_1)\tau, p(0,1), p(1,1), p(1,0)) \tag{A.5}$$

Where F is defined by A.4. The results are reported below.

TABLE A.1: Estimated structural parameter, effect, and interpretation as multiplicative effect on $E(Y_{ji})$.

Parameter		$Pr(z^{j*} = z_j)$		Effect (interpretation)	
$\hat{\alpha_0}$	norm. to 0	$\Phi_1(0)$	.102	$e^{-(\alpha_2-300)\tau}$ (implicit tax)	.523
$\hat{\alpha_1}$	-1.670	$\Phi_2(0)$	.134		
$\hat{\alpha_2}$	-1.399	$\Phi_3(0)$	.215	e^{β} (moral h./liquidity)	1.100
$\hat{\alpha_3}$	-.928	$\Phi_1(1)$	.112		
β	.095	$\Phi_2(1)$	.076	$\frac{K_0}{K_1}$ (cash tr. spillovers)	.992
$-(\alpha_2 - 300a_1)\tau$	.648	$\Phi_3(1)$	.214		
$P(1,0)$	.565			$\frac{P(1,0)}{P(1,1)}$ (lock-in)	.565
$P(1,1)$	1				
$P(0,1)$	.470			$\frac{P(1,1)}{P(0,1)}$ (activation)	2.125

Notes. The table reports the estimated structural parameters obtained by equating the structural interpretation in Table 1.6 to the average outcomes of compliers in treatment (estimated from the data) and of compliers in the control group (obtained by subtracting the effect in Table 1.5 to average outcomes of compliers in treatment). Normalizing α_0, this provides 8 linearly independent equations and 8 unknowns (leftmost column) which can be estimated and used to recovered the distribution of $Pr(z_{j*} = z_j)$ and effets of different components of *Garantie Jeunes*. The effects in the last column are multiplicative.

A.4 Model's Predicted Outcomes in the Case of Aeberhardt et al. (2020)

In this section I use the model estimated in Section 5.2 to obtain the predicted impact in the case of Aeberhardt et al. (2020) and comparing the obtained prediction with the actual effect they estimate. In the setting of Aeberhardt et al. (2020), the cash transfer is smaller, $b' \simeq 250$, and the phase-out of the cash transfer starts from the first euro earned, with $\tau' = 24\%$. Hence, the predicted probabilities of employment in different income brackets in treatment and control are reported in Table A.2.

TABLE A.2: Predicted probabilities of employment in the case of Aeberhardt et al. (2020)

$Pr(Y_{ji} = 1)$ in treatment group		
	Monthly income €1-€1100	Monthly income over €1100
1st year of enrollment	$(\Phi_1(0) + \Phi_2(0))\frac{K_0}{K_1'}e^{a_1(b'-(z_1+z_2)\tau')} \cdot P(0,1)$	$\Phi_3(0)\frac{K_0}{K_1'} \cdot P(0,1)$

$Pr(Y_{ji} = 1)$ in control group		
	Monthly income €1-€1100	Monthly income over €1100
1st year of enrollment	$(\Phi_1(0) + \Phi_2(0)) \cdot P(0,1)$	$\Phi_3(0) \cdot P(0,1)$

Where $K_1' = e^{\hat{\alpha_0}+a_1 b'} + e^{\hat{\alpha_1}+a_1(b'-z_1\tau')} + e^{\hat{\alpha_2}+a_1(b'-z_2\tau')} + e^{\hat{\alpha_3}}$, and the other parameters are identical to Section 5.3. Then, I can use estimates of $e^{-(\alpha_2-300a_1)\tau} = e^{-(a_1 z_2-300a_1)\tau}$ and $e^{\beta} = e^{a_1 b}$ from Table 1.7 (reported also in the third column of Table A.1)[2], knowing that $b \simeq 480$ and $\tau = 55\%$ in the case of Garantie Jeunes, and recover a_1 and z_2. Finally, I can calculate the predicted % increase in employment in Aeberhardt et al. (2020) for different values of z_1:

$$\Delta^{Aeberh.} \log \mathrm{E}(Y_i) = \frac{(\Phi_1(0) + \Phi_2(0))\frac{K_0}{K_1'} e^{a_1(b'-(z_1+z_2)\tau')} + \Phi_3(0))\frac{K_0}{K_1}}{\Phi_1(0) + \Phi_2(0) + \Phi_3(0)} \simeq .93 \quad \forall z_1$$

Which is very close to 7-13% negative effect found in Aeberhardt et al. (2020).

[2] I use only estimated obtained through wNLS as I will also need estimates of $\hat{\alpha}_j$

A.5 Additional Tables and Figures

Table A.3: Characteristics of youth at time of registration at YEC.

Quarter of registration	Number of registrations (1)	N. ever in GJ every 1000 (2)	N. with less than vocat. secondary qualification (3)	Mean age at registration (4)	Share of males (5)
2013q1	120,251	0.00	0.22	20.28	0.52
2013q2	106,620	0.00	0.23	20.26	0.50
2013q3	150,618	0.00	0.17	19.95	0.49
2013q4	149,523	0.37	0.19	20.31	0.52
2014q1	125,791	0.79	0.22	20.46	0.53
2014q2	105,165	0.92	0.22	20.32	0.50
2014q3	153,138	0.98	0.17	19.85	0.48
2014q4	145,520	2.16	0.19	20.22	0.52
2015q1	117,903	2.13	0.22	20.34	0.52
2015q2	101,984	3.87	0.22	20.21	0.50
2015q3	144,077	4.34	0.16	19.78	0.50
2015q4	132,399	10.36	0.18	20.17	0.52
2016q1	108,002	8.36	0.21	20.26	0.53
2016q2	96,003	9.27	0.22	20.08	0.50
2016q3	133,726	7.25	0.16	19.69	0.50
2016q4	114,930	16.62	0.18	20.05	0.53

Notes. The table reports summary statistics for each cohort of youths registering to YECs. Vocational secondary qualifications are defined as less than CAP/BEP diploma, obtainable after 2-years of professional vocational studies.

TABLE A.4: Number of youth enrolling in *Garantie Jeunes* by quarter and wave.

quarter	w13q4	w14q1	w14q2	w14q4	w15q1	w15q2	w15q3	w15q4	w16q1	w16q2	w16q3	w16q4	w17q1	w17q2	w17q3
2013q4	154														
2014q1	496	164													
2014q2	563	193	15												
2014q3	568	297	48												
2014q4	938	628	161	2											
2015q1	832	380	41	37	1185										
2015q2	1103	484	118	27	1416	1681									
2015q3	988	387	24	18	1230	1444	1184								
2015q4	1597	902	184	21	2410	2994	3082	188							
2016q1	1237	578	101	18	1915	2120	3372	111	80						
2016q2	1387	659	86	35	2053	2558	3558	160	211	670					
2016q3	1056	422	58	28	1536	1706	2564	111	200	454	393				
2016q4	1568	640	164	31	2673	3377	4498	216	261	794	770	532			
2017q1	1343	489	62	27	2089	2423	3976	142	292	731	986	523	851		
2017q2	1205	441	40	24	1880	1900	3026	97	265	585	706	320	1111	400	
2017q3	743	283	34	30	1081	1063	1649	73	191	324	379	146	660	202	27
2017q4	748	308	56	13	1443	1415	2345	114	238	462	490	267	709	289	31

TABLE A.5: Number of youths registering to YEC by quarter and wave.

yq	w13q4	w14q1	w14q2	w14q4	w15q1	w15q2	w15q3	w15q4	w16q1	w16q2	w16q3	w16q4	w17q1	w17q2	w17q3
2013q1	8118	5121	485	378	14276	18945	27608	1721	3571	6928	8848	4383	13587	5436	846
2013q2	7460	4459	389	743	12725	16766	24025	1493	3191	6001	8175	3848	12192	4531	622
2013q3	11558	7066	453	394	18498	23951	32568	2112	4296	8609	11056	5266	17334	6457	1000
2013q4	10186	6382	592	443	17615	23885	33580	2356	4344	8622	11400	5734	16531	6777	1076
2014q1	8196	5361	415	373	14777	20054	28218	1809	3726	7274	9617	4739	14621	5762	849
2014q2	7247	4589	364	707	12128	16778	23320	1525	3071	6195	8063	4074	11943	4531	630
2014q3	11793	7209	507	372	18655	24478	32848	2442	4413	8989	11102	5619	17096	6585	1030
2014q4	10026	6268	585	361	17175	22666	32470	2187	4419	8445	11045	6170	16081	6457	1165
2015q1	8066	5081	468	341	13779	18700	26701	1738	3366	6896	9005	4766	13060	5145	791
2015q2	7402	4523	338	441	12588	16242	22087	1399	2902	6079	7751	3857	11554	4190	631
2015q3	11942	6760	417	381	17658	23143	30636	2039	3995	8175	10473	5392	15987	6175	904
2015q4	9487	5685	664	378	15679	20885	29727	1657	3902	7473	10047	5548	14565	5731	971
2016q1	7489	4524	431	297	12903	17156	24730	1467	3398	6172	8186	4320	11584	4690	655
2016q2	6926	4064	308	474	11379	15607	21497	1145	3058	5854	7132	3489	10642	3906	522
2016q3	11047	6210	451	379	15805	21627	29398	1691	4013	7942	9645	4801	14562	5502	653
2016q4	7956	4845	555	419	13527	18021	26211	1402	3662	6703	8724	4620	12620	5035	630

TABLE A.6: Heterogeneity by employment contract.

