# Essays on Skills Formation

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*To my beloved family*

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# Abstract

This thesis examines the role of public policies and institutions in fostering human capital formation and utilization. In the first chapter, I analyze the impact of college remedial courses on students' performance and college completion, using administrative data from the department of Economics of an Italian university. My results suggest that the complex design of the policy studied hinders its effectiveness, as it fails both to boost students' results and to reduce college drop-out. In the second chapter, a joint work with Clémentine Van Effenterre, we study mothers' labor supply response to a reorganization of children's school schedule, promoted in France since 2013. We show that mothers react to this intervention by restructuring their working time in accordance to children's new schedule. Finally, in the last chapter, I investigate whether loosening employment protection legislation decreases firms and workers' investments in training. Exploiting a reform introduced in the United Kingdom in 2012, I find that reducing job protection in a period of negative wage growth does not lead to an increase of dismissals or a decline in training investments.

# Resumen

Esta tesis examina el papel que las políticas y las instituciones públicas pueden jugar para favorecer la formación y el uso del capital humano. En el primer capítulo, estudio el impacto de los cursos de recuperación ofrecidos en la universidad sobre los resultados de los alumnos y su probabilidad de acabar los estudios universitarios, utilizando datos administrativos de un departamento de Economía de una universidad italiana. Mis resultados sugieren que la complejidad de esta política estorba su eficacia, dado que no consigue mejorar ni el desempeño de los estudiantes ni disminuir su probabilidad de abandonar la universidad. En el segundo capítulo, un trabajo conjunto con Clémentine Van Effenterre, analizamos el impacto de una reorganización del horario escolar de los niños, introducida en Francia a partir de 2013, sobre las decisiones de trabajo de las

madres. Mostramos que las mujeres restructuran su semana de trabajo en función del nuevo horario de sus hijos. Finalmente, en el último capítulo, investigo el efecto de una disminución de la protección laboral sobre las decisiones de empresas y trabajadores de invertir en formación. Explotando una reforma introducida en Reino Unido en 2012, encuentro que reducir la protección laboral, en un periodo de crecimiento negativo de los salarios, no implica ni un aumento de despidos ni una disminución de inversiones en capital humano.

# Preface

Two main trends have characterized the economy of developed countries in the twentieth century, the evocative ”race between education and technology” and the so-called ”quiet revolution” in women’s labor supply.

The compelling and interconnected debates that have developed around these two expressions have greatly inspired my three ”essays on skills formation”. The growing literature on the dynamics of the supply of college graduates clearly influenced the first chapter of this work, where I focus on the importance of skills endowments in explaining educational attainments. The other two chapters deal instead with the role of institutions in promoting the formation and use of skills at work. In particular, in the second one, together with Clémentine Van Effenterre, we enter into the last chapter of women’s labor supply dynamics and analyze the impact of institutional constraints and flexible working schedules on mothers’ employment decisions. Finally, the works on the task-replacing technological change led me to study the factors shaping firms and workers’ incentives to invest in on-the-job training, by specifically focusing on the effects of employment protection legislation on training investments.

Before introducing the three chapters in more details, I find it opportune to briefly summarize the key elements of the ”race between education and technology”, the crucial steps in the ”women’s quiet revolution”, and the literature that has arisen around these topics.

The former expression was firstly coined by Tinbergen in 1974 (Tinbergen 1974) and lately became the title of a famous book by Goldin and Katz (Goldin and Katz 2009). According to these authors skill-biased technological change constantly increased the relative demand for skilled workers over the twentieth century. As the supply of educated labor followed the pace of demand, returns to education and the college wage premium, at their highest at the beginning of the century, consequently declined. However, starting from the 1980s, the rate of growth of high-skilled workers began to slow down. Accordingly, the college wage premium rose once more until recovering the levels encountered at the beginning of the century.

All researchers recognize the merits of Goldin and Katz's book in accounting for the main labor market trends that characterized an entire century. However, several studies also highlight that this work cannot account for two major (and complementary) trends that started in the early 1990s (Acemoglu and Autor 2012, Autor, Katz, and Kearney 2006, Autor, Levy, and Murnane 2003). First, earnings dynamics have been characterized by a rapid growth at the upper and lower deciles of the wage distribution, than at the median. Secondly, a similar u-shaped pattern has characterized employment, exhibiting a more rapid growth in high-skilled and low-skilled occupations, than in middle-skilled jobs. Acemoglu and Autor (2012), in their review of Goldin and Katz's book, define these trends as wage and job polarization. To account for them, the authors propose to increase Tinbergen canonical model of demand and supply of high- versus low-skilled workers with two elements. First, they introduce a distinction between high-, middle- and low-skilled workers. Secondly, they relax the equivalence between workers' skills and tasks, as this allows them to account for the fact that the assignment of skills to tasks can evolve over time, in particular when the set of tasks demanded in the economy is altered by technological advancements, or the dynamics of globalization. Observing that machines have replaced the routine tasks performed primarily by medium-skilled workers, a task-replacing technological change clearly becomes more suited to explain the recent trends of wage and job polarization. Yet, we are still far from understanding how to govern these phenomena (Acemoglu and Restrepo 2016).

Another strand of literature focuses on the slow-down in the supply of college graduates and highlights the challenges that raising educational attainment involves (Angrist, Lang, and Oreopoulos 2009, Dynarski and Scott-Clayton 2013, Oreopoulos and Dunn 2013, Scott-Clayton, Crosta, and Belfield 2014, Stinebrickner and Stinebrickner 2012, Turner 2004). These studies show that information asymmetries, financial constraints, motivation and preparation are all key factors to take into account when designing policies aimed at raising college completion.

Finally, it is important to stress that Goldin and Katz' work refers to the experience of the United States, and the same is true for most of the studies that analyze the topics of college enrollment and completion. However, phenomena like the rise in income inequality (Dustmann, Ludsteck, and Schönberg 2009, Piketty 2015), wage and job polarization (Goos and Manning 2007, Michaels, Natraj, and Van Reenen 2014), and the increase in college drop-out (De Paola and Scoppa 2014, Fack and Grenet 2015, Hübner 2012, Garibaldi, Giavazzi, Ichino, and Rettore 2012) are not confined to the United States and affect most European countries as well.

Concerning the other main trend, the so-called quiet revolution in women's labor supply, Goldin (2006) shows that, in parallel to the race between education and technology and strictly connected to it, the last century has been characterized by a slow convergence in the roles of men and women, and this is clearly true for all developed countries (Blau and Kahn 2013).[1] A narrowing has occurred between men and women in labor force participation, working hours, life-time working experience, occupations, and college majors. Women have even overtaken men in educational attainments. According to Goldin, these dynamics have occurred thanks to three fundamental changes in women's believes – especially of married women – that became clear in the 1970s: women changed their time horizon with respect to life-time labor force participation, from intermittent and brief to long and continuous, and this clearly affected their human capital investments; women changes the way they perceived their job, from a necessity to sustain household income to an expression of their own identity; women affirmed their role in the household decision making process. All these changes were favored by several and cumulative exogenous factors: the increase in demand for clerical workers in the 1920s; the creation of scheduled part-time work in the 1940s; the diffusion of electric household technologies since the 1950s; the spreading use of the contraceptive pill at the end of the 1960s. The changes in beliefs accordingly translated in a constant decline of women's income elasticity

[1] Even though the exact timing of this convergence may differ across countries, as highlighted again by Blau and Kahn (2013).

and the correspondent increase of the substitution elasticity in their Slutsky equation. These patterns have in turn found a reflection in a steady increase of women's labor force participation and hours worked.

Goldin concludes her seminal work by looking at the current situation – a prelude to Goldin (2014). Since 1990, female labor force participation rates and the fraction of women working full-time are no longer raising. The proportion of active women in their thirties is stacked at around 75 percent in the United States. Blau and Kahn (2005) claim that this is due to a strong reduction in women's own wage elasticity. According to Goldin, this pattern must instead be read in conjunction with the demographic changes that led women to postpone childbearing. If this is the case, how to manage work and family duties becomes the focus of the last chapter of the revolution in female labor supply.

Goldin (2014) claims that in this last chapter the gender gap in participation and wages may well be eliminated if only women's quest for flexibility in the work environment was satisfied. In some occupations working longer hours and/or a regular presence at work might indeed be more rewarded than in others. This could be the case, in particular, in those professions where it is important to build solid relationships with co-workers, attend frequent meetings, take key decisions, and perform tasks under pressure. The continuous presence at work and the availability to work long hours should be particularly valuable in these contexts, or, in other words, the cost of a flexible working schedule might be especially high in these occupations. This argument may well explain why the gender wage gap remains largest at the top of the wage distribution, as highlighted in the last review on the topic made by Blau and Kahn (Blau and Kahn 2016). As flexibility is particularly valuable for women, measuring and reducing its costs should then be the focus of the current gender debate, according to Goldin.

At the same time, other studies such as Wiswall and Zafar (2016) insist on the importance of gender preferences in explaining labor market choices and career patterns. And the experimental literature pioneered by Niederle (Buser, Niederle, and Oosterbeek 2014, Niederle 2014) shows that gender differences in reaction to competition exist and should not be neglected in the gender debate. Finally, Blau and Kahn (2013) warn that the introduction of "family-friendly" policies such as parental leave and part-time work may well boost female labor force participation, but also encourage part-time work and employment in lower level positions. This would explain why the proportion of active women in the United States has decreased in the last twenty years relative to many OECD countries that promoted these policies, but also why women in the United States are more likely than women in other countries to have full-time jobs and to work as managers or professionals. The debate on this last chapter of women's labor supply could never be more open.

With this work, I hope to contribute to these debates by drawing from the experience of three European countries. In the first chapter, I study the phenomenon of college drop-out in Italy. The fact that the United States, with a tough selection at entrance and very high tuition fees, have approximately the same college drop-out rate, at 20 percent, as Italy, with basically no selection at entrance and relatively low tuition fees, led me to think that students' preparation for college might play an important role in explaining this phenomenon. For this reason, I analyze the impact of college remedial education. In the second chapter, together with Clémentine Van Effenterre, we look at female labor supply in France. Studying the last chapter of the "quiet revolution" in a country where the proportion of active women is beyond 80 percent seems particularly appropriate to me. Finally, in the last chapter, I analyze the relationship between employment protection legislation (EPL hereafter) and on-the-job training in the United Kingdom, a country where job protection rises with seniority, more than 50 percent of workers engage in employer-financed training – against an European average that is lower than 30 percent – and yet, wage and job polarization are constantly increasing.

In detail, in **"Is College Remedial Education a Worthy Investment? New Evidence From a Sharp Regression Discontinuity Design"** I analyze the impact of college remedial courses on students' decisions and performance. To enhance college completion, an increasing number of higher-education institutions are introducing college remedial courses. The goal of these courses is to foster students' readiness for college. However, assigning students to remedial education may also discourage them from continuing their studies if they interpret this as a negative signal on their chances of succeeding in college, they fear a stigma effect, or they doubt to be able to manage the increase in workload these courses involve. Moreover, assignment to remedial education is usually based on the performance in a placement test that students are required to take prior to entering college. This implies that some students may even decide not to enroll in college if placed in remediation. To assess the overall implications of this initiative, and in particular to study its impact on college enrollment, drop-out and performance, I collected a novel data set from the department of economics of an Italian university that introduced its own remedial policy in 2009. To estimate causal effects, I implement a sharp regression discontinuity design, that exploits the cut-off rule used to assign students to remediation. Results indicate that students do not get discouraged when placed in remedial courses. However, the assignment to remediation does not trigger any positive and significant effect on either persistence in college, credit accumulation, or the probability of passing the college-level exam in the remedial subject. My findings, which differ from previous ones obtained in a similar context by De Paola and Scoppa (2014), suggest that the specific structure of college remediation may play an important role in determining its success. With these conclusions, I aim to contribute to the growing literature that analyzes alternative measures designed to enhance the supply of college-educated workers.

Next, in **"How Does Maternal Labor Supply Respond to Changes in Children's School Schedule?"**, joint with Clémentine Van Effenterre, we exploit a reform of the primary school schedule, that was implemented in France in 2013, to contribute to the debate on women's labor supply, along two specific dimensions. First, this intervention, that restructured and extended the total time children can spend in school, gives us the opportunity to understand to what extent the elimination of institutional constraints can further boost women's labor supply in the context of a developed country, characterized by high female labor force participation rates. Secondly, this reform allows us to investigate whether having a flexible schedule is especially costly for some women, as suggested by Goldin (2014). To analyze these issues, we compare employment decisions of mothers whose youngest child is in primary school with those of mothers whose youngest child is slightly older, in a difference-in-difference framework. With respect to the first dimension of response – the potential increase in labor supply driven by the implicit wage subsidy offered by the reform – we provide evidence that mothers reallocate their working hours over the week but do not increase the the total number of hours worked per week. Concerning the second dimension of response, we show that women do take into account that flexibility is costly when making their employment decisions. On the one hand, we see that women facing a higher cost of flexibility – i.e. those working in occupations where it is important to build solid relationships with co-workers, attend frequent meetings, take key decisions, and perform tasks under pressure – were already working longer hours before the reform. On the other hand, we observe that only women facing a low cost of flexibility – that is those working in professions where team work is less relevant, the worker is not responsible for important decisions, and work pressure is low - are able to immediately react to the reform, by restructuring their working schedule in accordance to the new timetable of their children. By combining the new insights of Goldin's theory, to the evidence provided by Blau and Kahn (2016), and to the literature on childcare,[2] these results hopefully enrich the gender debate, with a special regard to the European context.

Finally, in **"Does Employment Protection Legislation Affect Training Investments? Evidence from the United Kingdom"**, I test the hypothesis that in flexible labor markets, firms and workers may have less

[2] This literature comprises, among others, Bauernschuster and Schlotter (2015), Baker, Gruber, and Milligan (2005), Berlinski and Galiani (2007), Cascio (2009), Fitzpatrick (2010), Gelbach (2002), Havnes and Mogstad (2011).

incentives to invest in on-the-job training. Analyzing this topic is particularly important in the conceptual framework of the race between education and technology, as on-the-job training can be seen as a key tool to increase labor productivity and allow both firms and workers to cope better with rapid task-replacing technological changes. Moreover, the empirical evidence on the impact of EPL on training investments is basically non existent, despite the fact that in the current debate over the effects of flexible EPL, its negative consequence for training are often cited by its opponents.[3] In addition, many countries are considering the possibility to introduce the so-called unique contract to overcome a dual labor market structure. The main feature of this contract is that employment protection should raise with tenure and it is unclear how this could affect the level and timing of training investments.

The United Kingdom offers an interesting setting to analyze this topic as it is one of the few countries where workers receive high doses of training, and all workers are hired under the so called ”unified” contract, the closest version to the single contract that has been put in place so far. With this contract, firing costs rise with seniority, after an initial probationary period, with no protection. The length of this initial phase has been repeatedly modified and the last modification was introduced in 2012, in the immediate aftermath of the financial crisis. This intervention shortened the probationary period from two to one year of tenure. This allows me to study two issues. First, I can analyze the effect of loosening EPL during a recession, characterized by negative wage growth. Secondly, conditional on this effect, I can study how training levels evolve from the probationary phase to the following part of the contract and whether shortening the probationary period affects training investment. To do so, I compare, in a difference-in-difference framework, workers who have between one and two years of tenure, with those having more than two years of seniority,

[3] For a detailed review of the literature on the impact of EPL on labor market dynamics, see the third chapter of this work. Regarding the topic of on-the-job training, the seminal works of Acemoglu and Pischke, namely Acemoglu and Pischke (1996) and Acemoglu and Pischke (1999), clearly define the theoretical framework for any study on this matter.

who are not affected by the 2012 reform. My results show that the firing hazard of treated workers does not increase after the reform. This may happen because, in a period in which real wages are falling, firms might be less inclined to fire their workers. Secondly, conditional on this result, my findings suggest that training investments for treated employees do not decrease either, while training increases for workers with less than one year of tenure. These findings are encouraging with respect to the introduction of the unique contract, as it is currently discussed in many European countries. Moreover, they should be particularly insightful for the debate on skills-biased technological progress and the specific role that labor market institutions may have in helping workers coping with it.

To conclude, as the debates that have inspired this work evolve, so does my research agenda, but the focus remains on skills acquisition and utilization. In particular, my next works will focus on the impact of guidance program towards university choice on high-school graduates' decisions; on the effect of cultural norms in influencing gender preferences towards education and work; and on the impact of women's quest for flexibility on firms' organization. Maybe, in the twenty first century, women will have a decisive role in the race between education and technology.

# Contents

# List of Figures

# List of Tables

# Chapter 1

# Is College Remedial Education a Worthy Investment? New Evidence from a Sharp Regression Discontinuity

## 1.1. Introduction

Despite the increasing evidence on the overall gains of acquiring higher education (Kaufmann, Messner, and Solis 2013, Goldin and Katz 2007, Oreopoulos and Petronijevic 2013, Zimmerman 2014), 32 percent of tertiary students starting university in OECD countries do not graduate (OECD 2013). Both economists and policy makers have proposed several policies to enhance college completion, ranging from financial aid to mentoring services. One measure that is becoming increasingly popular is remedial education. Remedial courses are offered to first-year students who have weak academic skills. In the United States, about one-third of college students are required to take these courses, and public colleges alone spend between \$1 and \$4 billion in remediation (Martorell, McFarlin Jr, and Xue 2014). The rationale behind this initiative is that students might drop out of university because they lack the adequate preparation to succeed in their tertiary studies. However, the effect of assigning students to these courses might be ambiguous. On the one hand, remediation should help students to recover some basic skills in order to increase their college retention and improve their performance. On the other hand, there are several reasons why the assignment to remedial courses might increase their chances of dropping out. First, remedial courses usually do not count towards degree completion, but as a prerequisite for college-course attendance. Hence, they increase the overall workload for students who are assigned to them. Secondly, if a student is assigned to remediation, he might perceive this as a negative signal on his ability to pursue a college degree. Third, the assignment to remediation might generate a social stigma, as students who are placed in these courses might be considered as "less academically able" by their peers. All these factors may demoralize first-year students and increase their probability of quitting university. Remedial education might also have heterogeneous effects: it could help the weakest students, but discourage those who would have not expected to be placed in remediation. Finally, remediation can simply be ineffective in reducing students' drop-out probability. Given the increasing interest that colleges and policy institutions are showing for this measure, it is extremely important to identify in which context and for which type of student college remedial education could be useful.

In this paper, I provide new evidence on the effect of remedial education on students' decisions and performance. To do so, I collected a novel data set from the department of economics of an Italian university that introduced its own remedial policy in 2009. Students who want to enroll in this department have to take an exam, that assesses their readiness in math, logic, and verbal comprehension. If they score below a certain threshold in the math section, they are assigned to remediation. This has several consequences. Students have to take a remedial math exam and they cannot take the regular math exam until they have passed the remedial one. They can participate in a remedial course and have a minimum of five retakes for the remedial exam, over the course of the first year. However, if they are not able to pass any of these retakes during the first year, they are automatically re-enrolled in the first year the following September. To identify the impact of this remedial program, I implement a sharp regression discontinuity (RD) strategy that exploits the cut-off rule used to assign students to remediation.

The first outcome that I consider is the decision to enroll in college. Assignment to remedial education is based on students' performance in the placement test that they are required to take prior to entering in the department. This could lead to the undesired effect that some of them choose not to enroll if placed in remediation. The stigma effect might be especially important at this stage, when students still do not know their peers and how they will be judged by them. Moreover, if the performance in the entrance exam constitutes the first signal on the chances of succeeding in college, students may be strongly demoralized by a negative result.

Next, I study how the assignment to remedial education affects students' decisions and performance along their college career. Its impact might become visible in the short or in the long run. It could materialize in the first year, when students should make extra effort to recover their initial weaknesses and not to lag behind in their regular courses. Alternatively, its effect might appear later, when students have supposedly recovered from their initial difficulties. Moreover, this measure may only influence the probability of dropping out, or act first on students' performance in college, both in the subject of the remedial class and in the other courses. In principle, remediation should increase the chances of passing the college-level exam in the subject of the remedial course, given that its main goal is to help students to recover some basic notions on this topic. Meanwhile, the assignment to remediation might either boost students' effort in all subjects, or induce them to focus primarily on the remedial course, with a detrimental effect on their performance in the other subjects.

My results suggest that the assignment to remediation does not discourage students who are only just assigned to it from enrolling in the chosen department. At the same time, it does not improve either their overall performance or their performance in the subject of remediation. Importantly, placing students in remediation has no significant effect on their probability of dropping out during their college career, compared with those students who just avoid it. I do not find evidence of heterogeneous effects either. In particular, my estimates suggest that students who were high-performing in high school do not get more discouraged than low-performing ones if put in remediation, and that male students, usually more over-confident than female ones, do not get more demoralized when placed in remediation.

Until now, the empirical evidence on the impact of college remedial education has mostly been confined to the experience of American community colleges (Bettinger and Long 2009, Boatman and Long 2010, Calcagno and Long 2008, Martorell and McFarlin Jr 2011, Martorell, McFarlin Jr, and Xue 2014, Scott-Clayton and Rodriguez 2015). Overall, none of these studies finds significant and beneficial effects of remediation on drop-out reduction and college performance. To the best of my knowledge, only one paper by De Paola and Scoppa (2014) provides evidence for a different context. This study considers a remedial course organized in the academic year 2009/2010 at the University of Calabria, a southern Italian university. Using a fuzzy RD design, it finds large and positive effects of remedial education on credit accumulation and persistence in college.

My findings contribute to this growing literature in several respects. First of all, the novel data set I collected allows me to provide new insight on the effect of remediation outside American community colleges. It is worth noting that students enrolling in these institutions mainly come from disadvantaged backgrounds and are likely to have been rejected by private universities or to have interrupted their studies after high school and postponed the entrance into tertiary education. The importance of assessing the impact of remedial education for these students is unquestionable. Nonetheless, the absence of positive results for this population might be inconclusive with regard to the effect of this measure in other settings. In light of the different results of De Paola and Scoppa (2014), it appears particularly important to understand whether the success of remedial education depends exclusively on the population that receives it or hinges also on its structure.

Secondly, this paper makes a methodological contribution. Like De Paola and Scoppa (2014), most of the papers on this topic provide fuzzy RD estimates of remedial education attendance: they only consider students who are enrolled in college; next, they compare those who score close to the cut-off for being assigned to remediation in the placement test, and instrument the actual attendance of remedial courses with the assignment to remediation. As pointed out by Scott-Clayton and Rodriguez (2015), the validity of this estimation strategy relies on the assumption that assigning students to remediation has no direct effect on the analyzed outcomes. However, the assignment to remediation might directly generate a discouragement effect and influence students' decisions. For this reason, I provide sharp RD estimates of the direct effect of placing students in remediation.

Third, looking at the effect of assigning students to remediation gives me the possibility to analyze their initial response, the decision to enroll in college. It is particularly important to exclude the presence of a discouragement effect at this stage, in order to determine the optimal timing of this intervention (Stinebrickner and Stinebrickner 2012). Surprisingly the existing literature on college remedial education has not taken into account that the assignment to remediation may also affect this margin of decision, with the notable exceptions of Martorell, McFarlin Jr, and Xue (2014) and Scott-Clayton and Rodriguez (2015).

Finally, my data set allows me to track students over their career and to identify whether, when and how the assignment to remediation triggers a reaction. Analyzing at which point of students' careers the effect of this policy becomes evident and whether it affects both students' performance and their persistence in college is fundamental in order to structure the incentives and punishments associated with this policy (Stinebrickner and Stinebrickner 2014).

Overall, my results provide additional evidence that remedial education is unlikely to have a detrimental effect on students' decisions and performance. However, they also cast doubt on the effectiveness of this policy for undergraduate students not attending community colleges. In particular, my findings suggest that the specific structure of college remediation may play an important role in determining its success (Scott-Clayton, Crosta, and Belfield 2014).

The rest of the paper proceeds as follows. Section 1.2 reviews the literature on the measures proposed to reduce college drop-out and, in particular, on the estimated effects of remedial education. Section 1.3 describes the Italian university system, the remedial policy that I analyze, and introduces the data set. Section 1.4 describes the empirical strategy. Section 1.5 provides evidence of the validity of the RD design in the setting analyzed. Section 1.6 presents the main results, and section 1.7 explores potential heterogeneous effects. Section 1.8 offers a discussion of my findings. Section 1.9 concludes and suggests possible directions for further research.

## 1.2. Related literature

This study contributes to the growing literature that studies measures to enhance college completion. The first economic studies on the topic of higher education focused mostly on the design of efficient policies to boost college enrollment, especially among minority students and those coming from a disadvantaged socio-economic background. In the last decades, despite persistent differences by gender, family income and ethnic origin, enrollment rates in college have risen steadily across all socio-economic groups, at least in OECD countries. In contrast, completion rates have stagnated and time to completion has increased (OECD 2013, Turner 2004). As a consequence, these phenomena have attracted growing interest among economists. The first hypothesis that has been considered is the straightforward idea that a high drop-out rate and long time to completion might result from borrowing constraints. However, most of the papers that have analyzed this explanation (Bettinger 2004, Deming and Dynarski 2009, Dynarski 2008, Stinebrickner and Stinebrickner 2008) conclude that providing only financial support to students cannot ensure college completion. In other words, as stated by Scott-Clayton (2011), "money may well be necessary but insufficient to improve college outcomes". A series of related papers focus on the impact of alternative and cheaper measures, ranging from mentoring services to peer study groups (Angrist, Lang, and Oreopoulos 2009, Bettinger and Baker 2011, Garibaldi, Giavazzi, Ichino, and Rettore 2012). Others explore the effect of combining financial aid with these different forms of support, or simply of linking financial aid to students' performance (Angrist, Lang, and Oreopoulos 2009, Scott-Clayton 2011). As explained by Angrist, Lang, and Oreopoulos (2009), "the results suggest that the skills acquired in response to a combination of services and incentives can have a lasting effect, and that the combination of services and incentives is more promising than either alone".

These first conclusions of the literature and the increasing diffusion of remedial education, especially in American colleges, have induced economists to study how this measure affects college persistence and performance. Among the papers that focus on this type of intervention, Martorell, McFarlin Jr, and Xue (2014), Scott-Clayton and Rodriguez (2015), and De Paola and Scoppa (2014) are the closest to mine. The first two papers have been the only ones so far to look at the direct impact of assigning students to college remediation, with the use of a sharp RD design. None of them finds any significant evidence that this affects students' decision to enroll in college. However, like most of the studies on this topic, both Martorell, McFarlin Jr, and Xue (2014) and Scott-Clayton and Rodriguez (2015) focus on American community college students. As I previously mentioned, it is important to take into account the fact that these students, before enrolling in community colleges, have often applied and been rejected by private colleges. This implies that they might be conscious of their weaknesses, and that, as a consequence, they do not get discouraged if placed in remediation. When, on the contrary, the results of the entrance exam constitute the first signal on students' chances of succeeding in college, the assignment to a remedial course might be more likely to trigger a discouragement effect. In this paper I analyze this possibility.

The setting that I consider closely resembles the one studied by De Paola and Scoppa (2014). However, my paper adds to this both from the methodological point of view and in terms of outcomes analyzed. Moreover, it is worth considering that the two remedial interventions differ in several respects. In De Paola and Scoppa (2014), students assigned to remediation have to attend a remedial course, while in the context I study this is only optional.[4] However, in their setting students are not re-assessed at the end of the remedial course and they face no additional penalization. At the same time, the remedial course organized at the University of Calabria lasts 160 hours, and costs 1,000 euros per student. In contrast, the one offered in the setting I study lasts 21 hours and costs 2,000 euros per year.[5] A priori, it is not clear which combination of incentives and punishments could be more effective in boosting college persistence and performance, and my study sheds more light on this.

## 1.3. Institutional setting and data description

The Italian tertiary education system has been characterized until recently by the predominance of public universities, moderately low and progressive fees, no selection at entrance,[6] a high degree of managerial autonomy for each college,[7] and very low mobility of students. Moreover, since 2001 this system comprises two levels. The first leads to a Bachelor's degree, and should be acquired in three years, by completing a total of 180 credits. The second level consists of two-years Masters, which normally comprise a workload of 120 credits.

In this context, in 2004, the Italian Minister of Education introduced the requirement for all public universities to evaluate students' initial preparation in the core subject of the chosen field of studies, prior to en-

[4] Importantly, even if there are no attendance records, the department administration claims that almost all students assigned to remediation actually attend this course.

[5] For comparison, a standard college-level course typically consists of 60 hours. Moreover, for what concerns the cost of the remedial course, the university administration estimated that it basically corresponds to the salary of the professor in charge of the class.

[6] Apart from specific disciplines such as Medicine or Architecture.

