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Doctoral Thesis

Three Essays on the Economic Effects of Public Policies

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Declaration of Authorship

I, **Simone Chinetti**, declare that this thesis titled, "Three Essays on the Economic Effects of Public Policies" and the work presented in it are my own. I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this University.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Signed: **Simone Chinetti**

Date: **July 7, 2021**

"...And there is a lot of idiosyncrasy. But there are also regularities and phenomena. And what the data is going to be able to do – if there's enough of it – is uncover, in the mess and the noise of the world, some lines of music that actually have harmony. It's there, somewhere."

Esther Duflo

Abstract

This thesis contributes to the empirical strands of the economic literature that explore and investigate the effects of public policies on individuals and firms.

The first chapter provides a novel empirical test of human capital theory by studying whether increases in residual working life induce additional training. By exploiting a sizable pension reform, that affected all Italian workers, in a Difference-in-Differences setting there is evidence that an increase in the residual working life increases human capital investment. Additionally, the response to the reform was very heterogeneous and depending on gender, age profiles, education, martial status, sector of employment and firm size. However, the empirical evidence suggests to rule out that positive variations in human capital investment were directly sponsored by employers.

The second chapter studies the impact of the COVID-19 pandemic on female economists' research productivity. The analysis uses data from the SSRN web archive on 4,778 distinct pre-prints involving 8,651 authors from over 90 countries observed from January to November 2020. By estimating a Difference-in-Differences, the estimates show that, since the lockdown began, the number of working papers written by a female economist, alone or jointly with other researchers, uploaded on SSRN declined of about 20 percentage points and this negative effect persists up to about 4 months later. Declines in productivity, however, disappear during the school re-opening period suggesting that indeed childcare demand has been an important channel in causing women production drop. Finally, declines in productivity are not associated with increases in pre-prints' quality.

The third chapter provides novel empirical evidence on the effectiveness of public subsidies for SMEs by investigating the effect of a subsidy program taken place in *Campania* (South Italy) in 2013. By relying on a Difference-in-Differences approach, the empirical analysis demonstrates that the regional program was effective in increasing private firms' spending in innovative investment. However, the firms' response to

the program was also largely heterogeneous. In particular, there is evidence that the positive effect on investment comes from micro- and small-sized firms as well as firms operating in high tech sectors and high tech service firms. Nonetheless, it is not possible to reject the hypothesis that firms increased spending by about approximately the amount of the subsidy. Finally, the program had sizeable spillover effects on labour demand but not on firms' productivity.

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Last but not the least, I thank from the bottom of my heart my parents and my family all for the immense love, countless opportunities, trust and unconditional support they gave me throughout all these years. This dissertation is dedicated to them.

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To my family, to those who are still here, to those who are gone and to those who are next...

Chapter 1

Public Policies and Incentives: their effects on individuals and firms

The recent global economic and financial crisis has generated a large and prolonged intellectual debate and controversy about the virtues of the free market system and the role of government interventions in the economy.

Starting from the end of 2008, the escalation of the financial crisis and its subsequent ramification in all economic sectors has urged worldwide governments to increase their interventionist efforts in order to stabilize their economies and to revive economic growth. While in the very first moments government policies mainly regarded drastic measures to rescue troubled financial institutions and expansionary fiscal policies to counteract the economic downturn, the subsequent set of packages and interventions resulted in a series of wide-ranging policies all aimed at mitigating the undesirable consequences of the recession on market activity and public budgets, further exacerbated by the sovereign debt crisis in Europe.

More than ten years later after one of the greatest economic slowdowns in modern history, another global event has forced, with an even stronger emphasis and speed, worldwide governments to legislate massive public interventions with the aim of safeguarding the economy and public health. Indeed, the spread of the COVID-19 has represented one of the most challenging health and economic crises the world has experienced in the recent past, with declines in economic activity expected to be larger than those observed during the great recession.

Starting from China, the virus appeared in most developed countries and in many developing ones, with enormous life and economic costs. In many countries, governments faced a trade-off between containing the pace of infections and avoiding the collapse of the economy and implemented a mix of non-pharmaceuticals interventions, in order to contain the spread of the disease and its health costs. Many of these opted for strong limitations to circulations of people. Social distancing measures *de facto* implied a stop to several sectors, while a limited share of those considered as essential kept operating. This had important economic costs and determined a heated debate in the public opinion and businesses' representatives.

Public policies indeed provide to consumers, firms and workers a myriad of explicit and implicit incentives that end up to influence their choices and actions in a multitude of ways that, *a priori*, are not easy to predict and analyze.

In light of the relevance of the role of public authorities' decisions and policies in affecting economic agents' behaviour, the economic literature has always inquired into the effects of such public interventions. However, whether policies and incentives ruled by public administrations can be deemed as instruments to correct market inefficiencies or, on the contrary, as tools that further exacerbate them is only a matter of ideology that goes well beyond the aim of this thesis that, instead, aims at estimating the impact of public policies and incentives on individuals and firms in three different contexts.

In particular, the empirical analysis of the effects of public programs on economic agents' behaviour is the *leitmotif* of this thesis. The first essay broadly contributes to the labour economics literature and, in particular, to the empirical human capital literature that studies how variations in residual working life affect individual-level additional investment in human capital activities. In this case, I exploit a large unanticipated pension reform that suddenly tightened pension claim requirements, that I interpret as positive variations in residual working life, in order to investigate whether individual incentives in investing in human capital changed in the aftermath of the reform. The second essay, instead, contributes to the recent economic literature that explores the consequences, almost in real time, of the COVID-19 pandemic on the economy, by looking at the effects of lockdown policies on academic productivity and adopting also a gendered perspective. The third essay, finally, contributes to the empirical microeconometrics literature that investigates the relationship between public funding and private firms' investment in innovation as well as to the literature estimating the elasticity of investment to its costs, by focusing on a regional subsidy program for small and medium enterprises.

All the essays included in the present thesis exploit different and credible identification strategies that have the common ambition of clearly isolating and estimating a causal effect of public policies on the outcomes of interest.

Furthermore, the empirical evidence presented in the three essays is based on distinct individual-level datasets, that in some cases, and where part of them, have been obtained through direct hand-collection or through the implementation of web-scarping routines.

Finally, while some of the articles are more original than the others in terms of the research questions they aim to answer, all of them represent a contribution to the existing literature that, and with regards to the specific topic analyzed, has been proven to be inconclusive or relatively unexplored.

1.1 Pension reforms and human capital investment

The focus of the first essay is on the (unintended) effects of pension reforms aimed at tightening retirement eligibility requirements on middle-aged individuals' investment in human capital.

Many countries are experiencing a sustained increase in population ageing. The old-age dependency ratio, that is the ratio between the number of individuals aged 65 and above and the number of individuals in the working population (ages 20-64), in OECD countries was equal to 27.9 in 2015, up from 19.5 in 1975, and is projected to double by 2075 (OECD, 2017). Such adverse demographic transformation exerts mounting pressure on the financial sustainability of public pension systems: over the

1980-2015 period, public pension spending has increased from 5.5 per cent to 7.5 per cent of GDP. Spurred by these trends, many countries have implemented, especially in the aftermath of the great recession, or are considering to enact pension reforms that aim to encourage elderly labour force participation and contain pension expenditures, by reducing the generosity of retirement benefits or tightening eligibility criteria.

A rich literature has explored how changes in the public pension systems affect labour supply and employment at older ages, as well as the retirement and benefit claiming decisions (Seibold, 2019; Fetter and Lockwood, 2018; Manoli and Weber, 2018; Lalive, Magesan, and Staubli, 2017; Cribb, Emmerson, and Tetlow, 2016; Vestad, 2013; Behaghel and Blau, 2012; Liebman, Luttmer, and Seif, 2009). A number of papers have studied the effect of increasing the normal retirement age. The focus is mostly on employment, retirement and benefit claiming decisions of individuals *at risk of retirement*, that is those who are very close or past the eligibility for early retirement (Hanel and Riphahn, 2012). The general finding is that a higher normal retirement age leads to delayed labour market exit and benefit claiming. Other studies examine increases in the early retirement age. In these cases, the focus is mostly on employment and retirement effects at ages in between the old and the new early retirement age. These works generally report an increase in labour supply at these ages, but also provide evidence of program substitutions effects (Cribb, Emmerson, and Tetlow, 2016; Kline and Walters, 2016; Atalay and Barrett, 2015; Staubli and Zweimüller, 2013) on disability insurance (Borghans, Gielen, and Luttmer, 2014; Staubli, 2011; Karlström, Palme, and Svensson, 2008) or unemployment insurance (Inderbitzin, Staubli, and Zemüller, 2016; Lammers, Bloemen, and Hochguertel, 2013).

Recent and limited literature investigates, instead, how pension reforms that affect elderly labour force participation change labour demand at the firm level. For instance, Vigtel, 2018 shows that a decrease of the legal retirement age in Norway increases the hiring of senior workers, especially of blue-collars and with previous records of sickness. Other studies, exploiting increases in retirement age find negative effects on hiring, especially of younger workers (Boeri, Garibaldi, and Moen, 2017) and female workers

(Martins, Novo, and Portugal, 2009), while others document positive effects on young and middle-aged employment (see for instance Carta, D'Amuri, and Wachter, 2019).

Furthermore, there is evidence that pension reforms apart from their effects on labour supply and demand may alter also individuals' financial investment decisions. Indeed, variations in pension benefits generosity, mandated by pension reforms, are able to affect and influence individuals' accumulation of private wealth (Bottazzi, Jappelli, and Padula, 2011; Bottazzi, Jappelli, and Padula, 2006; Attanasio and Brugiavini, 2003).

Pension reforms that postpone retirement eligibility criteria end up to prolonging individuals' labour market activity. Very often individuals become aware of modifications of retirement rules at earlier stages of their life and before they come near to making those decisions. However, as workers get older, not only their investment in human capital declines but also their human capital depreciates at an increasing rate. These issues raise a question about the interaction of a longer working horizon and the need to invest against the obsolescence of workers' skills. In such circumstances, do individuals start changing their behaviour in the aftermath of the reform announcement or implementation? In particular, do pension reforms alter incentives of middle-aged workers in investing in human capital activities? The standard prediction from human capital theoretical models is that older workers are significantly less likely to be involved in on-the-job-training programs than relatively younger colleagues. The returns on such investments are disproportionally lower for older employees rather than younger ones given that these returns crucially should be expected to depend on the time left before retirement. However, there is evidence that the return to human capital investments is affected by the pension system if finite horizon economies are considered (Echevarria, 2009): by increasing retirement age, incentives on human capital formation are higher (Lau and Poutvaara, 2001a; Lau and Poutvaara, 2001b). This is a common result in many theoretical analyses, whom empirical evidence is however scarce, because postponed retirement lengthens the time period at the extensive margin over which individuals can appropriate the benefits from human capital investments, which translates into higher returns to education (Trostel, 1993). Hence, postponing retirement raises aggregate human capital because the return to education positively depends on the remaining active years.

Hence, Chapter 2 relates to the empirical human capital literature that studies how the design of public pension systems and their reforms affects individuals' investment in human capital by exploiting pension reforms as natural or quasi-experiments that allow the estimated effects to have a causal interpretation. In particular, I exploit the *Fornero* pension reform introduced in Italy at the end of 2011 as a source of quasi-experimental variation to assess the (unintended) causal effect of an increase in the residual working life on middle-age employees' human capital investments. This reform represents an ideal framework to assess the impact of pension reforms increasing minimum retirement requirements on older workers training, given that the 2011 pension reform has represented for almost all Italian workers a sudden tighten of the minimum requirements for claiming a public pension (with increases in residual working life up to 6-7 years) and also because the reform was rapidly implemented, with very limited grandfathered clauses, avoiding, crucially for the empirical analysis, any anticipation effects from both employees and employers.

To provide empirical evidence I exploit a rich survey that apart from data on human capital activities contains information, at the individual level, on age, gender, accrued years of contribution and sector of employment that allow me to predict for each individual in the sample, eligible to retire neither before nor after the 2011 pension reform, the Minimum Retirement Age (MRA) before and after the policy change. Indeed, the reform generated different changes in years until retirement eligibility among otherwise similar older workers, given that small demographic differences led to large differences in retirement delays for individuals. The different mandated retirement age by gender, age, sector and, mostly, by previously accrued years of contribution implies that individuals have been differently affected by the reform in terms of how much the length of the residual working period before retirement did increase.

Based on the MRAs in the post-reform and pre-reform period, I construct a measure of exposure, at the individual level, to the pension reform. This time-invariant measure of policy-induced shock, given by the difference in MRAs, provides the size of the reform-induced shock that mirrors the lengthen of the employees' residual working life, relative to the previous requirements in place before the *Fornero* reform. Hence, I exploit increases in the distance to retirement that in the literature are also known as the "horizon effect" within a Difference-in-Differences approach.

The Difference-in-Difference estimates, whose detailed discussion is available in Chapter 2.4, give the following 5 main results: *i*) there exists a positive effect on human capital investment due to the increase in the length of the residual working life because of the pension reform; *ii*) the higher the education endowment, the higher the human capital investment; *iii*) there is large heterogeneity in the response to the policy among different socio-economic groups, type of employment sector and firms; *iv*) oldest workers, that is those aged between 57 and 64 years, in the aftermath of the reform did not modify their propensity in investing in training activities; *v*) positive variations in human capital are not to be ascribed to firms sponsoring training activities.

1.2 Short-run effects of COVID-19 lockdown policies on academic productivity

Chapter 3 in this thesis explores the consequences of the recent public policies aimed at containing the spread of the COVID-19 pandemic on academic productivity. The contribution of this research to the existing literature is manifold: first, this paper is among the few that investigates, causally, the short-run effect of the current health crisis on the productivity level of female economists employed in academia by also testing whether the sudden increase in childcare demand, due to the lockdowns, has played a role in causing the widening of the gender production gap; second, it provides new empirical evidence to the small but growing literature that assess the impact of policy measures adopted to face the pandemic and the subsequent response of economic agents; third, it contributes to the economic literature that studies the determinants, the causes and the consequences of the productivity gap between female and male academics; fourth, the analysis is based on a sample of data, observed from January to July 2020, that allows to estimate, almost in real time, the causal effect of interest.

The literature on the economic effects of the epidemics started almost as soon as many governments implemented lockdowns to contrast the COVID-19 pandemic at the beginning of 2020. The interest of researchers in evaluating such policies is due to the fact that lockdowns are extremely costly. From a social and psychological perspective, keeping people away from friends and extended family members will significantly impact on their well-being. School closures will slow down children's development with likely long run consequences and will impair the possibility of their parents, especially mothers, to work. From an economic perspective, closing economic (non-essential) activities will cause a dramatic GDP reduction, leaving many workers without income and many entrepreneurs without cash-flow. Governments normally support workers with transfers to avoid a sudden decrease of their consumption level and provide liquidity to firms in order to guarantee at least their continuity, but those policies require a huge amount of financial resources that very likely translate into additional public debt. Heavily indebted countries might find it hard to refinance their debt on the market, and their increased borrowing cost, with the consequent further increase in debt, will force them to implement restrictive fiscal policies in the future, even though monetary policy accommodating fiscal policy may alleviate these funding issues. Overall, economic growth will most likely slow down and unemployment will most likely be persistently high for many years after.

Several studies exploit theoretical models to assess the impact of lockdown measures on the spread of the virus, and many of these incorporate an explicit epidemiological structure into a macro model (SIR-Macro models). Examples include, among others, Alvarez, Argente, and Lippi, 2020, Atkeson, 2020, Berger, Herkenhoff, and Mongey, 2020, Collard et al., 2020, Eichenbaum, Rebelo, and Trabandt, 2020, Glover et al., 2020 and Piguillem and Shi, 2020. Fewer works, instead, investigate empirically the role of public policies to contain the contagion (see for instance: Bonaccorsi et al., 2020; Dave et al., 2020; Di Porto, Naticchioni, and Scutinio, 2020; Fang, Wang, and Yang, 2020; Friedson et al., 2020; Hsiang et al., 2020). Other papers have questioned the effectiveness of non-pharmaceutical interventions (*i.e* lockdowns) on mortality and economic growth by looking at historical epidemic episodes such as the Spanish flu (see Barro, Ursua, and Weng, 2020; Carillo and Jappelli, 2020; Correia, Luck, and Verner, 2020; Moller Dahl, Hansen, and Jensen, 2020; Hatchett, Mecher, and Lipsitch, 2007).

The literature, however, has started not only to investigate whether such policies are effective in containing the pandemic, but also its likely real-time consequences on economic agents (and their behavior). In particular, many papers began to study the labour market effects of the current health crisis, by looking at the behavior of labour demand (Dalton, Kahn, and Mueller, 2020; Kahn, Lange, and Wiczer, 2020a) and labour supply (Kahn, Lange, and Wiczer, 2020b). As governments mandated social distancing rules, many firms switched to flexible working arrangements (or home-working), and scholars inquired how many and what types of jobs can be done at home (see for instance Basso et al., 2020, Dingel and Neiman, 2020).

Differently from past recessions, the COVID-19 pandemic is affecting the most economic sectors that, because of their specific characteristics, are not able to comply with social distancing, where usually the share of female employment is higher. Indeed, it is reasonable to think that the current crisis is going to produce further labour market inequalities across gender, evidence that appears supported by data (see for instance Alon et al., 2020). Furthermore, raising inequalities in the labour market are also accompanied by further inequalities within the family given that, since the lockdown began, the amount of time devoted by women to household duties and childcare has considerably increased worldwide (Adams-Prassl et al., 2020; Del Boca et al., 2020; Hupkau and Petrongolo, 2020).

This paper builds on this strand of the literature and contributes, further, to the literature analyzing gender gaps in the academic sector. Despite a substantial growth of the share of female in the economic field of academia, their share is still lower than other fields. For instance, Lundberg and Stearns, 2019, using data on the share of female faculty in top-50 departments for several science and social science disciplines from 2002 to 2012, find that female economists remains within the lowest group, along with physics, math, and engineering, and far below the biological and other social sciences.

Potential explanations for the low representation of women include differences in the preference for competitive environments (Buser, Niederle, and Oosterbeek, 2014; Niederle and Vesterlund, 2007) or in bargaining abilities in the labor market (Blackaby, Booth, and Frank, 2005), the presence of children and differences in child-rearing responsibilities (Bertrand et al., 2018; Bertrand, 2013), and gender-based discrimination (Goldin and Rouse, 2000).

However, Ceci et al., 2014 find economics to be an "outlier" among academic fields because of "*a persistent sex gap in promotion that cannot readily be explained by productivity differences*". Many feel that economics, in particular, remains a male-dominated field. Indeed, a recent survey of American Economic Association in 2019 describes the economics profession as a highly competitive environment that is hostile to women and, in particular, related to experiences of discrimination in invitations to participate in research conferences, associations, and networks.

Studies investigating the labour market outcomes of female economists in academia, overall, find that women in academic economics receive unequal treatment, along several different dimensions: promotions (Ginther and Kahn, 2014; Ginther and Kahn, 2004), hiring opportunities (De Paola and Scoppa, 2015; Williams and Ceci, 2015), editorial standards for submissions (Hengel, 2017) and lower rate for grant proposals (Lee and Ellemers, 2015; Wennerds and Wold, 1997; Broder, 1993).

By exploiting daily-level data on working paper retrieved from the two most popular economics working paper series on SSRN, that is *Economics Departments Research Papers* and *Economics Research Centers Papers*, I assess whether in the aftermath of lockdown policies female economists' productivity has fallen by estimating a Difference-in-Differences model.

The identification strategy, that allows my estimates to have a causal interpretation and described with greater details in Section 3.2, exploits the lockdown policies enacted as a result of the COVID-19 pandemic as an exogenous shock that has caused substantial disruptions to academic activities, requiring academics to conduct research, teach, and carry out household duties at home.

The main results of Chapter 3 - whose detailed description is contained in Chapters 3.3, 3.4 and 3.5 - are the following: *i*) lockdown measures had a significant impact on female economists' productivity; *ii*) the number of papers written by a female economist, alone or jointly with other researchers, uploaded on SSRN declined of about 20 percentage points and this negative effect persists up to about 4 months later; *iii*) declines in productivity, however, disappear during the school re-opening period suggesting that indeed childcare demand has been an important channel in causing women production drop; *iv*) declines in productivity were not associated with increases in pre-prints' quality.

1.3 Public incentives and firms' innovative investment

Chapter 4 of this thesis contributes to literature investigating the effectiveness, in terms of input-additionality, of public funding for innovation activities in enhancing firms' investment expenditures by verifying empirically whether public funds substitute or complement private investment in innovation. Furthermore, it contributes to empirical studies that aim to estimate the relationship between *R&D* investment and its price. Differently from the wide available evidence on the topic, in this study I explore also whether public funding for innovation activities may have different effects depending on the economic sectors in which firms operate (that is the manufacturing and the service sector) and whether a differential investment pattern in innovation exists according to the level of knowledge intensity of the business within the two sectors. Finally, I develop, also, a simple hypothesis test to evaluate whether increases in innovative investments, because of public funds, are compatible with the hypothesis of complementarity ("crowding-in" effects) between public support and private efforts in innovative spending or not.

In the aftermath of the global economic and financial crisis *R&D* and innovation activities have experienced huge negative declines. In the OECD countries as a whole the growth rate of GDP fell by 3.5% in 2009, while business *R&D* investment dropped by 4.2% (OECD-STI, 2014). Investment in *R&D* has exhibited, at this highly aggregate level, a pro-cyclical behavior over the last twenty years, according to data published by the OECD. The growth rate of GDP and of gross domestic *R&D* investment have been positively correlated over the period 1996-2016. This mirrors mostly the behavior of business *R&D*, since the correlation between GDP and of public *R&D* expenditure growth rates has been negative across that same period, which is suggestive of a mildly counter-cyclical behavior on average. The potential threat to long-term growth derived from reduced business *R&D* effort in downturns may thus have been partially mitigated by public policies. In addition, several sources of market failures that lead to a sub-optimal provision of *R&D* investment justify the governments' promotion of research and innovation activities, both public and private. Using different policy instruments, the primary goal of policymakers is to achieve a level of *R&D* investment which is socially optimal.