	Open-ended (1)	Temporary (2)	Agency jobs (3)	Apprenticeships (3)
ITT effect 1st semester of exposure	-0.000303	0.000686	0.00107	0.00125
	(0.00133)	(0.00165)	(0.00144)	(0.00137)
Total n.obs	3194961	3194961	3194961	3194961
ITT effect 2nd semester of exposure	0.000169	-0.00039	0.00174	0.000578
	(0.00236)	(0.00174)	(0.00154)	(0.00125)
Total n.obs	2379924	2379924	2379924	2379924
ITT effect 2nd year of exposure	0.000869	0.00503	0.00374	0.00118
	(0.00298)	(0.00358)	(0.00235)	(0.00174)
Total n.obs	2665714	2665714	2665714	2665714
Mean for control 1st semester of registration in YEC	0.084	0.155	0.078	0.031
Mean for control 2nd semester of registration in YEC	0.109	0.184	0.081	0.034
Mean for control 2nd year of registration in YEC	0.138	0.191	0.086	0.037
LATE 1st semester of exposure	-0.0197	0.0444	0.0691*	0.0808**
	(0.0351)	(0.0438)	(0.0390)	(0.0366)
LATE 2nd semester of exposure	0.00454	-0.0104	0.0467***	0.0155
	(0.0257)	(0.0190)	(0.0164)	(0.0137)
LATE 2nd year of exposure	0.0159	0.0927***	0.0689***	0.0218*
	(0.0219)	(0.0259)	(0.0169)	(0.0131)
LATE 1st semester after enrollm.	0.0178	-0.00661	-0.00730	-0.00607
	(0.0193)	(0.0195)	(0.0137)	(0.0109)
LATE 2nd semester after enrollm.	0.0262	-0.00221	0.0833**	-0.0153
	(0.0828)	(0.0663)	(0.0421)	(0.0630)
LATE 2nd year after enrollm.	0.0159	0.0927***	0.0689***	0.0218*
	(0.0219)	(0.0259)	(0.0169)	(0.0131)

Notes. The table reports the main results obtained following the rolling diff-in-diff methodology developed in Section 3. The upper panel reports weighted averages of the $DID^h_{w,c}$ coefficients where exposure is between 1 and 4 quarters or above 4 quarters. The lower panel reports the estimates of LATE of *Garantie Jeunes* on employment, hours worked and wages (earnings per hour) obtained according to Equation 1.2. Standard errors are bootstrapped and reported in parenthesis.

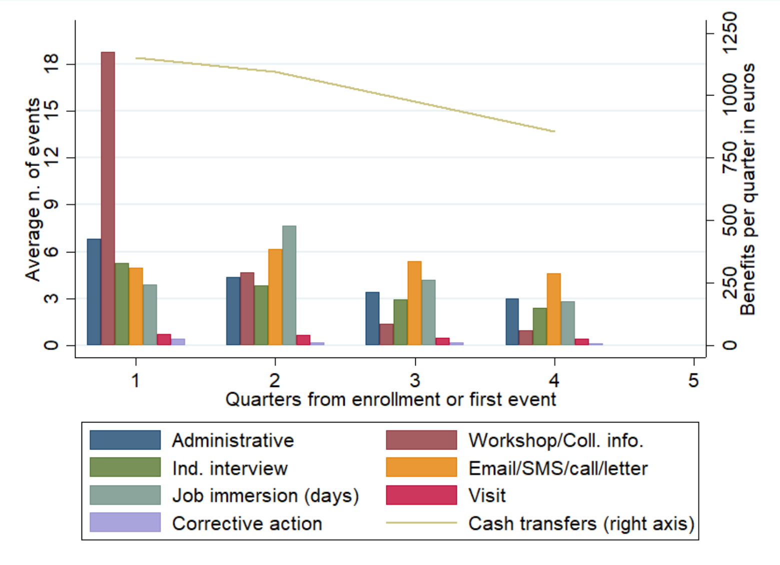

FIGURE A.2: Average number of events, by kind of event, and average benefits for participants in *Garantie Jeunes*.

Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *Garantie Jeunes*. The cash transfers series plots instead the average amount of benefit to youths participating in *Garantie Jeunes*, basing on when the actual transfer of money is recorded in the information system I-Milo.

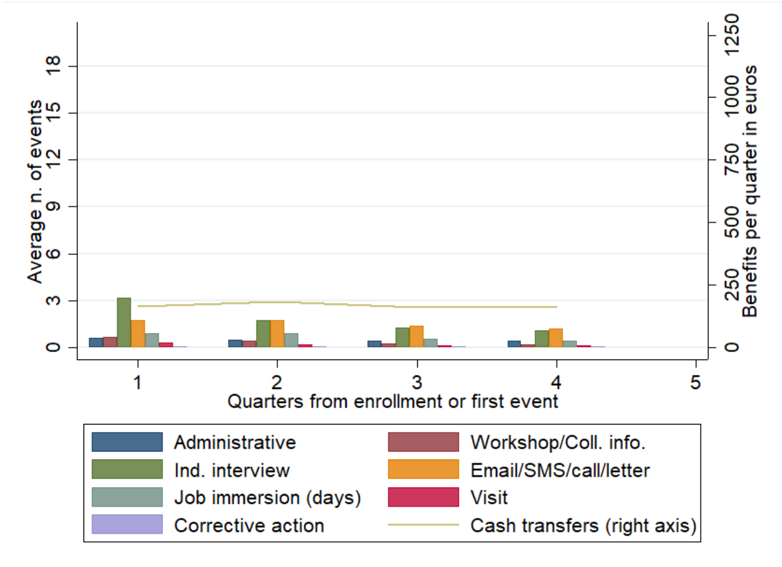

FIGURE A.3: Average number of events, by kind of event, and average benefits for participants in standard program available at YECs, *CIVIS*.

Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from enrollment in *CIVIS*. The cash transfers series plots instead the average amount of benefit to youths participating in *CIVIS*, basing on when the actual transfer of money is recorded in the information system I-Milo.

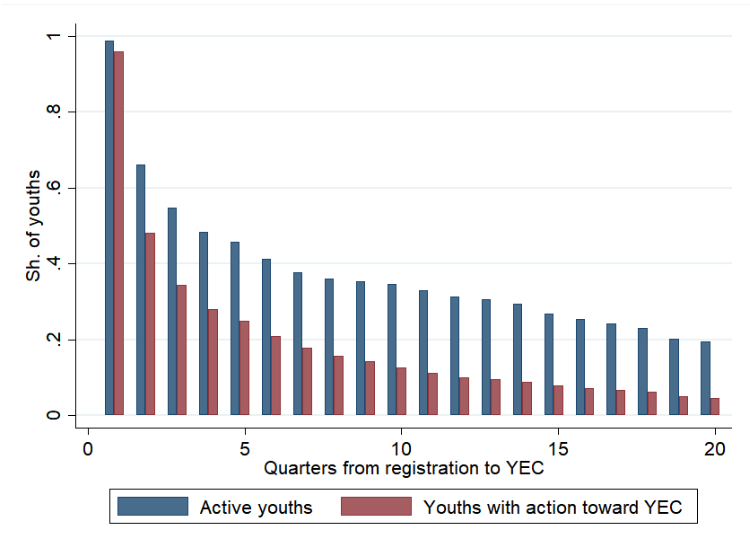

FIGURE A.4: Share of youth considered active at the YEC and youths who actually undertake action toward a YEC from time of registration.

Notes. The figure plots the average frequency of occurrence of an event as reported in the I-Milo information system of YECs, limited to the sample of interest, over quarters from registration at the YEC. "Active youths" are considered those whose file records any kind of action in the quarter. The red series reports instead youths for which a "youth toward YEC" action is recorded.

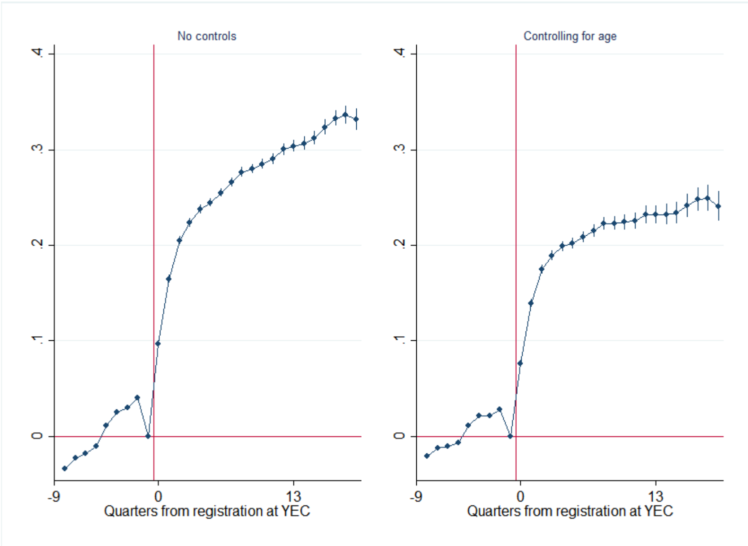

Figure A.5: Average employment rates in the quarters precedent/following registration at YEC, controlling or not for age.

Notes. The figure plots coefficients of a regression of an employment dummy on quarters from registration, cohort and YEC fixed-effects (left panel), adding age fixed effects (right panel).

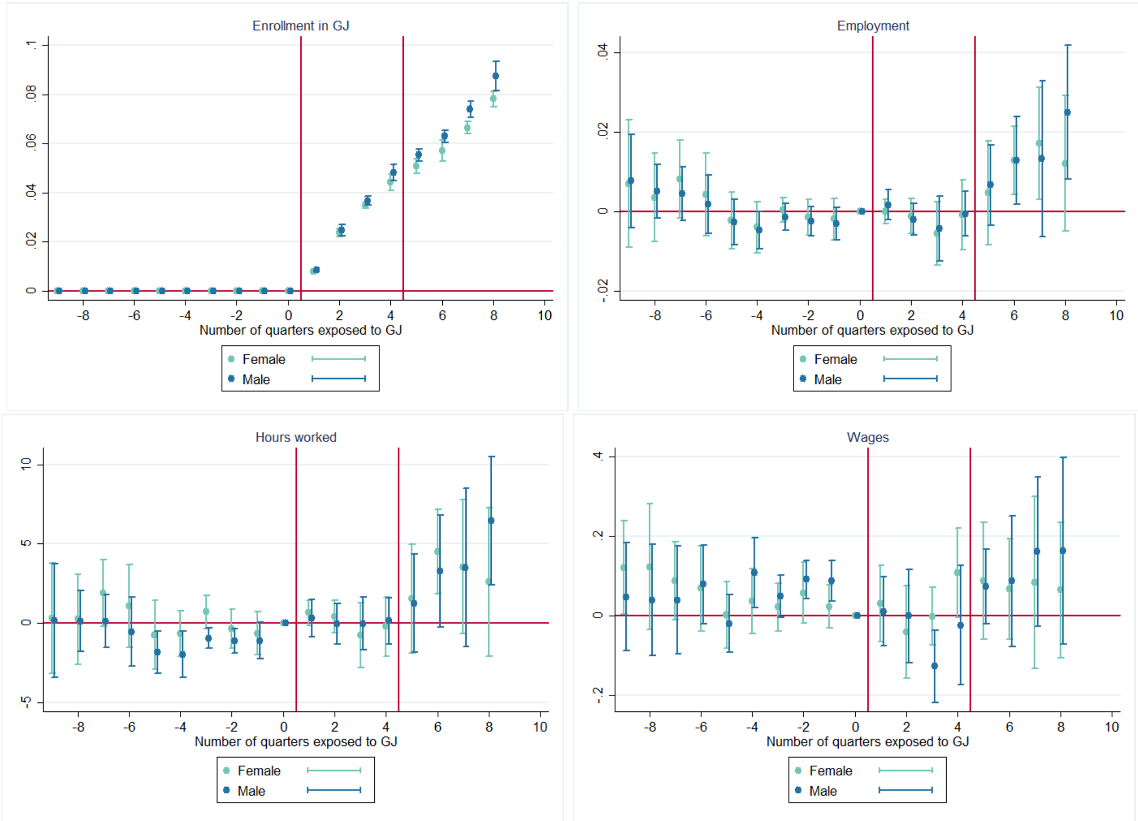

FIGURE A.6: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by gender.