[7] Combined with the fact that public funds are allocated on the basis of the number of enrolled students and the number of graduates.

rollment in a Bachelor program (Ministerial Decree n.270/2004). The rationale behind this initiative was the belief that lack of preparation could be the main cause of a 30 percent drop-out rate at this level. Universities were free to decide how to tackle the possible educational gaps resulting from this evaluation. Some departments chose to introduce remedial courses.[8]

To study the impact of this measure, I personally collected a data set at the economics department of an university located in the north of Italy. In the academic year 2009/2010, this department introduced a selective and specific entrance exam, using a standardized test created by an external institution only for economics, and currently used by another 14 economics departments. This exam consists of three sections, testing respectively math skills, verbal comprehension skills, and logic skills. It serves the double goal of selecting students into the department and assigning some of the admitted students to remedial education. Admission to the department is based on the weighted average of the scores in the three sections, plus the grade in the high school final exam. The assignment to "additional educational duties" (henceforth OFA, the Italian acronym) is instead based only on students' performance in the math section of this exam. Each year the department sets a threshold math score, and students who obtain that score or less are automatically assigned to OFA. The threshold is indicated in the exam instructions, and changes slightly each year. The entrance exam takes place each year at the beginning of

[8] In response to the minister's recommendation, each college, and within colleges, each department, built its own strategy. Over the last decade, most departments have introduced a non-selective entrance test to assess the basic skills of their first-year students in the core subjects of the department. Others took the opportunity to also introduce a limited-enrollment rule, so that the entrance test acquired the double goal of selecting the best students, and assessing their basic knowledge. Concerning those students who perform poorly in the placement test, some departments limited themselves to organizing compulsory remedial courses with no additional check, incentive or penalization scheme for the students who fail to catch up. Others created strong incentive schemes, ranging from not allowing students to sit the regular exam in their weak subject – until they had passed a remedial exam – to re-enrollment in the first year for students who fail to pass the remedial exams.

September. After a few days, students receive their results, separately for each section, and they are informed whether they have been admitted to the department – and whether they have been assigned to remediation. Importantly, all this information is publicly available on the university website. From that moment, students have to decide whether they want to enroll or not, and they have to pay the enrollment fees by December, at the latest. College-level courses start at the end of September and the remedial course takes place in October. It focuses exclusively on math and its aim is to give students the opportunity to acquire the notions necessary to pass the college-level exam. It is followed by a remedial math exam, which is normally scheduled in December or January – always after the enrollment deadline. Then, over the course of the year, students have at least other four possibilities to retake this exam, the last one being in July. The college-level math course takes place during the first year as well. Students can attend the classes, but cannot take the final exam, before passing the remedial one. However, they can take all the other first-year exams. Finally, if students fail to pass any of the remedial math retakes during the first year, they are automatically re-enrolled in the first year the following September. This implies that in the second year, they can attend courses but they cannot take any exams. This penalization ends as soon as they are able to pass the remedial exam.

In the database I collected, I observe all the 2,928 students who participated in the entrance exam over the first four years since its introduction. I have information on their age, sex, nationality, city of residence, type of high school attended, and location of the high school. Moreover, I have the results of the entrance exam, separately for each section. Furthermore, I can track the decision to enroll, for those students who are admitted, the decision to drop out during the course of studies, the grade in the regular math exam, the number of credits accumulated, students' grade point averages, and where relevant their time to complete a degree. For those students who are assigned to remediation, I know their score in each remedial math exam they take, and the date of each exam.

Three features of the data are worth mentioning from the raw analysis of summary statistics: first, table 1.1 shows that 95 percent of students who take the exam are admitted, which implies that the margin of selection is not particularly binding in this department. Secondly, table 1.1 also indicates that a large majority of students, approximately 77 percent of my sample, are assigned to OFA. This figure is very similar to the percentage reported in Scott-Clayton and Rodriguez (2015). It is important to take this into account, when thinking about the plausibility of a stigma effect in this context. Third, table 1.2 makes clear that students who are assigned to OFA are different from the others, in terms of baseline characteristics. The percentage of immigrants is higher among the former – although, in absolute terms, it is quite low; the same is true for the percentage of students performing poorly in high school, and for the proportion coming from a vocational school.

Table 1.3 shows the descriptive statistics for the outcomes studied here. Interestingly, in this context, students in need of remediation are more likely to enroll than those who are not assigned to OFA. My data set allows me to follow those students who take the entrance exam at the department of economics, but then decide to enroll in another department of the same university.[9] The analysis of their choices, reported in the appendix, tables 1.11, 1.12 and 1.13, suggests that this pattern – lower probability of enrollment as the performance in the entrance exam improves – is explained by the fact that students who perform better in this entrance exam are the ones who have more chances of being accepted in other departments. Regarding the drop-out decision, the figures in table 1.3 suggest that students with OFA are more likely to leave the department after one year. Finally, credits accumulated and the probability of passing college-level math are also lower for students placed in remediation compared with those who avoid it. In light of these figures, we might be tempted to conclude that the assignment to the remedial course has been detrimental to students in this context. The next section will describe how to identify its actual impact, at least for those students who are at the margin of being assigned to it.

[9] Unfortunately I cannot track the choices of those students who take the entrance exam and then either enroll in a different university or do not enroll at all in college; in the appendix I label this outcome as an ”unknown choice”.

To conclude the descriptive analysis, table 1.4 focuses on students who are assigned to remediation and enroll in the department. The first row shows that almost ninety percent of these students pass the remedial math exam over the course of the first year. The second row indicates that out of those who passed the remedial exam, ten percent drop out from the department, while out of those who did not pass it, this proportion rises to sixty percent.

## 1.4. Empirical strategy

The goal of this paper is to provide causal estimates of the impact of assigning students to remedial education on their college performance. The simple comparison between the sample means in the outcomes of interest of students in remediation and the others cannot help us to identify this effect, as the two groups are rather different in terms of baseline characteristics. Even when explicitly controlling for these covariates, simple OLS estimates would probably tend to downward bias any positive effect that the assignment to remediation might have. This is because students might also differ in terms of unobserved characteristics, such as self-esteem or aspirations, which may in turn have an influence on the outcomes considered.

However, following Martorell, McFarlin Jr, and Xue (2014) and Scott-Clayton and Rodriguez (2015), the rule used to assign students to remediation can be exploited to identify the effect of interest using a regression discontinuity (RD) design. The intuition is the following. The assignment to remediation is completely determined by the score in the math section of the entrance exam. Clearly, we can expect the performance in this test and students' subsequent performance in college to be somehow related. However, it seems reasonable to assume that this relationship will be smooth. This should also be the case around the score that determines the assignment to remediation. At the same time, the fact that only those students who score below this threshold are assigned to remediation, while those who score above it are not, generates a sharp discontinuity in the treatment as a function of the test score. Therefore, under the assumption that nothing else changes at that threshold, any discontinuity in the relationship between the outcomes and the math score, around the cutoff value, could be interpreted as evidence of a causal effect of assigning students to remediation.

Imbens and Lemieux (2008) formalize this idea using Rubin's potential outcomes framework. In general, when considering the impact of a policy intervention, we can imagine that, for each individual $i$, there exists a pair of "potential" outcomes: $Y_i(1)$ if he is exposed to the treatment, and $Y_i(0)$ if not. The causal effect of receiving the treatment for this individual would be $Y_i(1) - Y_i(0)$. Unfortunately, this difference can never be observed. In the same way, in the RD setting, we can imagine that there are two underlying relationships between the average outcome of interest and the assignment variable $X$ – here, the performance in the math test – represented by $E[Y_i(1)|X]$ and $E[Y_i(0)|X]$. Crucially, in the typical RD setting, all the individuals on one side of a certain cutoff value $c$ of the assignment variable are exposed to the treatment, and all those on the other side are denied it – in the context under study, all students who score below a certain threshold in the math section of the entrance exam are assigned to remediation, while all those who score above it can avoid it. Therefore, we can only observe $E[Y_i(1)|X]$ to the left of the cutoff and $E[Y_i(0)|X]$ to its right. However, this allows to estimate the following expression:

$$lim_{\epsilon \to c^+} E\left[Y_i | X_i = c + \epsilon\right] - lim_{\epsilon \to c-} E\left[Y_i | X_i = c - \epsilon\right]$$

which will identify the average treatment effect at the cutoff $c$, $E[Y_i(1) - Y_i(0)|X = c]$, under the assumption that the underlying functions $E[Y_i(1)|X]$ and $E[Y_i(0)|X]$ are continuous in X, around the cutoff $c$. Basically, this continuity condition allows to use the average outcome of those just above the cutoff (who avoid the treatment) as a valid counterfactual for those just below it (who received the treatment). For this condition to be plausible, we must be willing to assume that "all other factors" determining $Y$ evolve "smoothly" with respect to X. Importantly, this will be the case only if individuals have imprecise control over the assignment variable. Then, even though some would be especially likely to have values of $X$ near the cutoff, everyone will have approximately the same probability of having an $X$ that is just above or just below the cutoff. In other words, in a neighborhood around the threshold, the variation in the treatment will be as good as randomized. And as in a randomized experiment, this implies that the distribution of both the unobservable and observable factors that influence the outcomes of interest should not change discontinuously at the threshold.

In the setting analyzed here, assuming that individuals have imprecise control over the assignment variable means that students should not be able to exactly determine their performance in the exam, and that this should be, at least in part, driven by chance. At the same time, it is also important that no one could manipulate the grades in the grading process (Jacob and Lefgren 2004). In the context under study, the entrance exam is created by an external institution and graded by a computer. Hence, it is hard to think of a way in which students, or professors in the department could have precise control over the math score. Nonetheless, in the next paragraph I am going to provide formal evidence that this is not the case.

To implement the RD design, following Imbens and Lemieux (2008), I will estimate a local linear (LL) regression:

$$\begin{aligned} Y_i &= \alpha + \beta_1 D_i + \beta_2 NormScore_i + \\ &+ \beta_3 D_i NormScore_i + W_i \pi + CohortFE + \varepsilon_i \end{aligned} \tag{1.1}$$

restricting the estimation sample to those students who score in a small neighborhood around the threshold, $c - h \leq X_i \leq c + h$. Here $Y$ represents the outcome of interest. In this setting it will be, alternatively, enrollment in college, drop-out in the first or second year, credits accumulated by the end of the first or second year, or performance in college-level math. $D$ is a binary variable being equal to one at and below the threshold for remediation assignment and 0 otherwise. $NormScore_i$ represents the distance between the score in the math section of the entrance exam and the cutoff that determines the assignment to remediation. $W_i$ is a vector of controls including sex, immigration status, performance in the high-school final exam, an indicator variable for the type of high school attended, and high school province fixed effects. Finally I include cohort fixed effects. In this regression, the main coefficient of interest is $\beta_1$, which measures the discontinuity in the intercept in the relationship between the outcome of interest and the performance in the math test. Under the assumption that none of the actors in this setting could have precise control over the assignment variable, this discontinuity will identify the causal impact of assigning students to remediation, for those students who score close to the cutoff. In what follows, I will report the estimates of $\beta_1$ for three different values of the bandwidth $h$, respectively $(h = 2,\ 1,\ 2.5)$.[10] Following Imbens and Lemieux (2008), to assess the robustness of the RD estimates, in all the tables, I will also show the results from a flexible polynomial regression on the entire sample such as the following:[11]

$$\begin{aligned} Y_i &= \alpha + \beta_1 D_i + \beta_2 NormScore_i + \beta_3 NormScore_i^2 + \\ &+ \beta_4 D_i NormScore_i + \beta_5 D_i NormScore_i^2 + \\ &+ W_i \pi + CohortFE + \varepsilon_i \end{aligned} \tag{1.2}$$

I will now provide some formal evidence that the RD design is a valid estimation strategy in this setting.

## 1.5. The validity of the RD design

Even if there is no direct way to test that individuals do not have precise control over the assignment variable, there are two procedures to

[10] To choose the bandwidths I simply compared students' baseline characteristics on the two sides of the remediation cutoff, rather than implementing optimal bandwidth procedures such as the one proposed by Imbens and Lemieux (2008). The largest bandwidth, $h = 2.5$ corresponds to the value of the math score above which baseline covariates start being significantly different on the two sides of the threshold.

[11] For each outcome considered, I selected the order of the polynomial based on the Akaike information criterion.

indirectly check for the validity of the RD design. The first is to examine whether the distribution of the assignment variable exhibits any discontinuity at the cutoff. As stated by Imbens and Lemieux (2008), in principle, the continuity of the density of $X$ at $c$ is not required, but a discontinuity is suggestive of a violation of the no-manipulation assumption. If in fact students manage to manipulate their math test score in order to be on a specific side of the cutoff, then we should observe an unusual concentration of students scoring just above or below the threshold. Figure 1.1 shows the distribution of this score. As the threshold for placing students in remediation is changed every year, I have normalized the grade so that the 0 corresponds to the cutoff. Each correct answer in the exam gives 1 point, while wrong answers are penalized by 0.25, and unanswered questions are not penalized. The normalized distribution looks left-skewed, with a mean of -2.78, indicating that students are performing quite poorly on average. There is, however, a lot of variation in students' performance, with a standard deviation of 3.65. More importantly for the RD design, it does not seem that a disproportionate fraction of students is concentrated just below the threshold, which would suggest that many of them are acting in order to be assigned to remediation. At the same time, there is no sign that students are answering just enough questions to avoid remediation, which would result in a discontinuity in the distribution above the cutoff. Therefore, this graphical analysis suggests that students do not have precise control over the assignment variable. A McCrary test (McCrary 2008) supports this conclusion, as the null hypothesis of no jumps in the distribution of the math score, at the cut-off, cannot be rejected.[12]

A second way to test for the validity of the RD design is to examine whether students' baseline observable characteristics, which might influence the outcomes of interest, exhibit any discontinuity at the cutoff in their relationship with the assignment variable. Baseline covariates such as the high-school final grade, sex or immigration status are, by definition, determined prior to the assignment to remediation. Hence, there should be no reason to expect a jump at the threshold in their relationship with the

[12] In detail, the point estimate of the difference between the frequency to the right and to the left of the threshold is 0.01 with a s.e. equal to 0.0075.

assignment variable. Again, any evidence of such discontinuities would suggest that students are in some way able to manipulate their performance in the entrance exam. As we can see from the graphical analysis, figure 1.2, there is no visible jump in the relationship between baseline covariates and the assignment variable at $c$. Table 1.5, showing all the estimated discontinuities, confirms this intuition, as no discontinuity is significant.

Therefore, in the context of analysis, the RD design appears a valid estimation strategy to identify the effect of assigning students to college remediation on their performance in college. The next session will discuss the results of this estimation procedure.

## 1.6. Results

The data set I collected allows me to study how students react to the assignment to remediation with respect to different decisions. In this section I illustrate and discuss the estimated impact on each of them.

*The decision to enroll.* One of the main contributions of my study is that I am able to analyze how undergraduate students immediately react to the fact of being put in remediation. I model this decision as a binary variable equal to 1 if a student enrolls in the department, once admitted to it, and 0 otherwise. Figure 1.3 plots it as a function of the score in the math section of the entrance exam. In detail, each dot represents the probability of enrolling averaged across all students obtaining a certain math grade, and the lines are linear fits of the dots, estimated separately on each side of the threshold. The graph shows two main facts: first, as the summary statistics also suggest, the probability of enrolling appears to decline as the grade in the math exam increases. Secondly, there does not seem to be a discontinuity in the probability of enrolling at the threshold, which suggests that assignment to remediation does not trigger an immediate discouragement effect. Table 1.6 shows the point estimates of the effect of being placed in remediation on the probability of enrolling in the department. The table confirms what the graphical analysis suggests:

in all the specifications I cannot reject the null that, on the margin for remediation, assigning a student to a remedial course does not affect his decision to enroll as compared with a student who scores just above the threshold.

*The decision to drop out after the first year.* In principle we would like a student to react constructively to the fact of being placed in remediation. He should consider it a signal that he has to work hard during the first year to succeed in his college studies, and that the assignment to remediation could be beneficial for him. If this is the case, we should expect him to have a lower probability of quitting the university than a student who avoids remediation. However, especially for a student at the margin for needing remediation, assignment to remediation might discourage him over the course of the first year; in the context of study, if a student does not pass the remedial exam in the first year, he cannot enroll in the second year; at the same time, he can sit up to five retakes. Nonetheless, the strong penalization associated with a failure might induce him to focus exclusively on the remedial exam, with the consequence of lagging behind in the other courses at the end of the first year. This can lead to the undesired result of increasing his probability of dropping out. The top-left panel of figure 1.4 plots the likelihood to drop out after one year as a function of the performance in the math section of the entrance exam. As expected, students who perform worse in the exam are more likely to quit the university. However, this relationship does not exhibit any discontinuity at the threshold for remediation assignment. The point estimates in the first row of table 1.7 suggest the presence of a discouragement effect for students scoring just below the threshold for remediation compared with those just avoiding it, but they are not significant in any of the specifications.[13]

[13] Importantly, I analyze all the outcomes for the entire sample of students who took the entrance exam. This means that drop-out is equal to 1 both for a student who attends the first year and then abandons the department, and for one who decided not to enroll at the very beginning of the year. I also assign to this student 0 credits, when looking at credit accumulation. For completeness, in the appendix, I also report the estimates of the effect of assigning students to remediation on post-entry outcomes, conditional on enrollment. The point estimates in table 1.14, still insignificant, are slightly weaker and

*Credits gained by the end of the first year*. The results discussed until now seem to exclude the possibility that assignment to remediation discourages students who barely need it. However, opponents of remedial policies argue that they can still be detrimental if they drain students' resources away from college-level courses. In particular, if students decide to focus on the remedial course before studying for college-level ones, they might end up gaining less credits during the first year. Figure 1.4 displays the credits accumulated in the first year as a function of the math grade in the entrance exam. The overall pattern indicates that weaker students take less credits over the course of the first year. However, assigning students to remediation does not seem to worsen this trend. The estimated jump in this relationship, second row of table 1.7, is negative, suggesting that students who are just assigned to remediation gain 1 to 4 credits less than students who just avoid it. This corresponds to a 10 to 20 percent decrease in credit accumulation with respect to the sample mean. Nonetheless, the estimated discontinuity is not significant in any of the specifications.

*Performance in the subject of the remedial course*. The main goal of remediation is to help students to recover some basic notions in the subject of the remedial course, to enable them to pass the college-level exam. Even if a student decides not to attend the remedial course, the mere assignment to it should induce him to work harder in the subject of remediation. Hence, assignment to remediation could have a positive effect on the probability of passing the college-level exam. The bottom-right panel of figure 1.4 suggests that this is not the case in the studied sample: the relationship between the probability of passing college-level math and the grade in math at the entrance exam exhibits no discontinuity at the threshold for remediation assignment. In table 1.7, the point estimates of the effect of assignment to remedial math on the probability of passing the regular math for students who are at the margin for remediation are unstable across the different specifications, and not significant in any of them.

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more unstable across specifications compared with the ones obtained for the sample of admitted students; however, they go in the same direction.

*Performance in the second year*. The beneficial effects of assigning a student to remediation, if any, might appear only in the second year, when he has supposedly recovered from his initial difficulties. This could be even more the case in a context where students placed in remediation do not seem to get discouraged or to lag behind their peers in the first year. If this is the case, we should observe that assignment to remediation has a positive effect on credits accumulated in the second year and a negative effect on second year drop-out. The graphical analysis in figure 1.4 goes against these conjectures. The graphs of second-year credits and second-year drop-out as a function of the math score in the entrance exam show no visible discontinuity at the threshold for assignment to remediation. Point estimates in table 1.7 suggest that assigning students to remediation increases the gap between them and those who avoid it over time, but no specification delivers significant results.

## 1.7. Subgroup analysis

Finding no significant results for the entire sample can mask heterogeneous and opposite effects for specific subgroups. On the one hand, assignment to remediation might be beneficial for students coming from a vocational school, or for those with a low performance in high school who may have low expectations about their chances of succeeding in college; on the other hand, it could demoralize high-performing students and students coming from the general track if they have higher priors about their ability to complete university. Moreover, several behavioral studies (Buser, Niederle, and Oosterbeek 2014, Niederle and Vesterlund 2010, Niederle and Yestrumskas 2008) suggest that men tend to be more overconfident than women; hence, male students might react worse than female students to being placed in remediation. The data set under analysis allows to test these predictions. Figure 1.5 shows the likelihood to enroll as a function of the math grade in the entrance exam for the following subgroups: male and female students, students at different intervals in the distribution of high-school final grades, and students coming from either a vocational high school or the general track. No visual discontinuity can be detected from the graphical analysis. Tables 1.8 and 1.9 present the estimated jumps in the enrollment decision, for all the subgroups. None of the estimates is significant, with the exception of those for males. However, these are not robust across different specifications. Moreover, they are not statistically different from the ones for females. Finally, it is important to bear in mind that testing for multiple subgroups corresponds to multiple hypothesis testing, and therefore it increases the probability of committing the TYPE I error. In detail, when testing for $n$ different interaction terms, the probability of getting $k$ significant p-values with zero true effects is given by:

$$p\left(k,n\right)=\binom{n}{k}\alpha^{k}\left(1-\alpha\right)^{n-k}$$

In order to take this into account, I also consider more conservative significance levels, such as those derived from the Bonferroni or the Benjamini-Hochberg correction procedures.[14] In this context the two procedures lead to the same conclusion. Using either of the two methods makes me unable to reject the null of no discontinuity in the enrollment decision at the threshold, as well as in the other outcomes, for all the subgroups.[15]

[14] The Bonferroni correction procedure prescribes that, when testing for $n$ hypothesis, and considering a significance level $\pi$, only hypotheses with associated p-values $\leq \dfrac{\pi}{n}$ should be rejected. The Benjamini-Hochberg method, instead, works as follows: given a set of hypotheses $H_1, H_2, ..., H_m$, let $p_1, p_2, ..., p_m$ be the corresponding p-values, and let $H_i$ denote the null hypothesis corresponding to $p_i$. Order the p-values $p_1, p_2, ..., p_m$ and let $k$ be the largest $i$ for which $p_i \leq \frac{i}{m}\alpha$; then reject all $H_i$ with $i = 1, 2, ...k$. The two procedures are similar but in general the Benjamini-Hochberg one has the advantage of minimizing the probability of committing the TYPE II error.

[15] The results for the other outcomes are not shown here but are available upon request.

## 1.8. Discussion

What conclusions can be drawn from these results? Is remedial education always ineffective at reducing college drop-out and improving students' performance? To answer these questions I start by making two considerations. First, the RD design, in general, delivers local estimates. In this setting, this means that my results might not be extended to those students who perform poorly in the placement test, presumably those who could most benefit from remediation. Secondly, to assess the external validity of my results, it is fundamental to bear in mind how Italy compares to other OECD countries in terms of the main outcome of interest; and it is crucial to understand to what extent the sample under study could be representative of the entire population of Italian undergraduate students. Figure 1.6 shows the 2011 average college completion rates respectively across 18 OECD countries, all Italian undergraduate programs, and for the students who enrolled in the economics department under study. Italy's completion rates are in line with the OECD average. Students enrolled in the economics department under study do just slightly better. Next, table 1.10 shows the characteristics of the sample of students I observe and those of the overall population of Italian undergraduate students. In the setting under study – and in economics departments in general – females are under-represented compared with other departments. However, the ability composition of the two populations is remarkably similar.[16] In light of these figures, it appears that my study will speak in particular for a male-dominated environment. If we consider that college completion rates are in general lower precisely for this type of students (OECD 2013, Turner 2004), my results can be particularly useful for policy makers. Moreover, the fact that the ability distribution in my sample reflects that of the overall Italian population of undergraduate students suggests

[16] Ability here is measured in terms of the performance at the high school final exam. The grade in this exam goes from 60 to 100. I then define as low-ability students those who score below 70, as medium-ability those who score between 70 and 89, and as high-ability those who score more than this. Importantly, the grades in the general track are comparable to those of the vocational high schools.

that my conclusions can be informative for other contexts.[17] Nonetheless, all these elements have to be considered if we want to extract policy recommendations from these results.

With respect to the previous literature, my study definitely casts doubt on the effectiveness of remedial education for undergraduate students not attending community colleges. The studies that analyze the impact of this measure in community colleges (Bettinger and Long 2009, Boatman and Long 2010, Calcagno and Long 2008, Martorell and McFarlin Jr 2011, Martorell, McFarlin Jr, and Xue 2014, Scott-Clayton and Rodriguez 2015) conclude that it does not improve the chances of succeeding in college in this specific context. On the contrary, the analysis conducted by De Paola and Scoppa (2014) on Italian undergraduate students suggests that, in an different setting, college remediation can prove to be effective in reducing drop-out and improving their performance. The population considered in my study is very similar, in terms of baseline characteristics, to the one examined by De Paola and Scoppa (2014). However, the remedial policies implemented in the two settings differ substantially, in terms of both their costs and the incentives and penalization schemes associated with the assignment to remediation. As a consequence, any comparison between these two studies has to take all these differences into account. However, precisely because of these differences, my results suggest that further research is needed to identify which component of a remedial policy determines its effectiveness. Any additional evidence is valuable in this respect.

## 1.9. Conclusion

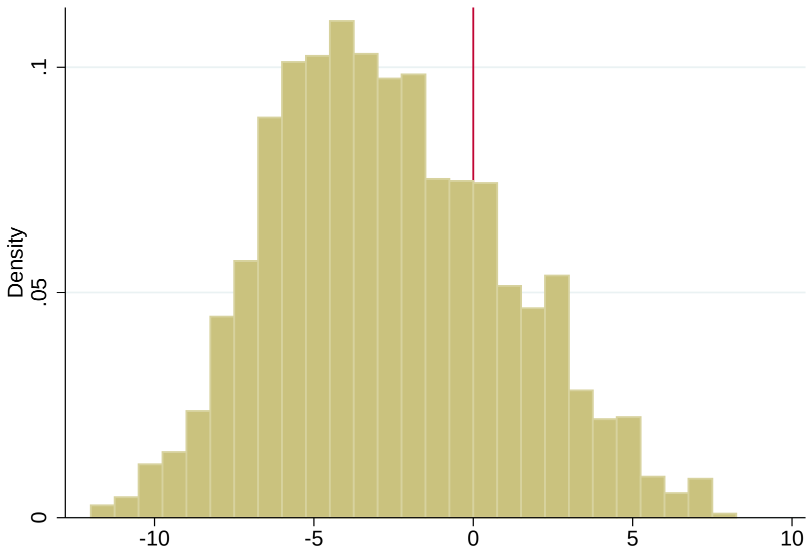
Policy makers and universities show a growing interest in college remedial education as a measure to reduce the severe problem of college drop-out. Identifying which aspects of remedial policies are more effective and for which types of students they could be more useful is of

[17] Here I am definitely ignoring the potential peer effects that could be generated by the sample gender composition.

primary importance. This study makes several contributions in this respect. First, it makes use of a novel and rich data set of Italian undergraduate students. This allows us to improve our understanding of how college students react to the provision of information on their ability and performance-enhancing incentives delivered by the assignment to remediation. Secondly, following Scott-Clayton and Rodriguez (2015), I make use of a sharp regression discontinuity design to estimate the effect of the assignment to remediation, rather than remediation attendance, on students' decisions and performance. This allows me to estimate whether the assignment to remediation immediately discourages students from enrolling in the chosen department. Third, my data set enables me to track students over their career and to identify whether, when, and how they react to the fact of being put in remediation. Finally, information on students' baseline characteristics allows me to detect heterogeneous effects. I find no significant evidence that assigning students to remediation affects their decision to enroll. The estimated effects on post-entry outcomes suggest the presence of a discouragement effect, but none of them is significant. An accurate subgroup analysis indicates that there is no evidence of heterogeneous effects. These results, combined with the ones produced by the previous literature, open several avenues for future research. Should colleges devote more time and resources to remediation? Or should they focus on identifying which students should be assigned to these courses, as suggested by Scott-Clayton, Crosta, and Belfield (2014)? And do the incentives and sanctions that follow the assignment to remediation affect its effectiveness? Exploiting the variation in the structure of remedial policies offered by the other Italian departments of economics that use the same placement test might prove extremely useful to clarify these points.