Implementing effective innovation policies is not an easy task, even though theoretical arguments and empirical evidence support public intervention in this regard. Policy makers have imperfect information about which innovation projects are deterred, and to what extent, as a consequence of knowledge spillovers or of firms' financial constraints, and about whether the social benefits of supporting them would exceed social costs and when. Ex-post policy evaluation becomes then an important tool to help and check the effects of a policy given the institutional and business environment. It can also provide useful information to revise it.

From a policy perspective it is essential to know whether public support addresses in practice common sources of underinvestment in innovation (Busom, Corchuelo, and Martínez-Ros, 2014). Even if support programs have positive effects on some measures of firm performance, this does not prove that these programs reach firms that face financing or other market failures that often affect innovation.

Several issues have to be taken explicitly into account when analyzing direct support - loans or grants - to firms in particular: *i*) allocation of support is not random, but a result of a firm's decision to apply for it and the public agency to award it; *ii*) perceived barriers to innovation may affect the resulting allocation; *iii*) returns to innovation may differ significantly across the firms' productivity distribution, and *iv*) allocation of support and returns to innovation might differ across manufacturing and service industries.

This research is related to studies on firms' *R&D* and innovation investment choices during the recent crisis and to studies evaluating the impact of public support on these decisions. Earlier studies have shown that business *R&D* investment is pro-cyclical on average, both at the aggregate and firm level (Beneito, Rochina-Barrachina, and Sanchis-Llopis, 2015; Fabrizio and Tsolmon, 2014; Cincera et al., 2012; Aghion et al., 2012; Aghion et al., 2010) suggesting that procyclicality is mainly driven by the joint or separate action of market imperfections and knowledge spillovers, generating not only a static market failure but also inducing a dynamic misallocation of *R&D* investment over the cycle.

The study of the effectiveness of different policy instruments used by governments and public agencies - subsidies, loans, tax deductions, and so forth - to provide incentives to increase private *R&D* and innovation investment has been the focus of evaluation research for some time (see Zúñiga-Vicente et al., 2014 for a survey and Czarnitzki and Hussinger, 2018). In general, empirical studies show that innovation subsidies may have the potential for encouraging firms to engage in *R&D* and to invest more intensely.

Furthermore, several studies report that public support for innovation can affect firms' economic outcomes beyond innovation efforts and productivity, such as firm survival and employment as well as a reduction in the potential risk of delaying, quitting or stopping further innovation projects (García-Quevedo, Segarra-Blasco, and Teruel, 2018; Czarnitzki and Delanote, 2017; Hottenrott and Lopes-Bento, 2014; BEIS, 2014; Cerulli and Potí, 2012; Mohnen et al., 2008).

Most of the empirical studies, furthermore, argue that the effectiveness of public policies in sustaining innovation efforts in the private sector crucially depends on the size of the firm and on the technological intensity of the sectors in which firms operate. Traditionally, the literature has analyzed the effectiveness of public support on innovation by considering only the manufacturing sector (the more *traditional* one), while neglecting whether public incentives could have had effects on the service sector as well. Many advocates that in manufacturing industries most innovations are based on the ability to generate new knowledge by engaging in costly *R&D* activities. Knowledge, however, may be subject to spillovers that reduce the private return and therefore the incentives to carry them. In services, on the other hand, innovation sources may be more diverse whose private returns are usually less likely to be affected by this potential source of market failures. However, the service sector includes a large and very heterogeneous set of activities that differ from manufacturing in several respects. First, many produce mostly intangible outputs, and the intangibility of many services means that they may be affected, to a greater extent than manufacturing industries, by issues derived from asymmetric information regarding service quality and properties. Indeed, many services consist precisely on the provision of information, whose quality and value may be uncertain until it is consumed providing more room for problems such as adverse selection and moral hazard. Given that asymmetric information can generate market failures, this is likely to affect costs and rewards of innovating (for instance, they can raise the cost of capital for corporations, reducing investment in general; see Choi, Jin, and Yan, 2013). Nonetheless, there is evidence that support coming from public funding can have the same effectiveness for service firms as in the manufacturing sector. Indeed, several studies report that investment in *R&D*, supported also by public incentives, and the introduction of innovations in services are significantly correlated, and that innovations affect productivity, as in manufacturing (Peters et al., 2014; Segarra-Blasco, 2010; Musolesi and Huiban, 2010; Arvanitis, 2008;

Lööf and Heshmati, 2006).

Hence, as recent developments in the scientific fields of psychology, neurosciences and behavioral economics suggest, *R&D* activities are likely to play an increasing role in generating new types of innovations across all industries, and especially in the service sector.

The empirical analysis exploits an investment subsidy program taken place in southern Italy in 2013 targeted to small and medium enterprises (SMEs) operating in the region *Campania* (south Italy), where firms were invited to submit proposals for new innovative projects and only those that scored above a certain threshold received a subsidy. This program represents an interesting case study since participating firms willing to be financed in developing innovative activities must operate (and be located) within the region boundaries and it obliged participating firms to request funding only for brand new investment projects and to develop them only with regional support. By matching regional data on subsidy recipients and non-recipients firms with balance sheet data, provided by Bureau Van Dijk, I estimate a Difference-in-Differences model by comparing the average expenditures of public incentive recipients and non-recipients firms, identified by taking advantage of the assignment scheme of the subsidy, before and after the program implementation.

The results, whose discussion is available in Chapters 4.4, 4.5, 4.6 and 4.7, give the following results: *i*) because of the subsidy, treated firms increased their level of spending in innovative investment projects, relative to the pre-treatment period; *ii*) I find that the positive effect on investment comes from micro- and small-sized firms as well as firms operating in high tech sectors and high tech service firms; *iii*) I am not able to reject the hypothesis that firms increased spending by about approximately the amount of the subsidy; *iv*) the program had sizeable spillover effects on firms' labour demand but not on productivity.

Chapter 2

Later life human capital investment Evidence from the unintended effects of a pension reform

2.1 Introduction

Do pension reforms alter incentives of middle-aged workers in investing in human capital activities?[1] Many Europeans countries are facing a sustained increase in the average age of their working population, and the European Commission, 2007 has pointed out the need of favoring middle-aged workers' skills updating along with lifelong learning in response to the growing pressures brought by globalization and technological changes on the labour market[2].

According to standard human capital theory, an individual's life-cycle can be distinguished in four different phases (Blinder and Weiss, 1976). During the first two phases

[1] *The views expressed in the article are those of the author only and do not involve the responsibility of the Italian Institute of Public Policy Analysis.*

[2] In fact, a skilled and educated workforce is recognized as one on the key factors for improving the productivity of firms and countries economic development and growth (Acemoglu and Pischke, 1999; Acemoglu and Pischke, 1998; Evans, Honkapohja, and Romer, 1998; Mankiw, Romer, and Weil, 1992; Lucas, 1988; Romer, 1987). Furthermore, when tastes and technologies are changing rapidly, human capital investments are essential to maintain high levels of competitiveness and of employment. Without a workforce that is continually acquiring new skills, it is difficult to reap all the returns from technological progress. Moreover, not having enough of the right skills in the workforce may further aggravate inequalities.

the individual acquires formal education and provides labour, by improving also human capital. The third phase comprises mainly employment with minimal or null human capital investments, reaping the benefits of previous accumulated knowledge. The fourth phase, instead, only regards retirement. The standard prediction from these theoretical models is that older workers are significantly less likely to be involved in on-the-job-training programs than relatively younger colleagues because the returns on such investments are disproportionately lower for older employees. Indeed, these returns crucially should be expected to depend on the time left before retirement. Also early retirement institutions, human capital depreciation (Neuman and Weiss, 1995) and lower learning ability and flexibility of senior employees cause lower incentives in providing older workers with investments in training, also in light of the view that they cannot benefit from the dynamic complementarities that characterize human capital accumulation as younger ones (Cunha and Heckman, 2007; Heckman, 2000).

Hence, a recognized problem is that senior workers and their employers have only a short time to recoup their investment in skills before retirement occurs (Ben-Porath, 1967; Becker, 1962). This problem raises the question whether pension policies that increase minimum retirement age, therefore forcing affected senior employees to stay longer in the labour market, can contribute to stimulate training investments. Indeed, pension reforms aimed at increasing minimum age and contribution requirements crucially alter the probability of retirement of a given individual by directly increasing the length of his residual working horizon. As predicted by the theory, (positive) variation in the distance to retirement affects training benefits given that it widens the payback period of human capital investments. Therefore, within a life-cycle model of human capital investments, variations in minimum retirement age affect the start of the fourth phase, and thereby also the turning point between the second and third phases. It can be predicted that a lower probability of retirement, implied by the lengthen of the residual working life, increases the likelihood that future training benefits can be reaped both by the old worker and the firm, and therefore increases the incentive to invest.

In this paper, I exploit the *Fornero* pension reform introduced in Italy at the end of 2011 as a source of quasi-experimental variation to assess the (unintended) causal effect of an increase in the residual working life on middle-age employees' human capital investments. I refer to the unintended effects given that the pension reform's main aim was not to directly have effects on human capital investments but rather on retirement age and pension benefits. Italy and the *Fornero* reform represent an ideal framework to assess the impact of pension reforms increasing minimum retirement requirements on older workers training for a number of reasons. First, Italy has one of the oldest populations among advanced economies, well above the OECD and the EU averages, and low labour market participation at older ages. Second, the *Fornero* reform has represented for almost all older Italian workers a sudden tighten of the minimum requirements for claiming a public pension, implying that for almost all of them residual working life increased considerably (up to 6-7 years). Third, the pension reform was rapidly implemented, with very limited grandfathered clauses, avoiding, crucially for the empirical analysis, any anticipation effects from both employees and employers. Fourth, soon after its approval, a prolonged and inflamed public debate occurred implying that the majority of the population understood (or at least were aware of the consequences brought by) the policy.

In order to provide causal evidence, I rely on a Difference-in-Differences approach where my treatment variable is given by a time-invariant measure of policy-induced shock. That is, I construct a measure of exposure to the pension reform, at the individual level, by relying on the difference of the Minimum Retirement Age (MRA) in 2017, that is the post-reform period, and 2011, the pre-reform period. Hence, the variation in MRAs provides the size of the reform-induced shock that mirrors the lengthen of the employees' residual working life, relative to the previous requirements in place before the *Fornero* reform. Hence, I exploit increases in the distance to retirement, that in the literature are also known as the *horizon effect* (also as *forward looking* or *perspective* effect). Individual-level data on labour market histories and human capital investments are drawn from the Participation, Labour and Unemployment Survey

(PLUS) a bi-annual survey administered by the Italian Institute of Public Policy Analysis (INAPP). I consider the survey's waves that go from 2007 to 2017, that is the years around the *Fornero* pension reform, and a sample of individuals aged between 40 and 64 years with at least 10 and less than 40 years of accrued years of contribution, eligible to retire neither before nor after the 2011 pension reform. I develop this empirical test of human capital theory predictions, that is if a lengthen in residual working life induce additional human capital investment, by looking at three different outcomes. The first I consider is the probability that individual i during the last 12 months prior the interview has attended some kind of human capital activities aimed at improving or updating her skills or knowledge. In particular, these activities refer to seminars, conferences, training courses or professional refresher courses and, hence, I focus on activities that human capital theory defines as formal on-the-job training[3]. Then, I extend the empirical analysis by looking at other two outcomes that have not been investigated in the literature: the probability that individual i paid for her on-the-job training, conditional on having invested in human capital activities, and the role of firms in inducing investment in training. Given that mandated positive variations in MRA translate in an increase of the payout period of the investment, older workers may find profitable to increase their stock of knowledge by directly investing in it in order to bargain a higher wage. Hence, I test whether the willingness of middle-age workers in investing directly (and so paying for it) in human capital changed in the aftermath of the reform. Finally, I explore also the role of firms in inducing its middle-aged workers in participating in training programs given that the human capital section of the survey contains a specific question on whether the employers has strongly recommended the workers to attend or sponsored the training activity (without, however,

[3]In general, human capital refers to both formal training (formally organized activities such as apprenticeships, workshops, and courses) and informal training (learning-by-doing or work experience). While the Mincer, 1962 definition of on-the-job training includes both types of activities, Arrow, 1962a, instead, highlights with more preponderance the importance of learning-by-doing. Furthermore, training can be also distinguished in general and specific training. The former represents skills that can be used at many other firms, and are portable across companies as individuals change jobs, whereas, the latter is by definition only valuable to the firm providing the training. However, the focus in this paper is on formal on-the-job training, but however the data I exploit do not allow me to discern between general or specific training investment.

implying that the firm or the employer paid for it). Indeed, analogous individual-level human capital effects can be also be found in a model of firms' investment: when the working life of employees increases, if workers are not perfectly mobile, overall firms' investment in human capital increases too (Acemoglu and Pischke, 1999; Acemoglu and Pischke, 1998). Despite I cannot directly observe in the data whether training is directly financed by firms, I can still explore this channel by looking at the role of firms in suggesting older workers to update their human capital[4].

According to my estimates, I find that the causal effect of an increase in the length of the residual working life, due to the *Fornero* pension reform, has a positive effect on human capital investment. For each additional year increase in MRA, the probability that an individual invest in human capital goes up of about 0.7 p.p. (that is about 1.7 percent when re-scaled in terms of sample mean). However, the response to the reform was very heterogeneous and mainly driven by men (0.9 p.p. for each additional year or re-scaled in terms of sample average about 2.5 percent) and married women (1.3 percentage points). Furthermore looking at the age profile of individuals, I find that increases in human capital investment occur only for those workers known as prime-aged (both men and women) and middle-aged (only men). In terms of sector of employment and firms' economic sector of activity, I find that the positive effect on human capital investment comes from self-employed individuals (1.5 p.p. or to about a 4 percent increase when compared to the sample mean) and from those who are employed in firms operating in the service sector (0.8 p.p.).

In addition, I explore also whether the hypothesis of complementarity between education attainment and investment in human capital, as theory states, holds empirically. My estimates suggest that, due to the pension reform, individuals with higher education have a higher probability of investing in human capital, and this relationship emerges more strongly for the sample of men and married women.

[4]It has to be said that for the empirical test of the human capital effect, the information on whether training is directly financed by the firms is not required as I focus on the effect of the lengthening of working life on training investment and not on the incidence of the human capital investment at the firm level.

Finally, I (indirectly) investigate the role of firms in providing training for their employees as well as the willingness of affected workers in directly investing (that is paying for investment) in human capital. Overall, for individuals employed in very small-sized firms (those with 1-9 employees) for a 1 year increase in their residual working life, the probability of attending training activities increased by about 1.8 percentage points (7% if compared to the sample average). A comparable magnitude is found when I split the sample not only by firms' size by also by economic sectors. For individuals employed in small-sized firms operating in the service sector, the probability of human capital investment goes up of about 2 percent. With regards to the propensity of individuals in paying for human capital activities I do not detect any statistically significant effect in the aftermath of the reform, whereas, I find that for each additional year in the lengthening of the residual working life affected individuals experienced a decrease of about 0.8 percentage points in the probability that the training activity was sponsored by the firms where they are employed.

This paper is related to several strands of the literature. Most importantly, I contribute to the empirical studies related to the human capital theory that estimate the effect of variations in pension requirements on training activities. However, only few papers use individual-level data and assume an endogenous process of human capital investment[5] by exploiting policy variations that more credibly are able to deliver estimates that can be interpreted as causal effects. These papers usually exploit pension reforms showing that an increase in the working life, implied by increase in mandated retirement age, has sizable, positive and statistically significant effects on human capital accumulation (Gohl et al., 2020; Brunello and Comi, 2015). Similar results can be found also in Bauer and Eichenberger, 2017; Fan, Seshadri, and Taber, 2017 and Battistin et al., 2012 where they show also that increases in mandated minimum early retirement age substantially reduce retirement probability. By the same token, Fouarge and Schils, 2009 show that generous early retirement options significantly reduced

[5] Fan, Seshadri, and Taber, 2017 relying on a structural model shows that curtailing pension benefits leads to increase in human capital accumulation, providing empirical evidence that the assumption of exogenous human capital process in many theoretical models is not supported by data.

older worker human capital accumulation, or that, instead, pension reforms aimed at curtailing early retirement benefits are able to induce workers in increasing their stock of human capital (Montizaan, Corvers, and De Grip, 2010). However, several other papers reached opposite conclusions finding that training incidence decreases with age (Bassanini et al., 2005; De Grip and Van Loo, 2002).

A closer strand of the literature, instead, analyzes how variations in residual working life affects firms or employers training investment decisions. With regards studies exploiting the *Fornero* reform, it has been showed that firms more affected by the 2011 pension reform, because of a higher share of retained older workers that otherwise would have been retired, increased investment in human capital (Quaranta and Ricci, 2017) provided that they were funded externally (Berton, Guarascio, and Ricci, 2018) or partially financed through funds co-managed with unions (Berton, Guarascio, and Ricci, 2017).

Furthermore, other studies, mainly at the firm level, have showed also that investments in human capital benefit overall firm performance (Martins, 2020; Dostie, 2018; Almeida and Carneiro, 2009). This study is also, indirectly, related to the literature that analyzes the consequences of increases in retirement age, or more in general workforce ageing, and firms' productivity, overall performance and interactions with labour market institutions (see Brunello and Wruuck, 2020 for an extensive survey), channels not yet well understood. With regards health-related outcomes, Bertoni, Brunello, and Mazzarella, 2018 find that a postponement of minimum retirement age, because of a pension reforms, has a positive effect on the (self-reported) health of affected individuals. Concerning labour market institutions, despite the limited empirical evidence, economic reasoning suggests that higher employment protection should increase the incentives of firms-provided training. On this issue, Bratti, Conti, and Sulis, 2021 find that reducing EPL increase firm-provided training, whereas Messe and Rouland, 2014 show that higher EPL has no effect on the training of older workers. With regards to productivity, Acemoglu and Restrepo, 2017 find that an increase in the share of older workers relatively to middle aged ones is positively associated to adoption of new technologies with ambiguous effects on overall labor productivity. Carta, D'Amuri, and Wachter, 2019, exploiting the same pension reforms as I do, find that a 10% increase in older workers does not harm employment growth of younger workers, leaving labor productivity and unit labor costs unchanged.

A further connection of this paper is with the literature studying how the characteristics of social security systems affect agents' behaviours, where most of the papers focus on how individuals' incentives to retire are determined by the legal retirement age (Manoli and Weber, 2016; Lalive and Staubli, 2015; Staubli and Zweimüller, 2013; Mastrobuoni, 2009) or by pension benefit rules (Liebman, Luttmer, and Seif, 2009; Krueger and Pischke, 1992).

This paper, finally, speaks to the strand of the literature that uses variation in mortality rates in order to assess variation in human capital accumulation (for an extensive survey see Bloom, Kuhn, and Prettner, 2019) which, however, provides mixed findings (Hansen and Strulik, 2017; Oster, Shoulson, and Dorsey, 2013; Lorentzen, McMillan, and Wacziarg, 2008; Jayachandran and Lleras-Muney, 2009; Acemoglu and Johnson, 2007; Kalemli-Ozcan, Ryder, and Weil, 2000). Nonetheless, these studies suffers of at least two criticisms. First, as discussed by Cervellati and Sunde, 2013 and Hazan, 2009 what matters the most for investment in human capital are the survival rates during adult life rather than the change in the life *per-se*. Second, variation in life expectancy is rarely random or unexpected, complicating causal estimation and results interpretation.

2.2 The Italian pension system and the 2011 reform

The Italian pension system, as well as that of many OECD countries, is characterized by a large first pillar, that is public pension funds, and by almost marginal second and third pillars, that is compulsory and voluntary private pension funds[6]. Specifically,

[6] In 2007, the implementation of the severance pay (Trattamento di fine rapporto, TFR) reform has introduced an automatic enrolment mechanism for voluntary pension funds. According to the reform,

the main pillar of the Italian public pension system is a compulsory *pay-as-you-go*, meaning that the contributions that workers and companies pay to the Social Security Institute are used to pay the pensions of those who have already left their job, that is those who are retired. Furthermore, the system offers two schemes under which claiming full retirement: the old age and the seniority pension schemes. They both feature requirements on age and on years of contributions. Under the old-age pensions scheme, individuals retire after having achieved a certain minimum age; whereas, under the seniority pensions scheme, individuals retire after having accrued a given number of years of contribution. Pension benefits are computed using a combination of defined-benefits (DB) and notional defined-contributions (NDC) methods. Specifically, under the DB regime benefits are computed according to the following earning based formula: $b = \rho N w_r$ where ρ is the accrual rate, N are years of contributions, and w_r is the average salary earned during the last r years of a worker's career. Under the NDC scheme, instead, social security contributions accrue into a notional account which are capitalized using a five-year moving average of the nominal GDP growth rate. They are then transformed into annual benefits through a transformation coefficient that depends on age at retirement and life expectancy.