Notes. The figure reports results of the rolling diff-in-diff approach for different gender sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation A.2. Cell-specific effects were obtained as in Equation A.1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

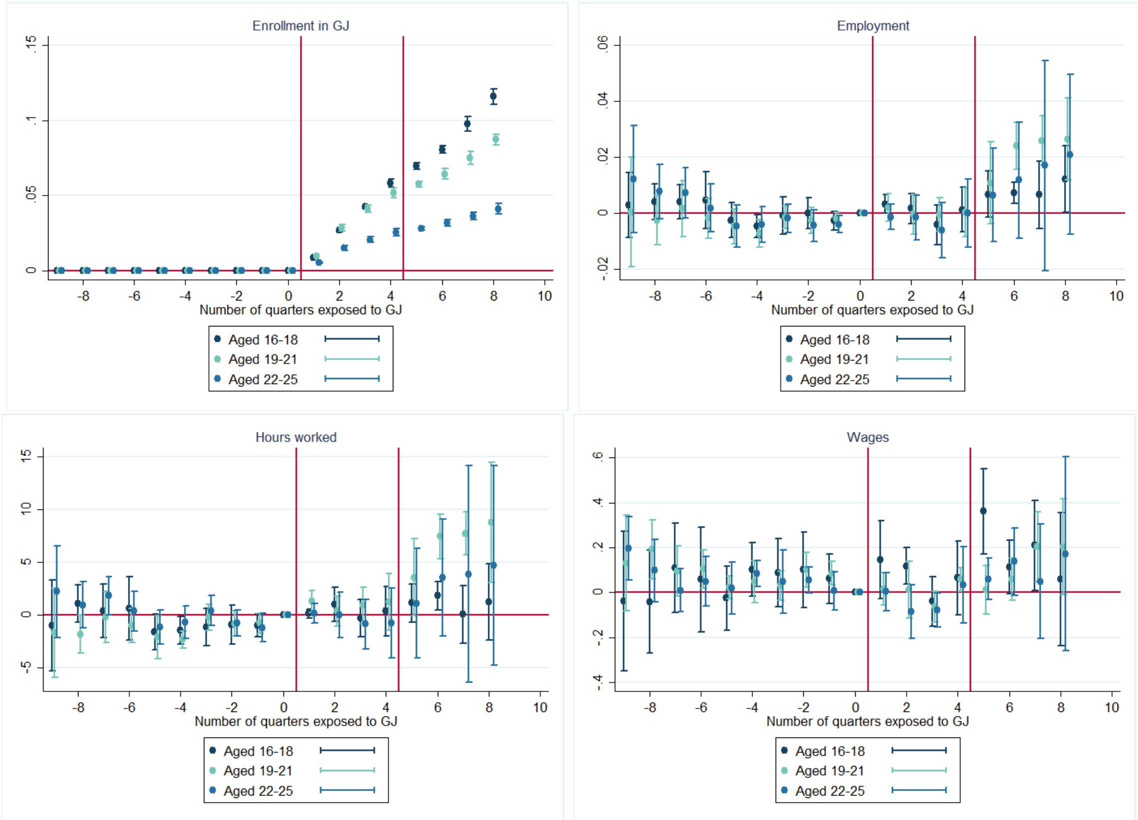

FIGURE A.7: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by age.

Notes. The figure reports results of the rolling diff-in-diff approach for different age sub-samples. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation A.2. Cell-specific effects were obtained as in Equation A.1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

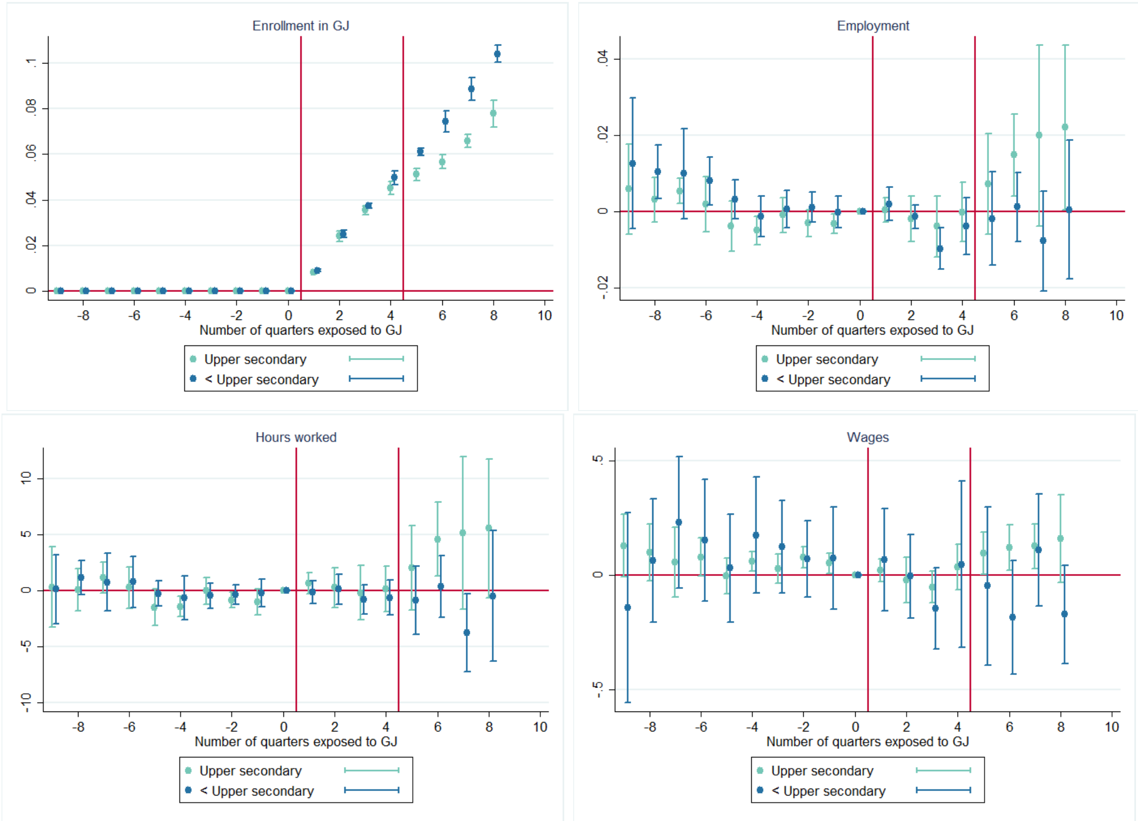

FIGURE A.8: Intent to treat (ITT) estimates using the rolling diff-in-diff approach by higher education degree attained.

Notes. The figure reports results of the rolling diff-in-diff approach for different sub-samples defined by higher education degree attained. The upper right panel reports the first stage effect, where the dependent variable is a dummy equal to one from the quarter of enrollment in *Garantie Jeunes* onward and the independent variable a dummy for exposure to *Garantie Jeunes*. The other three panel report the reduced-form coefficients: the dependent variables are employment, hours and earnings, while the independent variable is exposure to *Garantie Jeunes*. Point estimates are obtained as an average of cell-specific effects, weighted by the number of people in the cells, as in Equation A.2. Cell-specific effects were obtained as in Equation A.1. Standard errors are obtained by bootstrap sampling with clustering at the YEC-time since registration level, and confidence intervals are reported at 95% confidence level.

Appendix B

Appendix to Chapter 2

B.1 Additional tables and figures

TABLE B.1: Initial sample selection carried out by the Ministry of Labor

	nb of training episodes		
SI-CPF data (sept-2020)		5 309 119	
restriction to CPF data		4 123 472	
restriction to training which started		2 829 975	
restriction to years 2016 to 2019		2 129 073	
restriction to workers		1 195 601	
additional restrictions (dossiers non financed by training agency, duplicates, dossiers without CPF credit, CPF de transition, etc.)		1 098 487	
	2017	**2018**	**2019**
sample by year w/o 2016	251 032	359 990	310 483

Notes. The first line of the table corresponds to the number of training episodes in the extraction of the SI-CPF from September 2020. We first restrict to CPF data, because the SI-CPF is also used for keeping track of training financed with other devices. Then we restrict to training which started to remove draft training episodes. The restriction to workers is very important because a good share of CPF users are unemployed, although this share has decreased between 2015 and 2018. Then, we remove duplicates, training episodes without CPF credits (which must be an error), and *CPF de transition dossiers* as it is a different device. We also remove training episodes which are not financed by training agencies as our study focus on the changes of per-hour values of the CPF subsidy operated by training agencies. This leads to the removing of *PAD (parcours d'achat direct) dossiers* as they are financed by the public bank. *PAD dossiers* are a new type of CPF consumption, available from November 2019 where an individual can use its CPF on his own, on an app.