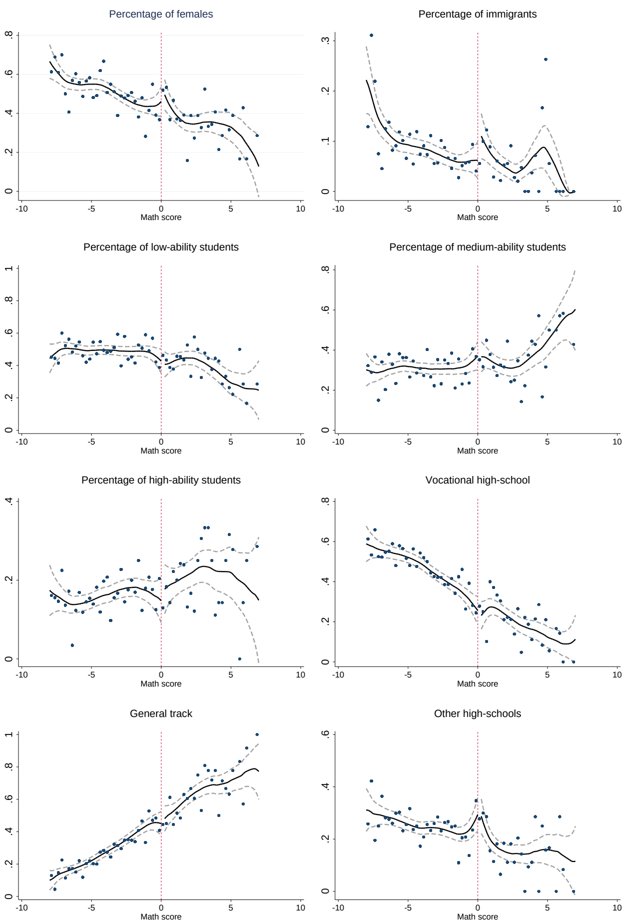
## 1.10. Tables and Figures

Figure 1.1: **Distribution of the assignment variable, relative to the cutoff**



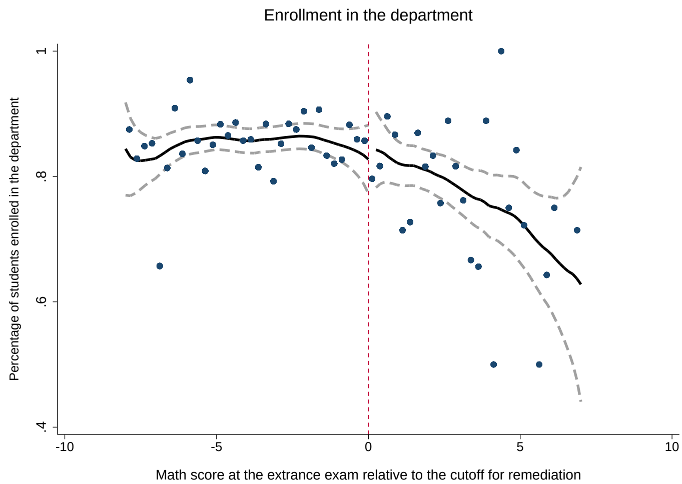

Source: author's elaboration of university administrative data.
Note: the figure reports the histogram of the math score in the entrance exam, which represents the assignment variable in this setting. The distribution is normalized so that 0 corresponds to the cutoff below which students are assigned to remediation. To construct this histogram, I considered the sample of all 2,927 students taking the entrance exam, over the years 2009-2012.

Figure 1.2: **Baseline characteristics as a function of the assignment variable**



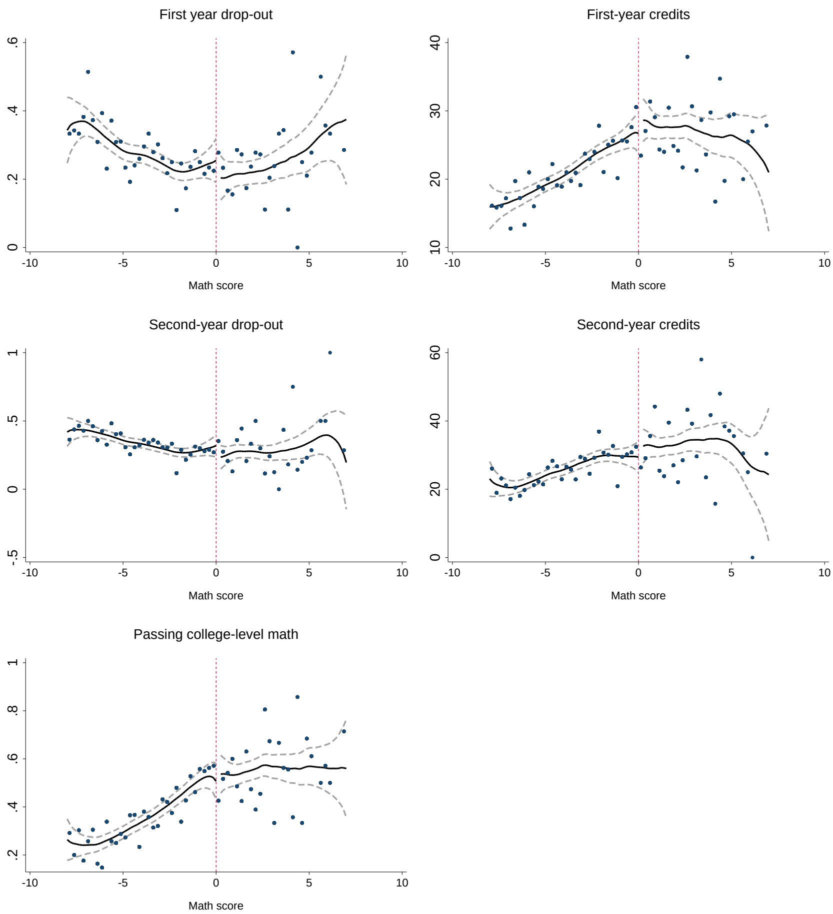

Source: author's elaboration of university administrative data.
Note: in each graph, the dots represent averages of the outcome variables computed for each value of the assignment variable (math score). The lines correspond to linear fits of the dots, computed separately on each side of the cutoff for remediation assignment. To construct these graphs, outliers on each side of the remediation cut-off are excluded. As a result, the sample is restricted to those students admitted to the department – over the years 2009-2012 – who scored within an interval around the remediation cut-off of width 8. No discontinuity could be detected even when outliers are included.

Figure 1.3: **Enrollment decision as a function of the assignment variable**



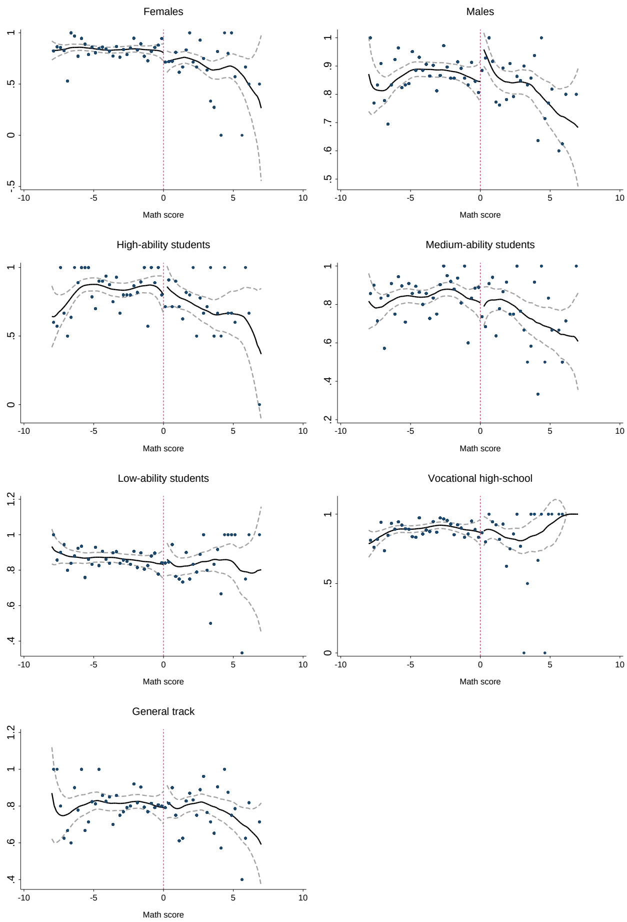

Source: author's elaboration of university administrative data.
Note: the dots represent averages of the outcome variable, enrollment, computed for each value of the assignment variable (math score). The lines correspond to linear fits of the dots, computed separately on each side of the cutoff for remediation assignment. To construct these graphs, outliers on each side of the remediation cut-off are excluded. As a result, the sample is restricted to those students admitted to the department – over the years 2009-2012 – who scored within an interval around the remediation cut-off of width 8. No discontinuity could be detected even when outliers are included.

Figure 1.4: **Post-entry outcomes as a function of the assignment variable**



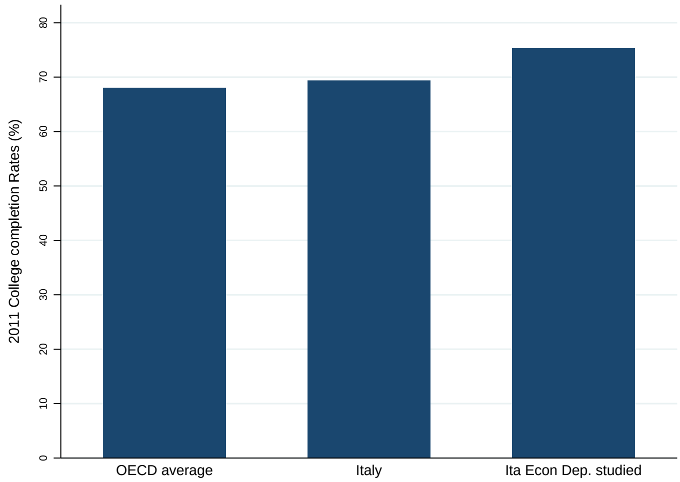

Source: author's elaboration of university administrative data.
Note: the dots represent averages of the outcome variables, computed for each value of the assignment variable (math score). The lines correspond to linear fits of the dots, computed separately on each side of the cutoff for remediation assignment. To construct these graphs, outliers on each side of the remediation cut-off are excluded. As a result, the sample is restricted to those students admitted to the department – over the years 2009-2012 – who scored within an interval around the remediation cut-off of width 8. No discontinuity could be detected even when outliers are included.

Figure 1.5: **Enrollment decision, for each subgroup, as a function of the assignment variable**




Source: author's elaboration of university administrative data.
Note: each graph represents the enrollment decision as a function of the assignment variable, for each subgroup considered. The dots represent averages of the dummy variable "enrollment", computed for each value of the assignment variable (math score). The lines correspond to linear fits of the dots, computed separately on each side of the cut-off for remediation assignment. To construct these graphs, outliers on each side of the remediation cut-off are excluded. As a result, the sample is restricted to those students admitted to the department – over the years 2009-2012 – who scored within an interval around the remediation cut-off of width 8.

Figure 1.6: **Proportion of students who enter tertiary education and graduate with a first degree in 2011**




Source: OECD Education at Glance, 2013, and Italian Ministry of Education website.
Note: the data for the OECD average completion rates were collected through a special survey undertaken in 2012 by the OECD. For half of the countries, completion rates are constructed using a true cohort method, and represent the proportion of graduates (within N years) among a given entry cohort. The completion rates for the other countries are calculated from cross cohort methods as the ratio of the number of students who graduate with an initial degree during the reference year to the number of new entrants in this degree n years before, n being the number of years of full-time study required to complete the degree. I follow this method to construct the figures for Italy, using the data provided by the Ministry of Education. The 2011 completion rates then refer to ratio of the number of students who got an undergraduate degree in the academic year 2010/2011 to the number of students who enrolled in an undergraduate program in 2008 – as it should take 3 years to complete such a Bachelor's degree.

Table 1.1: **Students admitted to the department of Economics studied**

| | Admitted |
|---|---|
| Over participants (%) | 0.95 |
| Placed in remediation (%) | 0.77 |
| Observations | 2,785 |

Source: author's elaboration of university administrative data.
Note: In the first row, the table reports the total number of students admitted to the economics department studied between 2009/10 and 2012/13; in the second row, the proportion of students admitted among those who took the entrance exam; and in the third row, the percentage of students who were assigned to remediation, among the admitted students.

Table 1.2: **Baseline characteristics of the estimation sample**

| | Admitted | In remediation | Not in remediation | Difference |
|---|---|---|---|---|
| Female | 0.48 | 0.52 | 0.36 | 0.15*** |
| Immigrant | 0.08 | 0.08 | 0.06 | 0.02** |
| Low high-school grade | 0.46 | 0.47 | 0.40 | 0.07*** |
| Medium high-school grade | 0.33 | 0.32 | 0.36 | -0.04* |
| High high-school grade | 0.18 | 0.17 | 0.21 | -0.04** |
| Vocational track | 0.41 | 0.47 | 0.22 | 0.26*** |
| General track | 0.36 | 0.28 | 0.62 | -0.34*** |
| Other high schools | 0.23 | 0.24 | 0.17 | 0.08*** |
| Observations | 2,785 | 2,132 | 653 | |

Source: author's elaboration of university administrative data.
Note: the table reports the percentage of students belonging to a specific subgroup (indicated in the first column) with respect to the entire group of admitted students (column 2), the group of students assigned to remediation (column 3), and the group of students that avoided remediation (column 4). The last column reports the difference between the sample means in column 3 and 4, and indicates whether it is statistically significant or not.

Table 1.3: **Sample means of the outcomes of interest**

| | Admitted | In remediation | Not in remediation | Difference |
|---|---|---|---|---|
| Enrollment | 0.84 | 0.85 | 0.79 | 0.06*** |
| 1st year drop-out | 0.27 | 0.28 | 0.24 | 0.05** |
| 2nd year drop-out | 0.34 | 0.35 | 0.28 | 0.07*** |
| 1st year credits | 22 | 21 | 27 | -7*** |
| 2nd year credits | 26 | 25 | 33 | -8*** |
| Passing college-level math | 0.40 | 0.35 | 0.55 | -0.20*** |
| College-level math grade | 22 | 22 | 24 | -2*** |
| Observations | 2,785 | 2,132 | 653 | |

Source: author's elaboration of university administrative data.
Note: the table reports the mean of the outcomes considered (indicated in the first column) for the entire group of admitted students (column 2), for the group of students assigned to remediation (column 3), and for the group of students that avoided remediation (column 4). The last column reports the difference between the sample means in column 3 and 4, and indicates whether it is statistically significant or not.

Table 1.4: **The remedial math exam**

| | Passed | Not passed |
|---|---|---|
| Over enrolled (%) | 0.88 | 0.12 |
| Dropping out (%) | 0.10 | 0.60 |

Source: author's elaboration of university administrative data.
Note: in the first row the table reports the percentage of students that passed/did not pass the remedial math exam in the first year, among those who were assigned to remediation and enrolled in the department. The second row reports the percentage of students that dropped out among those who passed the remedial exam, first cell, and among those who did not pass it, second cell.

Table 1.5: **Estimated discontinuities in baseline characteristics**

| | Local linear (±2) | Local linear (±1) | Local linear (±2.5) | Polynomial |
|---|---|---|---|---|
| Female | -0.08 | -0.09 | -0.06 | -0.01 |
| | (0.07) | (0.10) | (0.06) | (0.04) |
| Immigrant | -0.05 | -0.07 | -0.04 | -0.02 |
| | (0.04) | (0.05) | (0.03) | (0.02) |
| Low high-school grade | 0.06 | 0.01 | 0.10 | 0.05 |
| | (0.07) | (0.10) | (0.06) | (0.06) |
| Medium high-school grade | -0.02 | 0.02 | -0.06 | -0.07 |
| | (0.07) | (0.09) | (0.06) | (0.05) |
| High high-school grade | -0.04 | -0.02 | -0.03 | 0.01 |
| | (0.05) | (0.08) | (0.05) | (0.04) |
| Vocational high-school | 0.02 | 0.13 | -0.01 | 0.00 |
| | (0.06) | (0.09) | (0.05) | (0.04) |
| General track | 0.02 | -0.07 | 0.04 | -0.02 |
| | (0.07) | (0.10) | (0.06) | (0.04) |
| Other high-schools | -0.04 | -0.07 | -0.03 | 0.02 |
| | (0.06) | (0.09) | (0.05) | (0.03) |
| Observations | 844 | 458 | 1,042 | 2,662 |

Source: author's elaboration of university administrative data.
Note: the first column indicates the dependent variable. Estimation methods: local linear regression (LL) in column 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students admitted to the economics department under study, over the years 2009-2012. In columns 2, 3, and 4, the sample is restricted to the students whose score in the math placement test lies within an interval, around the remediation cut-off, of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.6: **Estimated discontinuities in the decision to enroll**

| | Local linear ($\pm 2$) | Local linear ($\pm 1$) | Local linear ($\pm 2.5$) | Polynomial |
|---|---|---|---|---|
| No controls | -0.01 | -0.06 | -0.00 | -0.01 |
| | (0.05) | (0.08) | (0.05) | (0.04) |
| Cov.+Cohort FE | -0.01 | -0.07 | 0.00 | -0.01 |
| | (0.05) | (0.08) | (0.05) | (0.04) |
| Observations | 844 | 458 | 1,042 | 2,662 |
| Sample mean | 0.82 | 0.83 | 0.82 | 0.79 |

Source: author's elaboration of university administrative data.
Note: in the second row I control for the high-school final grade, type of high-school attended, gender and immigrant status; cohort fixed-effects are also included. The sample mean refers to the group of students whose score in the math placement test lies, respectively 2, 1 and 2.5 above the cutoff. Estimation methods: local linear regression (LL) in columns 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students admitted to the economics department under study, over the years 2009-2012. In columns 2, 3, and 4, the sample is restricted to the students who scored within an interval around the remediation cut-off of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.7: **Estimated discontinuities in post-entry outcomes**

| | Local linear (±2) | Local linear (±1) | Local linear (±2.5) | Polynomial |
|---|---|---|---|---|
| 1st year drop-out | 0.05 | 0.05 | 0.03 | 0.02 |
| | (0.06) | (0.08) | (0.05) | (0.05) |
| Sample mean | 0.22 | 0.21 | 0.21 | 0.24 |
| 1st year credits | -2.29 | -3.79 | -0.67 | -0.31 |
| | (2.61) | (3.94) | (2.35) | (2.21) |
| Sample mean | 27.44 | 28.13 | 27.92 | 27.20 |
| Passing math | -0.03 | -0.06 | 0.03 | 0.03 |
| | (0.07) | (0.10) | (0.06) | (0.05) |
| Sample mean | 0.52 | 0.54 | 0.54 | 0.55 |
| Observations | 844 | 458 | 1,042 | 2,662 |
| 2nd year drop-out | 0.13* | 0.11 | 0.05 | 0.03 |
| | (0.08) | (0.11) | (0.07) | (0.06) |
| Sample mean | 0.29 | 0.25 | 0.27 | 0.28 |
| 2nd year credits | -5.62 | -5.62 | -2.28 | 1.32 |
| | (4.23) | (6.14) | (3.79) | (3.52) |
| Sample mean | 31.59 | 32.90 | 32.56 | 32.84 |
| Observations | 584 | 316 | 723 | 1,960 |

Source: author's elaboration of university administrative data.
Note: the first column indicates the dependent variable. In all regressions I control for the high-school final grade, type of high-school attended, gender and immigrant status; cohort fixed-effects are also included. The sample mean refers to the group of students whose score in the math placement test lies, respectively 2, 1 and 2.5 above the cutoff. Estimation methods: local linear regression (LL) in columns 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students admitted to the economics department under study, over the years 2009-2012. In columns 2, 3, and 4, the sample is restricted to the students who scored within an interval around the remediation cut-off of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.8: **Estimated discontinuities in the decision to enroll, by subgroup (I)**

| | Local linear ($\pm 2$) | Local linear ($\pm 1$) | Local linear ($\pm 2.5$) | Polynomial |
|---|---|---|---|---|
| Female | 0.12 | 0.05 | 0.11 | 0.09 |
| | (0.09) | (0.13) | (0.08) | (0.08) |
| Observations | 352 | 205 | 440 | 1,265 |
| Sample mean | 0.75 | 0.73 | 0.75 | 0.71 |
| Male | -0.12* | -0.19** | -0.09 | -0.08 |
| | (0.06) | (0.08) | (0.05) | (0.05) |
| Observations | 492 | 253 | 602 | 1,397 |
| Sample mean | 0.87 | 0.91 | 0.87 | 0.84 |
| Vocational hs | -0.05 | 0.07 | -0.03 | -0.06 |
| | (0.10) | (0.15) | (0.08) | (0.08) |
| Observations | 269 | 131 | 331 | 1,072 |
| Sample mean | 0.86 | 0.90 | 0.86 | 0.86 |
| General track | -0.01 | -0.16 | 0.00 | 0.01 |
| | (0.08) | (0.12) | (0.07) | (0.07) |
| Observations | 401 | 221 | 493 | 982 |
| Sample mean | 0.79 | 0.79 | 0.79 | 0.79 |

Source: author's elaboration of university administrative data.

Note: the first column indicates the subgroup considered. The sample mean refers to the group of students – belonging to the subgroup considered – whose score in the math placement test lies, respectively 2, 1 and 2.5 above the cutoff. Estimation methods: local linear regression (LL) in columns 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students admitted to the economics department under study, over the years 2009-2012, and belonging to the subgroup considered. In columns 2, 3, and 4, the sample is restricted to the students who scored within an interval around the remediation cut-off of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 1.9: **Estimated discontinuities in the decision to enroll, by subgroup (II)**

| | Local linear (±2) | Local linear (±1) | Local linear (±2.5) | Polynomial |
|---|---|---|---|---|
| High-ability | 0.01 | -0.17 | 0.01 | -0.07 |
| | (0.12) | (0.18) | (0.11) | (0.10) |
| Observations | 153 | 80 | 191 | 477 |
| Sample mean | 0.81 | 0.83 | 0.81 | 0.73 |
| Low-ability | -0.02 | -0.11 | -0.00 | 0.02 |
| | (0.08) | (0.11) | (0.07) | (0.07) |
| Observations | 389 | 203 | 482 | 1,206 |
| Sample mean | 0.82 | 0.83 | 0.82 | 0.83 |
| Medium-ability | 0.00 | 0.07 | 0.00 | -0.02 |
| | (0.09) | (0.16) | (0.08) | (0.08) |
| Observations | 278 | 158 | 340 | 882 |
| Sample mean | 0.82 | 0.81 | 0.82 | 0.77 |

Source: author's elaboration of university administrative data.
Note: the first column indicates the subgroup considered. The sample mean refers to the group of students – belonging to the subgroup considered – whose score in the math placement test lies, respectively 2, 1 and 2.5 above the cutoff. Estimation methods: local linear regression (LL) in columns 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students admitted to the economics department under study, over the years 2009-2012, and belonging to the subgroup considered. In columns 2, 3, and 4, the sample is restricted to the students who scored within an interval around the remediation cut-off of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 1.10: **Characteristics of Italian undergraduate students**

| | All Italian departments | Economics department studied |
|---|---|---|
| Female | 54.86 | 46.67 |
| Low high-school grade | 26.13 | 25.10 |
| Medium high-school grade | 51.84 | 53.80 |
| High high-school grade | 17.85 | 17.02 |

Source: author's elaboration of university administrative data.
Note: the table reports the percentage of students belonging to a specific subgroup, indicated in the first column, over the population of enrolled students in all Italian undergraduate programs (column 2), or over the enrolled students in the undergraduate program of the economics deparment studied (column 3).

## 1.11. Appendix

Table 1.11: **Students enrolled versus those who drop out at entry**

| | Enrolled | Not enrolled | Difference |
|---|---|---|---|
| Female | 0.47 | 0.55 | -0.08*** |
| Low high-school grade | 0.47 | 0.41 | 0.06** |
| High high-school grade | 0.17 | 0.21 | -0.04** |
| General track | 0.34 | 0.44 | -0.09*** |
| Math points | -2.66 | -2.23 | -0.43** |
| Over admitted | 0.84 | 0.16 | |

Source: author's elaboration of university administrative data.
Note: the table reports the characteristics of students enrolling in the department (column 2) and of those who drop out after the entrance exam (column 3). The last column reports the difference between the sample means in column 2 and 3, and indicates whether it is statistically significant or not. The variable math points refers to average score - relative to the cutoff - obtained in the math section of the entrance exam, by students in each of the two groups.

Table 1.12: **Students not enrolled in the department studied**

| | Other department | Unknown choice | Difference Difference |
|---|---|---|---|
| Female | 0.53 | 0.56 | 0.09*** |
| Low high-school grade | 0.43 | 0.39 | -0.07** |
| High high-school grade | 0.19 | 0.22 | 0.05** |
| General track | 0.49 | 0.40 | 0.05 |
| Math points | -1.76 | -2.57 | 0.02 |
| Over admitted and not enrolled | 0.42 | 0.58 | |

Source: author's elaboration of university administrative data.

Note: the table reports the characteristics of students who enroll in the same university but in a different department (column 2), and of those who either enrolled in a different university or decided not to enroll in college (column 3). The last column reports the difference between the sample means in column 2 and 3, and indicates whether it is statistically significant or not. The variable math points refers to average score - relative to the cutoff - obtained in the math section of the entrance exam, by students in each of the two groups.

Table 1.13: **The decision to enroll**

| | Other department | Unknown choice |
|---|---|---|
| Math points | 1.074** | -1.019 |
| | (.031) | (.025) |
| Female | 1.52** | 1.415** |
| | (.259) | (.208) |
| Immigrant | 0.405** | 0.211*** |
| | (.171) | (.097) |
| Low high-school grade | 0.902 | 0.741* |
| | ( .159) | (.114) |
| General track | 1.589** | 1.262 |
| | (.294) | (.199) |
| Observations | 2,662 | 2,662 |

Source: author's elaboration of university administrative data. Note: the table reports the estimation results of a multinomial logit regression where the outcome is a categorical variable equal to 1 if a student enrolls in the economics department, to 2 if he enrolls in a different department of the same university, and to 3 if he either enrolls in a different university or decides not to enroll in college at all. The first category is treated as baseline. Each column shows the relative risk-ratios of the other two possible outcomes for unitary changes in the regressors listed by row. In each regression I also control for the high-school final grade, the type of high-school attended, and cohort fixed-effects. Robust standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.14: **Estimated discontinuities in post-entry outcomes, conditional on enrollment**

| | Local linear ($\pm 2$) | Local linear ($\pm 1$) | Local linear ($\pm 2.5$) | Polynomial |
|---|---|---|---|---|
| 1st year drop-out | 0.06 | -0.00 | 0.05 | 0.03 |
| | (0.04) | (0.05) | (0.03) | (0.03) |
| Sample mean | 0.05 | 0.04 | 0.04 | 0.03 |
| 1st year credits | -1.85 | -1.60 | -0.90 | -0.01 |
| | (2.58) | (3.80) | (2.27) | (2.16) |
| Sample mean | 33.32 | 33.90 | 33.88 | 34.36 |
| Passing math | 0.01 | -0.02 | 0.06 | 0.06 |
| | (0.07) | (0.11) | (0.07) | (0.06) |
| Sample mean | 0.64 | 0.65 | 0.66 | 0.70 |
| Observations | 709 | 384 | 881 | 2,235 |
| 2nd year drop-out | 0.08 | 0.02 | 0.03 | 0.01 |
| | (0.07) | (0.10) | (0.06) | (0.05) |
| Sample mean | 0.13 | 0.12 | 0.11 | 0.10 |
| 2nd year credits | -3.53 | -0.41 | -2.02 | 2.92 |
| | (4.38) | (6.28) | (3.80) | (3.57) |
| Sample mean | 38.67 | 38.49 | 39.78 | 41.03 |
| Observations | 491 | 268 | 613 | 1,658 |

Source: author's elaboration of university administrative data.
Note: the first column indicates the dependent variable. In all regressions I control for the high-school final grade, type of high-school attended, gender and immigrant status; cohort fixed-effects are also included. The sample mean refers to the group of students whose score in the math placement test lies, respectively 2, 1 and 2.5 above the cutoff. Estimation methods: local linear regression (LL) in columns 2, 3, and 4; polynomial regression with 2nd-order polynomial in column 5. Estimation sample: students enrolled in the economics department under study, over the years 2009-2012. In columns 2, 3, and 4, the sample is restricted to the students who scored within an interval around the remediation cut-off of bandwidth h = 2, 1 and 2.5, respectively; in column 5, h = 8. Robust standard errors in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

# Chapter 2

# How Does Maternal Labor Supply Respond to Changes in Children's School Schedule?

## 2.1. Introduction

From 2008 to 2013, French children aged between two and eleven stayed in school four days a week for a total of 24 hours of classes. On Wednesday, they were supposed to stay at home. According to the Multinational Time Use Survey (Gershuny and Fisher 2013), as displayed in figure 2.1, women with children in the UK, Germany and Spain distribute their working time equally along the week. In contrast, French mothers work significantly less time on Wednesday than on the other working days of the week.

In January 2013, the French government approved a reform that restructured the weekly schedule of classes in kindergarten and elementary school. Following the suggestions of several chronobiologists, in order to lighten the daily workload of children, this intervention reduced the length of the instruction time per day; added an extra half day of classes in order to maintain invariant the total amount of weekly teaching hours; and aimed at compensating the shortening of each school day with the introduction of optional extra-curriculum activities, possibly without any additional cost for families.

Two elements of this intervention can affect mothers' employment decisions. First, the reorganization of the teaching time and, in particular, the introduction of classes on Wednesday morning, may induce mothers to restructure their own working schedule, in order to have a more continuous presence at work. Secondly, this reform delivers an implicit wage subsidy to those mothers who had to pay for private care services to look after their children on Wednesday morning. This may push mothers to work more, depending on the interplay between substitution and income effects.