Apart from the old-age and seniority schemes, there exists only one early retirement option called *Opzione Donna* introduced in 2004 on an experimental basis (and still in place), that, however, is only available for women. It allows to claim benefits before meeting the old-age or seniority pension requirements. Retiring early, however, comes at the cost of receiving sizably lower pension benefits. The cost of opting for it corresponds, on average, to a 35% reduction of the full pension benefit (Istituto Nazionale di Previdenza Sociale, 2016) given that pension entitlements under this option are computed applying the NDC regime to contributions accrued both before and after 1996.

the private sector workers' severance pay will be automatically paid into an occupational pension plan, and not anymore retained in the firm, if they do not opt out. However, according to Commissione di Vigilanza sui Fondi Pensione, 2019 only one-third private sector workers have a contract with a private pension fund, whose benefits are conditional on the eligibility for a public pension.

The private and public-sector social security tax rate is 33 percent: one-third is paid by the employee and two-thirds by the employer. For those who are self-employed and pay contributions to the Social security Institute the social security tax rate ranges between 24 and 34 percent. Retirement is not mandatory and working past retirement is allowed.

During the last three decades, the Italian pension system was dramatically revised through a long reform process aimed at improving its financial sustainability. Indeed, the progressive increase in Italian population aging has meant that pensions have to be paid for a longer period implying that the flow of Social Security Institute's income (represented by contributions) was not in balance with the amount of expenses (the pensions paid). In addition, the slowdown in economic growth has further decelerated contribution income. To cope with this situation, a series of reforms have been implemented, all aimed at bringing pension expenditure under control. In 1995, the *Dini* reform[7] introduced in the Italian pension system the notional defined-contribution (NCD) method, a way of computing pension benefits considered more actuarially fair given that it links the life-time paid contributions to total future pension benefits[8]. However, the transition from a defined-benefit (DB) to a notional defined-contribution (NDC) basis was gradual, involving only those who had less than 18 years of paid contribution before January 1, 1996.

Several legislative interventions from 1996 onward, motivated by public finance reasons, increased the requirements for claiming a pension, acting above all on those whose pension was computed according to the DB basis, but ending up also affecting the

[7]Three years earlier than the *Dini* reform another policy measure was legislated to try to curb pension expenditures. The *Amato* reform (legislative decree no. 503/1992) increased the requirements for claiming an old-age pension. According to the directives contained in the decree, the retirement age for old-age pensions, managed by the Social Security Institute, was raised from 55 to 60 for women and from 60 to 65 for men, while the necessary contribution years became 20 (15 before the reform). In addition, having fulfilled the requirements each worker was entitled to a pension calculated on the basis of the salary of the last 5 years according to the DB method.

[8]The introduction of the NCD method was motivated by the attribution of a freedom of choice to workers in relation to the age in which to claim the first pension. This principle of actuarial equity had not been applied in the computation of DB pension benefits, which, instead, pushed individuals to claim the pension as soon as possible, as the amount of the pension was not a function of the age of the worker at the start of the retirement period.

workers' pension requirements affected by the *Dini* reform. Overall, all these reforms aimed at increasing the retirement age and at curtailing pension benefits.

At the end of December 2011, the new technocratic government approved an emergency package of measures, the *Salva Italia* decree, in response to the pressure of the financial markets on the Italian sovereign debt that reached unprecedented levels. Among the emergency measures approved a substantial pension reform was introduced[9]. The reform, known as the *Fornero* reform (Law 22 December 2011 no. 201), entered into force in January 1, 2012 (ten days after its approval) and raised age and contribution requirements to claim old-age and seniority pensions, by reducing the number of new retirees and increasing the average age at retirement[10]. The new rules applied to all workers who did not accrue the right to claim either pension by the end of 2011[11]. Finally, the *Fornero* reform, in addition to increasing the mandated retirement age, changed the pension benefit formula for those who were still covered by the defined-benefit method of calculation (individuals with at least 18 years of accrued contribution by January 1996), moving them to the notional defined-contribution method for working years after 2011.

The technocratic government specifically targeted the pension system because it was

[9] Despite the pension reform was the central component of the decree, other measures were legislated aimed to increase taxation on real estate, cars, and consumption. The whole text of the law can be accessed at Decreto Salva Italia, Gazzetta Ufficiale.

[10] According to Fondazione Itinerari Previdenziali, 2020, after the implementation of the *Fornero* reform the (average) effective retirement age has increased. However, the the rise in the average age at which first pension installments are claimed differentially evolved. The highest increase, on average, has been experienced by women retiring under the old-age scheme (about 4 years and 6 months). For men, instead, the rise has been of about 7 months. With regards the seniority scheme, the (average) effective retirement age evolved according to the increase in the required years of accrued contributions (43 and 42 for men and women, respectively; whereas up to 2011 the requirement was set to 40 years of paid contributions). Women retiring under this regime faced an increases of about 2 years and 6 months, whereas men 2 years and 1 month. However, it should be reminded that retiring according to the seniority regime only implies requirements in terms of accrued years of paid contributions and not in age. For more details see Figure 2.3.

[11] An important feature of the reform is that grandfather clauses were very limited. They only applied to workers who were eligible to claim a pension under the old rules by December 31, 2011, and to a couple other specific categories. These are: workers *collocati in mobilità* according to law 223/91 and based on collective agreements signed before 31/10/2011; workers who, as of October 31, 2011, were beneficiaries of *prestazioni straordinarie a carico dei fondi di solidarietà di settore*; workers who, as of October 31, 2011, had ceased to work but had been authorized to continue to pay contributions. The lack of grandfather clauses meant the reform had an immediate effect on the retirement decisions of most Italian workers.

one of the main drivers of the increase in the national debt. In 2011, public pension spending amounted to 14 percent of the GDP, twice as much as the OECD average of 7 percent (OECD, 2011). This discrepancy between Italy and other OECD countries was due to a combination of more generous pension benefits and a more rapidly aging population. In 2011, 33 percent of the Italian population was over age 65, compared with only 23.6 percent among other OECD countries. Moreover, it was normal for retired workers to rely exclusively on public pensions. In 2009, only 12.5 percent of the working age population (16-64 years old) invested in private pension funds (OECD, 2011).

The reform raised the age requirement for old-age pensions, whilst leaving the contribution requirement (20 years) unchanged. The statutory retirement age was 60 (61) for women (women employed in the public sector) and 65 for men (irrespective of their sector of employment) in 2011. Absent the reform, it would have risen to reach 61 years and 10 months for women and 65 years and 7 months for men and women employed in the public sector in 2018. Per effect of the reform, the old-age statutory retirement age has gradually increased to reach 66 years and 7 months for both genders in 2018[12] (see Table 2.1). The change in the age requirement was thus considerably larger for women than for men.

In addition, the reform modified the rules for claiming seniority pensions. A "Quota system" was in place until 2011. Workers could retire as soon as their age and years of contributions summed to a certain "Quota", conditional on both surpassing a certain threshold. In 2011 the quota was set to 96, conditional on being at least 60 years old and having at least 35 years of contributions. Alternatively, workers could retire upon totalling 40 years of contributions, regardless of their age. The *Fornero* reform abolished the "Quota system" and it legislated that a seniority pension could be claimed upon totaling at least 41 years of contribution for women and 42 for men (irrespective of their age; see Table 2.2). Thus, workers planning to retire under the "Quota system"

[12]The reform allowed all individuals to retire at 70, as long as they have accrued at least 5 years of paid contribution.

faced a large increase in years until pension eligibility, up to 6-7 years.

However, the reform did not change the early retirement rules. The take-up of early retirement was very low before the reform because of the cut in benefits. After the reform, which heavily raised requirements for women, the take-up of *Opzione Donna* increased. As a result, the take-up of *Opzione Donna* remains limited involving only less than 65,000 women over the period 2008-2016 (representing around 20% of women who could have exercised the early retirement option; Istituto Nazionale di Previdenza Sociale, 2016).

2.3 Data and empirical strategy

Data. In this analysis, information on human capital accumulation activities and labour market histories come from the Participation, Labor and Unemployment Survey (PLUS) which is a biannual survey administered by the Italian Institute of Public Policy Analysis (INAPP) to a sample of Italian individuals, about 55,000 respondents per wave, and contains information on several aspects of the labour market with a complete coverage of the Italian population and in particular of all employees. Among the main features of this survey, it allows to investigate some specific aspects of the labor market referring to a series of sub-populations such as the entry to work of young people, the extension of the active life of the population in the elderly age classes, the participation of the female component the workforce up to the knowledge of the intensity, attitudes and ways of looking for a job with the possibility of analyzing these indicators together with variables such as income (from work and family), education and the family background of individuals, individual working histories, services in the area, health, etc.

In particular, crucially for the empirical analysis the survey provides a specific section where are collected all the information regarding human capital investment activities attended by respondents, apart from those connected with standard education. Specifically, individuals are asked if during the last 12 months they attended some kind of activity aimed at increasing their knowledge and competencies[13]; if they directly paid for attending them and if their employers (usually firms) sponsored the activity (that however do not necessarily imply that they paid in behalf of the worker)[14]. Hence, the availability of these data allows me to investigate the causal effect of an increase the residual working life period on later life human capital investment. Furthermore, the data coupled with precise information on education levels allows me also to check whether the level of schooling education correlates with additional investment in human capital. Finally, the richness of these data allows me to further investigate the propensity of individuals in investing directly (*i.e.* paying for) in additional human capital and what is the role of firms in inducing middle-aged workers in increasing or updating their knowledge level.

The empirical analysis builds on the most recent waves of the survey, that is from 2007 up to 2017, that include the years around the *Fornero* pension reform.

The PLUS data allows me to construct pension eligibility criteria because it includes information on age, gender, sector and type of employment and, importantly, on accrued years of contribution; this allows me to build for each individual the Minimum Retirement Age (MRA) on the basis of the eligibility rules in place each year.

Moreover, it collects information on expected retirement age (for individuals who are working at the time of the interview) but also on retired individuals by envisaging a specific question about the age at which the individual retired (as well as her sector of employment and years of accrued contributions) that represents a crucial piece of information to support the identifying assumptions and the soundness of the approach regarding the identification of the shock.

Despite the PLUS data has a longitudinal structure, where the panel follows a classic not rotated longitudinal design, the panel component across all the waves taken

[13]These human capital investment consist of: seminars, conferences, training courses or professional refresher courses.

[14]There are also some other interesting questions regarding the type of course chosen and the amount of hours spent per each activity. However, these questions are not included in all of the waves of the survey or, alternatively, these are asked in a format which is completely different from the same question asked in the two-years earlier survey. Furthermore, evidence suggests that it is more the incidence of a training spell than its duration that is relevant (Pischke, 2001).

into account is very short (about less than 3,000 individuals) forcing me to conduct the empirical analysis using repeated cross-sections.

The working sample is composed of individual level data concerning individuals aged between 40 and 64 years, with at least 10 and less than 40 years of paid contributions, eligible to retire neither before nor after the 2011 pension reform[15].

Identification of the shock. The reform generated different changes in years until retirement eligibility among otherwise similar older workers, given that small demographic differences led to large differences in retirement delays for individuals. The different mandated retirement age by gender, age, sector and, mostly, by previously accrued years of contribution implies that individuals have been differently affected by the reform in terms of how much the length of the residual working period before retirement did increase.

In order to estimate the increasing shift in the residual working life, I predict the minimum retirement dates under pre- and post-reform rules by drawing on information about individuals' gender, age, sector and years of contribution. I use as a starting point the contribution declared by the worker in each wave of the survey and I make two assumptions on their working histories: *i*) workers accrue full contributions (52 weeks per year) until retirement; *ii*) the predicted retirement date is the earliest date at which the worker can collect the first pension installment by claiming either an old-age or a seniority pension.

[15]For the sake of clarity, in each wave of survey I drop from the sample all those individuals that have eligibility criteria under the old-age pension scheme according to the pension rules in place in that year (I do not have to check for seniority requirements since I consider only individuals with less than 40 years of accrued contributions, but however I drop all of them that are eligible to retire under the "Quota" system up to 2011). Furthermore, I am able to drop from the sample all those individuals that after 2011 declare themselves as *esodato* (which is one of the question contained in the survey). An *esodato* is a worker who, when he comes close to retirement, has concluded an agreement with his company to leave his job in exchange for economic coverage until he actually reaches the pension. According to Istituto Nazionale di Previdenza Sociale, 2016, there have been 7 *salvaguardie* from 2011 (up to 2016) in order to ensure that these *esodati* would have been able to obtain pension installments even though they did not meet the *Fornero* eligibility rules. The total number of *esodati salvaguardati* amounts to about more than 101,837 individuals for a total cost, borne by taxpayers, of more than 9 billions of euro.

Assumption *i*) requires that individuals work year-long spells and full-time. Assumption *ii*) requires that most workers do not further delay retirement after becoming eligible for a public pension. While assumption *i*) may appear more problematic to believe and can imply and underestimation of the expected shock to the MRA[16], assumption *ii*) can be more easily checked by looking at the behaviour of individuals who retired in the past. In particular to show that indeed a significant share of individuals retire when they reach their minimum retirement age (MRA), I use the sample of individuals who declare themselves as retired in the PLUS data. By exploiting information on their effective retirement age (ERA), years of contribution and sector of employment for all individuals retired between 2005 and 2015, I compute the minimum retirement age for each individual retired in year t, with $t \in [2005, 2015]$, that I compare with their effective retirement age[17]. In this way, I define the distance to retirement, that is the difference between the MRA and ERA. If distance to retirement is zero, it means that indeed individuals retire when reaching their minimum eligibility requirement. In Figure 2.4 I plot the percentage of individuals retired, considering only the sample of pensioners, as function of distance to retirement. The figure clearly shows that when the distance equals zero, that is MRA equals ERA, more than one out of two individuals enter in retirement. If, instead, I take into account distance between -1 and +1, given that I am exploiting survey and not administrative data and there may be small errors in reporting ERA and years of paid contributions, this percentage increases up to 70 percent. Overall, it seems that assumption *ii*) provides sounded evidence in support for the identification of the shock.

Hence, to compute the individual level shock in the increase of the expected residual working life, that can also be interpreted as degree of exposure to the pension reform, I construct a time invariant measure of exposure to the shock, by taking the difference

[16] Bianchi et al., 2019 exploiting contribution histories from the Social Security Institute show that for several type of workers (in 2012) the median annual contribution is 52 weeks and the average is 45 weeks.

[17] I also take into account that the reform abolished the "waiting window", a rule whereby the first pension installment could be collected only 12 months after becoming eligible for either type of pension. However, I do not consider the sample of retired individuals in the 2017 wave given that for these individuals information on accrued years of contribution is not available.

between the expected MRA under the post-reform (at 2017) and under the pre-reform rules (at 2011), that is $shock_q = MRA_{2017} - MRA_{2011}$[18]. This measure of cross sectional variation in the exposure to the pension reform is based on the full interaction of all the characteristics necessary to determine the MRA, that is age, gender, years of contribution and sector of employment (whether it is private, public or if the individual is self-employed).

In Figure 2.1, I plot the percentage of individuals according to the values of the reform-induced $shock_q$, ranging between 2 and 7 years of expected increase in the residual working life (with an average value of 4 years and 7 months). According to the figure, individuals whose expected residual working life increased more than 3 years is about slightly less than 64% in the sample. Figure 2.2, instead, plots the reform-induced shock distribution in the length of the residual working life by gender. With regards men, about the 55 percent experience an increase in the residual working life greater than 3 years and this is coherent with the fact that Italian working men have more stable career trajectories and start working earlier than women. On the other hand, about the 75 percent of women in the sample experience increases in their expected residual working horizon greater than 3 years.

To better understand the source of cross-sectional variation in the exposure to the pension reform that I exploit in the empirical analysis, a simple example may be illustrative in explaining the shock. Table 2.3 considers six different individuals: 3 women (the first panel of Table 2.3) and 3 men (the second panel) all aged 59 years, however, with different years of paid contributions and sector of employment. Consider, for instance, Beatrice who is a private sector worker with 35 years of paid contributions. According to the pre-reform rules, she would have met eligibility criteria in accessing

[18]There are other papers that study the effects of the *Fornero* reform using as identification of the policy induced shock similar versions to that one I am exploiting in this paper. Bovini and Paradisi, 2019 examines how firms adjusted their hiring and firing decisions in response to the reform, Bianchi et al., 2019 the effects on internal labour markets. Carta and De Phillipis, 2019 the effect of the pension reform on the labour force participation of middle-aged individuals and their partners. Carta, D'Amuri, and Wachter, 2019 study the increase in retirement ages, due to the *Fornero* reform, on firms' economic outcomes. Boeri, Garibaldi, and Moen, 2017 studies how the reform affected youth unemployment. This paper contributes to their findings by using the *Fornero* reform as a tool to study human capital investment of middle-aged individuals.

to the public pension at 64 years if she had chosen to retire under the seniority scheme, or 60 years under the old-age or quota system. Hence, her minimum retirement age was 60 years. Under the post-reform rules, she can only choose to retire under the seniority or old-age regime. In both cases, her retirement age will be 66. Because of the *Fornero* reform her MRA increased, and the size of shock amounts to 6 years, that is the increase in the residual working life. Paola, instead, is a public sector worker with 26 years of paid contributions. Supposing she could have retired under the pre-*Fornero* rules, she would have retired at 61 years under the old-age requirements, which corresponds to her MRA. Following the rules in 2017, instead, now she would retire at 67 years, six years later than expected. Hence, women experienced the greatest and least heterogeneous increase in the residual working life.

Men, conversely, have been affected differently from the 2011 pension reform. Alessandro is a private sector employee with 35 years of contributions. If he could have retired according to the 2011 rules his MRA was 60 years, but because of the *Fornero* reform his MRA, according to the rules in place in 2017, is 67. That is a 7 years shock. Alternatively, Leornardo, a public sector worker, has 26 years of paid contributions. In 2011, his MRA was 65 years. Because of the reform, in 2017 his MRA equals 67 years, that is a two years shock. In this case, the source of variation in the shock for men is larger for men who would have retired under the quota system before the reform.

Empirical strategy. The *Fornero* Reform had at least two characteristics that are important for the empirical analysis. First, many workers experienced a substantial increase in their retirement-eligibility age, meaning that the reform represents an unexpected and substantial shock to the minimum requirements for pension eligibility. Second, as highlighted in Section 2.2, the decision and implementation lags of the reform were both very short, implying that anticipatory effects were likely negligible. Hence, the changes introduced by the reform provide a clean empirical setting to study how changes in the expected residual working life would affect workers' human capital investment.

The identification of the shock, described above, aims at evaluating the magnitude of the perspective effect (or the *forward looking effect*), it therefore studies the human capital investment of individuals who would not have been eligible to retire even under the pre-reform rules but whose MRA increased, due to the 2011 pension reform. Hence, using the variation in distance to retirement exclusively induced by the pension reform given by the cross-sectional time invariant measures of exposure to the policy, I estimate the following empirical model:

$$y_{iqt} = \beta shock_q \times post2011 + \delta_q + \alpha_t + \vartheta_i + \eta_{iqt} \tag{2.1}$$

where: y_{iqt} is an outcome of interest at the individual level i in year t at the shock level q. My main outcome of interest is a dummy variable that indicates whether individual i has participated to any activity involving human capital accumulation in the last 12 months in year t at the shock level q, then I also look for the propensity of individual i in paying for additional human capital investment and whether firms suggest employees to improve their knowledge; $shock_q$ is the change in the residual working life induced by the reform (as described above), that is a time invariant measure of exposure to the policy; $post2011$ is a dummy that indicates the post-reform period, that is years 2013, 2015 and 2017; α_t are year fixed effects, absorbing long term or cyclical developments that affect all individuals in the same way; δ_q are fixed effects at the shock level absorbing all pre-reform permanent differences in distance to MRA; ϑ_i is a vector of fixed effects at the individual level (marital status, region of residence, sector of employment, gender, age, years of contribution) absorbing cross sectional time-invariant heterogeneity among individuals. Finally, η_{iqt} is the error term. Standard errors are clustered at the age-sector of employment-gender-years of contribution level.

As usual in any Difference-in-Differences model, the coefficient of interest is β, that is the interaction between the treatment variable and the post-reform variable, which estimates the average human capital investment effect among individuals that experienced a larger or a smaller increase in MRA, exclusively depending on their degree of exposure to the policy, around its implementation.

Descriptive statistics. Before turning to the discussion of the Difference-in-Differences estimates, I briefly provide some descriptive statistics by starting with some graphical evidence, where I arranged individuals in two groups only for graphical and descriptive evidence purposes. In Figure 2.5, Panel 2.5a shows that the declared expected retirement age increases more around the reform (that is from 2011) and individuals more exposed to the change in the minimum retirement age (most treated; *i.e.* $shock_q > 3$) expects to stay active in the labour market two more years with respect to the least affected group. Panel 2.5b, instead, shows that individuals more exposed to shock expect a lower of pension income relative to job earnings, given that for these individuals the pension benefits share computed according to the NDC method is higher. Overall, trends for both groups followed more or less the same patterns.

With regards the main outcome variable of interest, that is the participation in human capital activities, Figure 2.6 shows the age profiles of the average participation (Panel 2.6a) and by three different age classes (40-47, 48-56, 56-64; Panel 2.6b; that I also exploit in the empirical analysis) by the degree of exposure to the increase in the residual working life. Panel 2.6a shows that for individuals who experienced an increase in the MRA greater than 3 years, average participation in human capital investment is higher, mostly, along all the age profiles of the individuals included in the sample. This finding is also confirmed by looking at Panel 2.6b. Indeed, individuals whose shock is higher than 3 years have an higher participation relative to the least shocked ones: a difference of about 6 p.p. between the first age class. Concerning the middle and oldest age class, this difference reduces in size even though most shocked ones still display a higher participation. Finally, Figures 2.7 and 2.8 shows the average trends in human capital investment according to exposure to the shock and also by gender. Figure 2.7 shows that individuals most shocked by the change in the minimum retirement age (shock greater than 3 years) display, on average, higher participation rate in activities involving human capital accumulation (seminars, conferences, training courses or professional refresher courses) in the aftermath of the *Fornero* pension reform, whereas in previous years their average participation was essentially the same as those least treated by the reform-induced shock. Looking, instead, at differences in gender (Figure 2.8) most treated men after the late-2011 pension reform remarkably increased their human capital accumulation relative to the least treated group, especially in 2013 and 2015. On the other hand, women independently of the size of the reform-induce shock display, more or less, the same average participation rate.