Table B.2: Placebo estimates, training course specification

	(1)	(2)	(3)	(4)
VARIABLES	$X_{qjft} - \hat{\hat{X}}_{qjft}$	$\ln x_{qjft} - \ln \hat{\hat{x}}_{qjft}$	$\ln N_{qjft} - \ln \hat{\hat{N}}_{qjft}$	$p_{qjft} - \hat{\hat{p}}_{qjft}$
c_{qft+1}	-0.0177	0.000559	-0.0172	-0.154
	(0.0172)	(0.00207)	(0.0165)	(0.125)
Observations	17,760	17,760	17,760	17,760
Years	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	IV	IV	IV	IV

	(1)	(2)	(3)	(4)
VARIABLES	$X_{qjft} - \hat{\hat{X}}_{qjft}$	$\ln x_{qjft} - \ln \hat{\hat{x}}_{qjft}$	$\ln N_{qjft} - \ln \hat{\hat{N}}_{qjft}$	$p_{qjft} - \hat{\hat{p}}_{qjft}$
$c_{q\tilde{f}t+1}$	-0.00354	0.000264	-0.00359*	-0.0594
	(0.00223)	(0.000518)	(0.00202)	(0.0371)
Observations	17,760	17,760	17,760	17,760
R-squared	0.885	0.937	0.878	0.916
Years	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	FE	FE	FE	FE

Notes. The upper panel reports placebo tests, obtained by estimating an IV regression of the relevant outcome in the pre-treatment period (2017-2018), residualized by the effect of any change in the subsidy cap in that period, on a lead of the effective subsidy instrumented by a lead of the subsidy cap. All regressions include year and training firm fixed effects and are weighted by the total value of the firm revenues. The lower panel reports placebo tests, obtained by estimating a reduced-form regression of the relevant outcome in the pre-treatment period (2017-2018), residualized by the effect of any change in the subsidy cap in that period, on a lead of the instrument. Column (1) uses as outcome total hours of training, Column (2) the average training duration, Column (3) the number of training courses, and Column (4) per-hour training prices. Regressions include fixed effects for training course and year. Standard errors are reported in parentheses and clustered at the training firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.3: Placebo estimates, training firm specification

VARIABLES	(1) $\ln REV_{Jt}$	(2) $\ln COST_{Jt}$	(3) $\ln \pi_{Jt}$	(4) $\ln L_{Jt}$	(5) $\ln E_{Jt}$
c_{Jt+1}	-0.00228	-0.00368	0.000393	-0.00950***	0.000185
	(0.00226)	(0.00263)	(0.00180)	(0.00309)	(0.000205)
Observations	7,174	6,792	6,742	6,478	6,884
Years	2017-2018	2017-2018	2017-2018	2017-2018	2017-2018
Estimation	IV	IV	IV	IV	IV

VARIABLES	(1) $\ln REV_{Jt}$	(2) $\ln COST_{Jt}$	(3) $\ln \pi_{Jt}$	(4) $\ln L_{Jt}$	(5) $\ln E_{Jt}$
$\tilde{c}_{Jt+1}$	-0.000506	-0.000810	8.73e-05	-0.00212***	4.24e-05
	(0.000571)	(0.000663)	(0.000461)	(0.000779)	(5.37e-05)
Observations	9,322	8,916	8,866	8,501	8,900
R-squared	0.973	0.980	0.907	0.971	1.000
Years	2017-2018	2017-2018	2017-2018	2017-2018	2017-2018
Estimation	FE	FE	FE	FE	FE

Notes. The upper panel reports placebo tests, obtained by estimating an IV regression of the relevant outcome in the pre-treatment period (2017-2018), residualized by the effect of any change in the subsidy cap in that period, on a lead of the effective subsidy instrumented by a lead of the subsidy cap. All regressions include year and training firm fixed effects and are weighted by the total value of the firm revenues. The lower panel reports placebo tests, obtained by estimating a reduced-form regression of the relevant outcome in the pre-treatment period (2017-2018), residualized by the effect of any change in the subsidy cap in that period, on a lead of the instrument. Columns use as outcomes revenues, costs, profits, total labor costs and total number of employees. Regressions include fixed effects for training course and year. Standard errors are reported in parentheses and clustered at the training firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4: Impact of changes in CPF subsidy on producers' revenues, costs, profits, labor costs and number of teachers

	(1)	(2)	(3)	(4)	(5)
VARIABLES	$\ln REV_{Jt}$	$\ln COST_{Jt}$	$\ln \pi_{Jt}$	$\ln L_{Jt}$	$\ln E_{Jt}$
$\ln c_{Jt}$	0.261***	0.0852	0.169**	-0.0534	-0.0150
	(0.0796)	(0.107)	(0.0791)	(0.110)	(0.0943)
Observations	9,604	8,900	8,816	8,472	8,792
Years	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	IV	IV	IV	IV	IV

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	c_{Jt}	$\ln REV_{Jt}$	$\ln COST_{Jt}$	$\ln \pi_{Jt}$	$\ln L_{Jt}$	$\ln E_{Jt}$
$\ln \tilde{c}_{Jt}$	0.242***	0.0630***	0.0202	0.0401**	-0.0126	-0.00361
	(0.0174)	(0.0189)	(0.0252)	(0.0185)	(0.0260)	(0.0227)
Observations	9,604	9,604	8,900	8,816	8,472	8,792
R-squared	0.846	0.970	0.964	0.822	0.954	0.957
Years	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019	2018-2019
Estimation	FE	FE	FE	FE	FE	FE

Notes. The upper panel reports the IV estimate of the effect of an increase in the natural log of the average effective training subsidy for a firms' customers on log revenues, costs, profits, labor costs and employment in the training firm. The lower panel of the table reports the reduced-form estimate of the effect of an increase in the natural log of the average subsidy cap for a firms' customers on log revenues, costs, profits, labor costs and employment in the training firm. All regressions include year and training firm fixed effects and are weighted by the total value of the firm revenues. Standard errors are reported in parentheses and clustered at the traning firm level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.5: Effect on entry(/exit)

VARIABLES	(1) $\ln c_{qt}$	(2) $\ln n_{qt}$
$\tilde{c_{qt}}$	0.125***	-0.0159
	(0.0314)	(0.0128)
Observations	9,592	9,592
R-squared	0.975	0.981
Years	2018-2019	2018-2019
Estimation method	FE	FE

Notes. Column (1) reports the first-stage relationship between the average subsidy cap fo a specific training kind and the average effective subsidy rate. Columns (2) reports reduced form estimates of the relationship between subsidy caps and average number of firms offering a training kind. All regressions include fixed effects for year and for training course (an interaction of the training title, online/offline, and training establishment). Standard errors are reported in parentheses and clustered at the interaction between industry financing centers and the training kind category. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.6: Effect on net prices

VARIABLES	(1) p_{qjft}
c_{qft}	-0.0923***
	(0.0268)
Observations	49,038
Years	2018-2019
Estimation	IV

Notes. The table reports the IV estimates of the effect of a change in the effective subsidy on the training price. All regressions include fixed effects for year and for training course (an interaction of the training title, online/offline, and training establishment). Standard errors are reported in parentheses and clustered at the interaction between industry financing centers and the training kind category. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

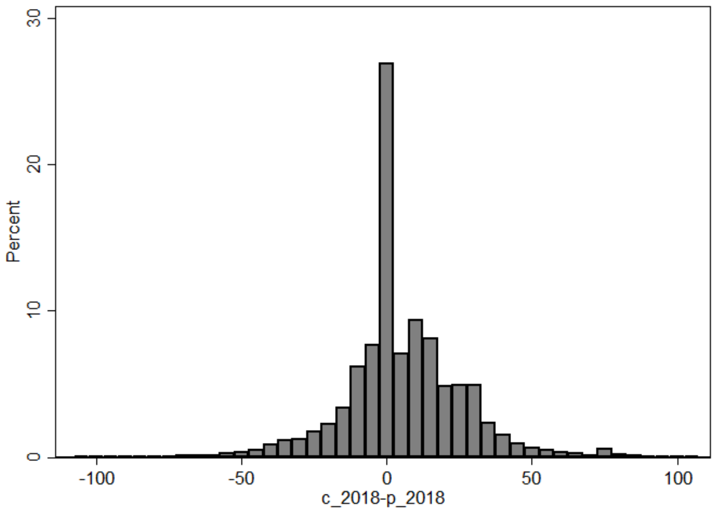

FIGURE B.1: Distribution of the difference between maximum subsidy rate and prices

Notes. The figure reports the histogram with width fixed at 5 of the difference between subsidy caps fixed by industry-specific financing centers and price of training episodes observed in the SI-CPF data in 2018.

Critères de prise en charge OPCA sur le CPF

Identification OPCA

Raison Sociale OPCA : ACTALIANS
Branche (s) professionnelle(s) couverte(s) par l'OPCA [2] : Professions libérales, Hospitalisation Privée, Enseignement Privé
Numéro (s) CCN :
Et/ ou
Code(s) IDCC : 2264,2691,2101,1951,1996,1147,1619, 2564,1875,959,2543,1726,2332, 2205,1921,2785,2706,240,1000,1850,

I. Informations CPF sur site institutionnel de l'OPCA

Informations générales sur le CPF [3] : http://www.actalians.fr/employeurs/cpf.asp
Conditions de prise en charge du CPF : [3] http://www.actalians.fr/employeurs/iso_album/dpc_cpf_ref2452_version_web.pdf

II. Conditions de prise en charge des OPCA au titre de l'agrément du 0.2 % CPF

A. Coût pédagogiques au titre de l'agrément 0.2% CPF

La prise en charge des coûts pédagogiques est-elle plafonnée ? : oui

Si oui, quel est le montant plafonné de prise en charge du coût horaire pédagogique (en euros HT) ?