Analyzing mothers' response along these two dimensions is equally important. Regarding the organization of the working time, having a flexible schedule can be especially costly for some women, as suggested by Goldin (2014). In this recent contribution, she shows that "most of the residual gender gap in earnings exists because hours of work in many occupations are worth more when given at particular moments and when the hours are more continuous. [...] Much has to do with the presence of good substitutes for individual workers when there are sufficiently low transactions costs of relaying information. In many workplaces employees meet with clients and accumulate knowledge about them. If an employee is unavailable and communicating the information to another employee is costly, the value of the individual to the firm will decline. Equivalently, employees often gain from interacting with each other in meetings or through random exchanges. If an employee is not around that individual will be excluded from the information conveyed during these interactions and has lower value unless the information can be fully transferred in a low cost manner." As Goldin (2014), other studies show that women value flexibility when making their career choices. In particular, Flabbi and Moro (2012) demonstrate this point by estimating, with the use of CPS data, a labor market search model in which jobs are characterized by work hours' flexibility. Similarly, Wiswall and Zafar (2016) analyze choices of undergraduate students who are presented sets of occupations with different characteristics, and find that women, on average, have a higher willingness to pay for jobs with greater work flexibility (lower hours, and part-time option availability). In light of these recent contributions of the literature, it appears especially important to gain more insight on the importance of the cost of flexibility, and to understand which mothers are more sensitive to the allocation of their working hours.

As to the second dimension of response - the potential increase in labor supply driven by the implicit wage subsidy by the reform - this intervention gives us the opportunity to understand to what extent the elimination of institutional constraints can further boost women's labor supply in the context of a developed country, characterized by high female labor force participation rates. Some studies suggest that women's wage elasticity may slow down as their employment rates rise. Citing Goldin (2006), this would reflect "a fundamental transformation in how women view their employment. [...] Most women now perceive their work as a fundamental aspect of their satisfaction in life and view their place of work as an integral part of their social world." As a consequence, both their income and substitution elasticities tend to decrease, as this transformation takes place. Still, policies like parental leave and part-time arrangements seem to strengthen mothers' attachment to the labor market even in countries with high levels of female participation to the labor market (Aaronson and French 2004, Baker and Milligan 2008, Blau and Kahn 2013, Booth and Van Ours 2008, Lalive and Zweimüller 2009, Schönberg and Ludsteck 2007).[18] In this context, it is less clear how mothers would react to an expansion of the time children can spend in school or public childcare, as the evidence on these types of interventions is mostly confined to countries with relatively low levels of female labor force participation (Bauernschuster and Schlotter 2015, Baker, Gruber, and Milligan 2005, Berlinski and Galiani 2007, Cascio 2009, Fitzpatrick 2010, Gelbach 2002, Havnes and Mogstad 2011). In countries like France, where the proportion of active women is as high as 83 percent, above the OECD average, some mothers might simply substitute private care services for the public one, as suggested by Havnes and Mogstad (2011), without increas-

[18] Even though they can have negative effects on wages and career progression - though, not always persistent in the long-run.

ing their working hours. Others could decide to switch from part-time to full-time work. In this paper we are able to study these hypothesis.

To estimate mothers' labor supply response to this intervention, we choose to focus on mothers whose youngest child is between six and eleven years old and we compare the evolution of their employment decisions to that of mothers whose youngest child is between twelve and fourteen, in a difference-in-difference framework. To conduct this analysis we mainly use the quarterly data of the French Labor Force Survey from 2009 to 2014.

Our results show that treated mothers do react to the 2013 reform. In particular, their probability of working on Wednesday rises by five percentage points. However, there is no evidence that labor force participation is affected by this intervention. Moreover, neither weekly working hours nor the probability of working full-time rise in response to it. Overall, these findings imply that treated mothers reorganize their working time in accordance to their children's new school schedule, but that they do not react to the implicit wage subsidy this reform provides.

To better understand which mothers drive this response, we consider the role of different factors, such as the structure of the family or the characteristics of the job held, and we also study how these different spheres of a woman's life interact with each other. First, we investigate the importance of the family context. Traditionally, the literature on the impact of childcare policies on female labor supply has analyzed the response of single and married mothers separately. In this paper, we enrich this analysis by considering whether women's reaction depends also on the characteristics of the other components of the family, notably their children and their partner. Secondly, we add to the literature by studying whether the work environment influences women's response. In particular, we are interested in understanding whether their bargaining power at work and the cost of flexibility at work, as defined by Goldin (2014), play a role in defining their reaction. To measure a worker's bargaining power we consider the worker's tenure, whether the woman has a permanent or temporary contract, and whether she works in occupations that favor part-time contracts. To identify which professions reward more a regular and prolonged presence at work – or, in other words, impose a higher cost of flexibility – we exploit the O*NET classification of occupations. This online platform, created by the United States Ministry of Labor, regroups jobs on the basis of the skills used and activities performed at work. Following Goldin (2014), we focus on those characteristics which seem particularly relevant to measure the cost of flexibility, such as the degree of time pressure, the organization of the work schedule, and the importance of interpersonal relationships with co-workers.

This analysis delivers several results and two are particularly important. First, we find that women's bargaining power at work does influence their response. In detail, we show that the effect of the reform, in the first year of its implementation, is confined to women with permanent contracts, at least one to five years of tenure, and working in occupations where part-time contracts are prevalent. Secondly, we provide evidence that women do take into account that flexibility is costly when making their employment decisions. On the one hand, we show that women facing a higher cost of flexibility, i.e. those working in occupations where it is important to build solid relationships with co-workers, attend frequent meetings, take key decisions, and perform tasks under pressure, were already working longer hours before the reform. On the other hand, we observe that only women facing a low cost of flexibility – that is those working in professions where team work is less relevant, the worker is not responsible for important decisions, and work pressure is low – are able to immediately react to the reform, by restructuring their working schedule in accordance to new timetable of their children. Therefore, these results show that to fully understand women's response to the relaxation of institutional constraints, it is important to consider the characteristics of the work environment in which they operate.

To conclude our analysis, we also study fathers' reaction to the reform and find no evidence that this intervention affects their employment decisions. On the one hand, this result supports the findings of the recent strand of the literature establishing the importance of cultural norms as determinants of gender identity and women's employment decisions (Fortin 2005, Bertrand 2011, Fernandez 2011). On the other hand, it shows that, precisely because a strict division of roles within the household persists even in a context of high female labor force participation, limiting institutional constraints can help modify these cultural believes.

Overall, our findings have several policy implications. First, they prove that, even in developed countries, where female participation to the labor market is high, women are sensitive to the presence of institutional constraints. Secondly, they show that to assess the overall impact of policies directed to specific members of a family, it is always important to consider how they affects all households members. Third, they suggest that both career's incentives and workers' bargaining power influence their reaction to institutional constraints. Finally, the fact that mothers do not react to the implicit wage subsidy offered by this reform provides some support to the hypothesis that women's wage elasticity might indeed be weaker in countries with high female labor market participation rates. However, it might also indicate that three additional hours of childcare are not enough to generate a substitution of work for leisure.

Importantly, so far we are estimating the short-run impact of this reform. In the long-run, its implications might change. First, more women might take advantage of the extracurricular activities to increase their working hours. In this respect, we have to take into account that it might take some time for contracts to be renegotiated, which implies that our results might be downward-biased. At the same time, this short-run analysis allows us to identify which category of workers can quickly react to changes in institutional constraints – namely those with enough bargaining power and working in occupations characterized by a low cost of flexibility. Secondly, a more regular working schedule might eventually affect the career path of mothers, by allowing them to perform more tasks and occupations, and by expanding their chances of receiving on-the-job training and promotions (Landers, Rebitzer, and Taylor 1996). Hence, it will be important to monitor the evolution of women's response. Finally, the general-equilibrium effect of this reform will have to be considered. In particular, it will be interesting to analyze how mothers' response to this reform will affect their co-workers and the overall organization of their work environment.

The paper proceeds as follows. Section 2.2 gives a detailed description of the French primary school system and how this has been affected by the 2013 reform. Section 2.3 describes the data used to conduct this analysis. Section 2.4 introduces the identification strategy, the main results and robustness checks. Section 2.5 analyses potential channels and consequences of these results. Section 2.6 concludes.

## 2.2. The French primary school system

The French educational system is divided into three stages: elementary education, for children aged six to eleven; secondary education – in turn divided into middle school (*collège*) and high school (*lycée*) – that terminates with the *baccalauréat*, normally taken at the age of eighteen. With this diploma pupils can access tertiary education. Education is compulsory since the age of six till sixteen. However, parents can send their children to free public pre-kindergarten (*école pre-maternelle*) already when they are two, or to kindergarten (*école maternelle*) at the age of three. By now, 23 percent of two-years old children and 95 percent of children aged three to five attend this pre-school stage (Goux and Maurin 2010). With the "*Loi d'orientation sur l'éducation*" or Jospin Law of 1989, primary school has been divided into three cycles. The first one, which comprises the first two years of nursery school is called "cycle of first learning"; the last year of kindergarten together with the first two years of elementary school form the "cycle of fundamental learning"; finally the last three years of elementary school constitute the "cycle of in-depth learning". Importantly, public primary schools are financed by municipalities. The private sector comprises mainly religious schools and enrolls fourteen percent of all primary school pupils.

With respect to the structure of the school calendar, France has always been one of the countries with the longest period of holidays, longest number of hours per year, and longest school day, in primary school.

Since the introduction of compulsory primary education in 1882 (*Loi Ferry*) until the end of the 1960s, children spend five full days at school, with a break on Thursday and Sunday, for a total of 30 hours per week. In 1969, Saturday afternoon is abolished, the break in the middle of the week is advanced from Thursday to Wednesday, and two hours of physical activities are added to the school week.

It is only with the development of the chronobiology in the 1980s that an intense debate on the optimal structure of the school schedule spreads out. Experts of this discipline point out that primary school children need more frequent holidays and a shorter day at school. As a consequence, the Jospin Law restructures the school year in 36 weeks over five periods, and reduces by one hour the weekly schedule. Moreover, in 1991, a ministerial decree gives municipalities the possibilities to adopt a four-days schedule. Only a few choose this possibility. In 1995 it is the Ministry of education that relaunches this option by selecting a pool of pilot schools to experiment the four-days school week. From that moment, several municipalities start to consider this option. Finally, in 2008, under an harsh debate, the four-days schedule is extended to all primary schools in France and weekly hours are reduced from 26 to 24. Nonetheless, in 2013, under the pressure of chronobiologists, the Minister of Education reintroduces the four-and-half days school week.

In particular, with the 2013 reform, the school day is shortened by 45 minutes; in order to maintain invariant the total amount of weekly hours, an half day is added, mainly on Wednesday morning, and exceptionally on Saturday; and municipalities are invited to provide free extra-curriculum activities for children, for a total of three weekly hours; these should compensate for the reduction of the daily instruction time. Importantly, municipalities are given the possibility to implement the new schedule either in the year 2013-14 or in 2014-15. 20 percent of them chose to do it in 2013; the rest adopts the new system only in 2014. Regarding private schools, these have the freedom to chose whether to implement the 2013 reform or not at all, and, by the end of the academic year 2014-2015, fifteen percent of them, comprising 13.5 percent of French pupils attending a private school, adopt the new schedule.[19]

[19] In our data we cannot tell whether a family sends their child to a public or a private school. We can only observe the aggregate proportions of students enrolled in public and

Finally, it is important to notice that both the 2008 and 2013 reforms affect only kindergarten and primary school children. In middle and secondary school, pupils have at least 24 hours and a half of classes per week, spread over five days, and this schedule has not modified since a long time.

## 2.3. Data description

Our study relies on the use of several databases. First, we use the 2009-2014 waves of the French Labor Force Survey (*Enquête Emploi en Continu*) or FLFS. This data set collects information on work-related statistics with quarterly interviews to a representative sample of the French population. From the FLFS we extract data on women's age, level of education, marital status, present and past labor market status, income, and the structure of the household in which they reside. Crucially, we exploit the information on the municipality of residence, the number of children women have, and their age.

Secondly, in order to identify the timing of the implementation of the reform across municipalities, we exploit the Enrysco database. This is an administrative data set that has been created by the French Ministry of Education and provides a precise description of the weekly teaching schedule for each school, in each municipality.

Finally, to better investigate the mechanisms that drive women's response to the reform, we use the United States Department of Labor Occupational Information Network, or O*NET. This database, available online, classifies occupations on the base of the activities performed and skills used at work. There are eight broad categories: abilities, interests, knowledge, skills, work activities, work context, work style, and work

private schools every year and these remain stable over the years of implementation of the reform. In other words, it does not seem that some families are moving their children from one type of school to the other because of the reform. Overall, this implies that our estimates might be slightly downward-biased as around twelve percent of families in our sample are not affected by the reform (corresponding to the 87 percent of the fourteen percent of children attending private schools.)

values. Following Goldin (2014), we focus on the work activities and work context, which comprise several aspects of the work environment that can help us understand women's reaction to the reform.

# 2.4. Empirical analysis

## 2.4.1. Identification strategy

To identify how a change in children's school schedule influences their mothers' labor supply behavior, we adopt a difference-in-difference strategy. We define a woman as being treated if her youngest child is affected by the 2013 reform. Next, we choose to compare mothers whose youngest child is between six and eleven, with those whose youngest child is between twelve and fourteen – corresponding to the age-interval of middle school pupils. The graphical analysis of pre-treatment trends in the labor supply measures we have chosen, figure 2.2, supports this choice, as the employment decisions of the treatment and control group exhibit a comparable evolution.

We decide to exclude mothers with children aged two to five from the treatment group for several reasons. First and most importantly, even though the evolution of several labor supply measures is similar among mothers with children in kindergarten and those with older children, the level of the participation rate to the labor market, as well as several observable characteristics, vary substantially between these two groups, as shown in table 2.1. As a consequence, even if from an econometric point of view it would be correct to include mothers of children in kindergarten age in the treatment group, the interpretation of the results and mechanisms behind these would probably differ depending on the age of the youngest child. Secondly, mothers with children between two and three were already entitled to receive childcare subsidies prior to the introduction of the reform. As a consequence, contrary to mothers of older children, they might react to the reform by simply substituting one form of care for another.[20] Moreover, only 30 percent of women whose youngest child is two years old actually send him/her to kindergarten (Goux and Maurin 2010). For all these reasons, we prefer to exclude mothers with children in kindergarten age from the treatment group. However, in the appendix, we show that our main results do not change if we include them in the analysis.

Finally, in the main regressions, we restrict our sample to mothers living in municipalities that introduced the reform in 2013, for which we can already observe the response all along the first year of the new regime.[21]

On the basis of these choices, we run the following specification on

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[20] To study if this is the case, we plan to use the CNAF data set of recipients of childcare subsidies, which provides household levels data on the use of two subsidies: the CLCA (*Congé de libre choix d'activité*), an early childhood parental leave, and the CMG (*Complément mode de garde*), a standard childcare allowance for parents with children younger than four.

[21] The results on the sample of mothers living in municipalities that introduced the reform in 2014 are available upon request. We do not find any evidence that the reform has an impact on these mothers. However, it has to be noticed that, with the available data, we can observe the effect of the reform on this group for just one quarter.

In this respect, it is also important to consider the following. In principle, to identify the effect of the reform, we could exploit the variation over time and across municipalities in the implementation of the reform. In this way, we would compare mothers whose youngest child is in the affected age-range and live in municipalities that introduced the reform in 2013, with the same group of mothers who live in municipalities that postponed the implementation of the reform to 2014. However, we prefer not to adopt this strategy for two reasons. First, the comparison of the pre-trends in labor supply measures for these two groups of mothers – figure 2.3 – reveals that their dynamics seem to diverge before the implementation of the reform. Therefore, it is hard to claim that, absent the reform, the evolution of labor supply would have been the same across these groups. This concern is also confirmed by a formal test on the parallel trend assumption. In a regression model that compares the evolution of labor supply for these two groups of mothers, we include a battery of dummies taking value one for mothers "treated in 2013", in the three waves before September 2013. A test on their joint significance leads us to reject the null for all the outcomes considered. Secondly, by adopting this strategy we would be able to study only the impact of the reform in his first year of implementation, given that from 2014 onward, all municipalities adopt the new schedule. As it might take some time for its effect to manifest, we think that considering only its short-run impact would considerably limit the objectives of our analysis.

mothers living in "2013 municipalities", whose youngest child is between six and fourteen years old:

$$
\begin{aligned}
Y_{icmt} &= \gamma_m + \delta_t + \pi * X_{icmt} + \alpha * Yst\_Child\_btw\_6\_11_c \\
&+ \beta * Yst\_Child\_btw\_6\_11 * Post\_Sep\_2013_{ct} + u_{icmt}
\end{aligned} \tag{2.3}
$$

Here $i$ stand for each interviewed woman, $c$ for the age of the youngest child, $m$ for the municipality of residence and $t$ for the wave in which the woman is interviewed. $Y_{icmt}$ represents the outcome considered. As anticipated, the main ones are labor force participation, the choice of working part-time or full-time, weekly working hours, weekly working days, and the decision to work on each specific day of the week.[22] The vector $X_{icmt}$ includes all the individual variables that can affect women's labor supply decisions. These include age, age squared, level of education, number of children, marital status, and presence of other members in the household; $\alpha$ measures the impact of having the youngest child in primary school age. The main coefficient of interest is $\beta$ that should capture any deviation from a parallel evolution in the outcome of interest between the treatment and the control group, due to the implementation of the new schedule in primary school. In all regressions we also include municipality of residence, $\gamma_m$, and wave of interview fixed effects, $\delta_t$. Finally, in all specifications, standard errors are clustered at the municipality level to account for any correlation of the outcomes for women residing in the same municipality.

### 2.4.2. Main results

Tables 2.2 and 2.3 show the main results. As expected, the 2013 reform does not trigger any response at the extensive margin – table 2.2,

[22] To measure these outcomes we construct, respectively: a dummy equal to one if the woman belongs to the active population; a dummy equal to one if the woman works part-time, a continuous variable indicating the number of hours worked on average per week, one measuring the number of days worked per week, and a dummy equal to one if the woman works on a specific day of the week.

column 1. Point estimates in table 2.2, column 2 and 3, suggest that, after the implementation of the reform, treated mothers are less likely to work part-time and tend to work more hours. However, these coefficients are not precisely estimated. In contrast, column 4 indicates that the reform has a significant impact on the number of days worked per week, as treated mothers work on average half a day more, from a pre-reform level of four days and half. In table 2.3, we can see that, accordingly, their probability of working on Wednesday increases by five percentage points, significant at five percent significance-level, while their likelihood of working on each other day of the week does not change with respect to the pre-reform period, in comparison with control mothers.[23]

Taken together, these results imply that mothers react to this intervention by adapting their working time to their children's new teaching time schedule, without increasing their overall labor supply. In other words, they do not take advantage of the implicit wage subsidy this reform gives them. We can think about several reasons why this is the case. First, it might take some time to renegotiate working contracts, which implies that the effect on hours worked and the incidence of part-time contracts might become visible only after the first year of implementation of the reform. Secondly, it might simply be the case that wage subsidy implicit in the reform is not large enough to trigger a substitution effect of work for leisure. Third, the fact that some municipalities chose to concentrate the extracurricular activities in a few days, rather than spread them along the week might prevent mothers from taking advantage of them. Finally, at least in the first year of implementation, mothers might perceive the new extracurricular activities to be of low quality, when compared to the alternative after-school-care options. To investigate these last two hypothesis, we plan to exploit a survey, conducted by the National Agency of Family Transfers (CNAF in the French acronym) and the Association of French

[23] It has to be noticed that, in the FLFS, the decision to work on each days of the week is measured only from 2013 onward. However, the fact that the reform has a significant impact also on the number of days worked per week shows that the effect on the probability of working on Wednesday does not merely depend on the span of time over which the outcomes are observed.

Mayors (AMF) in the spring of 2014, and providing, for a sub-sample of municipalities, the exact schedule of the extra-curricular activities and the type of activity offered to children.

### 2.4.3. Robustness checks

For the difference-in-difference strategy to accurately identify the effect of interest, we need to assume that, absent the reform, the evolution of mothers labor supply would have been the same for the treated and control group (parallel-trend assumption). In other words, we should check that our estimates are not capturing the effect of other factors that affect treated and control mothers in a different way.

To support this assumption, besides the visual inspection of the pre-treatment trends in labor supply measures, we can conduct a series of robustness checks. Here we focus on the decision to work on Wednesday, as the outcome measuring the number of days worked per week is complementary to this one. However, in the appendix, we report the robustness checks for this outcome as well. We start in table 2.4. In the first column we report the baseline estimates for the probability of working on Wednesday. The second column looks at the effect of the reform in its first year of implementation, 2013-14, in municipalities that postponed the introduction of the new schedule to the academic year 2014-15. In these municipalities, mothers having their youngest child in primary school are not more likely to work on Wednesday, compared to mothers whose youngest child is in middle school. Next, the third column shows the estimates of a triple difference model that exploits the municipalities that postponed the introduction of the reform as a third dimension of comparison:

$$
\begin{aligned}
Y_{icmt} &= \gamma_m + \delta_t + \pi * X_{icmt} + \\
&+ \rho * Mun2013 * Post\_Sep\_2013_{mt} + \\
&+ (\alpha + \theta * Mun2013_m + \mu * Post\_Sep\_2013_t \\
&+ \beta * Mun2013 * Post\_Sep\_2013_{mt}) * \\
&* Yst\_Child\_btw\_6\_11_c + u_{icmt}
\end{aligned} \tag{2.4}
$$

This specification should control for the influence of any other factor that affects treated and control mothers differently, but that is common across municipalities that introduce the reform at different points in time. Once again, the impact of the reform remains significant, as indicated by the p-value of the sum of $\mu$ and $\beta$.[24]

In tables 2.5 and 2.6 we change the size of the treatment and control group to show that our results are not sensitive to the definition we adopted. In particular, in table 2.5, we can see that restricting the treatment group does not alter substantially the magnitude of the effect, and the impact of the reform remains significant in all the columns. Table 2.6 shows, instead, that restricting or expanding the control group does not affect either the magnitude or the significance of the reform coefficient.

Finally, figure 2.4 provides a graphical analysis of the treatment dynamics. In particular, it shows the coefficients of the leads and lags in the treatment, estimated with this regression:

$$
\begin{aligned}
Y_{icmt} &= \gamma_m + \delta_t + \pi * X_{icmt} + \alpha * Yst\_Child\_btw\_6\_11_c \\
&+ \sum_{k \geq t-2} \beta_k * Yst\_Child\_btw\_6\_11 * Leads\_Lags_{ck} + u_{icmt}
\end{aligned} \tag{2.5}
$$

[24] These robustness checks deliver the same results when the outcome considered is the number of days worked per week, as shown in table 2.14. For this outcome, we can also check the impact of a placebo reform. In detail, in the fourth column of table 2.14 we exclude from the sample the post-treatment period and we pretend that the reform was implemented at the beginning of 2013. As we can see there is no evidence that this fake treatment affects women's working schedule.

The first thing to be noticed is that the coefficients on the leads are jointly insignificant, with a corresponding p-value of 0.1703. However, there is some evidence that mothers might have started to react to the reform as soon as it was announced, in the second quarter of 2013, as the coefficient on the first lag is individually significant.

Nonetheless, the dynamic response after the implementation of the reform suggests that it takes at least one quarter for the effect to become stable.

Overall, these tests seem to corroborate the validity of our identification strategy.

# 2.5. Potential mechanisms and short-term implications

## 2.5.1. Main factors influencing women's response

To better understand our results, it is important to identify which type of mothers are most responsive to the reform. We can think about three factors that can influence mothers' response, namely the family context, women's bargaining power at work, and, following Goldin (2014), the cost of flexibility at work. With the expression "family context" we refer to the woman's marital status, but also to the characteristics of the other members of her family. Here, we focus in particular on the family income, proxied by the partner's level of education,[25] and the total number of children the woman has. A priori, the effect of each of these factors is ambiguous. On the one hand, single mothers, as bread-winners, might need to work more than married mothers, independently of the institutional constraints they face. On the other hand, they might be entitled to receive subsidies that can weaken their incentives to work. The employment decisions of married mothers surely depend on their husband' earnings, and

[25] Unfortunately, labor and family earnings are very badly reported in the FLFS, and therefore we choose to rely on the level of education as an indirect measure of living standards.

total family income. On the one hand, the higher is the husband's income, the lower should be the incentives to work for the woman. On the other hand, an argument of assortative mating would suggest that high-skilled men will be more likely to be married to high-skilled women, and these, in turn, might have a strong taste for work, independently of their family resources. Finally, the larger is the number of children a woman has, the more difficult could be for her to manage family and work duties. However, raising children is costly, and the larger is their number, the stronger could be the incentives for mothers to work in order to sustain the family income. Traditionally, the literature that studies the effect of childcare expansions has focused only on the comparison between married and single women. Nonetheless, in light of all these arguments, we think that it is important to analyze whether the response is heterogeneous along all these dimensions spanning the family context.

Women's bargaining power at work is another factor that can affect their response to this reform, and to changes in institutional constraints, in general. In particular, we can think that this factor might influence the timing of the response, as some women may have the possibility to renegotiate their working schedule quicker than others. Several elements determine a worker's bargaining power. We focus on the type of contract the woman has, the length of her tenure, and the occupation she holds.[26] As for the latter, we assume that the frequency of part-time contracts for a certain occupation might be a good indicator of women's bargaining power in that profession. Therefore, we regroup occupations according to this criterion.[27]

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[26] In principle, the number of employees in the worker's firm might affect her bargaining power. Unfortunately, this variable is badly measured in the French Labor Force Survey, and therefore we cannot analyze its impact.

[27] In detail, in order to identify what we call part-time intensive occupations, we proceed in two steps. We looked first at the population of part-time women and we selected occupations that represented more than five percent of part-time workers. Secondly, we looked at occupations for which the part-time rate of women was the highest. Finally, we selected the seven occupations that were in both categories: intermediate health and social workers, middle management (business and firms), civilian members and public service employees, administrative business employees, commercial workers, employees

Finally, we take advantage of this reform to test Goldin's theory (Goldin 2014) regarding the cost of flexibility at work. It is plausible to think that in some occupations working longer hours and/or a regular presence at work might be more rewarded than in others. This could be the case, in particular, in those professions where it is important to build solid relationships with co-workers, attend frequent meetings, take key decisions, and perform tasks under pressure. The continuous presence at work and the availability to work long hours should be particularly valuable in these contexts, or, in other words, the cost of a flexible working schedule might be especially high in these occupations. To identify how this factor affects women's employment decisions, we follow Goldin and exploit the O*NET database to construct a measure of this cost of flexibility. We consider five characteristics, namely: time pressure, which answers the question "How often does this job require the worker to meet strict deadlines"; frequency of decision making, referring to the incidence with which a worker is required to make decisions that affect other people, the financial resources, and/or the image and reputation of the organization; structured versus unstructured work, representing the extent to which the job is structured for the worker, rather than allowing her to determine tasks, priorities, and goals; contact with others, referring to the extent the job requires the worker to be in contact with others (face-to-face, by telephone, or otherwise) in order to perform it; establishing and maintaining interpersonal relationships, representing the importance of developing constructive and cooperative working relationships with others, and maintaining them over time. The importance of each of these aspects in every occupation is measured with a score ranging from zero to 100. Our measure of the cost of flexibility is the average of the standardized scores of these five characteristics. In particular, we regroup women's occupations in two groups, depending on whether the average score is below or above the median for the entire sample.

Clearly, other aspects of a woman's career can influence the value of flexibility. We refer in particular to the woman's level of education, to the type of position held, being it managerial, intermediary or an elemen-

who provide direct customer service, and craft unskilled workers.

tary occupation, and to whether she works in the public or the private sector. All these different dimensions of a job are also strongly interrelated as shown in table 2.7.[28] Moreover, women's career choices are obviously connected with the composition of her family. In particular, a pattern of assortative matching is clearly evident in the sample studied. The summary statistics reveal another important message. Table 2.8 describes women's employment decisions before the implementation of the reform. Clearly, women with a high level of education, working in managerial occupations, and with a high cost of flexibility are aware of the value that working longer hours has in their professions, as on average, they all work more than the other groups of women. This appears to be more important than a regular presence at work, as they are not more likely to work on Wednesday than other types of mothers.

Importantly, to analyze the heterogeneous response to the reform along all these dimensions, we have to make the hypothesis that the composition of the subgroups considered is not affected by the reform. At least in the short-run, it seems plausible to assume that the reform will not affect educational, marriage or fertility decisions. In the previous paragraph, we have also shown that this intervention does not affect the choice to work part-time or full-time. Below, we will further demonstrate that, in its first year of implementation, the reform does not affect the type of tasks performed at work. Therefore, even if women self-select into different occupations, work environments and family's structures, it appears that we can take these choices as fixed in the short-run.

The heterogeneity analysis, reported in tables 2.9, 2.10 and 2.11 provides interesting insights.[29] First, women's bargaining power at work does influence their response. In detail, table 2.9 shows that only women working in permanent contracts, with one to five years of tenure, and

[28] Here we inspect only the career choices and family composition of women with different levels of education. If we were to present these statistics starting from a different dimension, we would obtain a similar picture.