Concerning the other two outcomes I consider in the empirical analysis, Figure 2.9a plots the probability of individual i, who has attended some kind of human capital accumulation activity, in paying for it. Figure 2.9b, instead, shows the probability that human capital activities are directly sponsored (but not necessarily paid) by the firm[19]. Most affected individuals pay more often for taking part in training activities, even though I am not able to detect divergent patterns after the pension reform. Instead, for what concern human capital activities sponsored by firms, most shocked individuals, in the aftermath of the reform, appear less likely to be involved in training being suggested by their firm, as if firms encouraged least affected individuals, that absent the reform would have retired, to invest in additional activities.

Finally, in Table 2.4 I present some descriptive statistics of the working sample. The first 3 columns regards all the waves of the survey taken into consideration, whereas, the last 3 refer to the pre-reform waves. Furthermore, I differentiate each period by considering all the individuals contained in the sample and by distinguishing between those most treated (*i.e.* shock grater than 3 years) and least shocked. Overall, no remarkably differences there exist between least and most treated groups, either in the full sample or in the pre-reform waves, with the only exceptions regarding gender composition of the groups (men are over-represented in the least treated group) and the shares of private sector employee (considerably higher for least treated individuals)

[19]The sample, apart from being composed of individuals who attended some training activities, includes individuals who work or has worked for a firm.

and self-employed individuals (greater for most exposed to changes in MRA).

2.4 Results

Does an increase in the residual working life induce additional human capital investment?[20] As explained in Section 2.1, human capital theory predictions state that the value of human capital investment increases with the payout period of the investment, and the reform here studied indeed represents an unanticipated and exogenous shock that induces a sizable increase in the working life (*i.e.* an increase of the payout period of the human capital investment) affecting a large share of the middle-aged working population. Table 2.5 reports the results obtained from estimating equation (2.1) on the main outcome of interest outlined in Section 2.3, that is the probability that individual i has participated to any activity involving human capital accumulation in the last 12 months in year t at the shock level q.

In addition to baseline results involving all the individuals included in my sample (column (1) of Table 2.5), I also conduct a sample-split analysis by gender (columns (2)-(3) of Table 2.5), both because men and women have different MRA shocks and because they tend to have heterogeneous labour market performances.

I find that the causal effect of an increase in the length of the residual working life, that is an increase in the minimum retirement age, has a positive effect on human capital investment. Concerning all the individuals included in the sample without distinguishing by gender (see column (1) of Table 2.5), I find that if the length of the working life increases by one year, the probability of participating in activities aimed at improving human capital increases by 0.7 percentage points (statistically significant at the 1 percentage level). When evaluated at the sample mean of the dependent variable, the previous estimate translates in an average training participation of about

[20] In Appendix A, I present additional results, not discussed in this Section, based on an alternative definition of the treatment variable. Despite the coefficients measuring the causal effect of interest change their interpretation, these additional results are in line with the evidence presented here. However, the overall effects, that is the coefficients re-scaled in terms of sample averages, are 3-4 times larger than those obtained using the variation in the MRA as treatment variable.

1.7 percentage points. Instead, the gender-split analysis reveals that the effect is driven only by the response of men. For this group, an increase of 1 year in their residual working life implies a 0.9 p.p., or 2.5 percent in terms of sample mean, increase in human capital activities participation. For what concerns women, despite a positive coefficient, it is not statistically different from zero. These results are broadly in line with Montizaan, Corvers, and De Grip, 2010, who find that public sector workers affected by a pension reform, lowering the pension rights, implied an increase in training participation of about 2.7-3.2 percentage points.

As a first heterogeneity exercise I consider different age classes by looking at the response of human capital investment of individuals that more or less find themselves in the later part of their working life. In Table 2.6 I report the results for this exercise where columns (1), (2) and (3) report results for individuals aged 40-47, 48-56 and 57-64, respectively. Furthermore, the upper panel of the Table refers to all the individuals, whereas, the last two to men and women, respectively. The first striking result is that, independently of the gender, oldest individuals, that is those included in the age class 57-64, do not display any evidence of increase in human capital investment due to the reform. Secondly, again, by the same token women of all age class do not attend further activities connected with human capital investment. Looking at the first panel of Table 2.6, there is a positive and statistically significant effect for age classes 40-47 and 48-56. For the former class an increase of 1 years in the residual working life increases the probability of additional human capital investment of about 1.3 p.p.; instead, the latter class an increase of about 0.7 percentage points. In terms of sample mean, the previous estimates correspond to an average increase for each additional year of 3.6 and 1.9 p.p., respectively. Again, the gender-split exercise reveals that the whole variation is driven by men belonging to the 40-47 and 48-56 age classes. Youngest men, expecting at least one year increase in their working life, increase their participation in human capital activities of about less than 1.5 p.p. (3.9 percent in terms of the sample average for men), whereas those included in the age class 48-56 of about 1.1 percentage points, that corresponds at an average increase of 3%.

Furthermore, in Table 2.7 I look for the causal effect of an increase in the residual working life on human capital investment by splitting the sample according to the sectors, that is public, private or whether the individual is self-employed, in which the individual works. This splitting is motivated by the fact the these 3 different broad sectors of employment may require their workers to update their knowledge and competencies with a different degree and extent. Usually investment in additional human capital may be lower in the public sectors given that the procedures that public employees accomplish are very often standardized and may change very little over time. On the other hand, private sectors workers and also self-employed tend to be exposed to working environments that are more constantly and rapidly changing. Columns (1), (2) and (3) of Table 2.7 refer to public workers, private sector employees and self-employed individuals, respectively. The only statistically significant effect comes from individuals working as self-employed for whom an increase of 1 year in their residual working life implies an increase in the probability of human capital investment of about 1.5 percentage points (statistically significant at the 5 percent level), or in other words to about a 4 percent increase when compared to the sample mean. For public sector and private sector employees the coefficients of interest are positive but not statistically at the conventional confidence level.

Finally, I perform another heterogeneity split-sample analysis by considering only private sector and self-employed workers and distinguishing them according to NACE code of the firms where they work. Specifically, I define two broad firm-sectors, based on the statistical code of the economic sectors, that is the manufacturing and service sectors. The results, available in Table 2.8, show that, despite a positive coefficient for both group of workers, only workers whose firms belong to service sector increased (see column (2) of Table 2.8), at the conventional statistical level, their probability of training. In particular, for each additional 1 year increase in residual working life service sector employees increase their probability of participating in human capital activities by about 0.8 percentage points (2.1 p.p. in terms of the sample subgroup mean).

Does human capital investment correlate with initial level of education? Human capital theory suggests that, apart from age, formal individual human capital, that is the education level, very likely is able to affect the worker probability of training (Griliches, 1997). Theory argues that workers with higher human capital levels tend to accumulate more skills and knowledge with respect to individuals with lower education endowment, advocating that formal education and human capital investments are complimentary. Henceforth, theory suggest a positive correlation between education and training participation. To check whether this theoretical prediction is supported by data, I re-estimate equation (2.1) separately for three different education level groups, that is low (middle schools or lower), medium (high school) and high (bachelor or higher). Table 2.9 reports the results for this heterogeneity check. Column (1), (2) and (3) refers to low, medium and high education, respectively, whereas the first panel to the whole sample and the last two panels to men and women separately, respectively. Overall, I find that individuals with higher education have a higher probability of investing in human capital (see the first panel, column (3)). For them a 1 year increase in the residual working life due to the pension reform implies an increase in the probability of human capital accumulation of 1.4 p.p. or to a 2.3 p.p. average sample increase, suggesting that the higher the education level, the higher the propensity of training activities as predicted by theory. However, the complementarity between education level and human capital emerges strongly when looking at the sample of men. Indeed, for this group the coefficient measuring the causal effect of interest is positive for all the education levels considered, and it is also increasing in magnitude the higher the education endowment of the individuals, although with different statistical significance. While for low-educated affected men the coefficient of interest is positive (about 0.6 percentage points) but not statistically significant at the conventional significance level, statistical significance, instead, is found for medium-educated (0.7 p.p. for each additional year at the 10 percent level) and high-educated (1.7 p.p. for each additional year at the 1 percent level) individuals. In terms of the sub-sample means, these estimates imply an average increase in the training participation rate of 2 and 2.3 percentage points for medium and high educated individuals, respectively. On the other hand, the positive correlation predicted by theory seems less clear-cut and supported by data for the sample of women, even tough those with higher education have for each additional year increase in their residual life an increase in the probability of attending human capital activities by about 1.5 percentage points (2.5 p.p. increase in terms of the sample mean).

Further women heterogeneity? As discussed so far, the causal effect estimates relative to the increase of the residual working life, implied by the 2011 pension reform, on women human capital investment narrate a picture of the story where women did not modify their probabilities in attending training activities, differently from men, despite all of them expect to stay longer in the labour market, given that them are those who were hit the most from the *Fornero* reform. In this short section, I focus on a factor that may influence women decision in investing in human capital activities. To carry out this further heterogeneity exercise I split the sample of women into married and not married women, that is by distinguishing between female individuals that, in principle and according to solid empirical evidence, may be defined as more "family focused" (those who are married) and as more "career oriented" (those, instead, who are not married)[21]. Indeed according to the strands of the literature about gender economics and family economics (see, among many others, Goodpaster, 2010; Leigh, 2010; and Munasinghe, Reif, and Henriques, 2008), married women experience a higher opportunity costs in terms of work and investments due to the household chores burden they are subject to, and hence they may be less willing to invest or time-constrained in investing in additional human capital. However, an extension of the period they have to stay active in the labour market may provide married women higher incentives to invest in human capital as opposed to more "career focused" women.

To check this issue, I re-estimate the previous heterogeneity sample-spit exercises as well as the baseline specification (that in Table 2.5, column (3)) taking into account

[21] I consider as not married women those who declare themselves as: single, divorced or widows.

that the response of married women may be different from that of women that can be defined as more "career focused". Table 2.10 reports the results of re-estimating column (3) of Table 2.5 by distinguishing between married (columns (1), (2)) and not married women (columns (3), (4)). According to these estimates there exists a different response to the pension reform in relation to the martial status of the women. As reported by column (1) of Table 2.10, for 1 year increase in the residual working life married women increase their human capital investment probability of about 1.3 percentage points (the magnitude of the effect is the same when I control also for the number of kids and household size to take into account for family chores) translating into an average increase of about 3.6 p.p. if compared to the sample mean, whereas for those women whose martial status is different from being married the causal effect is negative, very close to 0 and not statistically significant.

Then, I re-estimated the results of Table 2.6 following the same reasoning of above. For what concerns women, Table 2.6 shows that independently of the age class taken into account the estimated causal effects were not statistically different from 0. In Table 2.11 I show that indeed, again, married women in their 40s (up to 47 years, those that in the labour economics literature are known as prime-aged individuals) increased their probability in investing in human capital. For each additional year increase the probability goes up of about 2.5 p.p. (that is a 6.8 percent increase with respect the sample average for this sub-sample of women); for the 48-56 and 57-64 age classes the coefficients of interest are positive, decreasing in magnitude, but not statistically significant at the conventional confidence levels. For what concerns not married women, all the estimated coefficients are not statistically different from zero and show a magnitude that decreases as age increases.

The last heterogeneity exercises involving women and their martial status concerns the relationship between education endowment and investment in human capital. In Table 2.9 it is shown that the positive correlation relationship between education, gender and investment in human capital was less clear-cut and supported by data for women rather than for men. This finding is again confirmed by looking at not married women (the second panel) in Table 2.12. Regarding married women (the first panel), despite none of the estimated coefficients are statistically significant, the positive relationship emerges: that is the higher the education level the higher the probability of attending in training activities.

Propensity to spend in additional human capital investment and the role of firms. Firms, usually, invest in the human capital of their workers in order to enhance employees productivity and their growth prospects. However, they choose to provide training, after a careful cost-benefit analysis, only if productivity improvements outweigh the costs. Furthermore, provided that productivity returns from training are increasing in training more rapidly than wage returns (as usually happens in imperfect labour markets), then firms will be willing to sustain the costs. Despite I do not observe whether training is financed and provided directly by firms, I can gauge some evidence by looking at indirect proxies for firms involvement in middle-age workers training participation. I start investigating the role of firms in inducing their workers in investing in human capital by looking at the probability that individual i, affected by the 2011 pension reform, invested in human capital activities by the size of firm at which she is employed. The results of this further heterogeneity check are available in Table 2.13, where columns (1)-(6) refer to firms whose size is 1-9 employees, 10-15, 16-25, 26-49, 50-249 and > 250 workers, respectively. According to these estimates, only employees working in very small-sized firms, that is those with at least 1 and maximum 9 employees, increased their probability of training. Indeed, for each additional year of residual working life this probability increases of about 1.8 percentage points, statistically significant at 1 percent level, translating into an average response, in mean terms, of about 7 percentage points. As a further check, I also distinguish individuals not only by the size of the firm where they work but also for two broad firm economic sectors, that is the manufacturing and service sectors. The results are available in Table 2.14 where the first panel is devoted to the manufacturing sector and the second one to firms operating in the service sector. For what concerns the manufacturing sector, individuals working in medium-sized firms (26-49 employees) saw a sizable increase in the probability of attending training activities, about to 4.8 p.p. for each additional year of delay in pension eligibility in the aftermath of the reform. With regards the service sector, individuals working in small-sized firms increased their probability of investing in human capital for 1 years increase in the residual working life of about 2 percentage points, or about 8.7 p.p. when evaluated at the sample average (as found by Berton, Guarascio, and Ricci, 2017 that, instead, use firm-level data).

Finally, I conclude the empirical analysis by looking at the other two outcomes I outlined in Section 2.3, that is the probability the firm sponsored the human capital activity and whether the individual directly financed her training. The results of this last investigation are in Table 2.15, where the first 3 columns are devoted to the willingness of the affected individual in paying for his human capital investment, whereas, the last column to the firm-sponsorship of the activity. In reference to the willingness to pay, I am not able to find a statistically significant effect, even if I distinguish individuals according to the median yearly-earnings of the sample, as proxy for individual budget constraint. For this outcome, the estimated coefficients are positive but not statistically significant at the conventional levels. On the other hand, for what concerns the probability that the employer sponsor the worker the training activity, I find that for one year increase in the residual life this probability goes down of about 0.8 percentage points (-1.6 p.p. when evaluated at the sample average).

Parallel trend assumption. As standard for the estimation of Difference-in-Difference models, I need to show that the trends in human capital investment participation would have been parallel for individuals with different exposure to the shock, absent the change in the pension rules. In order to test this assumption, I show that the difference in the participation in human capital activities of individuals more or less exposed to the shock was constant before 2011 and started changing exactly after the introduction of the new pension rules, from 2012 onward. Specifically, I estimate Eq. (2.1) by interacting the coefficient of the reform-induced shock with year-dummies (from 2007 to 2017) while omitting the year 2011 as reference category. That is, I estimate the following equation which consists in an event-study that estimates the baseline regression with different treatment years:

$$y_{iqt} = \sum_{\tau=2007}^{2015} \varphi_\tau shock_q \times \mathbf{1}(t=\tau) + \delta_q + \alpha_t + \vartheta_i + \eta_{iqt} \tag{2.2}$$

Equation (2.2) includes interactions between the shock variable and year dummies for every year excluded 2011. Under the assumption of parallel trends $\varphi_\tau \approx 0$ for τ<2011 (or at least not statistically significant at the conventional level of confidence). Figure 2.10 reports the point estimates for φ_τ in equation (2.2) and 95% confidence intervals regarding the main outcome of interest referred to all the individuals included in the sample (that is this is the dynamic version of the estimate reported in column (1) of Table 2.5). As showed by the Figure, the coefficients relative to the pre-reform period are all close to 0 and not statistically significant suggesting that individuals were on a parallel trend, whereas those relative from the post-reform period are positive and turn out to be statistically different from 0 from 2015 onward. Figure 2.11, instead, replicates Figure 2.10 splitting the sample according to gender. In this case, while for men the event-study confirms the common trend assumption during the pre-reform years and a strong and significant effect on the probability of human capital investment in the aftermath of the reform, women seem not to be perfectly on parallel trends before the *Fornero* reform. Figure 2.12 shows the event-study regarding one of the first heterogeneity exercise I carried out. Specifically, it reports the estimates relative to the probability of investing in human capital activities according to age classes (panel a), sector of employment (panel b), education (panel c) and economic sector of the firm where the individual is employed (panel d). The visual inspection of coefficients $\{\gamma_\tau\}_{2007}^{2011}$ for each sub-sample show that were substantially on parallel trend, excepts one relative to individuals employed in firms operating in the manufacturing sector. In the post-reform period, essentially the dynamic estimates goes in favour of the coefficients obtained by estimating its compact version counterpart, that is equation (2.1).

In Figure 2.13 are plotted the coefficient relative to the test of the parallel trend assumption considering only the sample of women and distinguishing them according to their martial status (married or not married, panel a) and by age classes (panel b, c) and education (panels d, e). 3 out of 5 figures clearly show that the considered sub-sample of women were on a parallel trend before the implementation of the reform, where in panels b and d some of the estimates coefficients were statistically significant (at the 10%) suggesting that the parallel trend assumption holds weaker than the previous cases. Furthermore, the majority of the post-reform coefficients shows a very flat dynamics over time apart from the fact that they are never statistically significant at the conventional level.

Figure 2.14 shows the event-study estimates of the last heterogeneity exercise regarding firm size (panel a) and the economic sector, that is the manufacturing (panel b) and service sector (panel c). The visual inspection of the coefficients, despite some of them above 0, do not evidence statistical significance in the majority of the cases in the pre-reform years. In the aftermath of the reform, clearly emerges the statistical significance of the coefficients associated with firms whose size is between 1-9 employees (panel 2.14a) and operating in the service sector (panel 2.14c).

Finally, Figures 2.15 and 2.16 graph the estimates relative to equation (2.2) and using as outcomes the probability in paying for human capital activities and the probability that the firm, where the worker is employed, sponsor the training activity, respectively. With regards to the pre-reform years, in both cases, there is evidence of parallel trend given that the estimated coefficients, despite being different from zero, are never statistically significant. Concerning Figure 2.15, in the aftermath of the pension reform, the probability that the individual pays for human capital investment has been very close to zero up to 2015, and slightly increasing in 2017. However, the post-reform coefficients are never statistically significant at the conventional level of confidence. With regards Figure 2.16, instead, during the post-*Fornero* reform years there has a been a decrease in the probability that the employer sponsors the training activity to the middle-aged workers, even though statistically significant only in 2015.

2.5 Conclusions

In this paper, I provide causal evidence for the theory of human capital accumulation. The standard prediction from human capital theory is that older workers are less likely to be involved in training activities than younger colleagues, given that senior workers and their employers have only a limited amount of time to recoup the investment in skill before retirement occurs.

However, whether pension policies that exogenously change the working life horizon by increasing the payout period for the human capital investment can stimulate additional training activities is an open empirical question.

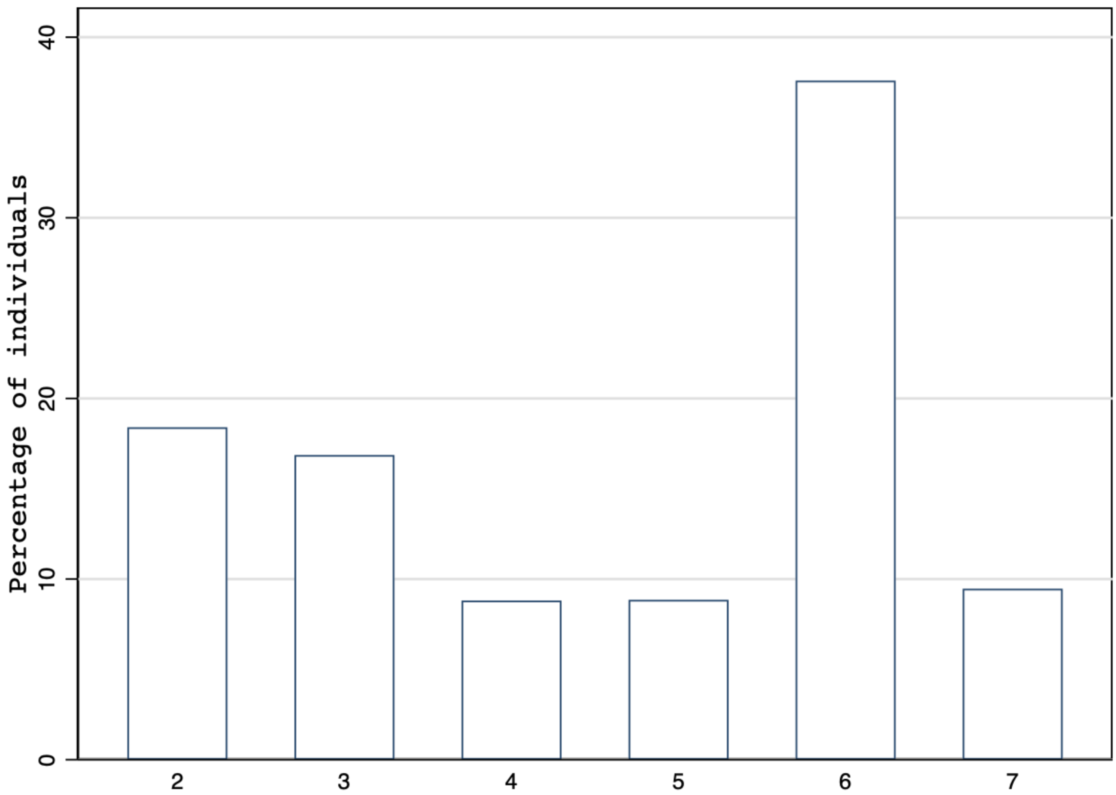
Specifically, I exploit a sizable pension reform, affecting all Italian workers from 2011, that abruptly increased minimum retirement age (MRA) requirements. The analysis is based on a sample of individuals aged between 40 and 64 years with at least 10 and less than 40 years of accrued years of contribution, eligible to retire neither before nor after the 2011 pension reform, and exploits a Difference-in-Differences approach where the treatment variable is given by a time-invariant measure of policy-induced shock, that is the variation in *pre* and *post* MRA, at the individual level, that mirrors the lengthen of the employees' residual working life.