	Heures compteur CPF	
	Coût horaire plafonné	Plafond global [4]
Pour l'accompagnement VAE	75 euros	24 h
Pour les actions CléA	27 euros	150 h
Liste COPANEF	60 euros	150 h
Liste COPAREF		
Liste CPNE	60 euros	150 h
Liste CPNE avec CPF abondé	[illegible]	[illegible]

Figure B.2: Example of conversion table

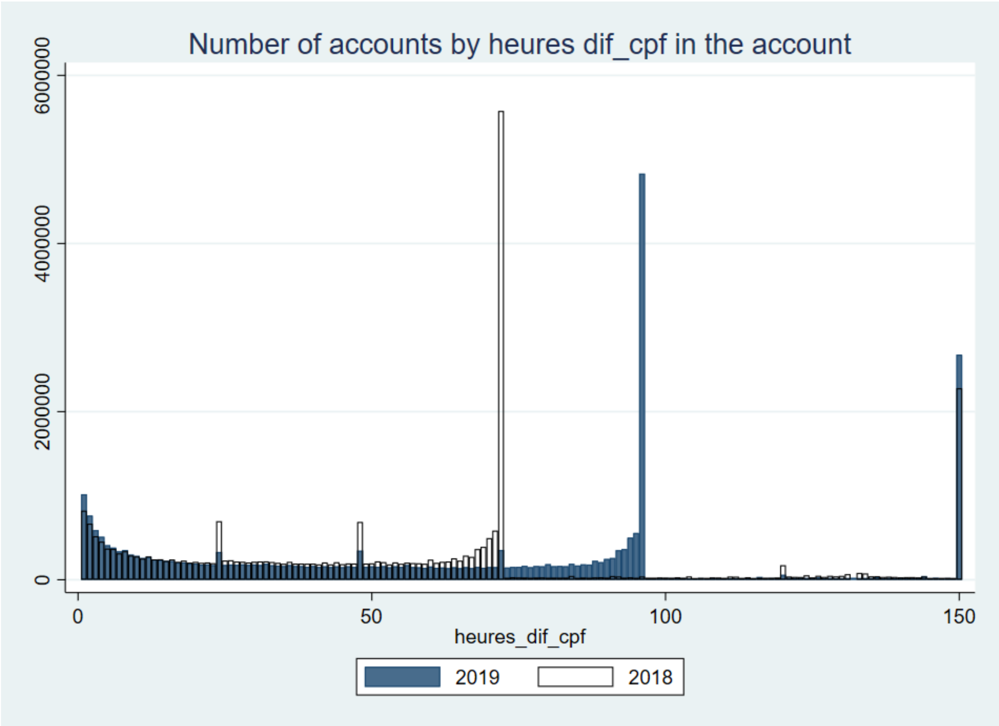

FIGURE B.3: Number of accounts by number of hours in the CPF account

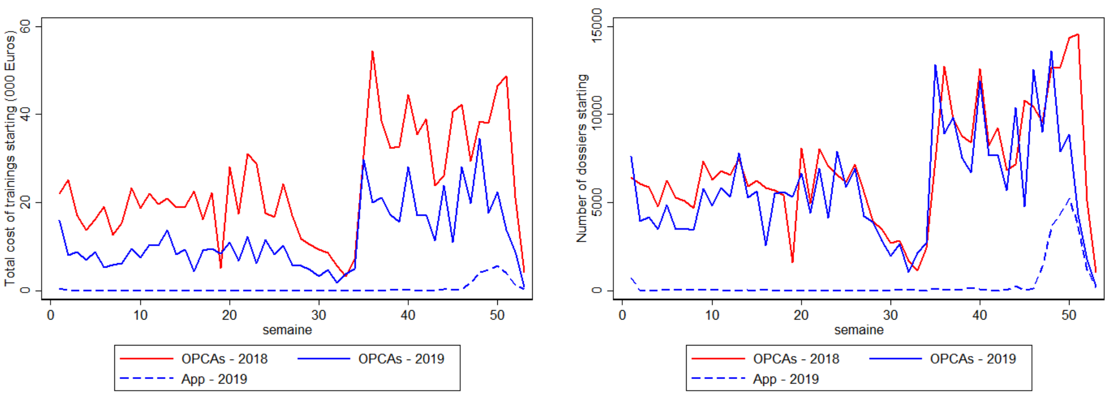

FIGURE B.4: Time series of total cost of trainings undertaken and number of trainings started each week, in 2018 and 2019, breaking down 2019 into trainings validated by industry financing centers and those initiated through the centralized mobile app

Correspondence betwen the industry agency and industry

Industry agency	Industry
ACTALIANS	Independant workers
AFDAS	Culture, communication, media, leisure
AGEFOS	Inter-industry and interprofessionnal
ANFA	Auto services
CONSTRUCTYS	Construction
FAFIEC	Engineering, studies and consulting companies
FAFIH	Hotels and restaurants
FAFSEA	Agricultural enterprises
FAFTT	Temporary work
FORCO	Retail and distribution
INTERGROS	Wholesale and international trade
OPCA 3+	Furniture, wood, construction materials and industry and the paper and cardboard intersector
OPCA DEFI	Chemicals, petroleum, pharmaceuticals, parapharmacy / veterinary, plastics
OPCA TRANSPORT	Transport
OPCABAIA	Banks, insurance companies, mutual insurance companies, general insurance agencies, assistance companies
OPCAIM	Metallurgy industries
OPCALIA	Inter-industry and interprofessionnal
OPCALIM	Food industry
UNIFAF	Health, social and medico-social sector
UNIFORMATION	Social economy

Figure B.5: Link between agencies and their industry

B.2 A Model of CPF with Discrete Choice and High Non-Monetary Training Costs

This Appendix sketches a simple model of CPF with discrete choice and high non-monetary costs of training to better understand a) why prices can be sometimes below the maximum subsidy cap, and b) why this should still not be a problem for IV estimation. For simplicity, let us ignore discretionary additions, $A_i = 0$. Assume then that training is discrete, i.e. consumers can either train for a fixed amount of hours $\bar{x_q} < \overline{x^{CPF}}$ or not train. Assume quasi-linear preferences with m_i as numeraire, and that consumers are heterogeneous in their utility from training $\phi_i(\bar{x_q}) = \phi(\bar{x_q}) + \eta_i$, where η_i can be interpreted as different benefits from training or different opportunity costs and is distributed as extreme values. Utilities from training and from not training are thus:

$$U_{i\bar{x_q}} = \omega - \max(p - \tilde{c}, 0) \cdot \bar{x_q} + \phi(\bar{x_q}) + \eta_i$$
$$U_{i0} = \omega$$

Normalizing $\omega = 0$, and keeping all other assumptions like in the previous section, McFadden et al. (1973) shows that aggregate demand is:

- If $p > \tilde{c}$, then $X^d(p) = n\bar{x_q} \cdot e^{-p\cdot\bar{x_q} + \tilde{c}\cdot\bar{x_q} + \phi(\bar{x_q})} / (1 + e^{-p\cdot\bar{x_q} + \tilde{c}\cdot\bar{x_q} + \phi(\bar{x_q})})$
- If $p \leq \tilde{c}$, then $X^d(p) = n\bar{x_q} \cdot e^{\phi(\bar{x_q})} / (1 + e^{\phi(\bar{x_q})})$

That is, when a CPF subsidy $\tilde{c}$ is introduced, demand shifts up only for those individuals who were already willing to pay some monetary costs to train, while those consumer who were unwilling to pay any price to train, possibly due to zero returns or high opportunity cost, remain in fact unwilling to train even if the subsidy covers the whole monetary cost.

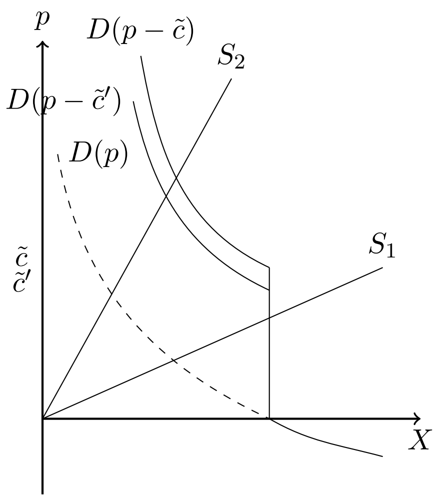

FIGURE B.6: Equilibrium with training as discrete choice and some individuals not willing to pay any monetary cost

Now, what is the equilibrium price and effective subsidy rates, when the subsidy cap $\tilde{c}$ varies? Suppose producers charge prices equal marginal costs (i.e., they can't discriminate consumers with CPF, for example if they are only part of their customers). Then, we will have a non linear first-stage, in red in Figure B.7: prices will tend to react to changes in the per-unit subsidy only when it "bites", i.e. when the subsidy is lower than the price and the effective subsidy changes. For example, when subsidies are reduced from $\tilde{c}$ to $\tilde{c}'$, and demand lowers from $D(p-\tilde{c})$ to $D(p-\tilde{c}')$, if supply is like S_2 and equilibrium prices were above the subsidy rate the cut will "bite" and lower price and quantity according to the elasticities of demand and supply. Conversely, when the per-unit subsidy is higher than the price, the cut in the subsidy doesn't bite, and the reaction of prices in equilibrium is null. Our IV estimator will necessarily use only instances where a variation in $\tilde{c}$ generates a variation in the effective subsidy c, and estimate the effect of the change of c on prices (i.e. the average slope of the black line left of the kink).

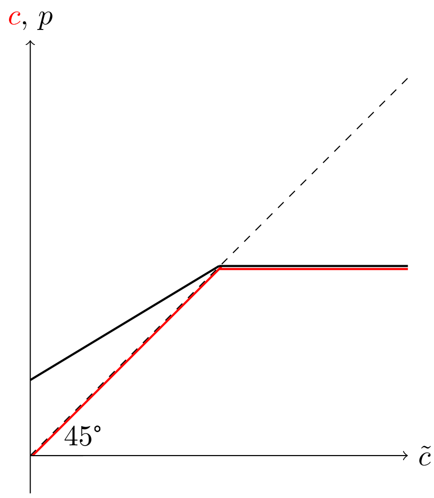

FIGURE B.7: Equilibrium prices as a function of per-hour value of the subsidy, with training as a discrete choice

Appendix C

Appendix to Chapter 3

C.1 Additional Tables and Figures

Table C.1: Evolution of the rules of Domestic Stability pact for Italian municipalities

Year	Target	Deficit rule	Accounting criteria	Others
1999	All municipalities	Zero growth	Cash	Initial sanctions: cut in transfers, ban on hires, cut on non-absenteeism bonuses
2000	All municipalities	Zero growth	Cash	
2001	>5,000	Max 3% growth	Cash	
2002	>5,000	Max 2.5% growth	Cash	Limit to current exp.
2003	>5,000	Zero growth	Cash+Accrual	
2004	>5,000	Zero growth	Cash+Accrual	
2005	>5,000		Cash+Accrual	Cur.+cap. exp. cannot grow more than pers. threshold (up to 10%)
2006	>5,000		Cash+Accrual	Current must be reduced, capital can grow within peronalized threshold
2007	>5,000	Zero growth	Cash+Accrual	
2008	>5,000	Zero growth	"Mixed"	
2009	>5,000	Personalized reduction goal (*)	"Mixed"	Additional sanctions: limits to borrowing, limits to current exp., larger cut to transfers and administrators' wages.
2010	>5,000	Pers. red. goal (*)	"Mixed"	
2011	>5,000	Zero-deficit	"Mixed"	
2012	>5,000	Zero-deficit	"Mixed"	Cut to transfers to municipalities >5,000
2013	>1,000	Zero-deficit	"Mixed"	
2014	>1,000	Zero-deficit	"Mixed"	
2015	>1,000	Zero-deficit	"Mixed"	
2016	All municipalities	Zero-deficit	Accrual	
2017	All municipalities	Zero-deficit	Accrual	
2018	All municipalities	Zero-deficit	Accrual	
2019	All municipalities	Zero-deficit	Accrual	

(*) Specifically, according to art.77 of L. 203/2008, municipalities are required to improve the 2007 balance, calculated on a "mixed" basis, a) If the municipality fulfilled the DSP and reported a deficit in 2007, 48% in 2009, 97% in 2010 and 165% for 2011; b) If the municipality fulfilled the DSP and reported a surplus in 2007, 10% in 2009, 10% in 2010 and 0% for 2011; c) If the municipality did not fulfill the DSP and reported a deficit in 2007, 70% in 2009, 110% in 2010 and 180% for 2011; and d) If the municipality did not fulfill the DSP and reported a surplus in 2007, 0% in 2009, 0% in 2010 and 0% for 2011. Requirments for 2011 were then modified by art.1 of L.220/2010.