[29] In these tables we only report the subgroups for which the effect of the reform is significant. In the appendix, tables 2.16, 2.17, 2.18 and 2.19 we show all the other subgroups.

working in occupations where part-time contracts are prevalent are able to re-organize their working schedule in accordance to the new school timetable of their children. Secondly, working in occupations characterized by a low cost of flexibility also helps women react immediately to the reform, as shown in table 2.10.[30] Accordingly to the pattern of correlations encountered in the descriptive analysis, the response to the reform is further driven by women working in elementary occupations, operating in the private sector – where women are less likely to occupy managerial positions – with secondary education, and a partner with secondary education – as reported in table 2.11. We also find that the probability of working on Wednesday increases mostly for mothers with one child, which are also slightly more numerous among lower educated women.

Overall, these results have two implications. First, they show that it is important to take into account the characteristics of the work environment in which women operate to fully understand their response to changes in institutional constraints. Secondly, these findings indicate that basically none of the dimensions considered here enhances the probability to react to the implicit wage subsidy delivered by this reform. In particular, these results seem to suggest that even for low-income households the wage subsidy might be too low to trigger any increase in women's labor supply.

### 2.5.2. Impact on fathers

In principle this reform might affect the employment decisions of both parents. Therefore, to identify all the implications of this intervention, we also analyze fathers' response. As shown in table 2.12, we find no evidence that men's employment decisions are influenced by a change in their children's school schedule. This result is to be considered together

[30] We also find evidence that these women work longer hours and are less likely to work part-time after the introduction of the reform. These results seem to suggest that the reform allows these women to catch up with those experiencing a high cost of flexibility. However, given that along the other dimensions of heterogeneity that are positively correlated with a low cost of flexibility, we do not find evidence of this reaction, we prefer not to put too much weight on these results.

with the fact that, among parents in employment, 76 percent of fathers worked on Wednesday before the introduction of this reform, against 56 percent of mothers. These numbers show that even in a country in which a high proportion of women participate in the labor market, a strict division of roles persists within households with children, and that institutional constraints bind only for women. As a consequence, removing barriers to work for women might play the double role of enhancing the attachment to the labor market, and of contributing to change gender identities.

### 2.5.3. Consequences

In table 2.13 we try to measure the short-term implications of a more regular working schedule. In particular, we investigate whether mothers might have higher chances of participating in training[31] or be more likely to change their position in their firm, when being present at work every day. Moreover, we check whether mothers increase at first their overtime hours, before renegotiating their regular schedule with their employer. We do not find evidence for these responses to take place in the first year after the implementation of the reform, and this is so in the entire sample, as in any subgroup considered.

However, we do not exclude that, in the long-run, a more regular presence at work might eventually affect these outcomes.

## 2.6. Conclusion

This paper brings several contributions to the literature on female labor supply, and three are especially insightful. First, our study shows that even in developed countries, where female participation in the labor market is high, women are affected by the presence of institutional constraints. Secondly, it indicates that both career's incentives and workers'

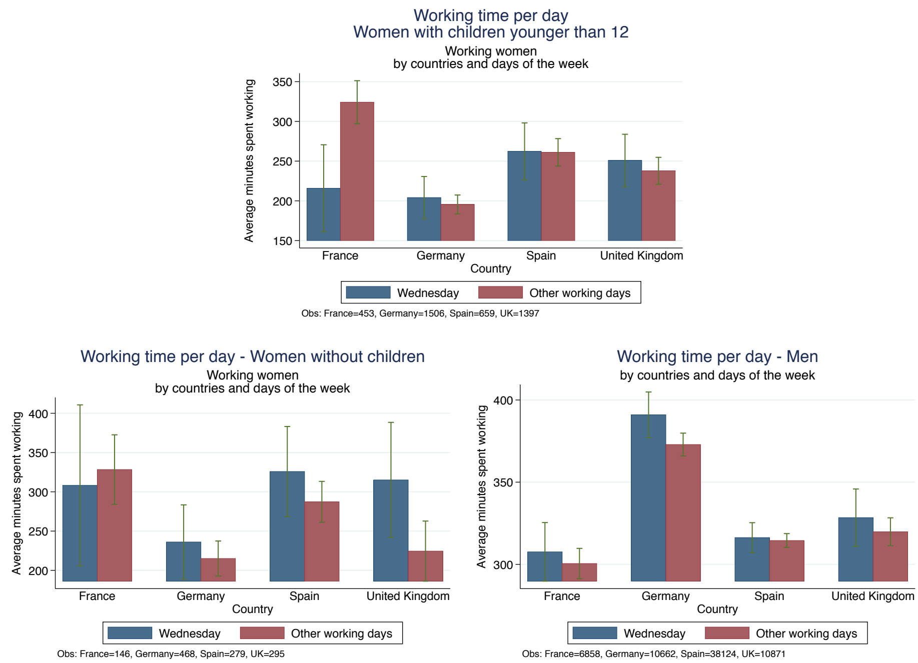
[31] We define as training the participation to stages, conferences, individual classes, or cultural activities

bargaining power influence women's response to government interventions and barriers to work. Third, it proves that institutional constraints bind only for women and that a strict division of roles within couples persist even in developed countries.

The next step will be to study the long-run implications of our findings. In particular, it will be important to analyze whether a more regular working schedule will allow women to perform more tasks and occupations, expand their chances of receiving on-the-job training and promotions, and affect their earnings profile. In parallel, the release of updated employer-employees data, the 2014 French DADS, will give us the possibility to study firms' and co-workers' reaction to this reform. Finally, it will be especially interesting to evaluate the impact of this intervention on children' school performance, as soon as the appropriate data to conduct this analysis will become available.

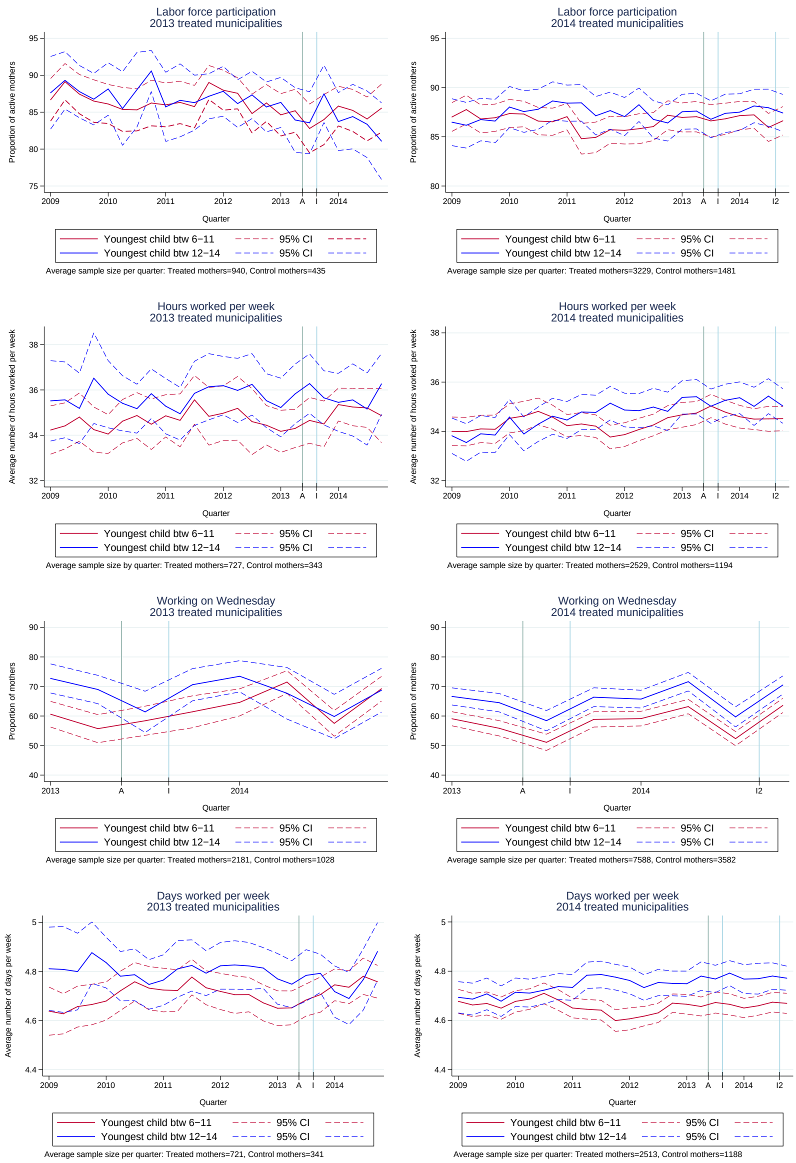
## 2.7. Tables and Figures

Figure 2.1: **Time Use across European countries**



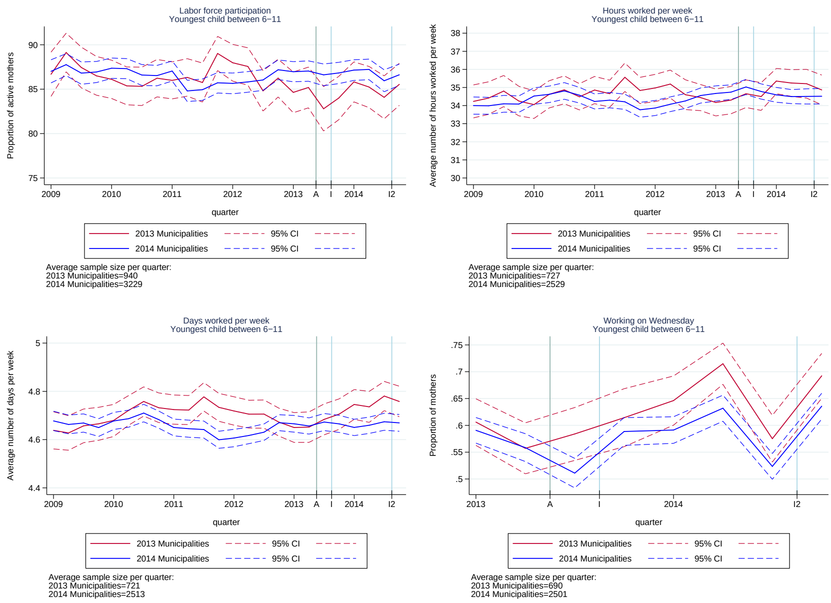

Source: Multinomial Time Use Study, 1991-2010 averages.
Note: the figures report a bar graph representing the average number of minutes spent at work by, respectively, mothers with children younger than 12 years old, women without children and men, in France, Germany, Spain, and the United Kingdom. Working time includes paid work, paid work at home, second job, and travel to/from work. To highlight the peculiarity of the French case, we show separately the working time declared for Wednesday from that reported for the other days of the week. The graph is constructed using the 1991-2010 averages of the Multinational Time Use Survey. Finally, we computed 95 percent-confidence intervals using means and standards errors obtained by estimating a regression of the outcome of interest on the treated category, clustering standard errors at the country level.

Figure 2.2: **Trends in mothers' labor supply measures by age of the youngest child**



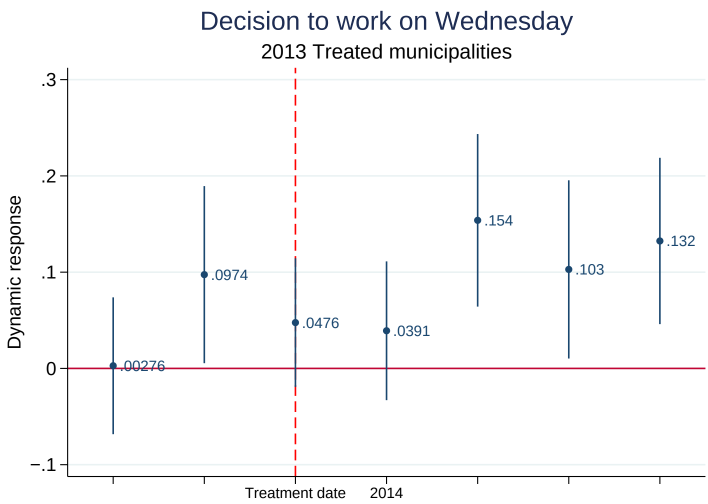

Source: French Labor Force Survey 2009-2014.
Note: the graphs show the evolution of different measures of labor supply over the period 2009-2014. In the graphs referring to 2013 municipalities, the sample is restricted to mothers living in municipalities that introduce the reform in 2013, and whose youngest child is between the age of six and fourteen. In the graphs referring to 2014 municipalities, the sample comprises instead mothers living in municipalities that introduce the reform in 2014, and whose youngest child is between the age of six and fourteen. We represent in red treated mothers, that is those whose youngest child is between six and eleven years old. Mothers whose youngest child is in middle school age, or control mothers, are represented in blue. The vertical bar named "A" corresponds to April 2013, when French municipalities announce in which year they will introduce the reform. The bar called "I" corresponds to September 2013, when 20 percent of municipalities implement the reform.

Figure 2.3: **Trends in mothers' labor supply measures across different municipalities**




Source: French Labor Force Survey 2009-2014.
Note: the graphs show the evolution of three labor supply measures between 2009 and 2014, for mothers whose youngest child is between two and eleven years old. We compare mothers living in municipalities that introduce the reform in 2013, in red, to those living in municipalities that postpone the implementation of the reform to 2014, in blue. The labor supply measures we consider are the proportion of active mothers, the number of hours worked per week, and the number of days worked per week. The vertical bar named "A" corresponds to April 2013, when French municipalities announce in which year they will introduce the reform. The bar called "I" corresponds to September 2013, when 20 percent of municipalities implement the reform.

Figure 2.4: **Dynamic response to the reform**




Source: French Labor Force Survey 2009-2014.
Note: in this graph we report the dynamic response to the reform concerning the decision to work on Wednesday. The coefficients are obtained from the estimation of regression 2.5 on the years 2013-2014. We also report 90-percent confidence intervals. The estimation sample includes all mothers living in municipalities that introduce the reform in 2013 and whose youngest child is between six and fourteen. The treatment date coincides with the last quarter of 2013. We also check the joint significance of, respectively, the leads and lags of the reform, and find that the former are jointly insignificant, with a corresponding p-value of 0.1703, while the latter are jointly significant at 5 percent significance level, with a p-value of 0.0473.

Table 2.1: **Pre-treatment means in covariates and outcomes by age of the youngest child - 2013 municipalities**

| | Ygst child 0-1 | Ygst child 2-5 | Ygst child 6-11 | Ygst child 12-14 | Ygst child 15-18 |
|---|---|---|---|---|---|
| Age | 31.1 | 34.4 | 40.4 | 44.8 | 47 |
| | (5.4) | (5.5) | (5.4) | (4.7) | (4.3) |
| Married | 0.52 | 0.56 | 0.60 | 0.63 | 0.64 |
| | (0.50) | (0.50) | (0.49) | (0.48) | (0.48) |
| Immigrant | 0.19 | 0.16 | 0.13 | 0.12 | 0.13 |
| | (0.39) | (0.36) | (0.33) | (0.32) | (0.33) |
| High education | 0.45 | 0.43 | 0.37 | 0.31 | 0.29 |
| | (0.50) | (0.50) | (0.48) | (0.46) | (0.45) |
| Secondary education | 0.37 | 0.37 | 0.42 | 0.45 | 0.45 |
| | (0.48) | (0.48) | (0.49) | (0.50) | (0.50) |
| Low education | 0.19 | 0.20 | 0.22 | 0.20 | 0.26 |
| | (0.39) | (0.40) | (0.41) | (0.40) | (0.44) |
| Number of children | 1.8 | 1.9 | 1.8 | 1.9 | 1.1 |
| | (0.98) | (0.91) | (0.04) | (0.79) | (0.29) |
| Labor Force participation | 0.64 | 0.79 | 0.86 | 0.87 | 0.86 |
| | (0.48) | (0.41) | (0.34) | (0.34) | (0.35) |
| Part-time work | 0.34 | 0.36 | 0.34 | 0.31 | 0.29 |
| | (0.47) | (0.480) | (0.47) | (0.46) | (0.45) |
| Hours worked per week | 34.3 | 34.1 | 34.6 | 35.7 | 36.2 |
| | (9.9) | (10.4) | (10.8) | (11.2) | (11.4) |
| Days worked per week | 4.6 | 4.6 | 4.7 | 4.8 | 4.86 |
| | (0.93) | (0.89) | (0.87) | (0.87) | (0.85) |
| Working on Wednesday | 0.48 | 0.52 | 0.56 | 0.67 | 0.67 |
| | (0.5) | (0.5) | (0.49) | (0.47) | (0.47) |

Source: French Labor Force Survey 2009-2014.
Note: the table presents the means of covariates and outcomes considered in the analysis, computed for each age-interval of mothers' youngest child. These values are calculated for the period before the implementation of the reform. The sample is futher restricted to those municipalities that introduce the reform in 2013.

Table 2.2: **Youngest child btw 6-14 - 2013 Treated municipalities**

| | Labor force participation | Part-time | Hours worked per week | Days worked per week |
|---|---|---|---|---|
| Treatment | 0.00670 | -0.0318 | 0.390 | 0.0956* |
| | (0.0160) | (0.0285) | (0.605) | (0.0505) |
| Observations | 32901 | 25483 | 25483 | 25483 |
| $R^2$ | 0.172 | 0.156 | 0.149 | 0.136 |
| F | 12.68 | 5.718 | 6.107 | 3.805 |
| Pre-treatment mean | 0.788 | 0.337 | 34.63 | 4.67 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform, obtained from the estimation of regression 3.6. The different columns refer to the outcome considered, being respectively labor force participation, column 1, the decision to work parti-time, column 2, number of weekly hours, column 3, and number of days worked per week, column 4. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household. The estimation sample comprises all mothers whose youngest child is between six and fourteen years old, and live in municipalities that introduce the reform in 2013. In column 2, 3, and 4, we consider only mothers who are employed at the time of the interview.

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

Table 2.3: **Working days - Youngest child btw 6-14 - 2013 Treated municipalities**

| | Monday | Tuesday | Wednesday | Thursday | Friday | Saturday | Sunday |
|---|---|---|---|---|---|---|---|
| Treatment | 0.0174 | 0.00934 | 0.0578** | -0.00327 | -0.00232 | 0.0198 | 0.0111 |
| | (0.0309) | (0.0290) | (0.0258) | (0.0265) | (0.0293) | (0.0245) | (0.0165) |
| Observations | 8282 | 8282 | 8282 | 8282 | 8282 | 8282 | 8282 |
| $R^2$ | 0.098 | 0.105 | 0.117 | 0.102 | 0.101 | 0.173 | 0.142 |
| F | 4.282 | 2.936 | 3.070 | 4.675 | 3.529 | 2.794 | 1.5 20 |
| Pre-treatment mean | 0.7152 | 0.7807 | 0.5940 | 0.7568 | 0.7536 | 0.1852 | 0.0676 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform on the decision to work each day of the week. They are obtained from the estimation of regression 3.6. These outcomes are available only from 2013 onward. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household. The estimation sample comprises all mothers whose youngest child is between six and fourteen years old, and live in municipalities that introduce the reform in 2013. We consider only mothers who are employed at the time of the interview.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.4: **Decision to work on Wednesday- Robustness checks**

| | Main regression | 2014 municipalities | DDD |
|---|---|---|---|
| Treatment | 0.0579** | 0.00789 | 0.0008 |
| | (0.0258) | (0.0180) | (0.018) |
| Treatment in 2013 mun. | | | 0.0424 |
| | | | (0.0322) |
| Observations | 8282 | 26035 | 33333 |
| $R^2$ | 0.117 | 0.152 | 0.146 |
| F | 3.070 | 9.228 | 9.847 |
| P-value DDD | | | 0.061 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the results of different robustness checks for the effect of the reform on the decision to work on Wednesday. In column 1, we report the coefficient of the main specification, regression 3.6. Column 2 shows the coefficient of the impact of the reform in the year 2013/14, on mothers living in municipalities that postponed its introduction to the academic year 2014/15. In this column, we exclude mothers interviewed in the last quarter of 2014, as they are actually treated. Finally, column 3 reports the impact of the reform, estimated from a triple-difference model, as specified in regression 2.4. In this column, the sample size comprises all mothers whose youngest child is between six and fourteen, irrespective of their municipality of residence. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

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

Table 2.5: **Decision to work on Wednesday - Changing the definition of the treatment groups**

| | 6-14 | 7-14 | 8-14 | 9-14 | 10-14 |
|---|---|---|---|---|---|
| Treated group 6-11 | 0.0579** | | | | |
| | (0.0258) | | | | |
| Treated group 7-11 | | 0.0698** | | | |
| | | (0.0271) | | | |
| Treated group 8-11 | | | 0.0547* | | |
| | | | (0.0293) | | |
| Treated group 9-11 | | | | 0.0727** | |
| | | | | (0.0282) | |
| Treated group 10-11 | | | | | 0.0961*** |
| | | | | | (0.0348) |
| Observations | 8282 | 7376 | 6457 | 5526 | 4565 |
| $R^2$ | 0.117 | 0.126 | 0.134 | 0.149 | 0.161 |
| F | 3.070 | 2.641 | 1.903 | 2.841 | 3.004 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform on the decision to work on Wednesday. They are obtained from the estimation of regression 3.6. The first column reports the coefficient of the main specification, where the estimation sample comprises all mothers whose youngest child is between six and fourteen years old, and live in municipalities that introduced the reform in 2013. From column 2 onward, we consider only treated mothers, whose youngest child is progressively older. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

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

Table 2.6: **Decision to work on Wednesday - Changing the definition of the control groups**

| | 6-13 | 6-14 | 6-15 | 6-16 | 6-17 |
|---|---|---|---|---|---|
| Control group 12-13 | 0.0528* | | | | |
| | (0.0307) | | | | |
| Control group 12-14 | | 0.0579** | | | |
| | | (0.0258) | | | |
| Control group 12-15 | | | 0.0574** | | |
| | | | (0.0246) | | |
| Control group 12-16 | | | | 0.0479* | |
| | | | | (0.0245) | |
| Control group 12-17 | | | | | 0.0481** |
| | | | | | (0.0228) |
| Observations | 7325 | 8282 | 9180 | 10011 | 10775 |
| $R^2$ | 0.127 | 0.117 | 0.113 | 0.104 | 0.099 |
| F | 2.802 | 3.070 | 3.939 | 4.288 | 5.117 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform on the decision to work on Wednesday. They are obtained from the estimation of regression 3.6. The first column reports the coefficient of the main specification, where the estimation sample comprises all mothers whose youngest child is between six and fourteen years old, and live in municipalities that introduced the reform in 2013. From column 2 onward, we progressively enlarge the control group. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

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

Table 2.7: **Career choices and family characteristics by mother's education**

| | Low | Middle | High |
|---|---|---|---|
| Managerial and professional occupations | 0.02 | 0.02 | 0.34 |
| | (0.13) | (0.12) | (0.48) |
| Intermediary occupations | 0.09 | 0.16 | 0.39 |
| | (0.29) | (0.36) | (0.49) |
| Employees | 0.70 | 0.67 | 0.21 |
| | (0.46) | (0.47) | (0.41) |
| Low cost of flexibility | 0.79 | 0.62 | 0.30 |
| | (0.41) | (0.49) | (0.46) |
| Public sector | 0.23 | 0.28 | 0.30 |
| | (0.42) | (0.45) | (0.46) |
| Permanent contracts | 0.66 | 0.58 | 0.57 |
| | (0.47) | (0.49) | (0.50) |
| Tenure $\leq$ 1 year | 0.11 | 0.09 | 0.04 |
| | (0.31) | (0.29) | (0.20) |
| Tenure 1-5 years | 0.28 | 0.27 | 0.26 |
| | (0.45) | (0.45) | (0.44) |
| Single | 0.27 | 0.34 | 0.25 |
| | (0.44) | (0.47) | (0.43) |
| Middle-educated partner | 0.43 | 0.43 | 0.24 |
| | (0.50) | (0.50) | (0.43) |
| High-educated partner | 0.33 | 0.15 | 0.07 |
| | (0.47) | (0.35) | (0.26) |
| 1 child | 0.33 | 0.33 | 0.29 |
| | (0.47) | (0.47) | (0.46) |
| 2 children | 0.46 | 0.51 | 0.51 |
| | (0.50) | (0.50) | (0.50) |

Source: French Labor Force Survey 2009-2014.
Note: this table shows the career choices and family structures of mothers with different levels of education. With low and high cost of flexibility, we refer to the composite score we assign to occupations depending on the importance of certain aspects of the job for these professions, as defined by the O*NET online platform.

Table 2.8: **Pre-treatment means of selected outcomes by subgroups**

| | Working on Wednesday | Hours worked per week |
|---|---|---|
| Higher education | 0.60 | 36.18 |
| | (0.49) | (8.82) |
| Secondary education | 0.55 | 33.33 |
| | (0.5) | (10.52) |
| Low education | 0.66 | 31.25 |
| | (0.47) | (11.31) |
| Managerial occupations | 0.60 | 37.1 |
| | (0.5) | (7.9) |
| Intermediary occupations | 0.56 | 35.05 |
| | (0.5) | (8.9) |
| Elementary occupations | 0.56 | 32.56 |
| | (0.5) | (9.9) |
| Low cost of flexibility | 0.59 | 32.92 |
| | (0.5) | (11.5) |
| High cost of flexibility | 0.57 | 36 |
| | (0.5) | (9.38) |

Source: French Labor Force Survey 2009-2014.
Note: this table shows the means of two selected outcomes for different subgroups of mothers, in the period preceding the introduction of the reform. With low and high cost of flexibility, we refer to the composite score we assign to occupations depending on the importance of certain aspects for these professions, as defined by the O*NET online platform. In detail, the score is an average of the standardized scores given to five factors, namely time pressure, frequency of decision making, structured versus unstructured work, contact with others, establishing and maintaining interpersonal relationships. A detailed description of these characteristics and the scores assigned to them is given in section 2.5. We regroup women's occupations in two groups, depending on whether the average score is below or above the median for the entire sample.