According to my estimates, I find that the causal effect of an increase in the length of the residual working life, due to the *Fornero* pension reform, has a positive effect on human capital investment. For each additional year increase in MRA, the probability that an individual invest in human capital goes up of about 0.7 percentage points. However, the response to the reform was very heterogeneous and mainly driven by men and married women. Furthermore looking at the age profile of individuals, I find that increases in human capital investment occur only for those workers known as prime-aged (both men and women, that is individuals aged 40 to 47) and middle-aged (only men, those aged between 48 and 56). In terms of sector of employment and firms' economic sector of activity, I find that the positive effect on human capital investment comes from self-employed individuals and from those who are employed in (small-sized) firms operating in the service sector. Furthermore, my estimates provide evidence in support of the hypothesis of complementarity between education attainment and investment in human capital, given that individuals with higher education have a higher probability of investing in human capital. Finally, my estimates suggest to rule out that the positive variations in human capital investment, in the aftermath of the reform, were directly sponsored by employers.

This evidence, apart from being a novel test of human capital theory, may enrich the policy debates about pension policies, that usually do not consider human capital dynamics. My results suggest that policies aimed at increasing MRAs, mainly due to public finance motives, may have positive unintended consequences that may pay off also in terms of higher training, possibly because they may have contributed to extend relatively short working horizons and to increase the perceived benefits from additional training.

Figures and Tables

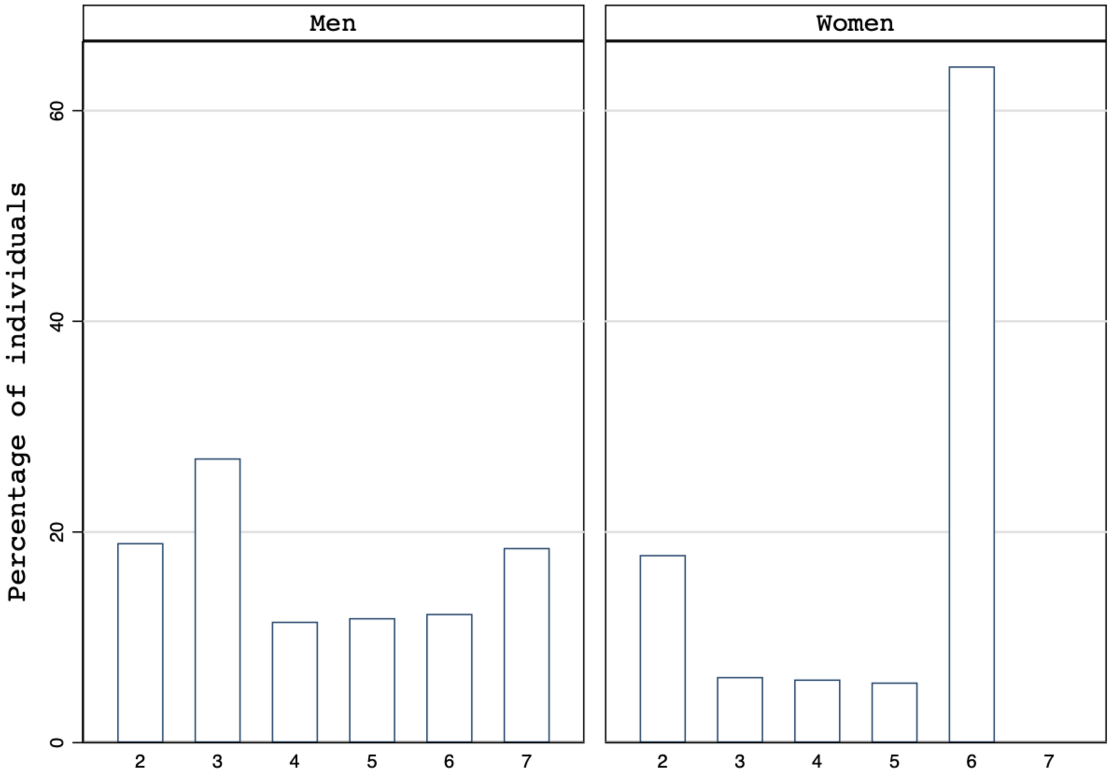
FIGURE 2.1: Shock distribution in the length of the "residual" working horizon (variation in pension rules between 2017 and 2011)



Source: PLUS (INAPP) 2007-2017.

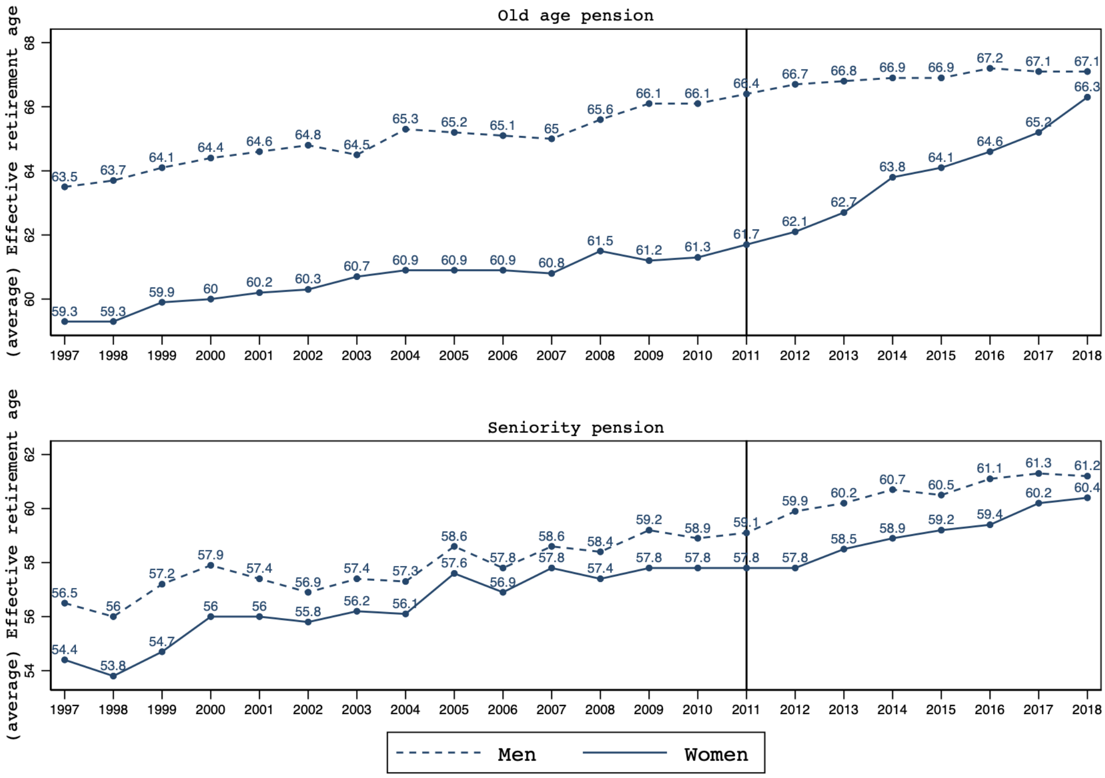
Notes: The figure displays the distribution of the reform-induced shock to the "residual" working horizon. It shows the distribution of the difference between the minimum retirement age (MRA, the age at which individuals can claim their first pension benefit, either old age or seniority) under the post reform pension rules (2015) and the MRA under the pre-reform rules (2011). The sample is composed of individuals aged between 40 and 64 years, with at least 10 and less than 40 accrued years of contribution, eligible to retire neither before nor after the reform. Data are at the individual level, the y-axis reports the percentage of individuals for any given value of shock.

FIGURE 2.2: Shock distribution in the length of the "residual" working horizon by gender (variation in pension rules between 2017 and 2011)



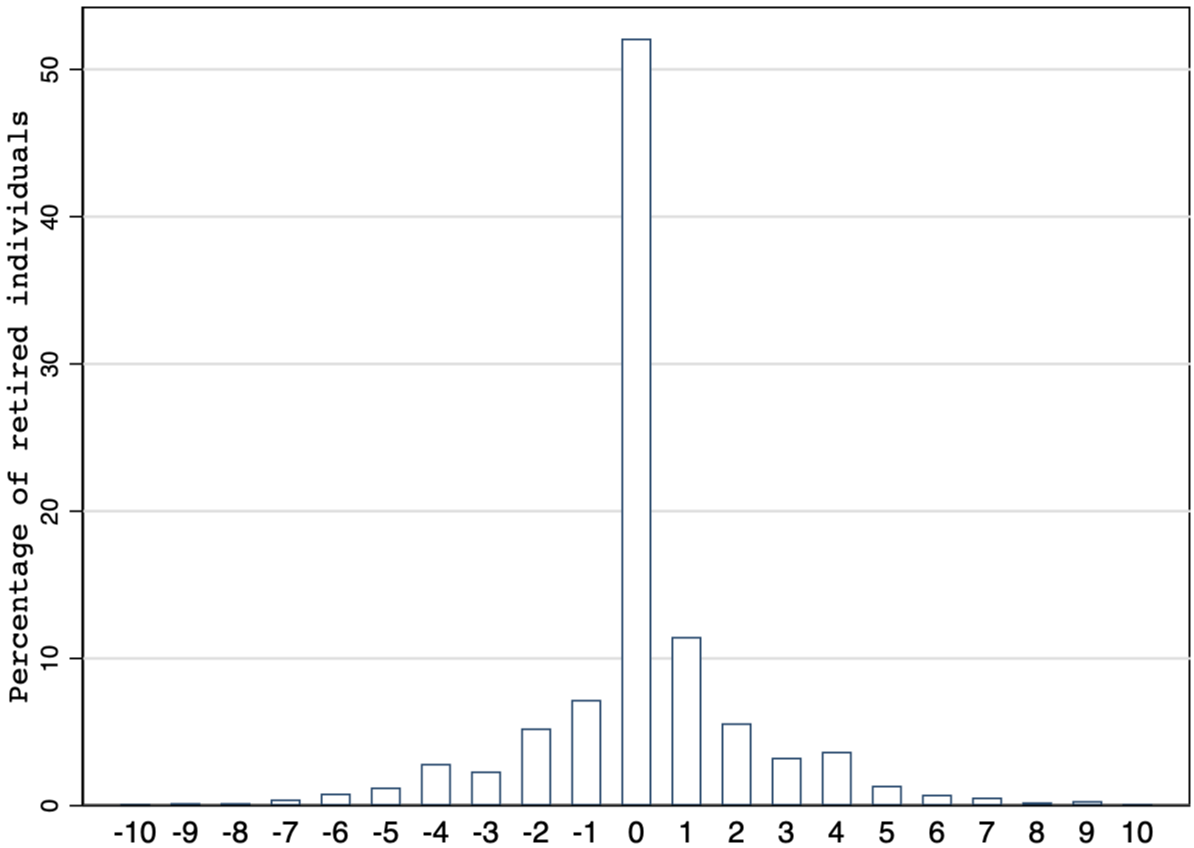
Source: PLUS (INAPP) 2007-2017.
Notes: The figure displays the distribution of the reform-induced shock to the "residual" working horizon, as in Figure 2.1, distinguishing by gender.

Figure 2.3: Effective (average) retirement age by gender and pension regime



Source: Fondazione Itinerari Previdenziali, 2020 (based on social security records).

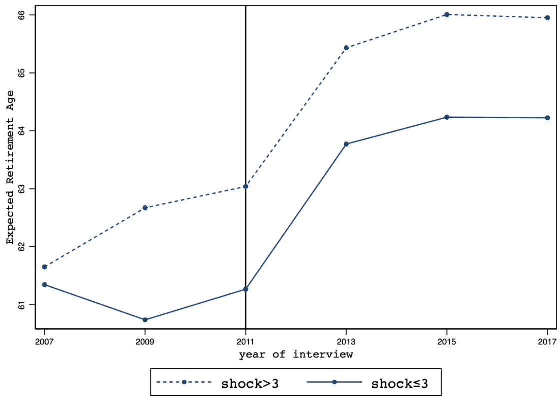
Figure 2.4: Percentage of individuals retired as function of distance to retirement (MRA_q - Retirement age)



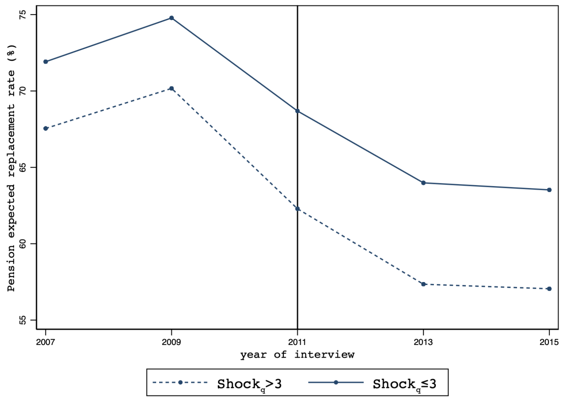
Source: Plus (INAPP) 2005-2015.

Notes: The figure plots the percentage of individuals who declare themselves as retired as a function of the distance to the minimum retirement age (MRA, the age at which individuals can claim their first pension benefit, either old age or seniority according to their gender and sector of employment). The sample of retired individuals is composed solely of those who entered in retirement between 2005 and 2015. Distance to MRA is the difference between the minimum retirement age according to the rules in place at the year of retirement and the individual's age at retirement. The Figure shows that individuals actually retire when they reach their MRA, *i.e.* when their distance to retirement approaches 0.

FIGURE 2.5: Declared expected retirement age and replacement pension income rate by exposure to the policy shock



(A) Expected retirement age

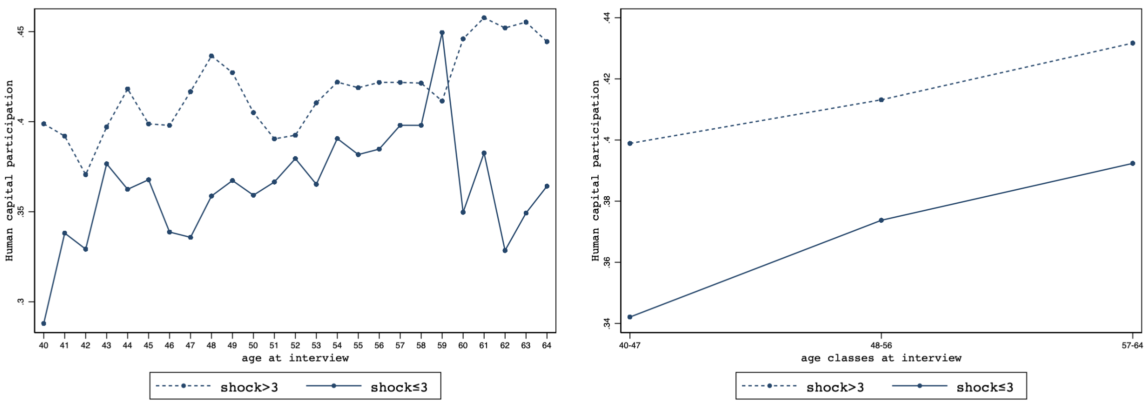


(B) Expected replacement rate

Source: PLUS (INAPP) 2007-2017.

Notes: Panel **(a)** shows that the declared expected retirement age increases more around after the reform (that is from 2011) for individuals more exposed to the change in the minimum retirement age (most treated; *i.e.* $Shock_q > 3$). Panel **(b)**, instead, shows that individuals more exposed to shock expect a lower of pension income relative to job earnings. The sample is composed of individuals aged between 40 and 64 years, with at least 10 and less than 40 accrued years of contribution, eligible to retire neither before nor after the reform. The question on expected retirement age and expected replacement rate (not available in the 2017 wave) are asked only to individuals who have been employed at least once during their life.

FIGURE 2.6: Age profiles of later life human capital accumulation by most and least treated

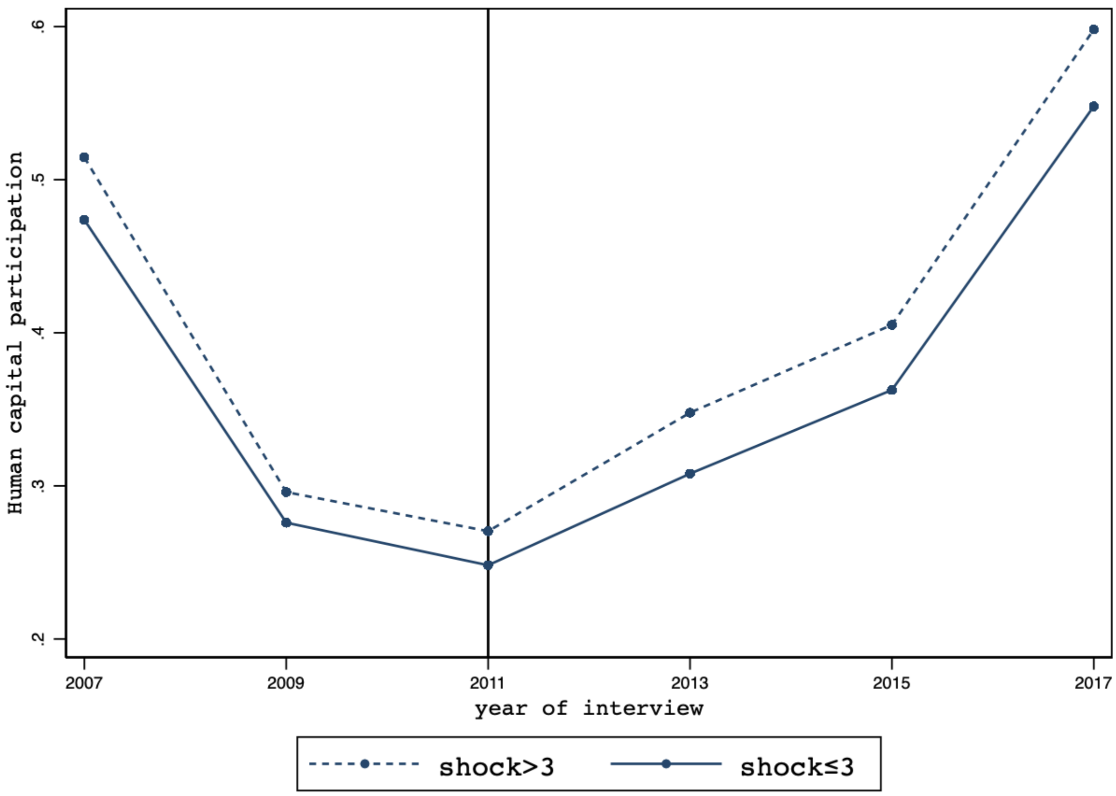


(A) Age profiles (B) Age classes

Source: PLUS (INAPP) 2007-2017.

Notes: The figures show human capital participation activity rate across ages (Panel **(a)**) included in the sample and across three age classes (Panel **(b)**). Individuals most shocked by the change in the minimum retirement age display, on average, higher participation rate in activities involving human capital accumulation, such as: seminars, conferences, training courses or professional refresher courses.

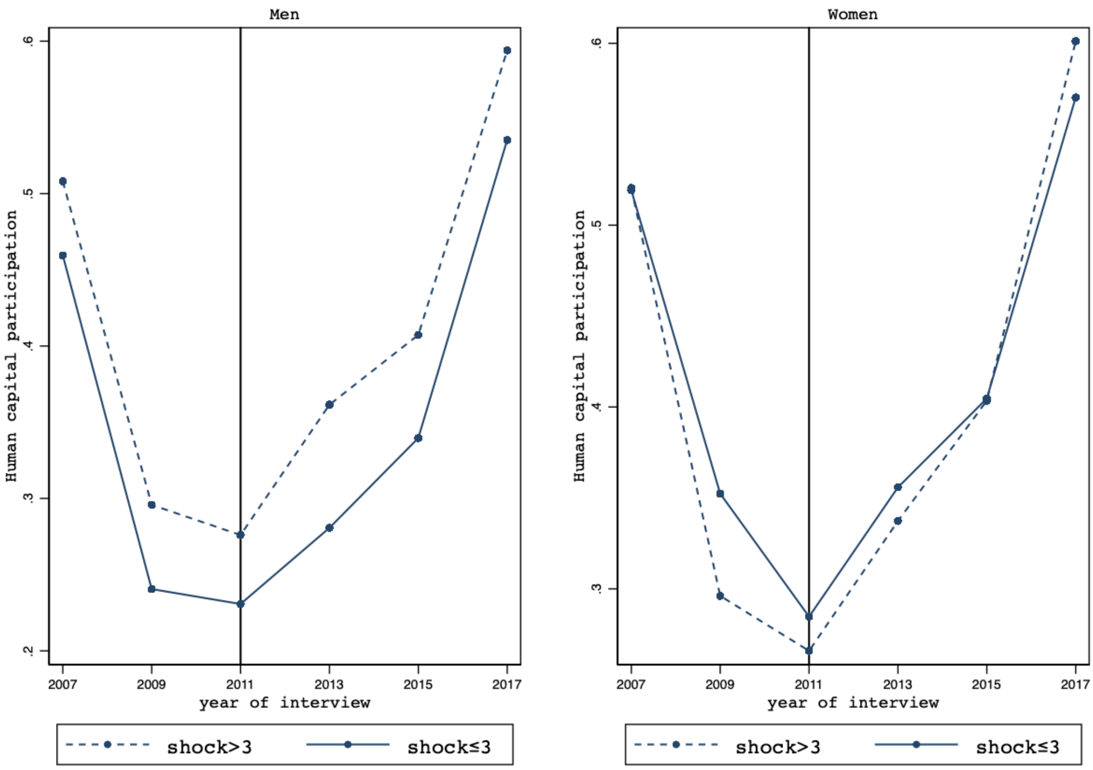
Figure 2.7: Human capital accumulation by most and least treated



Source: Plus (INAPP) 2007-2017.