TABLE C.2: Balance sheet items from pre-2015 model (CCOU) and post-2015 model (CCOX)

Item	quadro CCOU	voce CCOU	colonna CCOU			quadro CCOX	voce CCOX	colonna CCOX		
			Accrual	Cash	Residual			Accrual	Cash	Residual
Fiscal revenues	2	80	1	2	3	3	40	8	7	2
Non-fiscal revenues	2	310	1	2	3	3	60	8	7	2
Capital revenues	2	395	1	2	3	3	70+80	8	7	2
Capital transfers from state	2	345	1	2	3			8	7	2
Capital transfers from regions	2	350	1	2	3	2	230+240	8	7	2
Capital transfers from other PA	2	355+360	1	2	3			8	7	2
Fiscal federalism revenues	2	67	1	2	3			8	7	2
Borrowing	2	420	1	2	3	3	90+100	8	7	2
Entries for third-party services	2	425	1	2	3	3	110	8	7	2
Total entries	2	430	1	2	3	3	130	8	7	2
Current expenditures	3	5	1	2	3	5	20	8	7	2
Capital expenditures	3	10	1	2	3	5	30	8	7	2
Loans repayment	3	15	1	2	3	5	50	8	7	2
Expenses for third-party services	3	45	1	2	3	5	70	8	7	2
Total expenses	3	50	1	2	3	5	90	8	7	2

Notes. The Table reports the correspondance between the voices from the *Rendiconti di Bilancio* used, from the pre-2015 format (CCOU) and post-2016 (CCOX).

TABLE C.3: Descriptives, all dataset

	Mean	Sum	St.dev.	p10	p90	n
Period: 2007-2012						
Total budget	10,951	84,648,438	100,064	883	16,192	7,727
Fiscal revenues	3,322	25,594,384	25,768	169	5,490	7,727
Non-fiscal revenues	1,589	12,274,679	19,114	79	2,367	7,727
Revenues from capital transfers	2,032	15,743,330	31,624	84	2,999	7,727
Current expenditures	6,650	51,360,117	57,303	498	10,009	7,727
Capital expenditures	2,468	19,121,311	35,602	128	3,631	7,727
Total income declared	97,478,776	788,898,250,456	718,432,384	5,098,806	169,450,784	8,093
Labor income declared	51,525,562	416,918,271,815	375,114,298	2,455,260	93,827,240	8,092
Self-entrepreneurship income decl.	5,238,549	37,562,973,198	51,465,356	243,776	7,759,032	7,119
Capital income decl.	8,806,687	69,542,581,359	66,958,468	306,006	16,028,587	7,895
Freq. income 0-15,000	2,437	19,722,779	11,609	199	4,607	8,093
Freq. income 15,000-26,000	1,557	12,603,068	7,996	96	3,010	8,093
Freq. income > 26,000	1,057	8,550,627	8,871	37	1,770	8,093
Period: 2013-2015						
Total budget	11,822	90,906,228	104,032	841	17,369	7,690
Fiscal revenues	5,286	40,639,059	41,247	313	8,679	7,690
Non-fiscal revenues	1,665	12,800,792	21,936	79	2,441	7,690
Revenues from capital transfers	1,484	11,411,053	13,761	39	2,605	7,690
Current expenditures	7,056	54,257,627	68,998	500	10,842	7,690
Capital expenditures	1,726	13,273,888	16,147	55	2,955	7,690
Total income declared	102,013,032	817,859,931,229	748,240,320	5,164,882	179,283,536	8,018
Labor income declared	52,817,757	423,448,564,035	381,864,282	2,422,111	97,414,295	8,018
Self-entrepreneurship income decl.	8,868,396	65,073,767,021	68,182,576	514,083	14,906,539	7,338
Capital income decl.	7,860,670	61,767,158,815	59,140,644	275,023	14,428,147	7,858
Freq. income 0-15,000	2,248	18,021,255	11,117	172	4,266	8,018
Freq. income 15,000-26,000	1,529	12,261,596	7,591	97	3,008	8,018
Freq. income > 26,000	1,214	9,736,150	9,629	44	2,106	8,018
Period: 2016-2020						
Total budget	12,571	95,325,433	125,481	815	17,730	7,578
Fiscal revenues	4,990	37,804,407	41,966	293	7,996	7,578
Non-fiscal revenues	1,726	13,074,970	21,240	78	2,534	7,578
Revenues from capital transfers	1,225	5,563,300	10,314	36	2,121	4,547
Current expenditures	6,894	52,234,599	64,714	478	10,498	7,578
Capital expenditures	1,331	10,084,820	8,261	64	2,417	7,578
Total income declared	108,628,048	860,308,150,711	787,173,888	5,338,096	193,217,408	7,920
Labor income declared	57,564,814	455,891,261,262	411,181,109	2,564,747	107,659,706	7,920
Self-entrepreneurship income decl.	8,513,222	58,500,440,412	65,862,340	470,008	14,120,236	6,859
Capital income decl.	7,628,294	59,080,784,756	58,540,152	257,074	14,141,537	7,745
Freq. income 0-15,000	2,195	17,386,592	11,112	160	4,152	7,920
Freq. income 15,000-26,000	1,535	12,156,785	7,240	97	3,073	7,920
Freq. income > 26,000	1,354	10,722,296	10,257	50	2,393	7,920

Notes. The table reports descriptive statistics of the entire dataset after merging income data and municipalities' balance sheets. Monetary values are in current euros.

Table C.4: Descriptives, selected sample

	Mean	Sum	St.dev.	p10	p90	n
Period: 2007-2012						
Total budget	4,765	10,183,637	2,873	2,274	7,908	2,137
Fiscal revenues	1,628	3,478,210	1,002	766	2,686	2,137
Non-fiscal revenues	706	1,508,865	822	213	1,309	2,137
Revenues from capital transfers	901	1,925,305	1,391	158	1,880	2,137
Current expenditures	2,843	6,075,332	1,521	1,443	4,536	2,137
Capital expenditures	1,169	2,498,643	1,505	248	2,368	2,137
Total income declared	51,747,128	110,583,609,837	25,455,420	24,874,174	90,343,120	2,137
Labor income declared	27,900,349	59,623,045,052	14,404,516	12,626,165	49,714,625	2,137
Self-entrepreneurship income decl.	1,986,124	4,243,496,096	1,522,346	717,191	3,619,964	2,136
Capital income decl.	4,524,118	9,668,040,229	3,118,746	1,306,004	8,647,261	2,137
Freq. income 0-15,000	1,470	3,140,468	619	789	2,357	2,137
Freq. income 15,000-26,000	960	2,051,361	472	444	1,683	2,137
Freq. income > 26,000	503	1,075,380	295	197	942	2,137
Period: 2013-2015						
Total budget	4,890	10,450,384	3,341	2,095	8,503	2,137
Fiscal revenues	2,480	5,299,123	1,646	1,158	4,165	2,137
Non-fiscal revenues	703	1,502,354	769	212	1,312	2,137
Revenues from capital transfers	778	1,663,322	1,510	82	1,684	2,137
Current expenditures	2,942	6,286,324	1,761	1,453	4,750	2,137
Capital expenditures	861	1,840,097	1,596	90	1,918	2,137
Total income declared	54,157,236	115,734,016,066	27,003,766	25,515,796	95,119,464	2,137
Labor income declared	28,741,920	61,421,483,366	15,286,813	12,603,803	51,601,486	2,137
Self-entrepreneurship income decl.	4,163,909	8,898,273,937	2,417,464	1,724,584	7,471,494	2,137
Capital income decl.	4,074,462	8,705,786,539	2,813,466	1,224,264	7,724,896	2,137
Freq. income 0-15,000	1,313	2,805,748	567	697	2,104	2,137
Freq. income 15,000-26,000	948	2,026,461	459	450	1,660	2,137
Freq. income > 26,000	603	1,288,433	351	233	1,141	2,137
Period: 2016-2020						
Total budget	5,010	10,705,840	3,526	2,140	8,708	2,137
Fiscal revenues	2,264	4,838,701	1,386	1,100	3,667	2,137
Non-fiscal revenues	709	1,515,651	827	201	1,341	2,137
Revenues from capital transfers	613	785,755	882	76	1,354	2,137
Current expenditures	2,911	6,220,958	1,856	1,393	4,725	2,137
Capital expenditures	762	1,627,741	918	141	1,606	2,137
Total income declared	57,145,408	122,119,736,290	29,140,016	26,266,862	100,766,208	2,137
Labor income declared	30,973,055	66,189,417,900	16,770,620	13,338,547	55,720,576	2,137
Self-entrepreneurship income decl.	3,674,313	7,832,826,087	2,323,032	1,374,153	6,754,143	2,131
Capital income decl.	3,916,998	8,370,624,850	2,830,692	1,145,194	7,467,320	2,137
Freq. income 0-15,000	1,240	2,649,512	537	657	1,992	2,137
Freq. income 15,000-26,000	950	2,030,258	458	450	1,666	2,137
Freq. income > 26,000	680	1,453,323	393	265	1,279	2,137

Notes. The table reports descriptive statistics of the our sample after dropping municipalities from Valle d'Aosta and Bozen autonomous province, municipalities that were merged, and restrict to municipalities with no missing information between 2007 and 2018, having a number of inhabitants between 2000 and 8000 in the 2011 census, which comprise all the municipalities in the different bandwidths around the threshold of 5,000 we are going to use. Monetary values are in 2012 euros.