Table 2.9: **Decision to work on Wednesday - Importance of bargaining power**

| | Entire sample | Long term contracts | Prevalence of part-time contracts | Tenure 1-5 years |
|---|---|---|---|---|
| Treatment | 0.0579** | 0.0678** | 0.0686** | 0.1014*** |
| P-Value | 0.025 | 0.0446 | 0.0470 | 0.001 |
| Pre-treatment mean | 0.56 | 0.58 | 0.54 | 0.57 |
| Observations | 8282 | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the estimated effect for the entire sample. Column 2 displays the effect for mothers with long-term contracts. Column 3 shows the effects for mothers working in occupations in which part-time contracts are prevalent, i.e. those occupations in which most women work part-time and where part-time workers are mostly represented. Finally, column 4 focuses on mothers who have been working for more than one but less than five years with the current employer. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

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

Table 2.10: **Decision to work on Wednesday - Importance of cost of flexibility**

| | Entire sample | Low cost of flexibility | Elementary Occupations | Private sector | Secondary education |
|---|---|---|---|---|---|
| Treatment | 0.0579** | 0.0967*** | 0.1026** | 0.0863*** | 0.122*** |
| P-Value | 0.025 | 0.014 | 0.019 | 0.013 | 0.008 |
| Pre-treatment mean | 0.56 | 0.59 | 0.55 | 0.58 | 0.56 |
| Observations | 8282 | 8282 | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the estimated effect for the entire sample. Column 2 shows the effect for mothers working in occupations characterized by a low cost of flexibility. With low and high cost of flexibility, we refer to the composite score we assign to occupations depending on the importance of certain aspects for these professions, as defined by the O*NET online platform. In detail, the score is an average of the standardized scores given to five factors, namely time pressure, frequency of decision making, structured versus unstructured work, contact with others, establishing and maintaining interpersonal relationships. A detailed description of these characteristics and the score assigned to them is given in section 2.5. We regroup women's occupations in two groups, depending on whether the average score is below or above the median for the entire sample. Next, column 3 displays the effect on women working in elementary occupations. Column 4 refers to the impact on women working in the private sector. Finally, column 5 reports the effect on mothers with secondary education. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.11: **Decision to work on Wednesday - Influence of the family context**

| | Entire sample | Middle-educated partner | One child |
|---|---|---|---|
| Treatment | 0.0579** | 0.103** | 0.13*** |
| P-Value | 0.025 | 0.0255 | 0.001 |
| Pre-treatment mean | 0.56 | 0.58 | 0.59 |
| Observations | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the estimated effect for the entire sample. Column 2 shows the effect for mothers with middle-educated partners. Next, column 3 indicates the effect for women with one child. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.12: **Fathers with youngest child btw 6-14 - 2013 Treated municipalities**

| | Labor force participation | Part-time | Hours worked per week | Days worked per week | Working on Wednesday |
|---|---|---|---|---|---|
| Treatment | 0.00333 | -0.00671 | -0.263 | 0.0142 | 0.006 |
| | (0.0178) | (0.01) | (0.594) | (0.037) | (0.0284) |
| Observations | 25255 | 22827 | 22827 | 22827 | 7587 |
| $R^2$ | 0.123 | 0.128 | 0.198 | 0.169 | 0.080 |
| F | 2.886 | 1.401 | 5.894 | 1.518 | 2.458 |
| Pre-treatment mean | 0.96 | 0.04 | 42.2 | 5.05 | 0.76 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform on fathers' employment decisions, obtained from the estimation of regression 3.6. The different columns refer to the outcome considered, being respectively labor force participation, column 1, the decision to work parti-time, column 2, number of weekly hours, column 3, and number of days worked per week, column 4. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household. The estimation sample comprises all fathers whose youngest child is between six and fourteen years old, and live in municipalities that introduce the reform in 2013. In column 2, 3, 4, and 5 we consider only fathers who are employed at the time of the interview. Finally, in column 5 the sample is further restricted to the years 2013 and 2014 as the decision to work on Wednesday is not available for the previous waves of the FLFS.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.13: **Short-term consequences of the reform**

| | Task change | Training in the last quarter | Overtime hours |
|---|---|---|---|
| Treatment | 0.00603 | 0.0225 | 0.00315 |
| | (0.0297) | (0.0220) | (0.0184) |
| Observations | 25483 | 25451 | 25017 |
| $R^2$ | 0.148 | 0.170 | 0.076 |
| F | 5.559 | 20.69 | 4.341 |
| Pre-treatment mean | 0.15 | 0.14 | 0.06 |

Source: French Labor Force Survey 2009-2014.
Note: this table shows the effect of the reform on additional outcomes, such as the probability of changing task or position at work, the probability of engaging in training, and the probability of working overtime hours. The estimation sample comprises all mothers whose youngest child is between six and fourteen, and who live in municipalities that introduce the reform in 2013. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

## 2.8. Appendix

Table 2.14: **Number of days worked per week - Robustness checks**

| | Main regression | 2014 municipalities | DDD | Placebo |
|---|---|---|---|---|
| Treatment | 0.0956* | -0.0425 | -0.0424 | 0.0332 |
| | (0.0505) | (0.0305) | (0.0305) | (0.0573) |
| Treatment in 2013 mun. | | | 0.158*** | |
| | | | (0.0607) | |
| Observations | 25483 | 85186 | 109685 | 20400 |
| $R^2$ | 0.136 | 0.187 | 0.177 | 0.162 |
| F | 3.805 | 8.824 | 10.34 | 4.714 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the results of different robustness checks for the effect of the reform on the number of days worked per week. In column 1, we report the coefficient of the main specification, regression 3.6. Column 2 shows the coefficient of the impact of the reform in the year 2013/14, on mothers living in municipalities that postpone its introduction to the academic year 2014/15. In this column, we exclude mothers interviewed in the last quarter of 2014, as they are actually treated. Column 3 reports the impact of the reform, estimated from a triple-difference model, as specified in regression 2.4. In this column, the sample size comprises all mothers whose youngest child is between six and fourteen, irrespective of their municipality of residence. Finally, column 4 reports the estimated effect of a placebo reform. In this column the sample is restricted to mothers interviewed in the period before the implementation of the reform. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.15: **Youngest child btw 2-14 - 2013 Treated municipalities**

| | Labor force participation | Part-time | Hours worked per week | Days worked per week | Working on Wednesday |
|---|---|---|---|---|---|
| Treatment | 0.00333 | -0.0182 | 0.278 | 0.0814* | 0.0605*** |
| | (0.0163) | (0.0271) | (0.594) | (0.0487) | (0.0223) |
| Observations | 53461 | 39249 | 39249 | 39249 | 12867 |
| $R^2$ | 0.180 | 0.121 | 0.123 | 0.108 | 0.081 |
| F | 25.31 | 8.782 | 12.2 | 6.836 | 6.742 |
| Pre-treatment mean | 0.79 | 0.34 | 34.6 | 4.68 | 0.56 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the coefficients capturing the effect of the reform, obtained from the estimation of regression 3.6. The different columns refer to the outcome considered, being respectively labor force participation, column 1, the decision to work parti-time, column 2, number of weekly hours, column 3, and number of days worked per week, column 4. All regressions include age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household. The estimation sample comprises all mothers whose youngest child is between two and fourteen years old, and live in municipalities that introduce the reform in 2013. In column 2, 3, 4, and 5 we consider only mothers who are employed at the time of the interview. Finally, in column 5 the sample is further restricted to the years 2013 and 2014 as the decision to work on Wednesday is not available for the previous waves of the FLFS.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.16: **Decision to work on Wednesday - Importance of bargaining power - Other subgroups**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Short-term contracts | No prevalence of part-time contracts | Tenure $< 1$ year | Tenure $> 5$ years |
| Treatment | 0.047 | 0.023 | 0.088 | 0.034 |
| P-value | 0.177 | 0.620 | 0.132 | 0.241 |
| Pre-treatment mean | 0.55 | 0.62 | 0.66 | 0.57 |
| Observations | 8282 | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.
Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the effect for mothers with short-term contracts. Column 2 shows the effects for mothers working in occupations in which part-time contracts are not prevalent. Column 3 displays the impact on mothers with less than one year of tenure, and column 4 the effect on those with more than five years of seniority. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.
*** p<0.01, ** p<0.05, * p<0.1.

Table 2.17: **Decision to work on Wednesday - Importance of cost of flexibility - Other subgroups**

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | High cost of flexibility | Intermediary occupations | Managerial occupations | Public sector |
| Treatment | 0.017 | 0.062 | -0.115 | -0.000 |
| P-value | 0.641 | 0.227 | 0.102 | 0.989 |
| Pre-treatment mean | 0.57 | 0.55 | 0.57 | 0.54 |
| Observations | 7365 | 8209 | 8209 | 7465 |

Source: French Labor Force Survey 2009-2014.
Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the effect for mothers working in occupations characterized by a high cost of flexibility. With low and high cost of flexibility, we refer to the composite score we assign to occupations depending on the importance of certain aspects for these professions, as defined by the O*NET online platform. In detail, the score is an average of the standardized scores given to five factors, namely time pressure, frequency of decision making, structured versus unstructured work, contact with others, establishing and maintaining interpersonal relationships. A detailed description of these characteristics and the score assigned to them is given in section 2.5. We regroup women's occupations in two groups, depending on whether the average score is below or above the median for the entire sample. Next, columns 2 and 3 display the effects on women working, respectively, in intermediary and managerial occupations. Finally, column 4 refers to the impact on women working in the public sector. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.18: **Decision to work on Wednesday - Parents' education**

| | Mother's education | | | Father's education | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Low | Secondary | High | Low | Secondary | High |
| Treatment | 0.003 | 0.122*** | 0.008 | 0.033 | 0.103** | 0.010 |
| P-value | 0.960 | 0.008 | 0.838 | 0.422 | 0.0255 | 0.850 |
| Pre-treatment mean | 0.63 | 0.56 | 0.58 | 0.67 | 0.58 | 0.54 |
| Observations | 8282 | 8282 | 8282 | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Columns 1 to 3 reports the effects on mothers with different levels of education. Columns 4 to 6 display the effects on mothers depending on their partner's education. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 2.19: **Decision to work on Wednesday - Mother's characteristics**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Single | Married | Young | Old | 2 children | $\geq 3$ children |
| Treatment | 0.059 | 0.052* | 0.204 | 0.043 | 0.003 | 0.083 |
| P-value | 0.385 | 0.098 | 0.196 | 0.109 | 0.927 | 0.496 |
| Pre-treatment mean | 0.61 | 0.57 | 0.53 | 0.59 | 0.57 | 0.58 |
| Observations | 8282 | 8282 | 8282 | 8282 | 8282 | 8282 |

Source: French Labor Force Survey 2009-2014.

Note: this table shows the effect of the reform on the decision to work on Wednesday for different subgroups. Column 1 reports the effect on single mothers and column 2 the impact on married mothers. Columns 3 and 4 display the effects on younger and older mothers. Columns 5 and 6 show the impact on, respectively, mothers with two children and those with three children or more. To conduct this analysis, we choose to estimate a regression on the entire sample in which all regressors are interacted with the subgroup considered, except for municipality fixed effects. Otherwise, all regressions include the standard covariates, namely age and age square, marital status, number of children, a dummy for immigration status, municipality and wave fixed effects, dummies for the level of education, and a dummy for the presence of other members in the household.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

# Chapter 3

# Does Employment Protection Legislation Affect Training Investments? Evidence from the United Kingdom

## 3.1. Introduction

”Given that fixed-term contracts are generally short, firms do not invest in training their temporary workers” declares, on the 15 March 2016, Olivier Blanchard to *Le Monde*, a renowned French newspaper. ”Many firms do not invest in their workers given that they are going to dismiss them after two years”, claims Albert Rivera, secretary of the Spanish party ”Ciudadanos”, at the popular TV program *El Objetivo*, on the 22 November 2015.

The theoretical argument behind these claims is straightforward: in a flexible labor market, the expected duration of an employment relationship is shorter than in a context characterized by rigid employment protection legislation (EPL hereafter). As a consequence, the incentives to invest in this relationship become weaker both for the employer and the employee. Investment in training should drop in this context. In turn, this should negatively affect the overall productivity of the economy (Delacroix and Wasmer 2007, Belot, Boone, and Van Ours 2007, Suedekum and Ruehmann 2003).

Despite the importance of this topic in the current political debate, there exists basically no rigorous evidence on the empirical relevance of this argument. There are several reasons why this is so. First, comparing workers who are subjected to different EPL regimes might confound the effect of this legislation with that of unobservable characteristics that differ between workers in permanent and temporary contracts. Secondly, exploiting reforms of EPL as natural experiments to identify the effect of interest presents the drawback that these interventions are likely to affect contemporaneously workers and job flows, and this makes it more difficult to isolate the effect on training investments. Finally, it has to be taken into account that training levels have been historically very low precisely in most of the countries that have recently undertaken major EPL reforms, such as Italy or Spain.

The United Kingdom offers a setting that allows to overcome most of these identification problems. First, its EPL and in particular dismissal laws equally apply to both fixed-term and permanent contracts. In this context, firing costs do not vary across contracts, but rise with seniority, after an initial probationary period. Secondly, the United Kingdom is one of the few countries – at least in Europe, where workers receive high doses of training, as shown in figure 3.1. Third, the British parliament approved in April 2012 a reform of EPL, by extending in particular the length of the probationary period from 12 to 24 months of tenure. This framework offers the opportunity to study several issues.

First, it allows me to bring new evidence on the effect of firing laws on workers' flows, and in particular on firing hazards, in the aftermath of a severe recession. The theory and past empirical contributions on this topic suggest that this reform, by lowering firing costs for firms, should increase the firing hazard of workers with 12 to 23 months of seniority. Moreover, it could possibly decrease the firing hazard of workers with less than 12 months of tenure, as some dismissals might be postponed to the second year. Nonetheless, the coincidence of this reform with a period of wage moderation might mitigate or even neutralize its effects (Disney, Jin, and Miller 2013).

Secondly, this setting gives me the possibility to analyze how the existence of a probationary period affects the timing and levels of training investments. In principle, we would expect training to be particularly important in the first months of an employment relationship. However, firms might be reluctant to invest in training workers until their quality is completely revealed during the probationary period. No evidence exists on this issue.

Finally, this reform gives me the chance to study how extending a probationary period affects both firing and training decisions of British firms. Regarding the latter, as the expected duration of the employment relationship decreases, firms might decide to further postpone some training investments. However, it is not obvious that they will be willing to do so for as long as two years, the length of the new probationary period.

To test all these hypothesis, I use the 2009-2015 two-quarter longitudinal version of the UK Labor Force Survey. The descriptive analysis of the timing of training shows that, despite the existence of a probationary period, up to 25 percent of newly hired workers engage in training in the first two months in a firm, and this proportion steeply decreases to fall below 15 percent for workers with four years of tenure. To study the effect of the 2012 reform of EPL, I adopt a difference-in-difference strategy. In detail, I compare the evolution of the firing hazard and training levels for workers with 12 to 23 months of tenure, with that of employees with 24 to 48 months of tenure, who are not affected by the reform. In a separate regression, I use this same control group to study how the reform affects these outcomes for workers with less than 12 months of tenure. My results suggest that the reform does not affect either the firing hazard or training levels for workers with 12 to 23 months of seniority. However, I find evidence that the proportion of workers engaged in training increases by roughly 10 percent for employees with less than 12 months of tenure. This might be due to the fact that the decrease in expected firing costs liberates resources that businesses decide to invest in training the newly hired.

The 2012 reform follows a previous intervention, implemented in 1999, that prescribed exactly the opposite, that is to reduce the probationary period from 24 to 12 months of seniority. Marinescu (2009) analyzes the effect of that reform on dismissals up to 2004 and finds that the 1999 reform substantially decreases the firing hazard of workers with 12 to 23 months of tenure. Moreover, the shortening of the probationary period lowers the firing hazard for workers with less than 12 months of tenure, as well. Marinescu (2009) interprets this last finding as evidence that firms improve their recruiting practices in response to the increase in expected firing costs, determined by the reform.

There are at least two factors that might prevent the 2012 reform from having the opposite effect of the 1999 one. First, in the United Kingdom, the 2008 recession is followed by a slow recovery phase characterized in particular by a low wage growth (Disney, Jin, and Miller 2013). Secondly, if firms have developed good recruitment practices following the 1999 intervention, it might take some time to disinvest in the selection process, or firms might simply be unwilling to do so. Both these factors might neutralize the potential negative effects of the reform, at least in its first year of implementation.[32]

Regarding training, my findings are encouraging with respect to the introduction of the so-called unique contract, as it is currently discussed in many European countries. The existence of a probationary period in the United Kingdom – and envisaged in the various proposals for the unique contract – does not seem to prevent businesses from investing in training employees in this phase. Moreover, an overall decrease of expected firing costs might allow firms to invest more resources in productive activities, such as training. Nonetheless, these results do not reject the possibility that in a dual labor market, workers in fixed-terms contracts might be penalized not only in terms of job security, but also with respect to the access to training, as suggested by Cabrales, Dolado, and Mora (2014).

The paper proceeds as follows. Section 3.2 describes the theoretical

[32] The reforms starts binding for employees with more than 11 months of tenure only in 2013, which means that I can rigorously analyze the impact it has only along the first year.

framework and reviews the literature on the effect of EPL. Section 3.3 presents the descriptive analysis of the data and the regression analysis on the impact of the 2012 reform. Section 3.4 discusses the results and section 3.5 concludes.

## 3.2. Theoretical framework

### 3.2.1. Goals and effects of Employment Protection Legislation

Employment protection legislation (EPL) is a set of norms governing the hiring and dismissal of employees. Historically, EPL has been designed to protect jobs and increase job stability, by limiting job destruction. Firing restrictions can also be rationalized in the presence of financial market imperfections that limit the ability of risk-averse workers to get insurance against dismissal (Boeri, Cahuc, and Zylberberg 2015, Pissarides 2001).

However, by limiting job destruction, EPL may entail a trade-off between allowing an efficient reallocation of workers and the need to protect employees. By now, there exist several studies showing, both from a theoretical and empirical perspective, that strict EPL reduces workers and jobs flows (Bassanini and Garnero 2013, Blanchard and Portugal 2001, Boeri and Jimeno 2005, David, Kerr, and Kugler 2007, Hopenhayn and Rogerson 1993, Kugler, Jimeno-Serrano, and Hernanz 2013, Kugler and Saint-Paul 2004, Kugler and Pica 2008, Marinescu 2009, OECD 1999, OECD 2004, Schivardi and Torrini 2008, Von Below and Thoursie 2010).

Besides workers and job flows, EPL can also influence the levels of employment and unemployment, but the direction of the effect is more ambiguous in this case, as it depends on whether EPL affects more hiring or layoffs. Most empirical studies so far show that restrictive EPL has a negative effect on employment levels, but there is less consensus on its impact on unemployment (Lazear 1990, Autor, Donohue III, and Schwab 2006, Behaghel, Crépon, and Sédillot 2008).

Autor, Donohue III, and Schwab (2006) and Kugler and Pica (2008) show that strict EPL can further affect the composition of the workforce, by increasing the chances for employed workers to stay in their jobs and reducing the chances for those without jobs to find employment. This implies that restrictive EPL might be particularly harmful for young workers when entering the labor market, and for women who tend to exit and reenter the labor market more frequently than men.

In addition, in the presence of rigid EPL on permanent contracts, employers may choose to hire more workers with fixed-term contracts or substitute capital for labor. In this respect, Blanchard and Landier (2002) provide evidence that the progressive liberalization of temporary contracts that took place in the 1980s in France led to increase flows from unemployment to fixed-term contracts, and to decrease flows from unemployment and fixed-term contracts to regular contracts. As a consequence, the share of temporary employment in the workforce increased. Similar findings have been encountered for the United States by Autor (2003) and for Spain by Dolado, García-Serrano, and Jimeno (2002). Moreover, David, Kerr, and Kugler (2007) show that the introduction of exceptions to the employment-at-will doctrine in some states of the United States induced employer to rely more on capital at the expense of labor in the production process.

The coexistence of strict EPL for workers in open-ended contracts and lighter regulation for temporary contracts may further affect the wage-setting process with knock-on effects on employment prospects of "outsiders" on short-term contracts. This can happen because the insiders can raise their wage claims, without fear of loosing their jobs, while any negative consequence on employment will be borne by the outsiders (Bentolila and Dolado 1994). Two recent studies, Van der Wiel (2010) and Leonardi and Pica (2013), conducted, respectively, on the Netherlands and Italy, provide empirical support for this theoretical prediction.

Importantly, through all the mechanisms just described, EPL can affect productivity. However, the theory does not offer any clear-cut prediction on the sign of this effect. On the one hand, there are several reasons why restrictive EPL may hinder productivity. First, this can happen to the extent that EPL reduces the ability of firms and workers to fully respond to market shocks and to reallocate from declining to growing sectors (Hopenhayn and Rogerson 1993); this channel can be especially important in industries characterized by rapid technological changes (Bertola 1994). Secondly, in the presence of rigid EPL, businesses may be discouraged from investing in projects that may have higher added value but face more volatile demand and thus require greater flexibility (Saint-Paul 2002). Third, high firing costs might force employers to retain unproductive workers. Finally, knowing that it is hard to fire them, employees may have less incentive to work hard in their jobs, and this can further lower productivity.

On the other hand, strict EPL can enhance labor productivity through at least three channels. First, it might induce employers to improve their recruitment practices and hence raise the quality of job matching, as suggested both by Marinescu (2009) and Malcomson (1999). Second, if higher labor costs induce businesses to shift away from hiring labor towards investing more in capital, this can eventually raise labor productivity – even though it can also decrease total factor productivity. Third – and this is what is tested in this paper – rigid EPL, by raising the expected duration of a job, can make both the employer and the employee more willing to invest in training (Acemoglu and Pischke 1996, Acemoglu and Pischke 1999, Delacroix and Wasmer 2007, Belot, Boone, and Van Ours 2007, Suedekum and Ruehmann 2003).

It is important to take into account that both investment in firm-specific skills and investment in general training may be affected by EPL.[33] Regarding firm-specific skills, it is logical to expect that their value will rise, both for the employer and the employee, as the expected duration of the employment relationship increases. However, EPL may also have an impact on general training and this is a priori more ambiguous. On the one hand, the seminal papers by Acemoglu and Pischke (Acemoglu

[33] To be clear, here I consider general human capital to include all skills that are identically useful to many firms, including the training company. In contrast, I define as firm-specific skills those that increase productivity only in the firm in which they are acquired.

and Pischke 1996, Acemoglu and Pischke 1999) prove that any factor that augments frictions in the labor market and generates wage compression can push firms to finance general training.[34] If the 2012 reform reduces labor market frictions, it might then weaken firms' incentives to provide general training. On the other hand, we might expect that, in a more flexible labor market where job mobility is enhanced, workers might be more willing to invest in general training, in order to be prepared for new job opportunities. The overall impact of the reform on general training is therefore unclear.

If the theory on the impact of EPL on productivity, and training investments in particular, gives ambiguous predictions, the empirical evidence is still too scarce to offer any convincing answer on the matter. In a cross-country study, Scarpetta and Tressel (2002) find that restrictive EPL reduces productivity growth in countries where wages do not offset higher firing costs. In an industry-level cross-country Bassanini, Nunziata, and Venn (2009) provide suggesting evidence that strict EPL has a depressing impact on productivity growth in industries where layoff restrictions are more binding.[35] Cabrales, Dolado, and Mora (2014), using PIAAC data for Spain, show that the large gap in employment protection between indefinite and temporary workers that characterizes the Spanish labor market is accompanied by large differentials in on-the-job training against the latter. They further provide cross-country evidence showing that on-the-job training gaps are quite lower in those European labor markets where dualism is less entrenched than in those where it is more extended. Autor, Donohue III, and Schwab (2006) show that the strengthening of EPL in some US states, by inducing firms to substitute capital for labor, leads to an increase in labor productivity. Cingano, Leonardi, Messina, and Pica (2013) obtain similar results using Italian data to examine a 1990 reform

[34] Wage compression denotes a situation in which training boosts productivity more strongly than pay, creating a wedge between the two that increases with the level of skill.

[35] Bassanini, Nunziata, and Venn (2009) identify these industries to be those where, in the absence of regulations, firms rely on layoffs to make staff changes. EPL should instead be less binding in industries where internal labor markets or voluntary turnover are more important.

that raised dismissal costs for firms with fewer than 15 employees only. Finally, Ichino and Riphahn (2005) suggest that strict EPL might indeed reduce workers effort, as they find evidence that increased job security in the Italian banking sector increases employees' absenteeism.

To conclude this literature review, it is important to notice that all the mechanisms described here are likely to be influenced by the context in which EPL is introduced or modified. In particular, the degree of wage stickiness, as well as the level of complexity of product market regulation may influence the actual impact of EPL on the variables considered so far. As for the former, when job security comes at the expense of low wage growth, its negative effects on workers flows and employment levels might be mitigated (Bertola and Rogerson 1997). Concerning the interplay between labor and product market regulations, both Krueger and Pischke (1997) and Kugler and Pica (2008) suggest that the effectiveness of a reform that removes or relaxes employment protection regulation will be smaller in a country with heavy administrative burdens on firms or strong restrictions on firms entry.

### 3.2.2. Tenure-dependent job security

The United Kingdom is a particularly interesting case to study, as it proposes an EPL model that aims at eliminating the segmentation between insiders and outsiders, while maintaining the possibility for the employer to hire workers on contracts with different lengths. In particular, workers rights at termination are equalized across contracts, and rise with seniority. However, unlike the ”single contract”, that many European countries are currently envisaging to adopt, the so called ”unified contract” that the United Kingdom has adopted does not entail an immediate gradual increase in termination costs with tenure. On the contrary, this contract prescribes a probationary period – whose length has been repeatedly modified – during which workers have neither the right to claim unfair dismissal nor are entitled to receive severance payments in case of dismissal. Only after this probationary period, workers obtain the right to sue their employer for unfair dismissal, and get entitled to severance payments that smoothly increase with years of seniority. The rational behind a probationary period is, on the one hand, to allow the firm to have a certain period of time to evaluate the quality of the new hire, without incurring in any cost in case the employment relationship results to be a bad match. On the other hand, the unified contract allows all workers to enjoy an increasing level of job protection after a relatively short period of time.

Marinescu (2009) clearly describes how this type of contract can affect hiring practices, monitoring efforts and the timing of dismissals. With respect to the recruitment process, the shorter the probationary period, the more firms should be willing to invest in the selection strategy to identify the best workers. At the same time, firms might decide to better monitor workers once they are hired, as the probationary period gets shorter. Concerning dismissal decisions, the existence of a large firing-cost wedge between the probationary period and the years following it, should lead the firing hazard to be much higher in this period than afterwards. Moreover, the firing hazard as a function of tenure may exhibit a spike right before the end of the probationary period if firms anticipate some dismissals to avoid incurring in higher firing costs after this period. Finally, the firing hazard in the probationary period should decrease as recruitment practices improve, while it should increase as monitoring efforts rise in this period. Marinescu (2009) exploits a reform of this legislation to test these predictions. In 1999 the British government shortened the probationary period from 24 to 12 months of tenure. The author finds that this intervention significantly decreased the hazard of termination for workers with 12 to 23 months of tenure. She also provides evidence that the firing hazard decreases for employees with less than 12 months of tenure, which is consistent with firms having increased the quality of new recruits after the policy change. In addition, she mentions that unemployment duration decreases after this policy change, training increases, and wages are unaffected, but she does not dedicate more space to these outcomes, as they are not the focus of her study.

In light of the increasing attention given to the practice of on-the-job training as a tool to increase labor productivity, and allow both firms and workers to cope better with rapid technological changes, it appears especially important to understand how EPL affects training decisions. The last reform of the length of the probationary period in the British unified contract offers the possibility to do so.

In April 2012, the British government approves a reform that restores the pre-1999 regime, by bringing back the length of the probationary period to 24 months for all employees hired from April onward. As a consequence, workers reaching 12 months of tenure in April 2013 up to those reaching 23 months in March 2014 loose the right to sue their employer for unfair dismissal and the entitlement to severance payments.

Theoretically, it is not clear how the presence of a probationary period affects training decisions. On the one hand, as the expected duration of the employment relationship steeply increases after this initial period, both the employer and the employee might prefer to postpone training investments to the post-probationary phase. On the other hand, firms might be willing to provide the newly hired the necessary training to make them operative. In addition, firms might want to use training as a screening device during the probationary phase, which would lead to observe a higher level of training in this period than afterwards. Studying how training decisions are affected by the presence of a probationary period is the first objective of this study. Next, it is also interesting to understand whether changing the length of the probationary phase influences the timing of training investments. In particular, the 2012 reform might induce firms to reduce training investments for workers affected by the intervention. However, it might also be the case that certain training practices cannot be delayed substantially, in which case such a reform would not affect much training levels for workers who see their job protection decreasing.

Importantly, the reform does not directly affect workers with less than 12 months of tenure, who were not entitled to any right at termination even before this intervention. Nonetheless, the 2012 intervention might also influence training decisions regarding this group of workers, and it is a priori not clear in which sense. On the one hand, the extension of the probationary period decreases the expected duration of the match for these employees as well. In addition, if firms choose to decrease their investment in recruitment practices in response to the extension of the probationary period, the expected quality of a match might decrease. These two mechanisms can make businesses even more reluctant to invest in training in the initial phase of the probationary period. On the other hand, the fact that expected firing costs decrease in the aftermath of the reform might induce firms to reallocate some resources to train new hired workers who are more likely to lack firm-specific skills. Hence, the third goal of this paper is to study how the reform affects training levels for workers with less than one year of tenure.

Finally, it is clear that any impact this reform might have on training practices will primarily depend on the effect that it has on hiring and firing decisions. Therefore, as a first step to understand all these mechanisms, this paper will provide a joint analysis of firing and training dynamics.

## 3.3. Empirical strategy

### 3.3.1. Data and descriptive analysis

To study the relationship between the UK EPL model and firing and training decisions of British firms, I use the 2009-2015 waves of the two-quarter longitudinal version of the UK Labor Force Survey, LFS, that offers the possibility to follow each individual for two consecutive quarters. I consider only individuals who are employed in the first quarter they are interviewed, as this allows me to study their firing hazard. Moreover, I restrict the sample of study to employees working at least 16 hours, as the reform only applies to this group of workers. Finally, I exclude workers on temporary contracts, as it is very rare to find some with more than one year of tenure.

The LFS gathers information on a wide range of labor force characteristics and related topics. In particular, years and months of tenure in a firm can be calculated for more than 99 percent of the sample.

Moreover, the longitudinal structure of the data set can be exploited to calculate the fraction of workers who get fired or dismissed in each tenure group, among those who are employed in the first quarter they take part to the survey. To do so, I proceed in this way. First, I identify individuals who are employed in the first quarter, and are either unemployed or in a new job in the second. Then, I use the fact that individuals who have left their job are asked for the reason why this happened, as this should help me to distinguish dismissals from other forms of separations. Table 3.1 shows the distribution of the answers to this question for individuals interviewed before April 2012. It has to be noticed that among workers leaving their job during the first quarter they are interviewed, around 30 percent of individuals declare that they have been dismissed or made redundant. Therefore, the first outcome of my analysis is a dummy equal to one for workers interviewed in the first quarter who have separated from their job between the first and the second time they are interviewed and declare to have been dismissed or made redundant. However, I also look at the effect of the reform on the overall separation hazard, without distinguishing for the reason of the separation. The first variable might indeed underestimate the actual number of workers being dismissed, if some individuals prefer to declare they have quit a job when they have actually been fired.[36] Figure 3.2 shows the evolution of the hazard of termination and the firing hazard as a function of seniority until 48 months of tenure, before and after April 2013, when the reform actually becomes binding. Note indeed that the modification of EPL introduced in April 2012 applies only to newly hired employees. Therefore, starting in April 2012, this intervention can only indirectly affect workers with zero months of tenure, up to those who reach 11 months of seniority in March 2013. However, the first workers who are directly affected by the reform are those who reach 12 months of tenure in April 2013, until those who reach 23 months of seniority in March 2014.