Notes: The figure shows that individuals most shocked by the change in the minimum retirement age ($Shock_q > 3$) display, on average, higher participation rate in activities involving human capital accumulation (seminars, conferences, training courses or professional refresher courses) in the aftermath of the *Fornero* pension reform, whereas in previous years their average participation was essentially the same as those least treated by the reform-induced shock.

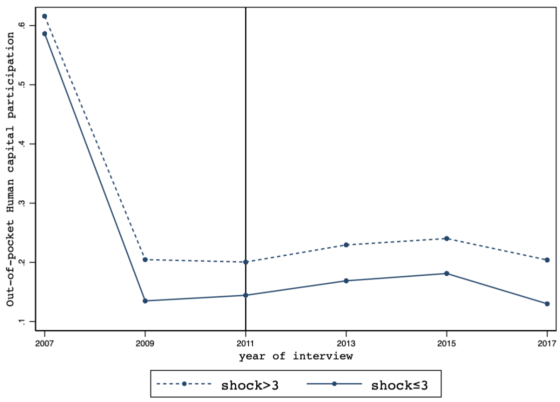
FIGURE 2.8: Human capital accumulation by gender and most and least treated



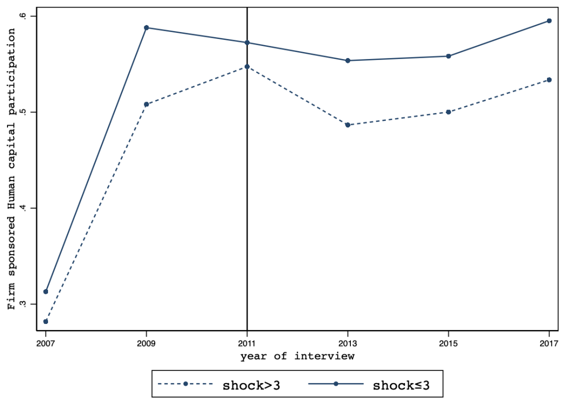
Source: PLUS (INAPP) 2007-2017.

Notes: The figure replicates Figure 2.7 distinguishing by gender. Women independently of the size of the reform-induce shock display, more or less, the same average participation rate. On the other hand, most treated men after the late-2011 pension reform remarkably increased their human capital accumulation relative to the least treated group.

Figure 2.9: Paid and firm-sponsored later life human capital accumulation by most and least treated



(a) Out-of-pocket HCA

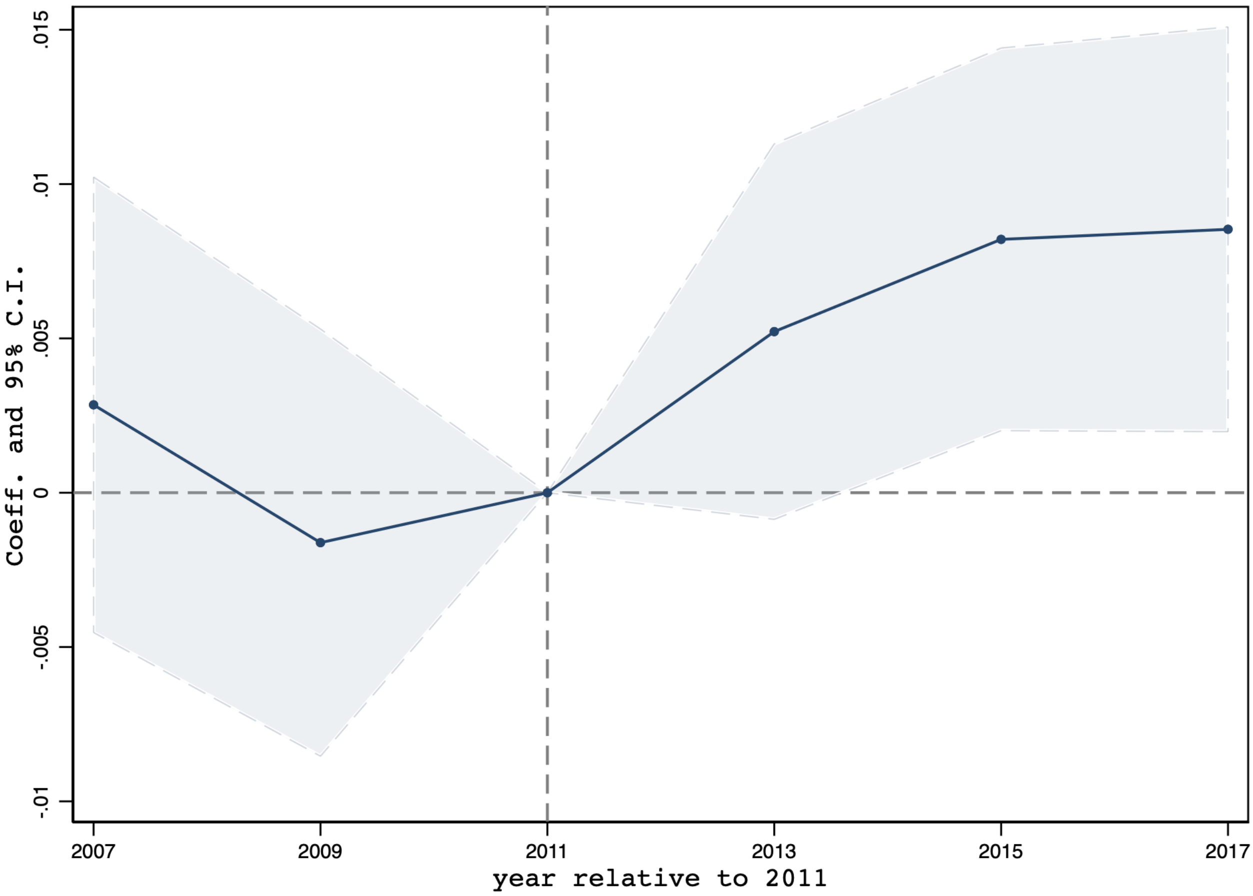


(b) Firm sponsored HCA

Source: Plus (INAPP) 2007-2017.

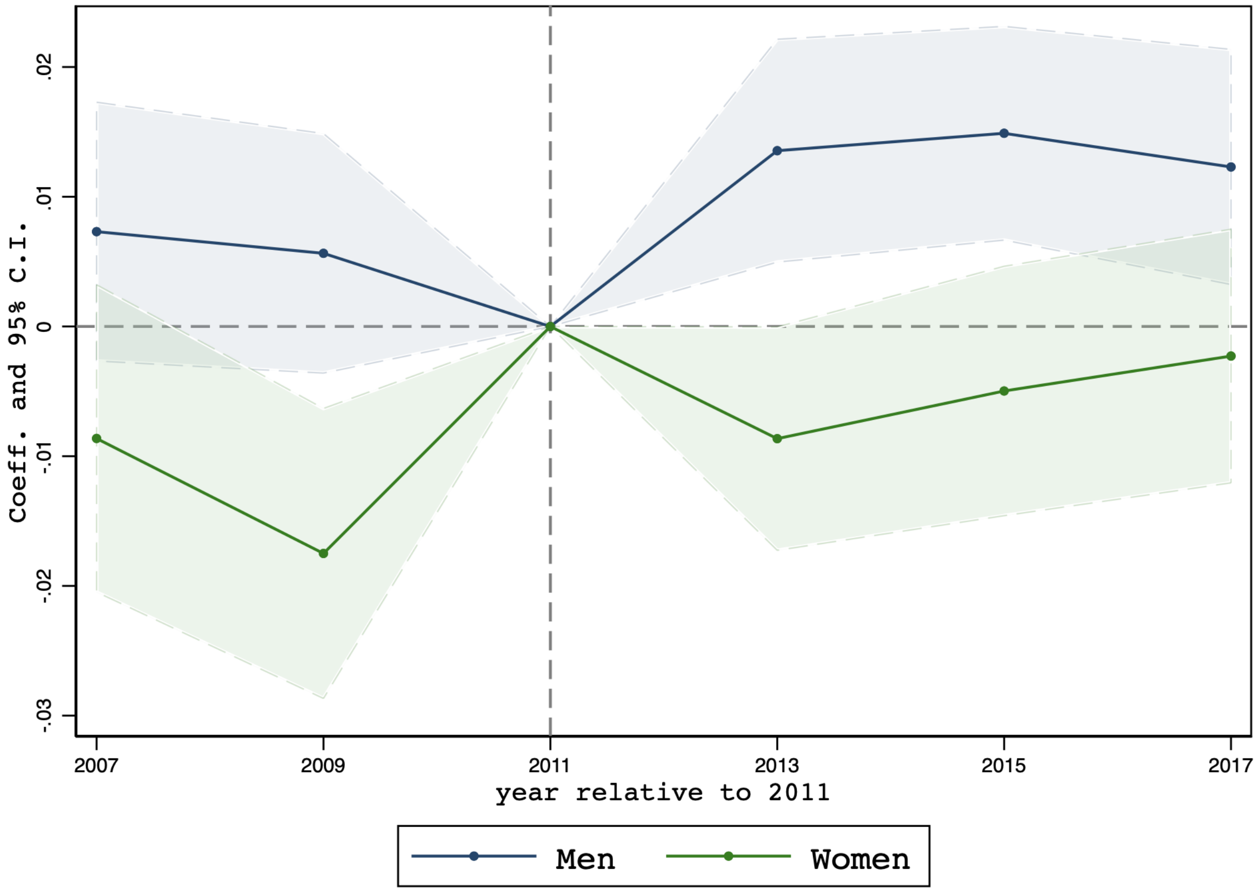
Notes: Panel **(a)** plot the probability of individual i, who has attended some kind of human capital accumulation activity, in paying for it. Panel **(b)**, instead, show the probability that human capital activities are directly sponsored (but not necessarily paid) by the firm; and the sample, apart from being composed of individuals who attended some training activities, includes individuals who work for a firm. Most affected individuals pay more often for taking part in training activities and are less sponsored by firms.

FIGURE 2.10: Event-study estimates



Source: PLUS (INAPP) 2007-2017.
Notes: Estimates based on equation (2.2). The dependent variable is a dummy variable that takes value of 1 if individual i has attended human capital activities in the last 12 months.

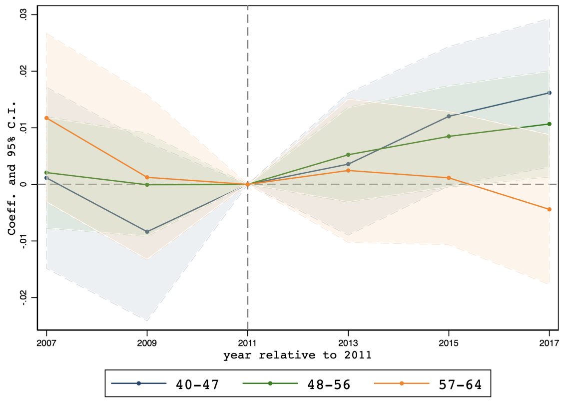
Figure 2.11: Event-study estimates by gender



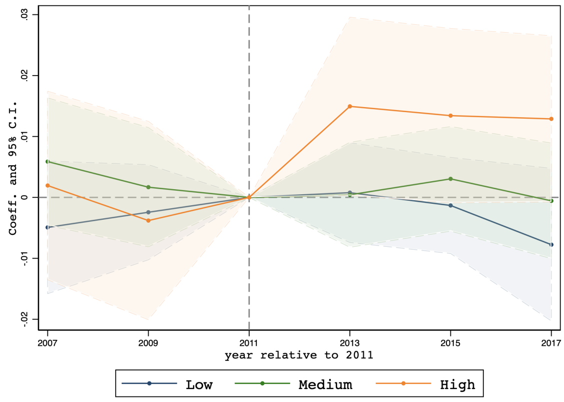
Source: Plus (INAPP) 2007-2017.
Notes: Estimates based on equation (2.2) distinguishing the sample by gender. The dependent variable is a dummy variable that takes value of 1 if individual i has attended human capital activities in the last 12 months.

FIGURE 2.12: Event-study estimates by:

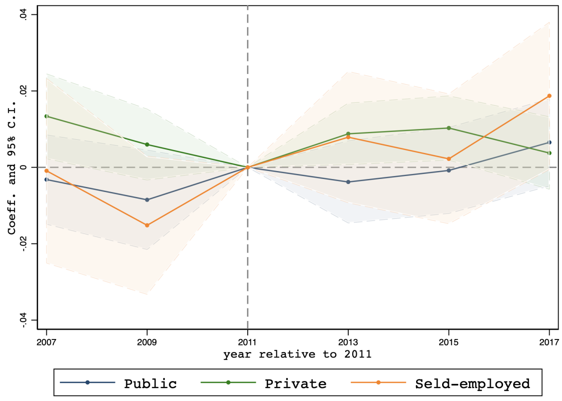
(A) Age classes



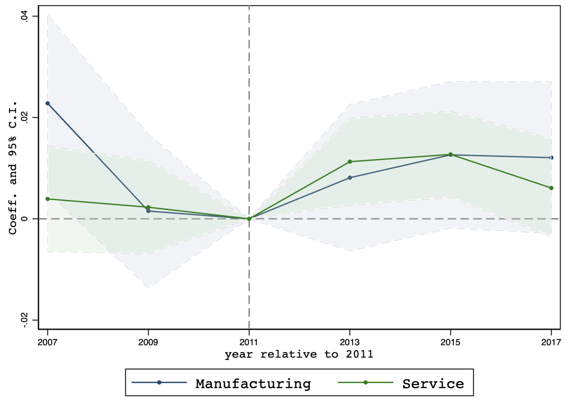
(C) Education



(B) Sector of employment



(D) Economic sector of the firm

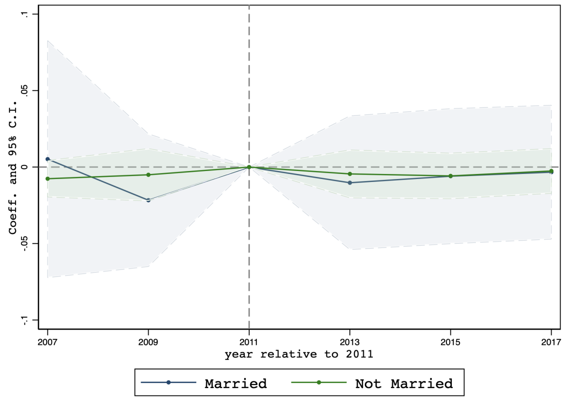


Source: PLUS (INAPP) 2007-2017.

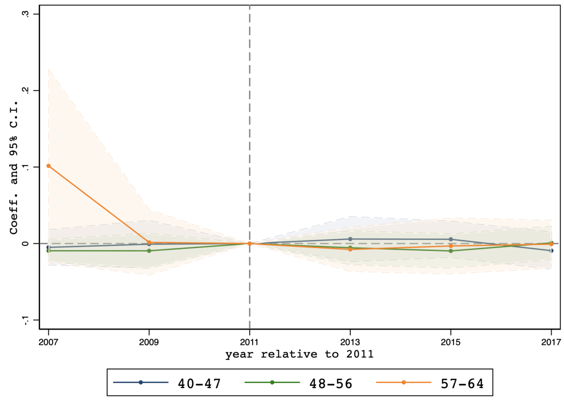
Notes: Estimates based on equation (2.2) distinguishing by each sub-sample. The dependent variable is a dummy variable that takes value of 1 if individual i has attended human capital activities in the last 12 months.

FIGURE 2.13: Event-study estimates, women only, by:

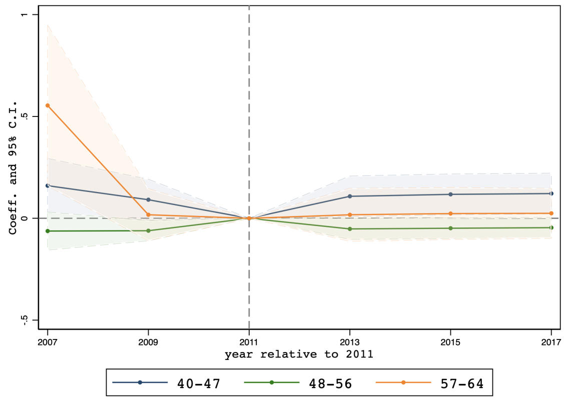
(A) Martial status



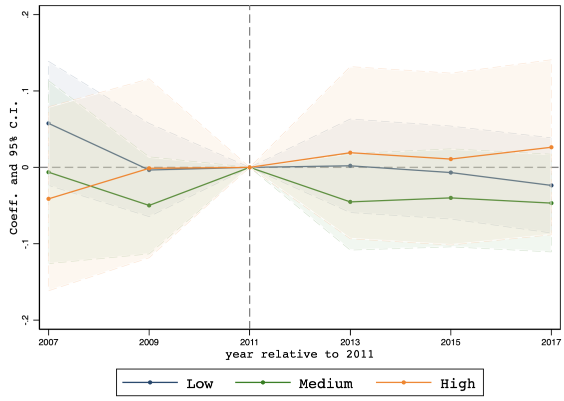
(C) Not married and age classes



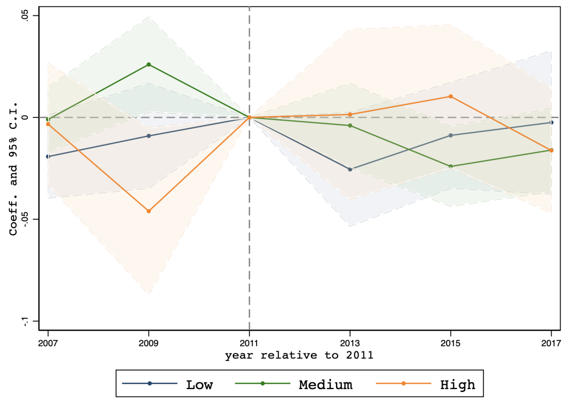
(B) Married and age classes



(D) Married and education



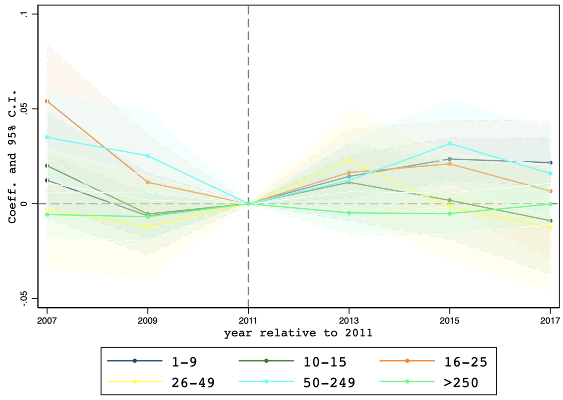
(E) Not married and education



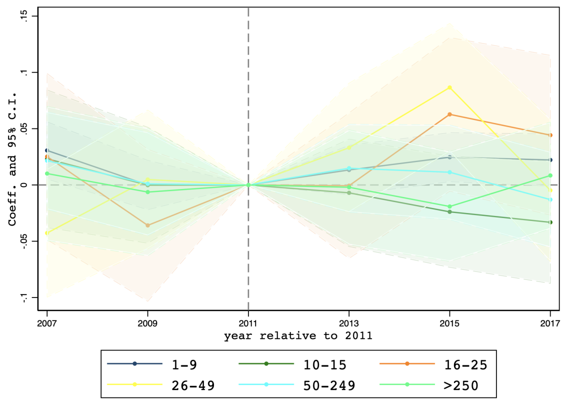
Source: PLUS (INAPP) 2007-2017.
Notes: Estimates based on equation (2.2) considering only the sample of women and distinguishing them according to their martial status (married or not married) and by age classes and education level. The dependent variable is a dummy variable that takes value of 1 if individual i has attended human capital activities in the last 12 months.