TABLE C.5: Optimal bandwidth for first stage and reduced form estimation

Panel A: estimated optimal bandwidth for first stage

Method	Opt. BW for the estimator	Opt. BW for the bias-correct.
mserd	880	1605
msesum	1761	1761
cerrd	576	1605
cersum	717	1761

Panel B: estimated optimal bandwidth for reduced form

Method	Opt. BW for the estimator	Opt. BW for the bias-correct.
mserd	1516	2417
msesum	1540	2459
cerrd	992	2417
cersum	1008	2459

The table reports optimal bandwidth calculated using Calonico et al. (2020) with long differences in net per-capita surplus (Panel A) and per-capita income (Panel B) as outcome, restricting to post-2013. A polynomial of order 1 is imposed and standard errors are clustered at the municipality level.

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Table C.6: Heterogeneity according to the percentage of neighboring municipalities which is treated

Bandwidth: 1000

	(1) Cutoff p50	(2) Cutoff p75	(3) Cutoff p90
High spillover	0.199	1.469	1.581
	(1.330)	(1.201)	(1.034)
Low spillover	1.223	0.744	-0.343
	(1.881)	(1.710)	(1.763)

Bandwidth: 1500

	(1) Cutoff p50	(2) Cutoff p75	(3) Cutoff p90
High spillover	-0.256	-0.037	1.244
	(1.554)	(1.212)	(1.191)
Low spillover	1.560	1.980	-0.530
	(3.656)	(3.601)	(3.002)

Bandwidth: 2000

	(1) Cutoff p50	(2) Cutoff p75	(3) Cutoff p90
High spillover	-0.050	0.668	1.236
	(1.862)	(1.811)	(1.600)
Low spillover	-1.026	-0.595	-1.509
	(3.472)	(2.803)	(2.783)

Bandwidth: 2500

	(1) Cutoff p50	(2) Cutoff p75	(3) Cutoff p90
High spillover	-0.327	-0.472	-0.093
	(1.453)	(1.524)	(1.361)
Low spillover	-1.202	0.512	-0.274
	(3.700)	(2.471)	(2.126)

The Table replicates the baseline reduced form estimates for municipalities above/below a cutoff in the percentage of population in surrounding municipalities which is treated. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

Table C.7: Effect of DSP on Per-Capita Surplus and Local Income

	(1)	(2)	(3)	(4)
VARIABLES	Surplus pC	Income pC	Surplus pC	Income pC
DSP	34.68	18.94	145.0***	-60.72
	(22.05)	(87.05)	(47.91)	(103.1)
Observations	5,256	5,256	3,177	3,177
R-squared	0.471	0.969	0.437	0.978
Years	2007-2015	2007-2015	2007-2015	2007-2015
Bandwidth	1500	1500	1500	1500
Region	NORD	NORD	CENTROSUD	CENTROSUD
Mean in 2012	-224.3	14334	-367.6	9899
Multiplier		-.546		.418
		[2.50]		[.748]
	(1)	(2)	(3)	(4)
VARIABLES	Surplus pC	Income pC	Surplus pC	Income pC
DSP	42.28**	-85.37	108.8***	-86.10
	(18.45)	(118.9)	(39.58)	(138.6)
Observations	7,008	8,176	4,236	4,942
R-squared	0.519	0.962	0.471	0.965
Years	2007-2018	2007-2020	2007-2018	2007-2020
Bandwidth	1500	1500	1500	1500
Mean in 2012	-224.3	14334	-367.6	9899
Region		NORD		CENTROSUD
Multiplier		1.147		.892
		[2.65]		[1.27]
	(1)	(2)	(3)	(4)
VARIABLES	Surplus pC	Income pC	Surplus pC	Income pC
DSP	71.54***	11.35	124.5**	-32.88
	(23.79)	(60.08)	(48.35)	(73.45)
Observations	3,504	3,504	2,118	2,118
R-squared	0.504	0.983	0.476	0.990
Years	2010-2018	2010-2020	2010-2018	2010-2020
Bandwidth	1500	1500	1500	1500
Mean in 2012	-224.3	14334	-367.6	9899
Region		NORD		CENTROSUD
Multiplier		-.158		.264
		[.839]		[.609]

Notes. The Table reports difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities between 1,000 and 5,000 inhabitants, from 2013 onward on their net per-capita surplus and income. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors in the last row are calculated with an IV regression of per-capita income on net surplus, instrumented by the DSP dummy.

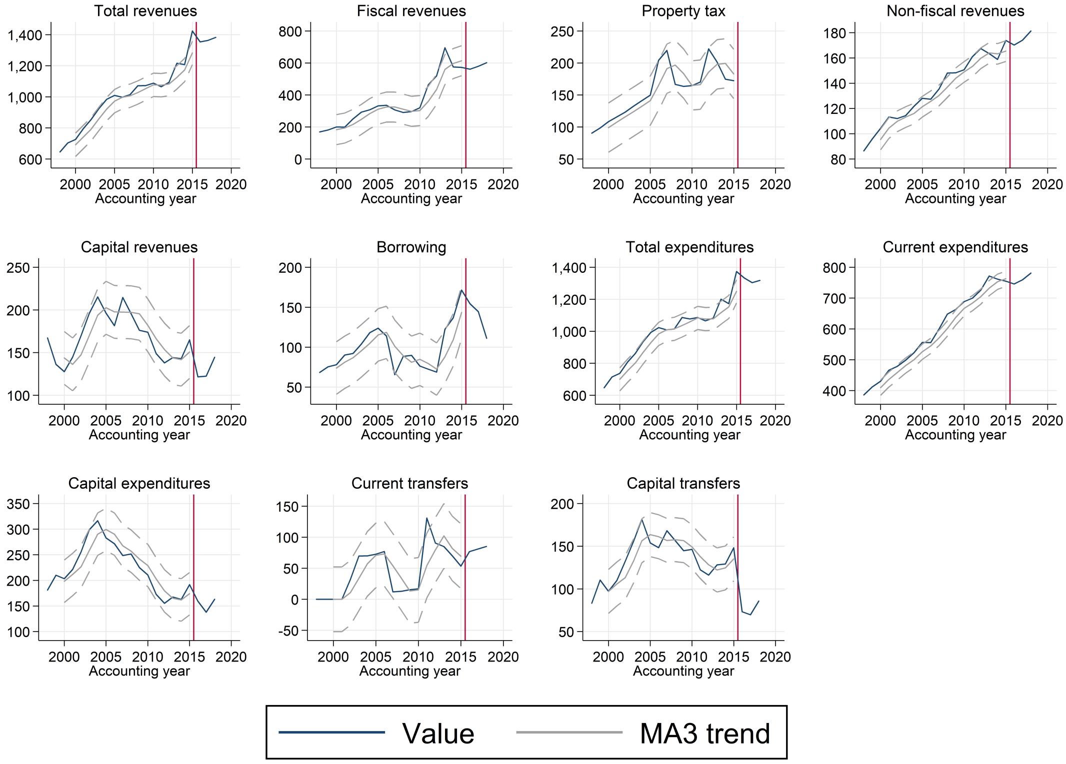

FIGURE C.1: Stability in match between balance-sheet variables in the pre-2015 model (CCOU) and post-2015 model (CCOX)

Notes. The Figure reports time series and MA3 trends of our main balace-sheet variables of interest, across the discontinuity of 15 in the balance sheets data.

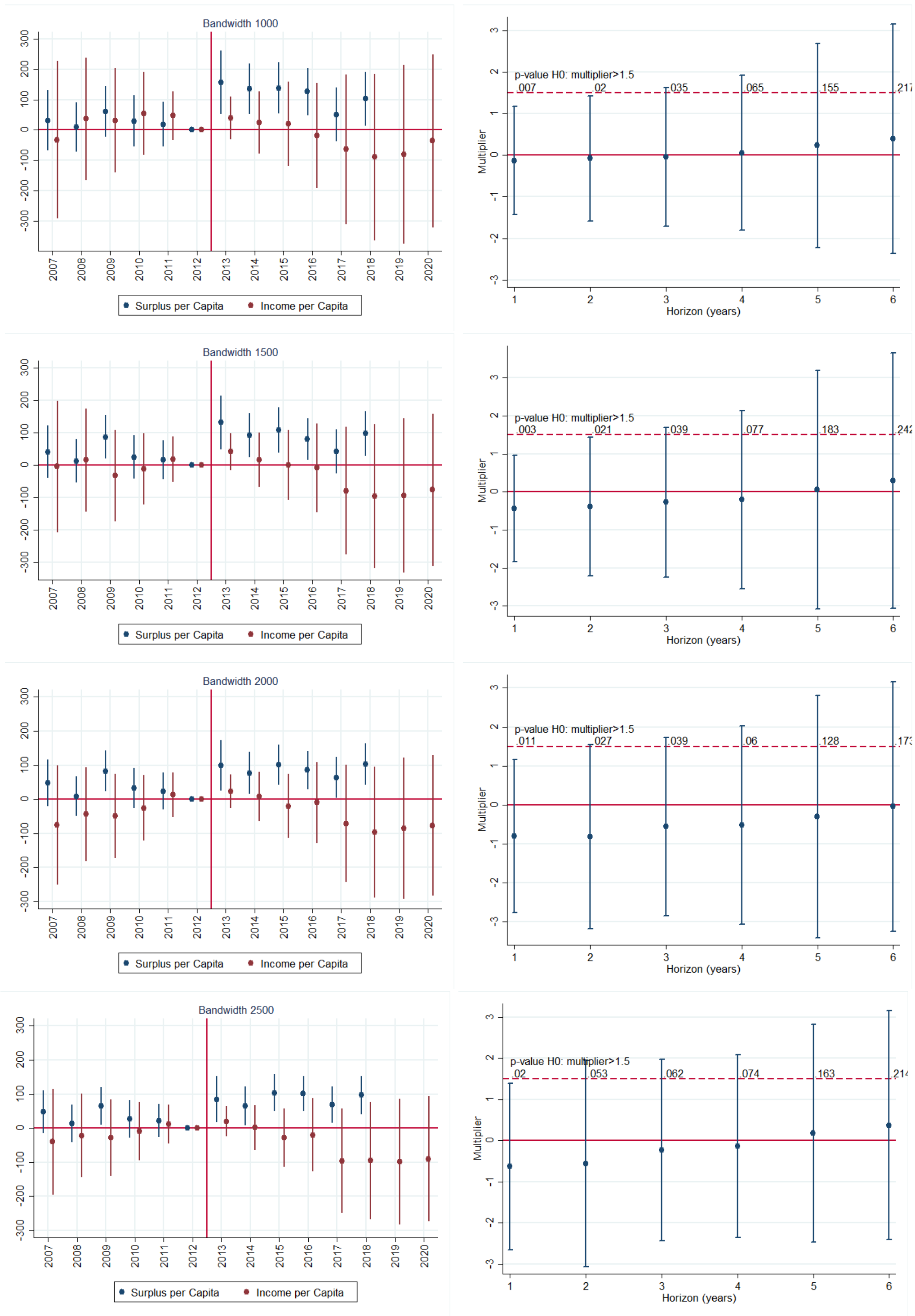

FIGURE C.2: Main Results Including Special Statute Regions

Notes. The figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level. The multiplier estimate and its standard errors on the right are calculated with an IV regression of per-capita income on net surplus, instrumented by the DSP dummy, keeping observations only up to a specific horizon after the shock. The p-value in the right figures refer to a one-sided test for the multiplier being below 1.5.