Three things are worth noticing in figure 3.2. First, focusing on the pre-reform period, represented in red, in line with the theoretical predictions described above, both the hazard of separation and the firing hazard

[36] Note also that five percent of individuals declare that their job ended because it was a temporary job, even though the sample comprises only individuals who had a permanent job. This supports the hypothesis that some individuals might give elusive answers to this question.

are higher in the probationary period than afterward. Moreover, the latter exhibits a spike right before the 12 months threshold where firing costs increase discontinuously. Secondly, in the post-reform period, the hazard of separation presents this jump at 23 months, when the length of the new probationary period terminates. Such bunching is less evident in the firing hazard, but clearly the spike at the end of the first year gets attenuated in this curve as well. Third, the firing hazard is lower for any tenure group in the post-reform period, which might be due to the fact that this period coincides with the recovery phase from the 2008 financial crisis. Finally, from these graphs it is hard to expect to find an impact of the reform on dismissals of workers with less than 24 months of tenure, but this will be the subject of the next paragraph.

Turning to training, the survey contains a series of questions on this topic. In particular, it asks whether the individual engaged in education or training related to his job in the past four weeks, from which I derive the binary variable ”Training in the last four weeks” – reported simply as ”Training” in the tables. Then, to those who answer positively to this question, the survey also asks to report whether the training was done at the workplace, outside the workplace, or both. From this last question, I create the variables called ”Workplace”, ”Off-site” and ”Mixed”. In the absence of more precise information on the content of the different forms of training and on who pays for them,[37] here I assume that workplace training is more likely to provide firm-specific skills than its alternatives.

Tables 3.2, 3.3, 3.4, and 3.5 present descriptive statistics for these variables, separated for different tenure groups, 3.2, several categories of workers, 3.3, and also split by occupation, 3.4, and industry, 3.5. These figures refer to individuals interviewed before the introduction of the reform, i.e. April 2012, who are employed, in a permanent job, and working at least 16 hours per week. Note that, despite being in the probationary period, employees with less than one year of tenure are slightly more likely to receive training than other groups, as shown in table 3.2. Moreover, it is interesting to notice that a higher proportion of female workers engage

[37] The LFS contains a question asking who pays the training fees, but the answer rate is too low for this information to be used.

in training than their male counterparts, as indicated in table 3.3. This is probably due to the fact that individuals working in the education and health sectors, who are mostly women, tend to receive more training than workers in other industries, as shown in table 3.5.

Figure 3.3 describes in detail the evolution of the probability of engaging in (the different forms of) training as a function of tenure, before and after the 2012 reform starts binding. The first thing to be noticed, in line with what is shown in table 3.2, is that training levels are much higher in the first months of the employment relationship, and then decrease steeply, at least for what concerns workplace training. This suggests that, despite the existence of a probationary period, firms are willing to provide new employees the skills needed to make the employment relationship productive.[38] Next, comparing the dynamic of workplace training, training outside the workplace, and combined training, it is interesting to notice that the declining pattern is evident only for workplace training. This might support the hypothesis that it is precisely this form of training that provides firm-specific skills, i.e. those skills that newly hired workers are more likely to lack. Moreover, this pattern is consistent with the hypothesis that this type of training might be used by the firm also to screen newly hired workers. Finally, it does not seem that training levels decrease after the implementation of the reform for workers affected by the reform. However, the regression analysis will offer a clearer answer on this point.

### 3.3.2. Regression analysis

To study whether the 2012 reform affects the dynamics of dismissals and training as a function of seniority, I adopt a difference-in-difference strategy. In detail, I choose to compare the evolution of the outcomes of interest for employees who are directly affected by the reform, those with 12 to 23 months of seniority, with that of workers who have between 24 and 48 months of tenure, using the following specification:

[38] Nonetheless, training levels might be even higher in the absence of a probationary period.

$$
\begin{aligned}
Y_{itm} &= \gamma_m + \delta_t + \pi * X_{itm} + \\
&+ \beta * Tenure\_btw\_12\_23m * Post\_April\_2013_{ct} + u_{itm}
\end{aligned} \tag{3.6}
$$

Here $Y_{itm}$ is either a dummy equal to one if the employee gets dismissed in the first quarter he takes part to the survey, or a dummy equal to one if the worker declares to have recently engaged in training; $gamma_m$ represents month fixed effects, $delta_t$ stands for tenure fixed effects, and $X_{itm}$ is a vector of covariates that includes dummies for sex, marital status, age groups, educational levels, full-time status, occupation and industry fixed effects.

The main regressor is $Tenure\_btw\_12\_23m * Post\_April\_2013_{ct}$ that takes value one for workers with 12 months of tenure from April 2013 onward, it switches to one for those with 13 months of tenure in May 2013 and so on, till March 2014 when it becomes one for workers with 23 months of seniority. This variable is instead always equal to zero for employees with 24 to 48 months of tenure. Therefore, $\beta$ should capture any deviation in the evolution of separations or training levels between the treated and the control group that is due to the implementation of the EPL reform. Importantly, to estimate this regression, I restrict the sample to individuals interviewed before April 2014, that is I study only the impact of the reform in its first year of implementation. This is to avoid that the control group gets to include employees who reach the two years of tenure after the reform, and therefore are potentially affected by its introduction.[39] In addition, the estimation sample does not include workers with less than 12 months of tenure, as the reform is likely to affect them and this possibility will be studied in a different regression. Moreover, I exclude observations from the second quarter of interview,

[39] In the appendix I also show the results of a regression that I estimate to study the effect of the reform for the entire period the data are available, i.e. April 2015. To do so, I keep in the control group only workers with 36 to 48 months of tenure, as these are all hired before the introduction of the reform. As shown in tables 3.18 and 3.19, the results of this specification look similar to the main ones.

as without observing individuals longer, I cannot compute the hazard of separation. Finally, in all the regressions, I use robust standard errors.

Figures 3.4 and 3.5 show the evolution of all the outcomes of interest over the period considered for the treatment and control group. Overall, the patterns look similar both for separation and training levels, especially in the pre-treatment period and for workers with 12 to 48 months of tenure. Moreover, note in figure 3.4 that, consistent with what is shown in the second panel of figure 3.2, dismissals exhibit a declining trend, unrelated to seniority. It is also interesting to notice that workplace training tends to increase for all workers over the period considered, second panel of figure 3.5. This is consistent with the theoretical prediction that, as dismissals decline, both firms and workers become more willing to invest in firm-specific human capital. However, in the third panel of figure 3.5, we can observe that training outside the workplace tends to decrease over this period, which goes against the theoretical predictions of Acemoglu and Pischke (Acemoglu and Pischke 1996, Acemoglu and Pischke 1999).

Regarding the effect of the reform, the dynamics of separations do not seem to be affected by the modification of EPL, at least in its first year of implementation. On the contrary, there appear to be some indications that training levels, and especially training conducted outside the workplace, increases – at least temporary – after the reform for workers with 12 to 23 months of tenure. This would be consisted with the hypothesis that workers are more willing to acquire general skills in a context in which more flexibility may favor mobility.

### 3.3.3. Workers directly affected

Table 3.6 shows the estimates of regression 3.6 for the hazard of separation and the firing hazard. These results suggest that extending the probationary period does not affect the odds that an employment relationship ends, and in particular it does not seem to influence firms' firing decisions.

Regarding training decisions, in line with the patterns seen in the graphs, the point estimates in table 3.7 suggest that training levels, and especially training conducted outside the workplace increase in the post-reform period, but the coefficients are not significant for any of the outcomes considered.

To test for the validity of the identification strategy, I also conduct a series of robustness checks, depicted in tables 3.8 and 3.9. In the first row of each table, I report the main results. In the second row, I add tenure-specific time trends to regression 3.6 and this does not seem to alter substantially the main coefficients and, above all, their significance. The main results are not sensitive to the choice of the control group either, as shown in the third row of tables 3.8 and 3.9, where the control group is restricted to employees with 24 to 36 months of tenure. Finally, in the last row of these tables, I report the impact of a placebo reform, where I pretend that the new legislation starts binding in April 2011 and I exclude the actual post-reform period – from April 2012 onward. The fact that none of the coefficients shows up significant suggests that in the main regressions I am not capturing the effect of persistence differences in the evolution of the outcomes of interest between the treatment and the control group.

Next, in tables 3.10 and 3.11, I study whether the reform affects differently workers who are close to the firing-cost wedge, compared with those having less seniority. Table 3.10 shows that the firing hazard seems to increase for employees with less than 16 or more than 19 months of tenure, but none of the coefficients shows up significant or statistically different from the other two.

Regarding training, table 3.11 provides some evidence that the timing and type of training is affected by the reform. In particular, workplace training might be in part anticipated from the last months of the second year to its central months – even though only the coefficient on workers with 20 to 23 months of tenure is significant and significantly different from the others.[40] This is consistent with the hypothesis that training might be used by businesses to screen workers when it is still easy to

[40] The p-values for the equality of this coefficient with that on workers with 12 to 15 months of tenure, and with that on workers with 16 to 19 months of seniority are both equal to 0.000.

dismiss them.[41] At the same time, training outside the workplace seems to increase in particular for employees at the beginning of their second year in the firm.[42] This might reflect the fact that precisely those workers who still face one year of probationary period might feel the need to acquire more general skills, in case their employment relationship were to end.

Finally, in tables 3.12, 3.13, 3.14 and 3.15, I present the subgroup analysis. Investigating whether this reform has heterogeneous effects is particularly important in light of the evidence provided by the literature that augmenting the flexibility of the labor market might be particularly harmful for certain groups, such as female and young workers. Tables 3.12 and 3.13 show that the reform does not seem to have heterogeneous effects with respect to separations.[43] Concerning training, there is some evidence that the impact of the reform might indeed be heterogeneous, as shown in tables 3.14 and 3.15. A few groups, notably males, workers with at least upper secondary education, young employees, employees working part-time, and those working in the public sector seem to react to the reform by investing more in training outside the workplace – even though only the coefficient on male workers shows up significant.[44] These groups – with the exception of those working in the public sector – might have in common a higher propensity to move. For this reason, they might be more willing to invest in general training, as soon as the labor market becomes more flexible. Females and low educated workers, as well as the highly educated, seem instead more likely to engage in workplace training following the implementation of the reform. This would appear

[41] Even though, there is no clear evidence that this increases the firing hazard right before the 23 months threshold.

[42] In the third column of table 3.11 the coefficient on workers with 12 to 15 months of tenure is statistically different from the one on workers with 16 to 19 months of seniority, but not from the one on workers with with 20 to 23 months of tenure.

[43] Except for workers with upper secondary education and those working in the public sector. However, the fact that only these coefficients show up significant might be the result of multiple testing, more than the true effect of the reform.

[44] Moreover, according to a Chow test, the coefficients of the males' regression are jointly statistically different from those of the females' regression, with a p-value equal to 0.0002.

at odds with the theoretical predictions, and indeed the coefficients are not statistically significant.

### 3.3.4. Workers indirectly affected

To study whether this reform has an indirect effect on workers with less than 12 months of tenure, I estimate the following regression

$$\begin{aligned} Y_{itm} &= \gamma_m + \delta_t + \pi * X_{itm} + \\ &+ \beta * Tenure\_btw\_0\_11m * Post\_April\_2012_{ct} + u_{itm} \end{aligned} \tag{3.7}$$

where I restrict the estimation sample to workers with less than 12, or 24 to 48 months of tenure. Moreover, I exclude from the sample individuals interviewed after March 2013. This is to avoid that the treatment group includes workers who, starting from April 2013, enter in a new job after having being dismissed because of the reform.

The main regressor here, $Tenure\_btw\_0\_11m * Post\_April\_2012_{ct}$, takes value one for workers who start a new job from April 2012 onward, it switches to one for those who are interviewed in May 2012 and have one month of tenure at that time and so on, till March 2013 when it becomes one for workers with 11 months of seniority. This variable is instead always equal to zero for employees with 24 to 48 months of tenure.

Table 3.16 shows that firms might indeed postpone some dismissals from the very first months of the employment relationship once the probationary period is extended, as the firing hazard of workers with less than 11 months of tenure significantly decreases by almost 30 percent. However, it must be said that this result is not very robust. As shown in the appendix, table 3.20, this coefficient decreases in magnitude and looses significance both when I include tenure-specific time trends and when I restrict the control group.

Concerning training, as shown in table 3.17, there is evidence that training levels, and especially training that is done both at the workplace and outside, increase for this group, with the introduction of the reform. Such result is robust both to the restriction of the control group and to the introduction of tenure-specific time trends, as shown in table 3.21, in the appendix. Moreover, the subgroup analysis – reported in tables 3.22, 3.23, 3.24 and 3.25, in the appendix – does not point to the presence of heterogeneous effects, except for what concerns educational levels. As shown in table 3.24, the reform does seem to increase training levels for workers with at least a high-school degree, but not for those with lower education. This finding is consistent with the hypothesis that a decrease in expected firing costs leads firms to redirect some resources to productive activities, such as training for the most skilled among the newly hired.

## 3.4. Discussion

### 3.4.1. Economic arguments

My results show that the firing hazard of treated workers does not increase following the implementation of the 2012 reform. In other words, this intervention does not produce the opposite effects as the one introduced in 1999, and analyzed by Marinescu (2009). There are at least two factors that might explain this. First, in the United Kingdom the 2008 recession is followed by a slow recovery phase characterized in particular by a declining trend in real-wages, as shown in figure 3.6. Bertola and Rogerson (1997) show that wage rigidity tends to undo the potential effects of EPL on employment flows. This is because, all else being equal, if wages cannot adjust to market shocks, firms have to respond by adjusting more their labor force. This implies that in a period in which real wages are falling, firms might be less inclined in firing their workers, even if it becomes less costly to do so. Secondly, if the increase in firing costs that followed the 1999 intervention leads firms to improve their recruitment practices, as shown by Marinescu (2009), it might take them some time to disinvest resources from the selection process. Alternatively, businesses might simply be unwilling to do so.

These two elements, the decline in labor costs and the improvements in recruitment methods, might mitigate and even undo the potential raising effect of the 2012 reform on dismissals.

Regarding training, my findings suggest that extending the length of a probationary period does not affect firms' and workers training decisions. Two factors might explain these results. First, if the 2012 reform does not affect separations, the expected duration of a job relationship should not change. As a consequence, firms' and workers' willingness to invest in this relationship should not decrease either. Secondly, even if both employers and employees were less inclined to invest in training during the probationary period, the fact that certain training practices cannot be delayed substantially might prevent training levels to decrease much in the second year of a job relationship.

### 3.4.2. Technical arguments

The strength of my findings hinges on the validity of the identification strategy I adopt. To identify the impact of the reform, I use a difference-in-difference strategy within the framework of an OLS model. As an alternative estimation method, I might have chosen to implement the difference-in-difference method within a duration model, as done by Marinescu (2009). Analyzing the evolution of the firing or training hazard as a function of tenure boils down to study the duration of an employment relationship until either dismissal or training take place. In principle, duration models are more suited to study phenomena implying that a certain amount of time has to pass for an event to occur.

In particular, in the context under study, a duration model would have four main advantages over an OLS model (Van den Berg 2001). First, it would take better into account of the time in which each person is at risk of experiencing the event of interest, the effect of tenure in other terms. Secondly, it would take into consideration the effect of right-censoring, that is the fact that, at the time of the interview some individuals have not yet experienced either training or a dismissal, so that the total length of time between entry and exit from their current state is unknown. Third, it would account for the fact that, in the sample studied, long tenures are over-represented, as all the jobs that have ended before the time of the interview are not observed. Finally, duration models nest the competingrisk models, that give the possibility to deal with the fact that, in this context, each individual has several potential destination states, namely a dismissal, a training spell, or the simple continuation of the employment relationship.

However, it has to be noticed that duration models become especially useful when one can follow the unit of observations for several periods. Here, individuals are observed only along two quarters, and this might limit the potential of a duration model over an OLS. Moreover, both in a duration model and in the OLS, the difference-in-difference strategy is valid only if the parallel-trend assumption holds. More importantly, studying the impact of the 1999 reform with the OLS model produces similar results to the ones obtained by Marinescu (2009).[45] This clearly gives additional support to the validity of the identification strategy adopted here. Nonetheless, all the conclusions drawn in this paper obviously rely on this.

## 3.5. Conclusion

This paper provides the first attempt to study empirically how employment protection legislation affects both firms' firing decisions and the occurrence of training investments. It shows that lowering firing costs during a period characterized by negative wage growth can have neutral effects on dismissals. Moreover, conditional on this result, it demonstrates that increasing the flexibility of the labor market does not need to hamper training investments.

Clearly, the results of this paper speak for the specific structure of employment protection legislation that is analyzed. Moreover, they are likely to be influenced by the particular economic conditions that characterize the period under study.

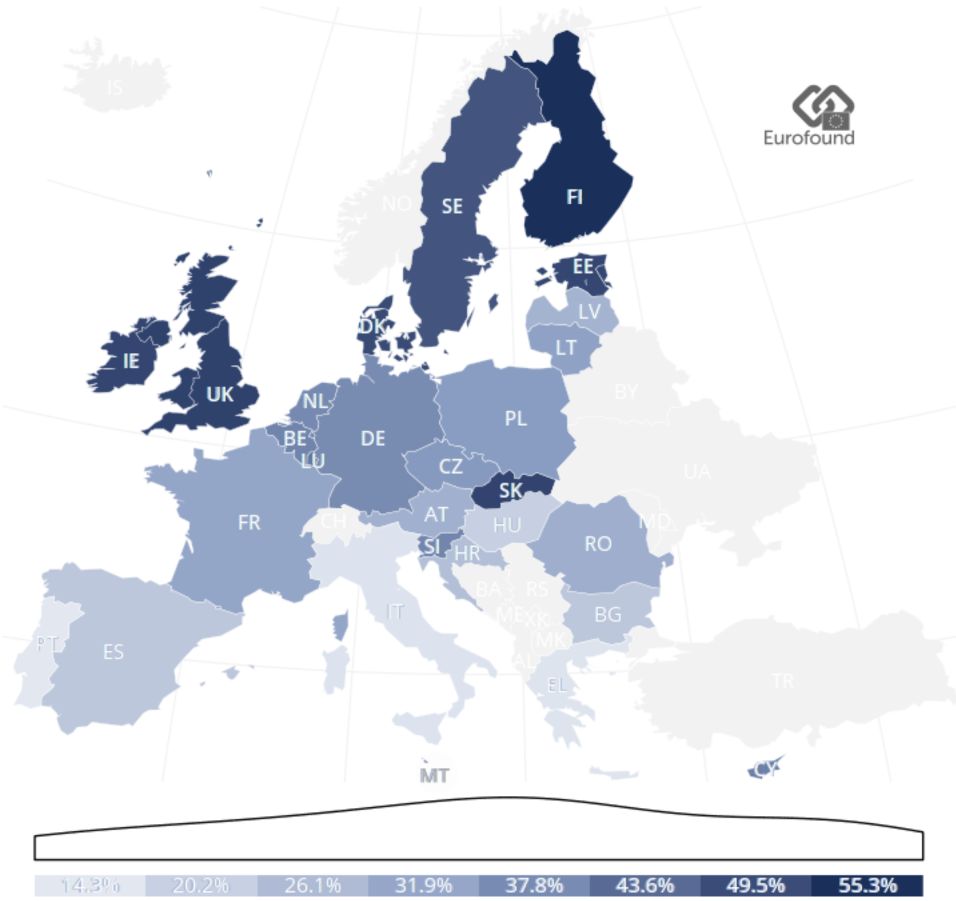
This implies that more research is needed to fully understand how job security legislation affects firms and workers decision to invest in training. However, in light of the increasing attention given to the instrument of the

[45] These results are not reported here but are available upon request.

single contract, it appears especially interesting to study the effects of its closer substitute, the UK unified contract. Understanding better how the previous reforms of the unified contract have affected training decisions might be a good starting point to expand this analysis.

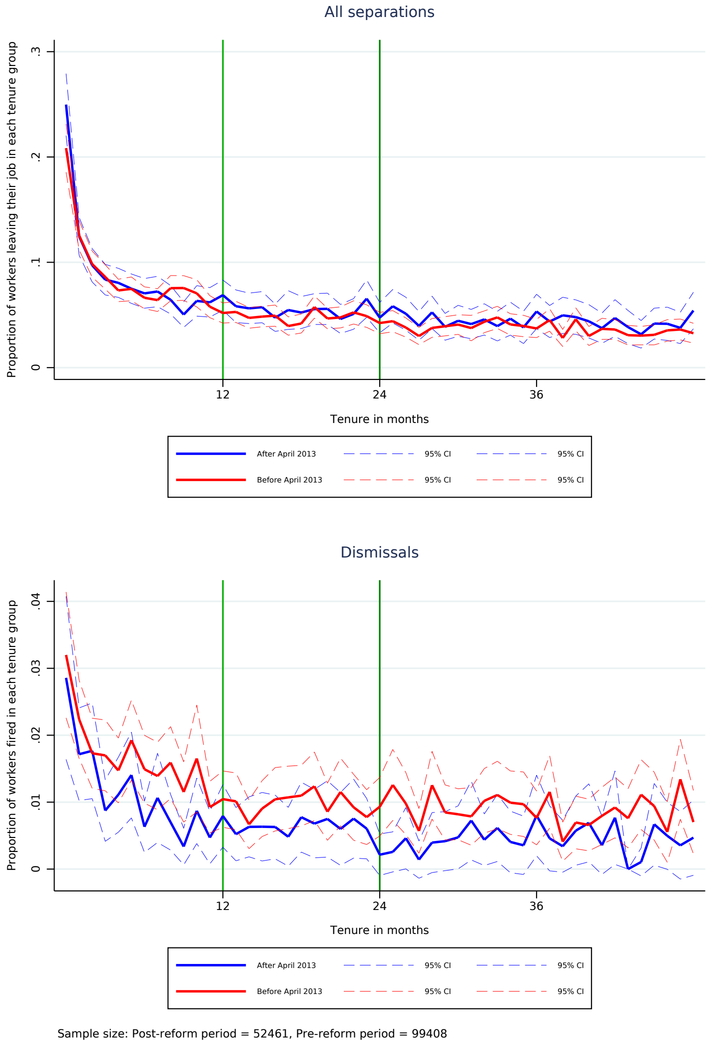
## 3.6. Tables and Figures

Figure 3.1: **On-the-job training across countries**



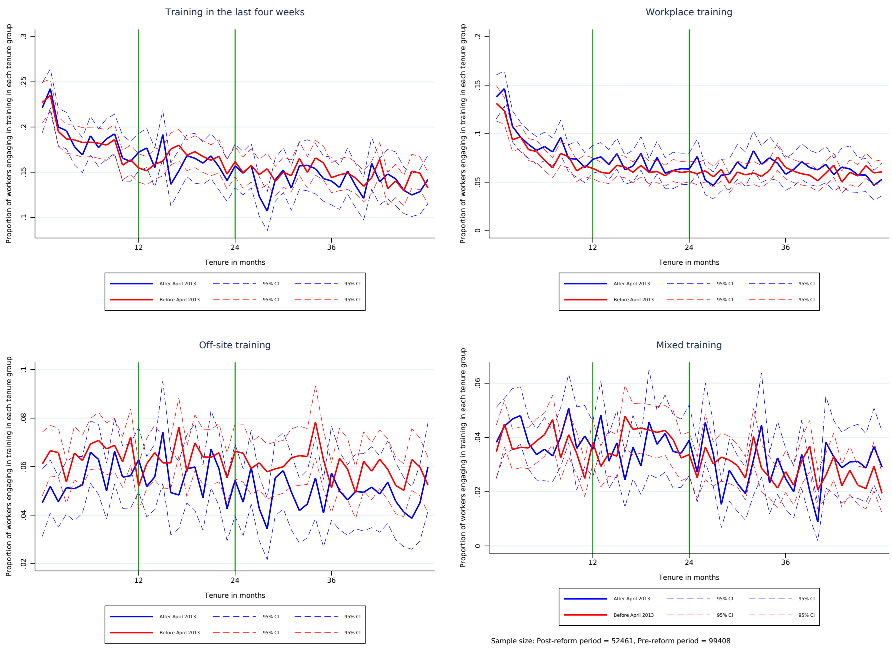

Note: The figure reports the percentages of workers per country that answered "Yes", when asked "Have you had on-the-job training in the past year?"

Figure 3.2: **Separation hazards as a function of tenure**



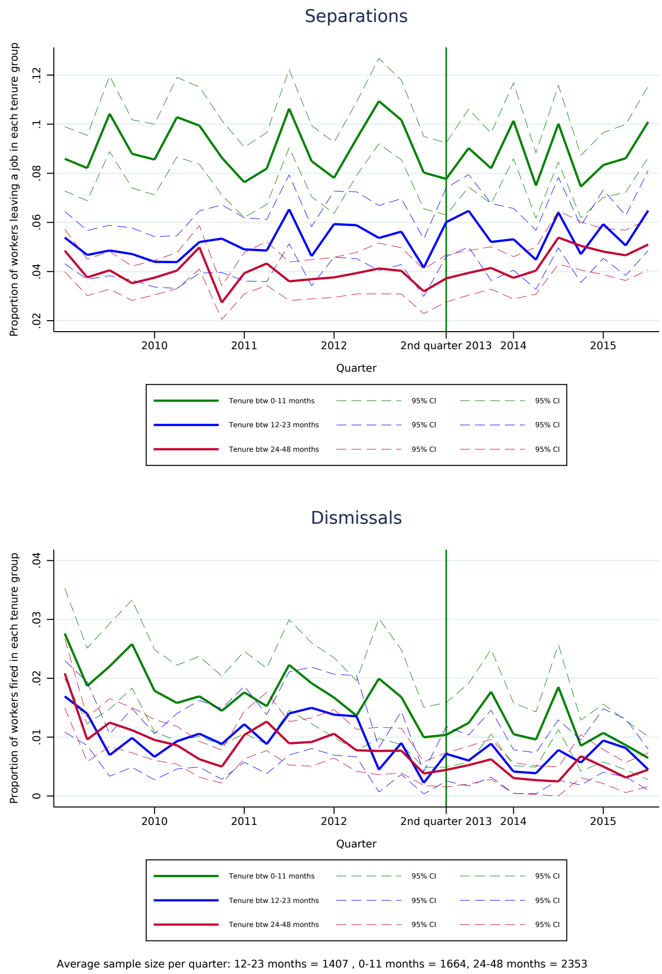

Note: The figure reports, respectively, the hazard of separation and the firing hazard as a function of tenure in the firm, before and after the 2012 starts to bind. The sample is restricted to workers who are employed when entering the survey, have a permanent job and work at least 16 hours per week. Only the first observation for each individual is retained.

Figure 3.3: **Training levels as a function of seniority**



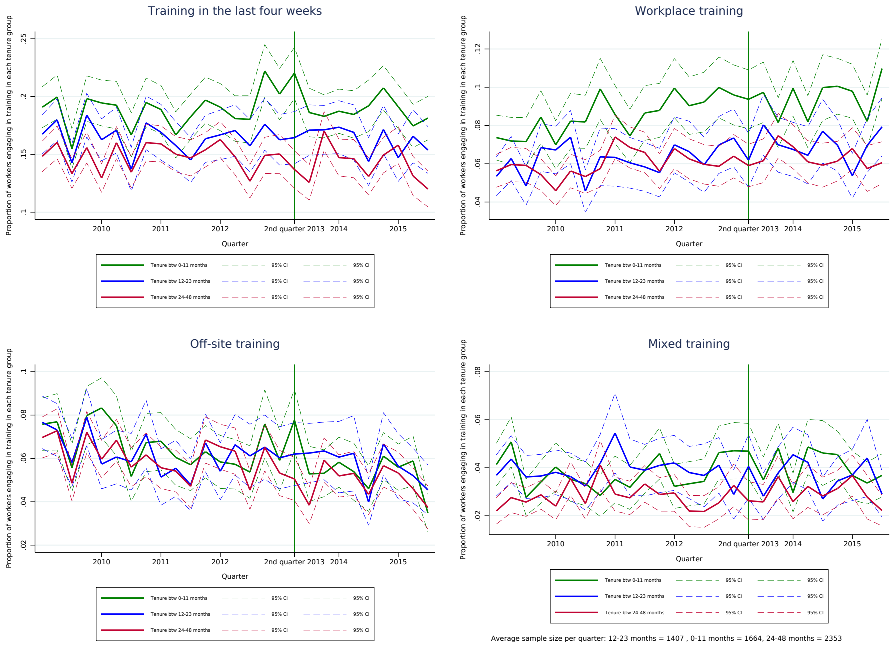

Note: The figure reports the probability of engaging in different forms of training as a function of tenure in the firm, before and after the 2012 starts to bind. The first graph refers to training in the last four week, in general terms. The second graph represents workplace training. The third one refers to training outside the workplace. The last graph represents mixed training. The sample is restricted to workers who are employed when entering the survey, have a permanent job and work at least 16 hours per week. Only the first observation for each individual is retained.