FIGURE 2.14: Event-study estimates by:

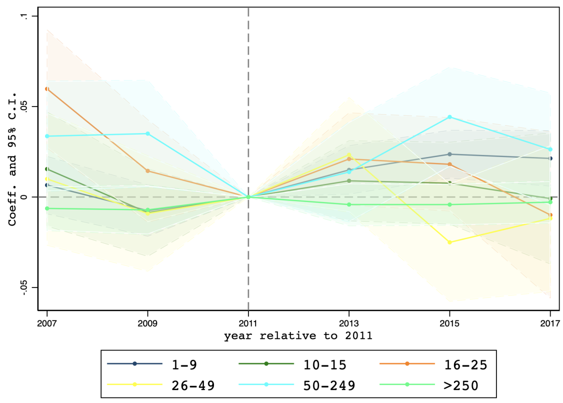
(A) Firm size



(B) Firm size, manufacturing sector

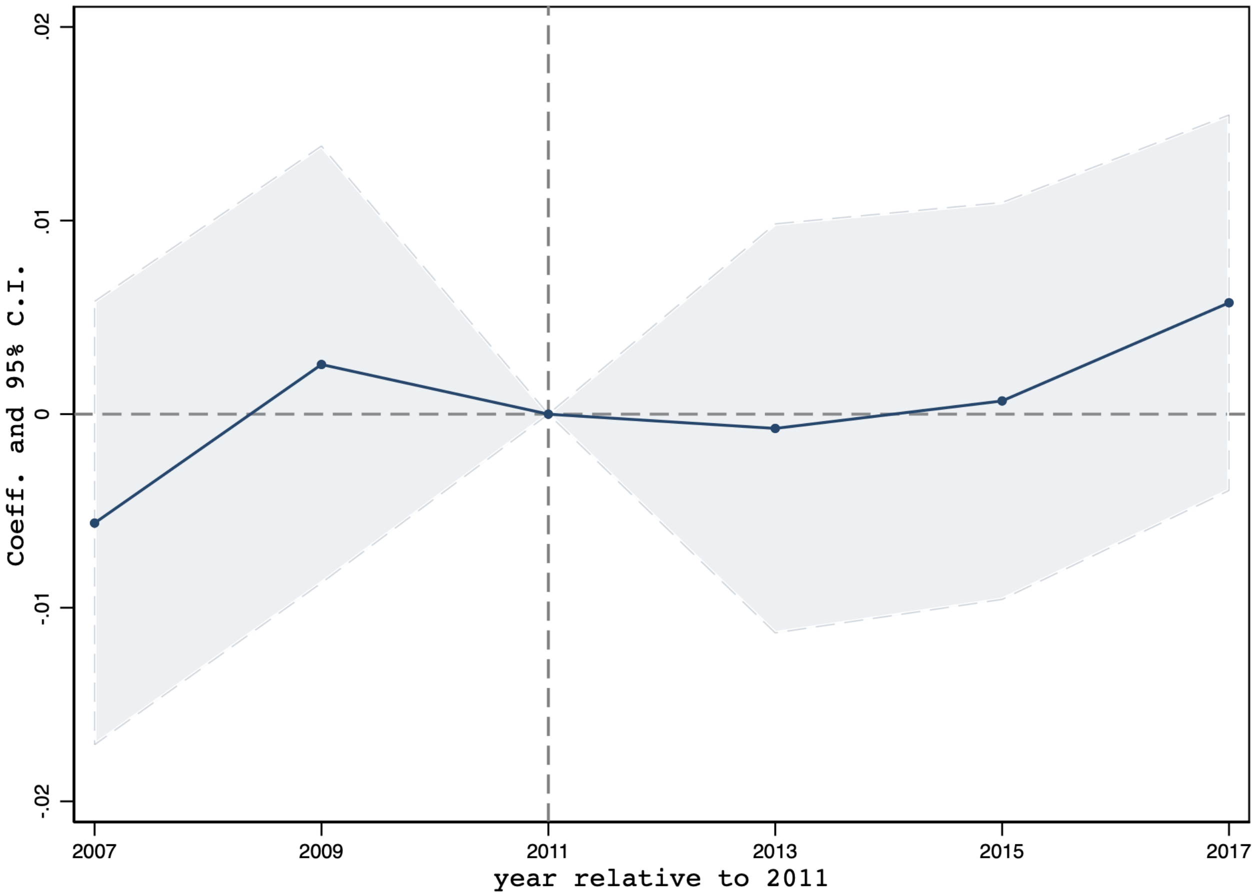


(C) Firm size, service sector



Source: PLUS (INAPP) 2007-2017.
Notes: Estimates based on equation (2.2) according to size of the firm where the worker os employed and its economic sector. The dependent variable is a dummy variable that takes value of 1 if individual i has attended human capital activities in the last 12 months.

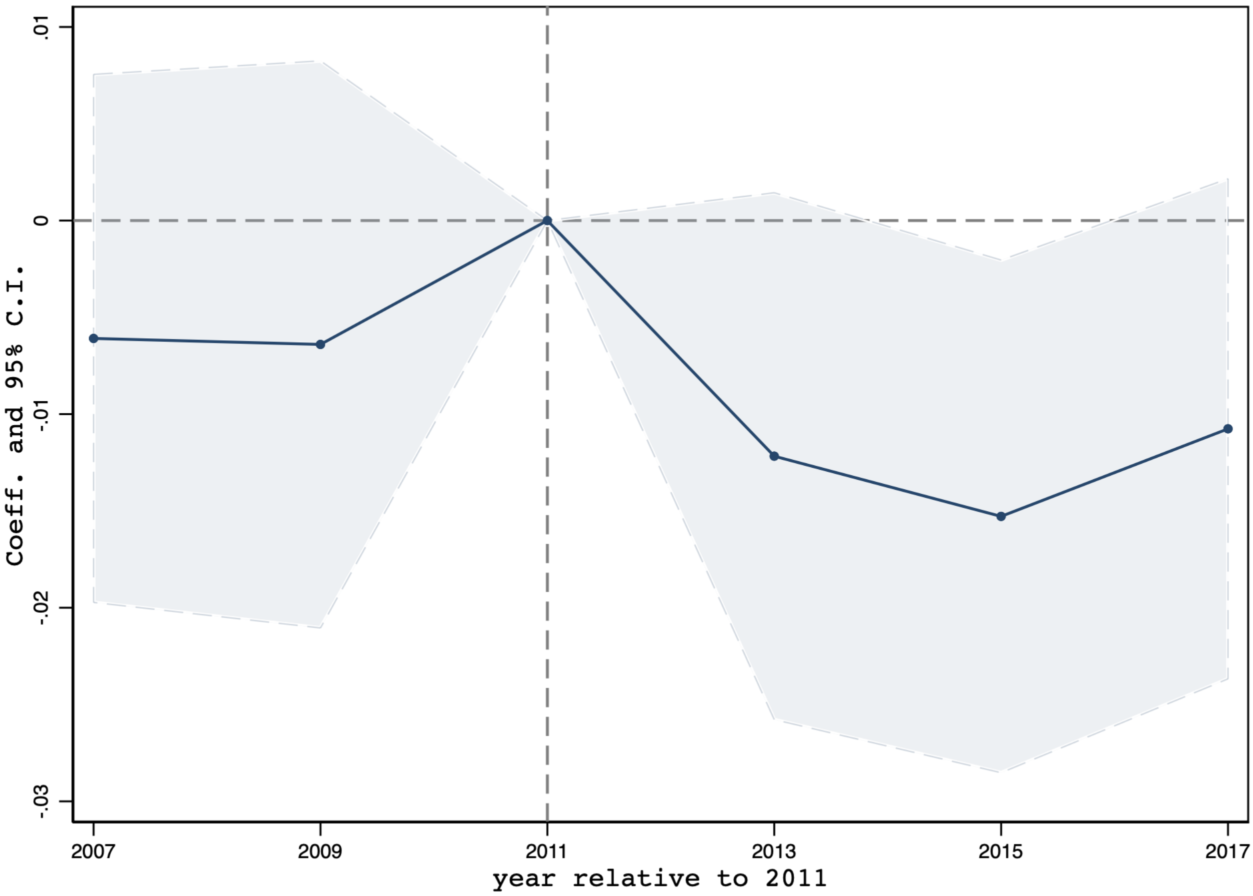
Figure 2.15: Event-study estimates



Source: Plus (INAPP) 2007-2017.

Notes: Estimates based on equation (2.2). The dependent variable is a dummy variable that takes value of 1 if individual i, conditional on having invested in human capital in the last 12 months, has paid for it.

FIGURE 2.16: Event-study estimates



Source: PLUS (INAPP) 2007-2017.
Notes: Estimates based on equation (2.2). The dependent variable is a dummy variable that takes value of 1 if the human capital activity has been sponsored by the firm/employer.

Table 2.1: Old age pension eligibility rules

	Men			Women		
Year	Public	Private	Self-employed	Public	Private	Self-employed
Before *Fornero* reform:						
2007	65	65	65	60	60	60
2008	65	65	65	60	60	60
2009	65	65	65	60	60	60
2010	65	65	65	61	60	60
2011	65	65	65	61	60	60
After *Fornero* reform:						
2012	66	66	66	66	62	63
2013	66	66	66	66	62	64
2014	66	66	66	66	64	65
2015	66	66	66	66	64	65
2016	67	67	67	67	66	66
2017	67	67	67	67	66	66
2018	67	67	67	67	67	67

Notes: Old age pension eligibility requires the legal retirement age (reported above) and at least 20 accrued years of contribution.

TABLE 2.2: Seniority pension eligibility rules

	Men				Women			
	Public-Private		Self-employed		Public-Private		Self-employed	
Year	only C	Quota	only C	Quota	only C	Quota	only C	Quota
Before *Fornero* reform:								
2007	39yc.	57y.+ 35yc.	40yc.	58y. + 35yc.	39yc.	57y.+ 35yc.	40yc.	58y. + 35yc.
2008	40yc.	58y.+ 35yc.	40yc.	59y. + 35yc.	40yc.	58y.+ 35yc.	40yc.	59y. + 35yc.
2009	40yc.	59y.+ 35yc.	40yc.	60y. + 35yc.	40yc.	59y.+ 35yc.	40yc.	60y. + 35yc.
2010	40yc.	59y.+ 35yc.	40yc.	60y. + 35yc.	40yc.	59y.+ 35yc.	40yc.	60y. + 35yc.
2011	40yc.	60y.+ 35yc.	40yc.	61y. + 35yc.	40yc.	60y.+ 35yc.	40yc.	61y. + 35yc.
After *Fornero* reform:								
2012	42yc.	-	42yc.	-	41yc.	-	41yc.	-
2013	42yc.	-	42yc.	-	41yc.	-	41yc.	-
2014	42yc.	-	42yc.	-	41yc.	-	41yc.	-
2015	42yc.	-	42yc.	-	41yc.	-	41yc.	-
2016	43yc.	-	43yc.	-	42yc.	-	42yc.	-
2017	43yc.	-	43yc.	-	42yc.	-	42yc.	-
2018	43yc.	-	43yc.	-	42yc.	-	42yc.	-

Notes: Under the seniority pension regime individuals can be granted eligibility when the number of accrued years of contribution reached a minimum amount; that is 39 in 2007, 40 between 2008 and 2011 and so on. When individuals retire using the option that I labelled as "only C" there is no binding age requirement. The second option, instead, available up to 2011 was the *Quota* system according to which individuals can retire if they have at least 35 years of contribution and a minimum age requirement. In the pre-reform period, self-employed and private-public employees were subject to different seniority pension rules, both in terms of "only C" and the *Quota* system, but not with respect gender. In the post-reform period requirement have been levelled out between sectors of employment but not with respect gender.

Table 2.3: Example of heterogeneity in gender, years of social security contributions, sector of employment, pension regimes and eligibility rules

	Pension rules in 2011:				Pension rules in 2017:			
	Seniority	Old age	Quota	MRA_{2011}	Seniority	Old age	MRA_{2017}	Shock:
Women, 59 years:								
Beatrice, C=35yc.; S=Priv.	64	60	60	**60**	66	66	**66**	6
Lucrezia, C=30yc.; S=Self-emp.	69	60	65	**60**	71	66	**66**	6
Paola, C=26yc.; S=Publ.	73	61	68	**61**	75	67	**67**	6
Men, 59 years:								
Alessandro, C=35yc.; S=Priv.	64	65	60	**60**	67	67	**67**	7
Francesco, C=30yc.; S=Self-empl.	69	65	64	**64**	72	67	**67**	3
Leonardo, C=26yc.; S=Publ.	73	65	68	**65**	76	67	**67**	2

Notes: This table reports an example of how individuals are differently affected by the increase in the MRA, given their accrued years of contribution, gender and sector of employment. The Table displays the age at which individuals can claim the old age or the seniority pension (including the *Quota* system in place before the *Fornero* reform). The minimum retirement age takes the first age of eligibility among the three pension regimes in the pre-reform period and among the two pension regimes in the post-reform period. C stands for the number of accrued years of contribution and S for the sector of employment (private, public or self-employed). Shock, the last column of the table, measures the difference between the minimum retirement age after and before the reform enacted at the end of 2011.

TABLE 2.4: Descriptive statistics

	2007-2015:			2007-2011 (pre-reform period):		
	All	$Shock_q > 3$ (most treated)	$Shock_q \leq 3$ (least treated)	All	$Shock_q > 3$ (most treated)	$Shock_q \leq 3$ (least treated)
Men	0.528	0.449	0.666	0.562	0.473	0.703
	(0.499)	(0.497)	(0.472)	(0.496)	(0.499)	(0.457)
Age	51.861	52.053	51.525	51.790	51.883	51.643
	(5.978)	(6.170)	(5.608)	(5.548)	(5.622)	(5.424)
Years of contrib.	25.946	24.661	28.202	25.750	24.318	28.044
	(7.904)	(7.767)	(7.634)	(7.745)	(7.421)	(7.704)
High educ.	0.283	0.327	0.206	0.242	0.278	0.185
	(0.450)	(0.469)	(0.404)	(0.428)	(0.448)	(0.388)
Married	0.577	0.574	0.582	0.291	0.274	0.318
	(0.494)	(0.494)	(0.493)	(0.454)	(0.446)	(0.466)
Household size	3.167	3.154	3.189	3.176	3.161	3.201
	(1.157)	(1.166)	(1.140)	(1.153)	(1.163)	(1.138)
If children	0.800	0.804	0.793	0.821	0.825	0.814
	(0.400)	(0.397)	(0.405)	(0.383)	(0.380)	(0.389)
Annual earnings	28,138.844	28,000.584	28,380.898	28,377.006	28,652.243	27,944.502
	(28,374.396)	(29,097.370)	(27,061.327)	(28,428.983)	(30,558.296)	(24,711.057)
Public sector	0.391	0.400	0.376	0.407	0.414	0.396
	(0.488)	(0.490)	(0.484)	(0.491)	(0.493)	(0.489)
Private sector	0.460	0.403	0.561	0.451	0.392	0.547
	(0.498)	(0.490)	(0.496)	(0.498)	(0.488)	(0.498)
Self-employed	0.149	0.198	0.063	0.142	0.194	0.057
	(0.356)	(0.398)	(0.244)	(0.349)	(0.396)	(0.232)
HAC	0.398	0.415	0.370	0.346	0.359	0.324
	(0.490)	(0.493)	(0.483)	(0.476)	(0.480)	(0.468)
Paid HAC	0.258	0.279	0.218	0.373	0.398	0.328
	(0.438)	(0.449)	(0.413)	(0.484)	(0.489)	(0.470)
Firm-sponsored HAC	0.497	0.477	0.534	0.430	0.409	0.467
	(0.500)	(0.499)	(0.499)	(0.495)	(0.492)	(0.499)
Obs.	53,977	34,386	19,591	20,600	12,681	7,919

Notes: The sample is composed of individuals aged between 40 and 64 years, with at least 10 and less than 40 accrued years of contribution, eligible to retire neither before nor after the reform. HAC stands for human capital accumulation. Mean averages and standard deviation in parentheses.

Table 2.5: *Forward-looking* effect on human capital participation activities

	All (1)	Men (2)	Women (3)
$shock_q \times post2011$	0.0069**	0.0093**	0.0034
	(0.0024)	(0.0032)	(0.0035)
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	no	no
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes
Obs.	53,977	28,478	25,499
R^2	0.1314	0.1043	0.1750
Adj. R^2	0.1299	0.1015	0.1722

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.6: *Forward-looking* effect on human capital participation activities by:

	Age class:		
	40-47	48-56	57-64
	(1)	(2)	(3)
		All:	
$shock_q \times post2011$	0.0131**	0.0074*	-0.0038
	(0.0048)	(0.0033)	(0.0049)
Obs.	13,600	27,289	13,088
R^2	0.1373	0.1353	0.1293
Adj. R^2	0.1332	0.1330	0.1245
		Men:	
$shock_q \times post2011$	0.0140*	0.0113*	-0.0007
	(0.0068)	(0.0046)	(0.0062)
Obs.	6,103	14,703	7,672
R^2	0.1181	0.1070	0.1109
Adj. R^2	0.1087	0.1026	0.1025
		Women:	
$shock_q \times post2011$	0.0088	0.0027	-0.0041
	(0.0069)	(0.0047)	(0.0082)
Obs.	7,497	12,586	5,416
R^2	0.1728	0.1831	0.1738
Adj. R^2	0.1658	0.1784	0.1629
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.7: *Forward-looking* effect on human capital participation activities by:

	Sector of employment:		
	Public	Private	Self-employed
	(1)	(2)	(3)
$shock_q \times post2011$	0.0042	0.0016	0.0154*
	(0.0043)	(0.0033)	(0.0064)
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes
Obs.	21,113	24,831	8,033
R^2	0.0793	0.0729	0.0876
Adj. R^2	0.0754	0.0696	0.0776

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.8: *Forward-looking* effect on human capital participation activities by:

	Firm's economic sector:	
	Manufacturing	Service
	(1)	(2)
$shock_q \times post2011$	0.0038	0.0083**
	(0.0055)	(0.0032)
Obs.	8,059	24,805
R^2	0.0766	0.0861
Adj. R^2	0.0664	0.0829
Year FE	yes	yes
Shock FE	yes	yes
Gender FE	yes	yes
Age FE	yes	yes
Martial stat. FE	yes	yes
Region FE	yes	yes
Sector FE	yes	yes
Y. of contr. FE	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.9: *Forward-looking* effect on human capital participation activities by:

	Education level:		
	Low	Medium	High
	(1)	(2)	(3)
		All:	
$shock_q \times post2011$	0.0005	-0.0013	0.0143**
	(0.0035)	(0.0033)	(0.0047)
Obs.	11,645	27,057	15,275
R^2	0.0726	0.1083	0.0769
Adj. R^2	0.0655	0.1054	0.0715
		Men:	
$shock_q \times post2011$	0.0067	0.0073^{+}	0.0173**
	(0.0048)	(0.0043)	(0.0060)
Obs.	6,694	14,319	7,465
R^2	0.0722	0.0840	0.0772
Adj. R^2	0.0597	0.0783	0.0661
		Women:	
$shock_q \times post2011$	-0.0039	-0.0114*	0.0154^{+}
	(0.0058)	(0.0048)	(0.0088)
Obs.	4,951	12,738	7,810
R^2	0.1011	0.1548	0.0916
Adj. R^2	0.0848	0.1489	0.0813
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.10: *Forward-looking* effect on human capital participation activities by:

	Married		Not married	
	(1)	(2)	(3)	(4)
$shock_q \times post2011$	0.0136*	0.0135*	-0.0009	-0.0008
	(0.0066)	(0.0066)	(0.0059)	(0.0059)
Year FE	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes
Age FE	yes	yes	yes	yes
Martial stat. FE	no	no	yes	yes
Region FE	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes
No. kids	no	yes	no	yes
HH size	no	yes	no	yes
Obs.	14,991	14,991	10,508	10,508
R^2	0.1633	0.1640	0.1990	0.1993
Adj. R^2	0.1586	0.1591	0.1924	0.1925

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.11: *Forward-looking* effect on human capital participation activities, women only, by

	Age class:					
	40-47		48-56		57-64	
	(1)	(2)	(3)	(4)	(5)	(6)
	Married:					
$shock_q \times post2011$	0.0245^{+}	0.0246^{+}	0.0089	0.0090	0.0042	0.0043
	(0.0139)	(0.0139)	(0.0083)	(0.0082)	(0.0152)	(0.0152)
Obs.	4,610	4,610	7,127	7,127	3,254	3,254
R^2	0.1644	0.1645	0.1693	0.1703	0.1682	0.1708
Adj. R^2	0.1536	0.1533	0.1613	0.1621	0.1507	0.1528
	Not married:					
$shock_q \times post2011$	0.0020	0.0022	0.0001	0.0000	-0.0051	-0.0063
	(0.0091)	(0.0091)	(0.0087)	(0.0087)	(0.0123)	(0.0122)
Obs.	2,887	2,887	5,459	5,459	2,162	2,162
R^2	0.2080	0.2088	0.2112	0.2114	0.1977	0.1992
Adj. R^2	0.1906	0.1908	0.2008	0.2007	0.1708	0.1716
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
No. kids	no	yes	no	yes	no	yes
HH size	no	yes	no	yes	no	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.12: *Forward-looking* effect on human capital participation activities, women only, by:

	Education level:					
	Low		Medium		High	
	(1)	(2)	(3)	(4)	(5)	(6)
	Married:					
$shock_q \times post2011$	-0.0051	-0.0049	0.0034	0.0036	0.0213	0.0213
	(0.0088)	(0.0088)	(0.0089)	(0.0089)	(0.0141)	(0.0141)
Obs.	2,731	2,731	7,585	7,585	4,675	4,675
R^2	0.1007	0.1011	0.1492	0.1508	0.0772	0.0772
Adj. R^2	0.0722	0.0718	0.1397	0.1411	0.0599	0.0599
	Not married:					
$shock_q \times post2011$	-0.0056	-0.0058	-0.0172*	-0.0172*	0.0073	0.0073
	(0.0108)	(0.0108)	(0.0078)	(0.0078)	(0.0135)	(0.0135)
Obs.	2,220	2,220	5,153	5,153	3,135	3,135
R^2	0.1320	0.1332	0.1777	0.1778	0.1391	0.1391
Adj. R^2	0.0966	0.0969	0.1635	0.1633	0.1139	0.1139
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
No. kids	no	yes	no	yes	yes	yes
HH size	no	yes	no	yes	no	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.13: *Forward-looking* effect on human capital participation activities by:

	Firm size:					
	1-9	10-15	16-25	26-49	50-249	>250
	(1)	(2)	(3)	(4)	(5)	(6)
$shock_q \times post2011$	0.0185***	-0.0003	-0.0040	0.0106	-0.0003	-0.0082
	(0.0042)	(0.0086)	(0.0091)	(0.0115)	(0.0086)	(0.0069)
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
Obs.	11,975	2,827	2,113	1,864	3,909	8,536
R^2	0.1012	0.0906	0.0944	0.1130	0.0767	0.0828
Adj. R^2	0.0945	0.0614	0.0550	0.0690	0.0554	0.0733

Notes: The estimates refer only to self-employed and private sector workers. Firm size refers to the number of employees, including the interviewed, working in the firm at the year of interview. Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^+ p < 0.10$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

TABLE 2.14: *Forward-looking* effect on human capital participation activities by:

	Firm size:					
	1-9	10-15	16-25	26-49	50-249	>250
	(1)	(2)	(3)	(4)	(5)	(6)
	Manufacturing sector:					
$shock_q \times post2011$	0.0109	-0.0282	0.0335	0.0480*	-0.0017	-0.0048
	(0.0083)	(0.0180)	(0.0264)	(0.0234)	(0.0171)	(0.0177)
Obs.	3,063	739	439	560	1,339	1,623
R^2	0.1218	0.1465	0.2300	0.2216	0.1207	0.1015
Adj. R^2	0.0958	0.0310	0.0391	0.0781	0.0588	0.0500
	Service sector:					
$shock_q \times post2011$	0.0208***	0.0057	-0.0081	-0.0026	0.0033	-0.0107
	(0.0049)	(0.0096)	(0.0101)	(0.0135)	(0.0105)	(0.0077)
Obs.	8,912	2,088	1,674	1,304	2,570	6,913
R^2	0.1039	0.1077	0.1134	0.1428	0.0909	0.0938
Adj. R^2	0.0949	0.0684	0.0642	0.0808	0.0586	0.0821
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 2.15: *Forward-looking* effect on human capital participation activities by:

	All: (1)	Paid Wage above median: (2)	Wage below median: (3)	Firm-sponsored (4)
$shock_q \times post2011$	0.0041	0.0056	0.0018	-0.0079$^+$
	(0.0036)	(0.0046)	(0.0055)	(0.0041)
Year FE	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes
Age FE	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes
Obs.	21,289	13,033	8,256	20,308
R^2	0.2081	0.2175	0.2036	0.0938
Adj. R^2	0.2048	0.2121	0.1949	0.0898

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 3

Academic productivity and pandemic evidence from female economists during the COVID19 crisis

3.1 Introduction

Working from home has become increasingly popular among firms and workers, especially during the last decade. Generally, this arrangement provides the worker with some degrees of flexibility on the number of days required to work from the office as well as on the daily hours of work. According to Bloom, Kretschmer, and Van Reenen, 2009, 1 out of 3 manufacturing firms in the EU and US offer opportunities to work from home to certain workers, however with wide variation across industries.