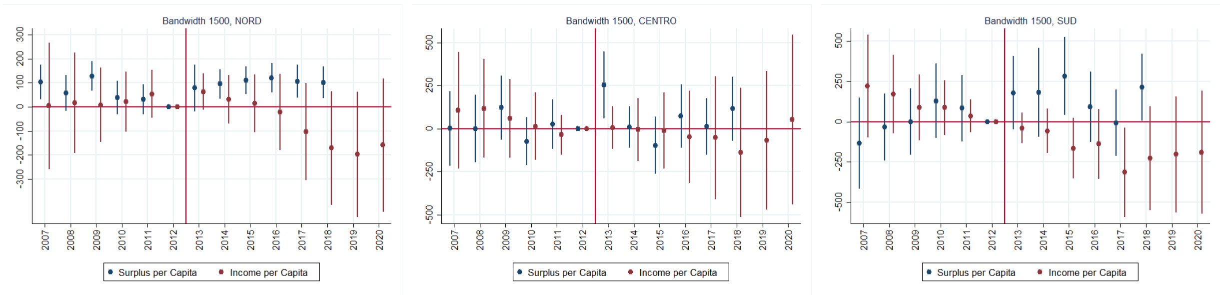

FIGURE C.3: Heterogeneity by Macroregion

Notes. The Figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

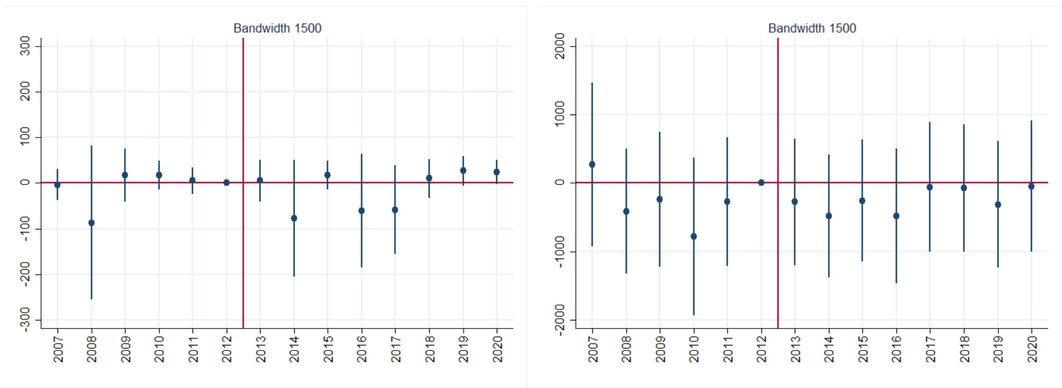

FIGURE C.4: Placebo test: no change in the amount of funds received from EU Cohesion policy (left panel) and from certified public investment (right panel)

Notes. The Figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

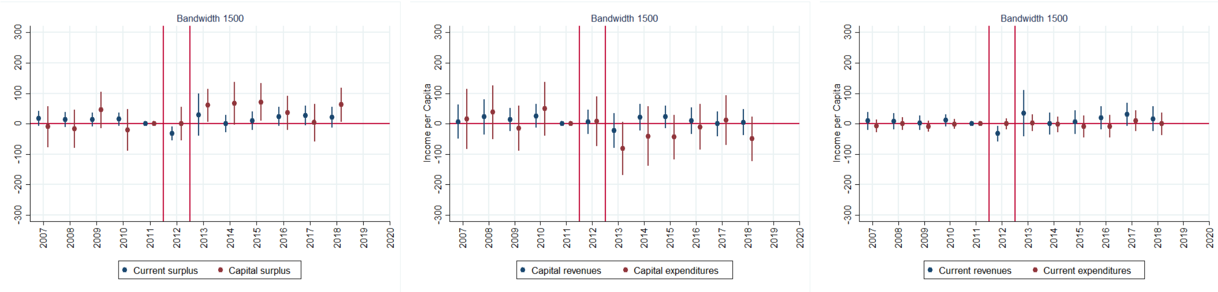

FIGURE C.5: Composition of the First Stage, Dynamic Specification

Notes. The Figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

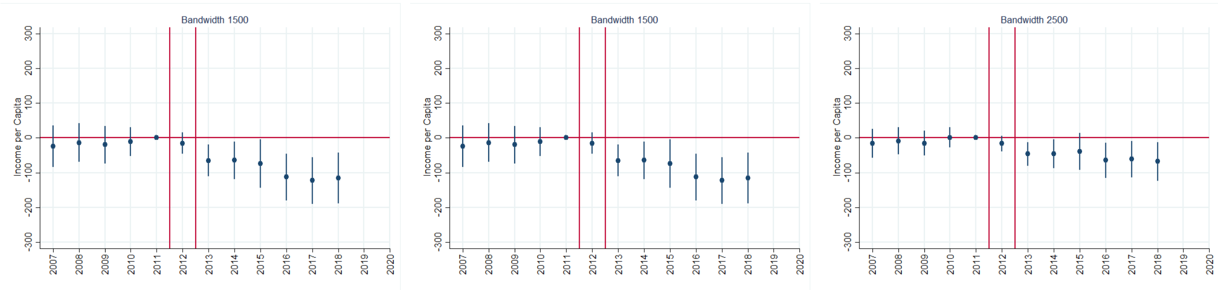

FIGURE C.6: Effect of DSP on Borrowing, Dynamic Specification

Notes. The Figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

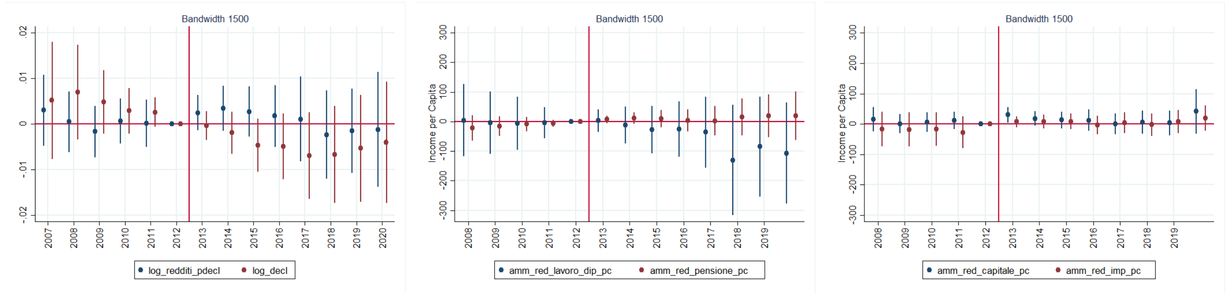

FIGURE C.7: Composition of the Reduced Form, Dynamic Specification

Notes. The Figures report difference-in-discontinuities estimates of the effect of the extension of the Domestic Stability Pact to Italian Municipalities below 5,000 inhabitants from 2013. All regressions include FEs for municipality and year, as well as controls for the interaction between the difference between population and the 5000 threshold and years FEs; and an interaction between the difference between population and the 5000 threshold, years FEs, and the dummy for treatment group. Standard errors are clustered at the municipality level.

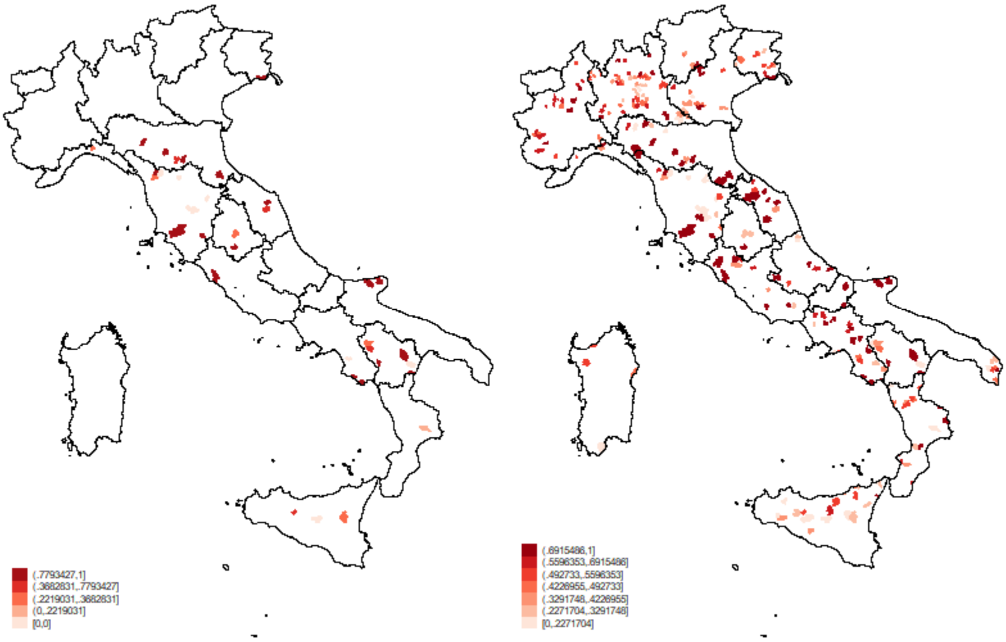

FIGURE C.8: Sample for Spillovers Analysis

Notes. The Figures reports municipalities that a) are between 5000 and 5000 inhabitants in 2011 b) in a 15 minutes by car range, they are surrounded only by municipalities in the 2500-7500 population bandwidth in 2011 (left panel) or population in neighboring municipalities is only up to 50% from municipalities above 7500 and below 2500. In shades of red, the share of of neighboring municipalities (15 minutes by car range) which is treated.

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