Figure 3.4: **Trends in separations by tenure group**



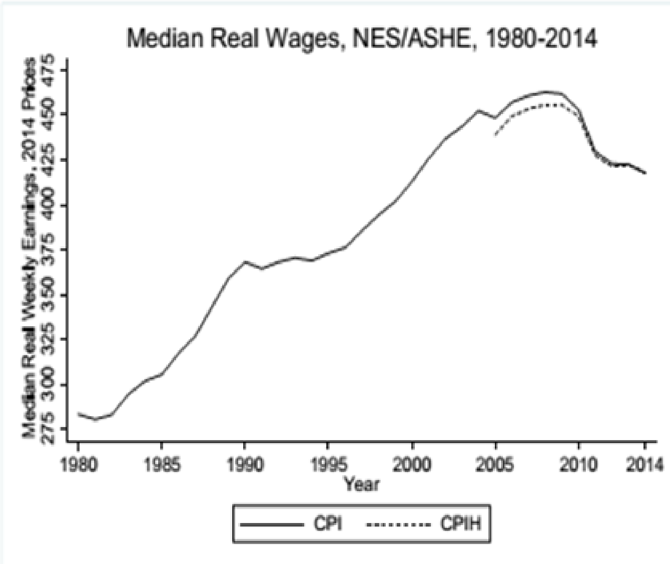

Note: The graph reports the trends in separations since 2009 to 2015. In detail, each dot represents quarterly means of the proportion of workers who leave their job, first panel, or get dismissed, second panel, and have either 0-11 months of tenure, green line, 12-23 months of tenure, blue line, or 24-48 months of tenure, red line. The sample is restricted to workers who are employed when entering the survey, have a permanent job and work at least 16 hours per week. Only the first observation for each individual is retained.

Figure 3.5: **Trends in training participation by tenure group**




Note: The graph reports the trends in training participation since 2009 to 2015. In detail, each dot represents quarterly means of the proportion of workers who have participated in training in the last four weeks, first panel, workplace traning, second panel, training outside the workplace, third panel, mixed training, fourth panel. Tenure groups refer to workers having either 0-11 months of tenure, green line, 12-23 months of tenure, blue line, or 24-48 months of tenure, red line. The sample is restricted to workers who are employed when entering the survey, have a permanent job and work at least 16 hours per week. Only the first observation for each individual is retained.

Figure 3.6: **Trends in real wages**




Notes: Annual Survey of Hours and Earnings (ASHE) weekly earnings numbers, updated from Gregg et al (2014a, 2014b), deflated by CPI and CPIH (from 2005).

Note: The graph reports the evolution of real trends in the UK from 1980 to 2015.

Table 3.1: **Reason for leaving the last job**

| | Reason for leaving the last job |
|---|---|
| Dismissed | 0.03 |
| Made redundant, voluntary redundancy | 0.27 |
| Temporary job ended | 0.05 |
| Resigned | 0.25 |
| Gave up for health reason | 0.05 |
| Took early retirement | 0.05 |
| Retired | 0.08 |
| Personal reason | 0.07 |
| Education | 0.09 |
| Other reason | 0.07 |
| Observations | 7,920 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the reasons cited for separation by workers who are employed in a permanent job in the first quarter they enter the LFS, and become unemployed between the first and the second quarter they are interviewed. The sample is restricted to the pre-reform period, i.e. it comprises only individuals interviewed before April 2012.

Table 3.2: **Proportion of workers engaged in training by tenure group**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Entire sample | 0.14 | 0.06 | 0.05 | 0.02 |
| | (0.35) | (0.24) | (0.23) | (0.16) |
| Tenure $\leq$ 11 months | 0.18 | 0.08 | 0.06 | 0.04 |
| | (0.39) | (0.27) | (0.24) | (0.19) |
| Tenure btw 12-23 months | 0.16 | 0.06 | 0.06 | 0.04 |
| | (0.37) | (0.24) | (0.24) | (0.19) |
| Tenure btw 24-48 months | 0.15 | 0.06 | 0.06 | 0.03 |
| | (0.35) | (0.24) | (0.24) | (0.16) |
| Tenure $\geq$ 49 months | 0.13 | 0.06 | 0.05 | 0.02 |
| | (0.34) | (0.23) | (0.22) | (0.14) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set. Note: the table reports the percentage of workers engaged in training for each tenure group. The sample comprises all individuals who are employed when entering the survey and with known tenure. Only the first observation for each individual is retained. The sample is further restricted to individuals interviewed in the pre-reform period, that is before April 2012.

Table 3.3: **Proportion of workers engaged in training by subgroup**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Higher education | 0.19 | 0.07 | 0.08 | 0.04 |
| | (0.39) | (0.26) | (0.27) | (0.18) |
| Upper secondary education | 0.14 | 0.06 | 0.05 | 0.02 |
| | (0.34) | (0.24) | (0.22) | (0.15) |
| Lower secondary education | 0.11 | 0.05 | 0.04 | 0.02 |
| | (0.31) | (0.23) | (0.19) | (0.14) |
| 16-34 years old | 0.16 | 0.07 | 0.06 | 0.04 |
| | (0.37) | (0.25) | (0.24) | (0.18) |
| 35-70 years old | 0.13 | 0.06 | 0.05 | 0.02 |
| | (0.34) | (0.24) | (0.22) | (0.14) |
| Male | 0.12 | 0.05 | 0.05 | 0.02 |
| | (0.33) | (0.22) | (0.21) | (0.15) |
| Female | 0.16 | 0.07 | 0.06 | 0.03 |
| | (0.36) | (0.25) | (0.24) | (0.17) |
| Full-time | 0.14 | 0.06 | 0.05 | 0.03 |
| | (0.35) | (0.24) | (0.23) | (0.16) |
| Part-time | 0.13 | 0.06 | 0.05 | 0.02 |
| | (0.34) | (0.23) | (0.23) | (0.14) |
| Private sector | 0.11 | 0.05 | 0.04 | 0.02 |
| | (0.32) | (0.22) | (0.20) | (0.14) |
| Public sector | 0.20 | 0.08 | 0.08 | 0.04 |
| | (0.40) | (0.28) | (0.27) | (0.19) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the percentage of workers engaged in training for each subgroup considered. The sample comprises all individuals who are employed when entering the survey and with known tenure. Only the first observation for each individual is retained. The sample is further restricted to individuals interviewed in the pre-reform period, that is before April 2012.

Table 3.4: **Proportion of workers engaged in training by occupation**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Managers and Senior Officials | 0.12 | 0.04 | 0.06 | 0.02 |
| | (0.33) | (0.21) | (0.23) | (0.14) |
| Professional occupations | 0.21 | 0.09 | 0.09 | 0.04 |
| | (0.41) | (0.28) | (0.28) | (0.20) |
| Associate Professional | 0.18 | 0.08 | 0.07 | 0.04 |
| | (0.39) | (0.27) | (0.25) | (0.18) |
| Administrative | 0.10 | 0.05 | 0.04 | 0.01 |
| | (0.31) | (0.22) | (0.20) | (0.12) |
| Skilled Trades Occupations | 0.10 | 0.04 | 0.04 | 0.02 |
| | (0.30) | (0.20) | (0.19) | (0.15) |
| Personal Service Occupations | 0.19 | 0.09 | 0.07 | 0.04 |
| | (0.39) | (0.28) | (0.25) | (0.19) |
| Sales Occupation | 0.10 | 0.06 | 0.03 | 0.01 |
| | (0.30) | (0.23) | (0.18) | (0.11) |
| Machine Operatives | 0.07 | 0.04 | 0.02 | 0.01 |
| | (0.25) | (0.19) | (0.15) | (0.10) |
| Elementary Occupations | 0.06 | 0.03 | 0.02 | 0.01 |
| | (0.24) | (0.18) | (0.15) | (0.07) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the percentage of workers engaged in training for each occupation considered. The sample comprises all individuals who are employed when entering the survey and with known tenure. Only the first observation for each individual is retained. The sample is further restricted to individuals interviewed in the pre-reform period, that is before April 2012.

Table 3.5: **Proportion of workers engaged in training by industry**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Agriculture | 0.06 | 0.02 | 0.02 | 0.01 |
| | (0.24) | (0.15) | (0.15) | (0.11) |
| Energy | 0.12 | 0.05 | 0.05 | 0.02 |
| | (0.33) | (0.22) | (0.22) | (0.15) |
| Manufacturing | 0.09 | 0.04 | 0.03 | 0.01 |
| | (0.28) | (0.20) | (0.18) | (0.12) |
| Construction | 0.10 | 0.04 | 0.04 | 0.02 |
| | (0.31) | (0.19) | (0.20) | (0.15) |
| Distribution | 0.08 | 0.04 | 0.03 | 0.01 |
| | (0.28) | (0.20) | (0.18) | (0.11) |
| Transport | 0.09 | 0.04 | 0.03 | 0.01 |
| | (0.29) | (0.20) | (0.18) | (0.11) |
| Banking | 0.13 | 0.06 | 0.05 | 0.02 |
| | (0.34) | (0.23) | (0.22) | (0.15) |
| Public adm, edu, health | 0.21 | 0.09 | 0.08 | 0.04 |
| | (0.40) | (0.28) | (0.27) | (0.19) |
| Other services | 0.13 | 0.05 | 0.05 | 0.03 |
| | (0.33) | (0.22) | (0.22) | (0.16) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the percentage of workers engaged in training for each industry considered. The sample comprises all individuals who are employed when entering the survey and with known tenure. Only the first observation for each individual is retained. The sample is further restricted to individuals interviewed in the pre-reform period, that is before April 2012.

Table 3.6: **Impact of the 2012 reform on the hazards of separation**

| | (1)<br>All separations | (2)<br>Dismissals |
|---|---|---|
| Policy impact | -0.0000713 | 0.000327 |
| | (0.00551) | (0.00194) |
| Observations | 84228 | 84228 |
| Pre-treatment means | 0.05 | 0.01 |
| $R^2$ | 0.009 | 0.006 |
| F | 4.111 | 4.763 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.6. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.7: **Impact of the 2012 reform on the probability of engaging in training**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Policy impact | 0.0122 | 0.00158 | 0.00871 | 0.00222 |
| Observations | 84228 | 84228 | 84228 | 84228 |
| Pre-treatment means | 0.16 | 0.06 | 0.06 | 0.04 |
| $R^2$ | 0.044 | 0.014 | 0.017 | 0.022 |
| F | 23.50 | 6.721 | 10.13 | 8.595 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.

Note: the table reports the coefficients obtained through the estimation of regression 3.6. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

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

Table 3.8: **Impact of the 2012 reform on the hazards of separation - Robustness checks**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| Main results | -0.0000713 | 0.000327 |
| | (0.00551) | (0.00194) |
| Tenure-specific time trends | -0.00503 | -0.00124 |
| | (0.00637) | (0.00227) |
| Smaller control group | -0.0006286 | 0.0009829 |
| | (0.00599) | (0.002103) |
| Impact of a placebo reform | 0.0031599 | -0.0004181 |
| | (0.00549) | (0.0026341) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports a series of robustness checks on the impact of the 2012 on separations and dismissals. The first row reports the main results; in the second row I add tenure-specific time trends; in the third one, I restrict the control group to workers with 24 to 36 months of tenure. The last row presents the impact of a placebo reform, obtained by pretending that the new legislation starts binding in April 2011 and excluding the actual post-reform period – from April 2012 onward. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure - except for the third row where the control group is restricted to workers with 24 to 36 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.9: **Impact on the probability of engaging in training - Robustness checks**

| | (1)<br>Training | (2)<br>Workplace | (3)<br>Off-site | (4)<br>Mixed |
|---|---|---|---|---|
| Main results | 0.0122 | 0.00158 | 0.00871 | 0.00222 |
| | (0.00877) | (0.00602) | (0.00582) | (0.00436) |
| Tenure-specific time trends | 0.0113 | 0.000158 | 0.00691 | 0.00447 |
| | (0.0101) | (0.00689) | (0.00666) | (0.00497) |
| Smaller control group | 0.01000 | 0.000219 | 0.00730 | 0.00263 |
| | (0.00949) | (0.00657) | (0.00623) | (0.00468) |
| Impact of a placebo reform | -0.00345 | -0.00552 | 0.00355 | -0.00136 |
| | (0.00880) | (0.00582) | (0.00578) | (0.00465) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports a series of robustness checks on the impact of the 2012 on separations and dismissals. The first row reports the main results; in the second row I add tenure-specific time trends; in the third one, I restrict the control group to workers with 24 to 36 months of tenure. Finally, the last row presents the impact of a placebo reform, obtained by pretending that the new legislation starts binding in April 2011 and excluding the actual post-reform period – from April 2012 onward. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure - except for the third row where the control group is restricted to workers with 24 to 36 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.10: **Impact of the 2012 reform on the hazards of separation of different tenure groups**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| 12-15 months tenure | 0.003365 | 0.001402 |
| | (0.00747) | (0.00252) |
| 16-19 months tenure | -0.006085 | -0.002588 |
| | (0.00784) | (0.00257) |
| 20-23 months tenure | 0.00016 | 0.003384 |
| | (0.01355) | (0.00623) |
| Observations | 84228 | 84228 |
| $R^2$ | 0.009 | 0.006 |
| F | 4.06 | 4.7 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the impact of the reform, separately for workers with 12-15 months of tenure, for those with 16-19 months of tenure, and for those with 20-23 months of tenure, compared with workers with 24-48 months of seniority. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.11: **Impact of the 2012 reform on the probability of engaging in training by different tenure groups**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| 12-15 months tenure | 0.0266** | 0.00156 | 0.0189** | 0.00650 |
| | (0.0116) | (0.00759) | (0.00824) | (0.00563) |
| 16-19 months tenure | -0.000112 | 0.0134 | -0.00691 | -0.00633 |
| | (0.0137) | (0.0102) | (0.00821) | (0.00700) |
| 20-23 months tenure | -0.0245 | -0.0326*** | 0.00296 | 0.00558 |
| | (0.0202) | (0.0107) | (0.0141) | (0.0118) |
| Observations | 84228 | 84228 | 84228 | 84228 |
| $R^2$ | 0.044 | 0.015 | 0.017 | 0.022 |
| F | 23.21 | 6.690 | 10.05 | 8.492 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the impact of the reform, separately for workers with 12-15 months of tenure, for those with 16-19 months of tenure, and for those with 20-23 months of tenure, compared with workers with 24-48 months of seniority. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.12: **Impact of the 2012 reform on the hazards of separation - Subgroup analysis (I)**

| | (1)<br>All separations | (2)<br>Dismissals |
|---|---|---|
| Males | 0.00191 | 0.000381 |
| | (0.00763) | (0.00290) |
| Observations | 42022 | 42022 |
| Females | -0.00153 | 0.000330 |
| | (0.00783) | (0.00250) |
| Observations | 42206 | 42206 |
| Higher education | 0.0121 | -0.00162 |
| | (0.00912) | (0.00213) |
| Observations | 34803 | 34803 |
| Upper secondary edu | -0.0213** | 0.000210 |
| | (0.0102) | (0.00409) |
| Observations | 16073 | 16073 |
| Lower secondary edu | 0.00543 | 0.00425 |
| | (0.0109) | (0.00458) |
| Observations | 19953 | 19953 |
| Older than 35 | -0.000795 | -0.00220 |
| | (0.00715) | (0.00256) |
| Observations | 47789 | 47789 |
| Younger than 35 | 0.00232 | 0.00175 |
| | (0.00792) | (0.00273) |
| Observations | 36439 | 36439 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.6, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.13: **Impact of the 2012 reform on the hazards of separation - Subgroup analysis (II)**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| Full-time | -0.000756 | -0.00125 |
| | (0.00602) | (0.00192) |
| Observations | 65497 | 65497 |
| Part-time | 0.00146 | 0.00626 |
| | (0.0133) | (0.00579) |
| Observations | 18731 | 18731 |
| Private sector | 0.001812 | 0.000736 |
| | (0.00471) | (0.00205) |
| Observations | 65375 | 65375 |
| Public sector | -0.00736 | 0.00171** |
| | (0.00874) | (0.000711) |
| Observations | 18853 | 18853 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.6, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.14: **Impact of the 2012 reform on the probability of engaging in training - Subgroup analysis (I)**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Males | 0.0169 | -0.00535 | 0.0209** | 0.00148 |
| | (0.0119) | (0.00776) | (0.00813) | (0.00598) |
| Observations | 42022 | 42022 | 42022 | 42022 |
| Females | 0.00619 | 0.0103 | -0.00565 | 0.00214 |
| | (0.0129) | (0.00942) | (0.00808) | (0.00633) |
| Observations | 42206 | 42206 | 42206 | 42206 |
| Higher education | 0.0265* | 0.0151 | 0.0126 | -0.000960 |
| | (0.0144) | (0.00976) | (0.0102) | (0.00691) |
| Observations | 34803 | 34803 | 34803 | 34803 |
| Upper secondary edu | 0.00978 | -0.0113 | 0.0130 | 0.00838 |
| | (0.0189) | (0.0121) | (0.0125) | (0.0101) |
| Observations | 16073 | 16073 | 16073 | 16073 |
| Lower secondary edu | 0.0206 | 0.0101 | -0.00360 | 0.0149 |
| | (0.0179) | (0.0136) | (0.00957) | (0.00921) |
| Observations | 19953 | 19953 | 19953 | 19953 |
| Older than 35 | 0.0144 | 0.00269 | 0.00715 | 0.00474 |
| | (0.0111) | (0.00803) | (0.00741) | (0.00498) |
| Observations | 47789 | 47789 | 47789 | 47789 |
| Younger than 35 | 0.0124 | 0.00145 | 0.0107 | 0.000808 |
| | (0.0128) | (0.00864) | (0.00836) | (0.00658) |
| Observations | 36439 | 36439 | 36439 | 36439 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.6, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.15: **Impact of the 2012 reform on the probability of engaging in training - Subgroup analysis (II)**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Full-time | 0.00979 | 0.00352 | 0.00563 | 0.000537 |
| | (0.0100) | (0.00705) | (0.00646) | (0.00507) |
| Observations | 65497 | 65497 | 65497 | 65497 |
| Part-time | 0.0186 | -0.00812 | 0.0204 | 0.00815 |
| | (0.0176) | (0.0108) | (0.0130) | (0.00809) |
| Observations | 18731 | 18731 | 18731 | 18731 |
| Private sector | 0.0101 | 0.00234 | 0.00423 | 0.00373 |
| | (0.00912) | (0.00630) | (0.00571) | (0.00459) |
| Observations | 65375 | 65375 | 65375 | 65375 |
| Public sector | 0.0211 | -0.000177 | 0.0300 | -0.00755 |
| | (0.0265) | (0.0177) | (0.0201) | (0.0127) |
| Observations | 18853 | 18853 | 18853 | 18853 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.

Note: the table reports the coefficients obtained through the estimation of regression 3.6, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have between 12 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.16: **Impact on the hazards of separation for workers with less than 12 months of tenure**

| | (1)<br>All separations | (2)<br>Dismissals |
|---|---|---|
| Policy impact | -0.00884 | -0.00676*** |
| | (0.00618) | (0.00214) |
| Observations | 73725 | 73725 |
| Pre-treatment means | 0.08 | 0.02 |
| $R^2$ | 0.030 | 0.009 |
| F | 9.151 | 6.206 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.

Note: the table reports the coefficients obtained through the estimation of regression 3.7. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 months or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

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

Table 3.17: **Impact on the probability of engaging in training for workers with less than 12 months of tenure**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Policy impact | 0.0224** | 0.00698 | 0.00306 | 0.0124*** |
| Observations | 73725 | 73725 | 73725 | 73725 |
| Pre-treatment means | 0.18 | 0.08 | 0.06 | 0.04 |
| $R^2$ | 0.049 | 0.020 | 0.018 | 0.022 |
| F | 24.24 | 8.062 | 9.194 | 8.539 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set. Note: the table reports the coefficients obtained through the estimation of regression 3.7. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 months or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

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

## 3.7. Appendix

Table 3.18: **Impact of the 2012 reform on the hazards of separation over two years**

| | (1)<br>All separations | (2)<br>Dismissals |
|---|---|---|
| Policy impact | -0.004073 | 0.000846 |
| | (0.00389) | (0.00136) |
| Observations | 68817 | 68817 |
| Pre-treatment means | 0.05 | 0.01 |
| $R^2$ | 0.010 | 0.006 |
| F | 3.67 | 3.44 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set. Note: the table reports the coefficients estimated with regression 3.6. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. They further include tenure, month, region of work, industry and occupation FE. The sample comprises workers employed in the first quarter, with a permanent job, working at least 16 hours per week, with either between 12 and 23, or between 37 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

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

Table 3.19: **Impact of the 2012 reform on the probability of engaging in training over two years**

| | (1)<br>Training | (2)<br>Workplace | (3)<br>Off-site | (4)<br>Mixed |
|---|---|---|---|---|
| Policy impact | 0.00268 | 0.00492 | 0.000893 | -0.00305 |
| | (0.00612) | (0.00417) | (0.00398) | (0.00308) |
| Observations | 68817 | 68817 | 68817 | 68817 |
| Pre-treatment means | 0.16 | 0.06 | 0.06 | 0.04 |
| $R^2$ | 0.043 | 0.015 | 0.017 | 0.023 |
| F | 17.62 | 5.408 | 7.244 | 7.205 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.6. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. They further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either between 12 and 23, or between 37 and 48 months of tenure. This allows me to estimate the effect of the reform up to its second year of implementation, i.e. till April 2015. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.20: **Impact on separations for workers with less than 12 months of tenure - Robustness checks**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| Main results | -0.00884 | -0.00676*** |
| | (0.00618) | (0.00214) |
| Tenure-specific time trends | -0.003453 | -0.0049* |
| | (0.0074) | (0.00297) |
| Smaller control group | -0.0048 | -0.005488 |
| | (0.00669) | (0.0024) |
| Impact of a placebo reform | -0.007328 | -0.000186 |
| | (0.00618) | (0.00302) |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports a series of robustness checks on the impact of the 2012 on separations and dismissals for workers with less than 12 months of tenure. The first row reports the main results; in the second row I add tenure-specific time trends; in the third one, I restrict the control group to workers with 24 to 36 months of tenure. Finally, the last row presents the impact of a placebo reform, obtained by pretending that the new legislation starts binding in April 2011 and excluding the actual post-reform period – from April 2012 onward. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 or between 24 and 48 months of tenure - except for the third row where the control group is restricted to workers with 24 to 36 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.21: **Impact on training for workers with less than 12 months of tenure - Robustness checks**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Main results | 0.0224** | 0.00698 | 0.00306 | 0.0124*** |
| | (0.00916) | (0.00662) | (0.00592) | (0.00455) |
| Tenure-specific time trends | 0.0185* | -0.00204 | 0.00527 | 0.0153*** |
| | (0.0106) | (0.00777) | (0.00676) | (0.00512) |
| Smaller control group | 0.0288*** | 0.0112 | 0.00475 | 0.0127*** |
| | (0.00973) | (0.00707) | (0.00617) | (0.00483) |
| Impact of a placebo reform | 0.00419 | 0.00423 | -0.00393 | 0.00375 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports a series of robustness checks on the impact of the 2012 on separations and dismissals for workers with less than 12 months of tenure. The first row reports the main results; in the second row I add tenure-specific time trends; in the third one, I restrict the control group to workers with 24 to 36 months of tenure. Finally, the last row presents the impact of a placebo reform, obtained by pretending that the new legislation starts binding in April 2011 and excluding the actual post-reform period – from April 2012 onward. The outcomes are indicated on top of each column. All regressions control for sex, age, marital status, education, full-time status and for whether the job is in the private or public sector. The regressions further include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 or between 24 and 48 months of tenure - except for the third row where the control group is restricted to workers with 24 to 36 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.22: **Impact on separation for workers with less than 12 months of tenure - Subgroup analysis (I)**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| Males | 0.0183** | -0.00403 |
| | (0.00922) | (0.00322) |
| Observations | 36963 | 36963 |
| Females | 0.000403 | -0.00316 |
| | (0.00861) | (0.00280) |
| Observations | 36762 | 36762 |
| Higher education | 0.0181* | -0.00255 |
| | (0.00952) | (0.00227) |
| Observations | 29754 | 29754 |
| Upper secondary edu | -0.000132 | 0.00222 |
| | (0.0139) | (0.00605) |
| Observations | 13934 | 13934 |
| Lower secondary edu | 0.00917 | -0.00242 |
| | (0.0142) | (0.00588) |
| Observations | 17817 | 17817 |
| Older than 35 | 0.0116 | -0.00403 |
| | (0.00824) | (0.00254) |
| Observations | 47789 | 47789 |
| Younger than 35 | 0.0114 | -0.00393 |
| | (0.00901) | (0.00319) |
| Observations | 33143 | 33143 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.7, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.23: **Impact on separation for workers with less than 12 months of tenure - Subgroup analysis (II)**

| | (1) All separations | (2) Dismissals |
|---|---|---|
| Full-time | 0.0116 | -0.00479** |
| | (0.0072) | (0.00232) |
| Observations | 56933 | 56933 |
| Part-time | 0.00465 | 0.000595 |
| | (0.0136) | (0.00529) |
| Observations | 16792 | 16792 |
| Private sector | 0.0131* | -0.00459* |
| | (0.00732) | (0.00252) |
| Observations | 57840 | 57840 |
| Public sector | -0.00858 | 0.00143 |
| | (0.00993) | (0.00310) |
| Observations | 15885 | 15885 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the coefficients obtained through the estimation of regression 3.7, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 3.24: **Impact on training for workers with less than 12 months of tenure - Subgroup analysis (I)**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Males | 0.0372*** | -0.00527 | 0.0210** | 0.0215*** |
| | (0.0128) | (0.00882) | (0.00873) | (0.00653) |
| Observations | 36963 | 36963 | 36963 | 36963 |
| Females | 0.00601 | 0.0206** | -0.0170** | 0.00253 |
| | (0.0130) | (0.00999) | (0.00769) | (0.00624) |
| Observations | 36762 | 36762 | 36762 | 36762 |
| Higher education | 0.0394*** | 0.0114 | 0.0117 | 0.0163** |
| | (0.0153) | (0.0109) | (0.0107) | (0.00721) |
| Observations | 29754 | 29754 | 29754 | 29754 |
| Upper secondary edu | 0.0575*** | 0.0362** | 0.00535 | 0.0157 |
| | (0.0210) | (0.0150) | (0.0130) | (0.0112) |
| Observations | 13934 | 13934 | 13934 | 13934 |
| Lower secondary edu | -0.0306* | -0.0336*** | -0.00615 | 0.00984 |
| | (0.0168) | (0.0124) | (0.00965) | (0.00889) |
| Observations | 17817 | 17817 | 17817 | 17817 |
| Older than 35 | 0.0244** | 0.0169** | -0.00517 | 0.0128** |
| | (0.0115) | (0.00857) | (0.00683) | (0.00566) |
| Observations | 40582 | 40582 | 40582 | 40582 |
| Younger than 35 | 0.0211 | 0.000200 | 0.00803 | 0.0127* |
| | (0.0132) | (0.00950) | (0.00869) | (0.00655) |
| Observations | 33143 | 33143 | 33143 | 33143 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.
Note: the table reports the estimates of regression 3.7, for different subgroups. The outcomes are shown on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The sample comprises workers employed in the first quarter, with a permanent job, working at least 16 hours per week, with either less than 12 or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

Table 3.25: **Impact on training for workers with less than 12 months of tenure - Subgroup analysis (II)**

| | (1) Training | (2) Workplace | (3) Off-site | (4) Mixed |
|---|---|---|---|---|
| Full-time | 0.0228** | 0.00108 | 0.00831 | 0.0135** |
| | (0.0106) | (0.00772) | (0.00669) | (0.00545) |
| Observations | 56933 | 56933 | 56933 | 56933 |
| Part-time | 0.0217 | 0.0279** | -0.0139 | 0.00749 |
| | (0.0183) | (0.0126) | (0.0128) | (0.00757) |
| Observations | 16792 | 16792 | 16792 | 16792 |
| Private sector | 0.0263*** | 0.00662 | 0.00942 | 0.0103** |
| | (0.00977) | (0.00693) | (0.00638) | (0.00471) |
| Observations | 57840 | 57840 | 57840 | 57840 |
| Public sector | 0.0136 | 0.0190 | -0.0311** | 0.0256* |
| | (0.0255) | (0.0199) | (0.0151) | (0.0143) |
| Observations | 15885 | 15885 | 15885 | 15885 |

Source: UK Labor Force Survey Two-Quarter Longitudinal Data Set.

Note: the table reports the coefficients obtained through the estimation of regression 3.7, for different subgroups. The outcomes are indicated on top of each column. Each row reports the subgroup considered. All regressions include tenure, month, region of work, industry and occupation FE. The estimation sample comprises workers who are employed in the first quarter, have a permanent job, work at least 16 hours per week, and have either less than 12 or between 24 and 48 months of tenure. Only the first observation for each individual is retained. Robust standard errors are included in parenthesis.

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

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