Few (experimental) studies evaluate the pros and cons of teleworking, finding that working from home increases productivity (Angelici and Profeta, 2020; Bloom et al., 2015), reduces employees' turnover (Bloom et al., 2015), absenteeism (Angelici and Profeta, 2020) and contributes to improvements in well-being and gender balance within the family (Angelici and Profeta, 2020).

The interest for the economic implications stemming from smart-working has been renewed during the COVID-19 pandemic. Indeed, governments to curb down the virus spread have adopted several non-pharmaceutical interventions that resulted also in a wide encouragement to businesses to switch their activities from face-to-face to remote, generating the largest home-working experiment in modern history.

However, the home-working experienced by many during the first wave of the pandemic is conceptually different from the usual remote flexibility arrangements proposed by firms. First, while allowed to work remotely, people were also forced to stay home because of lockdown measures. Second, with school closures many parents have found themselves to look after children while expected to continue to work from home, increasing dramatically childcare demand and household chores that are disproportionately shouldered by women that likely will further exacerbate gender inequalities within labour market outcomes.

Hence, how has the productivity of men and women been affected while home-working during the pandemic? While punctual, appropriate and real-time data (especially from the private sector) is often unavailable, it is possible to focus the attention on a sector that worldwide has suddenly adopted flexible work arrangements. Indeed, the academic sector, as governments mandated schools and universities closures, has shifted, almost immediately, its activities virtually without interrupting education and students' training. Furthermore, apart from teaching, many academic fields do not necessitate of a physical presence to conduct research.

In particular, in this research, I decided to focus on the economics field within the academic sector, given that economic research usually begins to circulate, well before peer-reviews, in the form of working papers allowing me to trace almost in real-time (on a daily basis) how economists' productivity evolved, especially during the lockdown period. Furthermore, working papers, despite not being already published, represent an objective measure of productivity for an academic and skipping even a single year of publications can severely affect (young-)academics' career.

Specifically, the research question aims to investigate how the productivity of female economists has been impacted by the current pandemic, also in light of the sudden increase of household chores borne by women due to school closures and social distancing measures. Ignoring the disproportionate effects of the COVID-19 pandemic on women's productivity risks backsliding on substantial progress for academic diversity (Woolston, 2020). The current policy and research debate, also, is focusing its attentions on the unequal impact of lockdown measures on households, and also the press reports several articles that describe the struggle of families (and especially women) in reconciling their careers (especially if in home-working) and family' chores. An example of these difficulties is reported by Minello, 2020, a social demographer at the University of Firenze, explaining how mothers (and fathers together) are facing a short-term reorganization of care and work time, and how these changes in productivity may affect their careers in the long-run ending up to exacerbate further gender inequalities in the academic sector.

Besides, there is already (mainly descriptive) evidence, despite being very preliminary, of the health shock crisis effect on female economists' productivity. According to Fuchs-Schundeln, 2020 there has been a reduction during the first 5 months of 2020 in the rate of paper submissions to ReStud by female economists of about 2 percentage points (whereas the share of male submissions slightly increased); Rasul, 2020 exploiting submissions to the Journal of the European Economic Association finds that the share of female submitters declined from 28 to 16%; Shurkov, 2020 finds a drop of 8 p.p. for six working paper series. Differently from these studies, I am the first to test directly whether the sudden increase in childcare demand, due to the lockdowns, has played a role in causing the widening of the gender production gap.

To achieve causal identification I exploit the begin of lockdown policies enacted as the results of the COVID-19 pandemic as an exogenous shock to (female) academic productivity within a Difference-in-Differences approach.

Data on daily published economics working papers, extracted through web-scraping techniques, are collected from the SSRN web archive. My sample is composed of 4,778 distinct pre-prints involving 8,651 authors (of which 2,217 female and 6,405 male economists) observed from January to November 17, 2020.

According to the DID estimates, within different bandwidth sizes measured in terms of number of days before and after the lockdown begin, there is evidence that the lockdown period has a significant negative impact on the number of working papers that a female economist, alone or jointly with other researchers, uploads on SSRN of about 20 percentage points, and this effect persists up to about 4 months later. These estimates yield, hence, consistent results with the idea that lockdown policies have affected negatively the labour market performance of female academic economists. However, for authors exposed to school re-openings within the same time frame, declines in productivity disappear suggesting that indeed childcare demand has been an important channel in causing women production drop.

Furthermore, by exploiting the individual author-level dimension of the data, that is tenure, I show that the dynamics of the daily number of female authors mirrors the dynamics of the gender production gap during the lockdown and the school re-openings period, highlighting, however, that tenured economists working within academia are those who most suffered (gained) during mobility restrictions (school re-openings) period.

In addition, I show that my results are very robust to a series of falsification tests that formally rule out the possibility that my estimates are driven by a seasonality concern.

Finally, I explore also whether the negative effect on female academic productivity, due to the lockdown, can be explained by an increase in quality (measured by two different proxies), as if researchers traded-off quantity for quality. The empirical estimates suggest that in the aftermath of the lockdown measures research quality did not change significantly, supporting the idea that my findings are unlikely to be driven by the shifts in research quality at the expense of quantity.

This research is related to two strands of the literature. The first relates to the likely unequal consequences of the COVID-19 crisis on labour market outcomes and productivity of men and women. Indeed, lockdown measures adopted by public authorities aimed at lowering the spread of the virus, of course, have hardly impacted the labour market. Past researches (see for instance Rubery and Rafferty, 2013; Hoynes, Miller, and Schaller, 2012) showed that the great recession tended to severely affect industries considered as more male-dominated (such as the construction and manufacturing sectors). In contrast, the COVID-19 crisis has impacted hardly economic sectors that do not comply with social-distancing (such as retail and touristic industries), where the share of female employment is (usually) higher (Alon et al., 2020). Hence, as the labour market participation of men and women differ across industries, it is reasonable to think that the labour market effects, implied by the health crisis, may have (further) unequal consequences across genders. Furthermore, social-distancing measures have also forced governments to shut down schools in an effort to curb down virus diffusion. School closures, however, further differentially affect men and women on home production given that childcare responsibilities and household chores (worldwide) are higher for women rather than for men (Del Boca et al., 2020; Hupkau and Petrongolo, 2020). In particular, Adams-Prassl et al., 2020 find evidence that, since lockdown measures started, women in the US, the UK and Germany are spending more time on active childcare and homeschooling than men.

The second strand of the literature, instead, refers to studies investigating the labour market outcomes of female economists in academia. Despite substantial progress in women's educational attainment over the past 30 years, the fraction of women in economics, at all stages of the educational and professional ladder, has remained stubbornly low (Lundberg and Stearns, 2019; Lundberg, 2018; Bayer and Rouse, 2016). Recent research has highlighted the unequal treatment of women in academic economics, along several different dimensions. Women in economics are less likely to be promoted, even conditional on productivity (Ginther and Kahn, 2014; Ginther and Kahn, 2004); male economists are less likely, relative to other fields, to exhibit a women preference in a hypothetical hiring scenario (Williams and Ceci, 2015); women receive less credit for co-authorship (Sarsons, 2017); female-authored papers in top economics journals are held to higher standards and go through a longer process of peer review (Card et al., 2019; Hengel, 2017); female economists tend to produce fewer papers than men (Lundberg, 2020), write fewer single author papers and prefer to maintain strong productive ties with a small circle of coauthors (Ductor, Goyal, and Prummer, 2018); female instructors in economics receive lower teacher evaluations (Mengel, Sauermann, and Zolitz, 2019; Boring, 2017); women are underrepresented in principles of economics textbooks (Stevenson and Zlotnik, 2018); and women face explicit hostility in an anonymous online forum with academic and professional purposes (Wu, 2018).

3.2 Data and empirical strategy

Data. I collected data from the Social Science Research Network (SSRN), a repository of pre-prints (working papers) with aim of rapidly disseminate academic research outputs in social science. In particular, through web-scraping techniques, I extracted all the working papers uploaded from January to November 17, 2020, on the repository' website regarding the *Economics Departments Research Papers* and *Economics Research Centers Papers*, that is the two most popular pre-prints series in the economic field available on SSRN. I gather information, at the daily and working paper level, on paper titles, authors' names, authors' affiliations, and their addresses.

From the whole number of records extracted, I kept only working papers that were submitted for online dissemination for the first time during the (almost) 11 months period of 2020. The final number of papers amount to 4,778 distinct pre-prints involving 8,651 authors from over 90 countries. In addition, by matching authors' names and their affiliations, I hand-collected their current job positions obtaining detailed descriptions of their employment status for a total of 93.5% of the authors' sample.

To impute the gender of each author, I used the genderize.io tool which predicts the genders based on their first names and provides a corresponding confidence level about the certainty of the assignment. All names that were assigned gender with a probability lower than 70% were checked and assigned manually by searching them on the web using their complete names and affiliations and then infer their genders from their profile photos[1].

[1] Additionally, I verified manually a random sample of 15% of the names with gender probability assignment above 70 percent in order to cast away any doubts about the accuracy of the imputed

The sample of authors consists of 2,217 female economists and 6,405 male economists.

Identification. The identification strategy of the likely negative effect on female economists' productivity exploits the lockdown policies enacted in response of the COVID-19 pandemic as an exogenous shock that has caused substantial disruptions to academic activities, requiring academics to conduct research, teach, and carry out household duties at home.

The validity of this approach rests on the assumption that the shock is exogenous with respect to the researchers' anticipated responses, that is it requires that none of the two academics gender groups strategically anticipated the COVID-19 outbreak by quickening the completion of their current research works because, otherwise, the estimated causal effect would be biased.

However, as the recent facts demonstrate the COVID-19 epidemic was regarded as a low-risk threat in many countries up to the end of February 2020 and no significant actions had been taken other than travel warnings for selected destinations. Indeed, as Figures 3.1a and 3.1b show, the number of countries enacting lockdowns or shelter-in-place policies is only increased after the declaration of a global pandemic by the World Health Organization on March 11, 2020. Hence, the possibility of strategical anticipation by researchers seems unrealistic given the rapid and dramatic developments of the COVID-19 pandemic.

By taking advantage of each author's country (of affiliation), I collect each country's lockdown start date from news sources and the United Nations' report. Based on this information, I assign to each author the respective lockdown date enacted in her country. Given that many of the papers included in my sample are co-authored, that is written by more than 1 author (84.5% of the total number of working papers), I compute the earliest lockdown date among those of each author for each working paper. However, if one paper has among its authors a female economist (or more) I

genders. Furthermore, during the hand-collection of the employment status for each author I again validated the imputed genders, finding that only in less than 1% of the cases the assignment was not correct.

always consider as the earliest date that of the female economist whose country first introduced lockdown policies.

Empirical strategy. To provide empirical evidence on the likely negative effect implied by the COVID-19 on female economists' productivity, the empirical strategy takes advantage of the temporal discontinuity implied by the enactment of lockdown measures. In particular, I aggregated the number of papers at the (normalized) daily level distinguishing them whether or not among their authors there is at least one female economist. The analysis, hence, leverages the pandemic shock within a Difference-in-Differences approach. Specifically, the empirical model reads as follows:

$$no.papers_{it} = \alpha + \beta F_i + \theta F_i \times LD_t + \lambda_t + \eta_{it} \tag{3.1}$$

where $no.papers_{it}$ represents the number of papers observed for gender i on day t, F_i is dummy variable that takes value of 1 if gender i is female, and 0 otherwise. The dummy variable LD_t equals 1 if day t occurs the day or after the lockdown measure was adopted and λ_t includes a set of daily time dummies that control for time trends. The parameter of interest, measuring the effect of COVID-19 shock on the productivity level of female economists (that I expect to be negative), is given by θ that estimates the effect of the lockdown on female academics' research productivity relative to male researchers' productivity.

Moreover, I choose to estimate my Difference-in-Differences using different bandwidths, measured in days, around the lockdown event given that I am able to follow daily working paper production from 90 days before and up to 240 days after lockdown policies were mandated. That is, I estimate the empirical specification starting with 3 different symmetric bandwidths of ±30, ±60 and ±90 days around day 0 and I extend the model using always 90 days before the lockdown as the pre-treatment period and 120, 150, 180, 210 and 240 days after the lockdown as the post-treatment periods to check whether and how long the effect is persistent over time.

Descriptive statistics. The whole number of working papers extracted equals 4,778 distinct pre-prints of which the 44.2 percent has been written by at least a female economist (see Table 3.1), even though the total number of female authors per each paper, on average, do not reach the unity. On average, each paper is written by almost 3 authors, has a length of about 47 pages, its abstract has been viewed about 111 times and downloaded almost 20 times over the sample period. Of these 4,778 pre-prints about 26 percent of them has been published during May and June (see Figure 3.3), that is the months during which production has been the highest. With regards those working papers wrote by at least one female economist (see the second panel of Table 3.1), these pre-prints are written, on average, by 3.2 authors of which slightly more than 1.4 is a female economist. In addition, for these papers the female intensity relative to the total number of authors equals 0.5, meaning that, on average, the author group is perfectly gendered balanced. These papers, furthermore, are viewed 103 times, downloaded 16 times and have a length of 48 pages. On the other hand, regarding the sample of papers written by only male colleagues, the total number of authors per paper is lower than the previous one equalling slightly more than 2 authors. While these working papers are online viewed 117 (that is 1.1 more if compared to the figure for female economists), these are downloaded 22 times (about 1.4 times more than women colleagues) but have a length fully comparable to the other group.

The total number of authors involved in the drafting of the 4,778 pre-prints equals 8,651 of which 2,217 are female economists (34.6 percent of the sample) and 6,405 are male economists. In Figure 3.4 I show the percentage composition of the authors by gender and countries of affiliation. Concerning only female economists, Figure 3.4a shows that less than 40 percent of the total number of female authors are affiliated with institutions based in the US, followed by Germany, UK, Italy, Russia and France. A similar pattern, furthermore, is observed for the sample of male researchers (see Figure 3.4b), although with slightly different percentage compositions.

Figure 3.5 shows the percentage distributions of the 8,651 authors by gender and job position. Concerning female authors, Figure 3.5a points out that around 20 percent are full professors (against the 35.6% of male colleagues, see Figure 3.5b), another 20 percent holds a position as assistant professors (17% of men; among those with tenure, that is senior assistant professors the respective shares for women and men are 1.9 and 1.5 percent), 13.8% are associate professors (13.6 p.p. men), 11.1 percent are PhDs (whereas men are about 9.8%) and finally, around 10.7 percent of them are economists outside academia (10 percent men). For the remaining positions, that is post-docs, research assistants, research fellows, researchers, senior economists, senior research fellows, senior researchers and students the percentage compositions are not alike between the two groups. Given the large heterogeneity regarding the positions held by the authors included in the sample, I grouped them into two categories based on whether they can be considered as tenured or not. In particular, I define tenured those holding a job position such as senior assistant-associate or full professor, economist or senior economist, researcher or senior researcher and senior research fellows. Consequently, all the remaining job positions are considered as untenured. In Figure 3.6, I plot the percentage compositions of authors according to their tenure and gender. According to Figure 3.6a, the 58.6% of women hold tenured positions, whereas those holding untenured jobs amount to 41.4 percent. On the contrary, men are more likely to hold tenured positions (69 p.p.) than untenured ones (31 percent).

In Figure 3.7a, instead, I plot the difference at the daily level between the number of papers written by at least one female economist with those written by only male colleagues. As the figure shows during the 90 days before the lockdown begins the productivity gap alternates positively and negatively. As the lockdown begins, during the subsequent 90-100 days the productivity gap considerably worsens given that the number of days where the difference, in terms of daily number of papers, is negative increased substantially, whereas, during the remaining 120 days it seems to behave as during the pre-lockdown period. In Figure 3.7b, instead, I plot the average daily number of new working papers by gender before, during the lockdown and in the aftermath of school re-openings. Indeed, during the pre-lockdown period, the average number of daily new pre-prints has almost been the same across gender, slightly above 6 for papers with only male economists and slightly lower than 6 for papers with at least a female academic among its authors. During the lockdown period, although male somewhat increased their productivity at the daily level, those written by at least one female researcher decreased. However, during the post-school re-opening period, although production for both groups is lower than the previous periods, daily production is almost identical. Finally, Figure 3.8 shows the average number of daily authors across periods, tenure and gender. According to it, whatever the period considered the average number of daily tenured men authors is higher than those untenured and also in comparison with female authors. For what concern women, if the average daily figure was higher for tenured rather than untenured, during the lockdown period the average number somewhat declined for tenured ones whereas it remained unchanged for untenured female economists. As shown also by Figure 3.7b, also in Figure 3.8 during the school re-opening phase for both gender groups and independently of the tenure the average daily number of authors considerably reduced, even though among women no remarkably differences emerge.

3.3 COVID-19 and female academic productivity

Table 3.2 reports the estimated effects of the pandemic shock on research productivity at the aggregate daily level using Equation (3.1) and different bandwidth sizes, measured in terms of days before and after the lockdown, as explained in Section 3.2.

The estimates yield consistent results with the idea that lockdown policies have increased household chores borne by especially by women that in turn may affect negatively their labour market performance. First, in line with the descriptive evidence, there is evidence that fewer working papers are produced by female economists relative to men, in general. Furthermore, since the lockdown began, there has been a significant and persistent reduction in female academics' productivity relative to that of their male colleagues up to about 4 months later the lockdown enactment, suggesting that in the aftermath of policies aimed at curbing down the epidemic curve the gender productivity gap has increased. Indeed, the coefficient of the interaction term of column (4) of Table 3.2, during the 120 days subsequent the lockdown, equals to -1.13 translating in an average reduction in productivity, measured in terms of number of papers, of about 19.6 percentage points relative to male academics' productivity.

In column (3) of Table 3.2 I report the results relative to the bandwidth of ±90 days. According to the estimates, the coefficient relative to the interaction term still displays statistical significance and amounts to about -1.31, translating in an average reduction in productivity by female economists, relative to that of men's, of about 22.6 percentage points. In column (2) of Table 3.2 are reported the coefficients estimated using a bandwidth of ±60 days. The estimated effects are not alike from those of columns (3) and (4) but in this case, overall gender differences in productivity do not display statistical significance, whereas the coefficient relative to the causal effect of interest suggests an average reduction in production of about 26 percentage points.

In column (1) I present the estimated coefficients using a bandwidth of ±30 days around the lockdown event. However, this specification should be taken with caution given the small number of observations and given that in a period from the beginning of the lockdown of such a short time the likely negative impact on productivity may not have fully materialized. To put it in other words, given the short time frame it is probable that the number of papers published is not alike from that of the previous 30 days, assuming that those published in the following 30 days after the lockdown began, refer to researches started in a much earlier period compared to the COVID-19 crisis. In this case, the reported coefficients are almost similar to those observed in column (2) but not statistically significant at the conventional confidence level.

Finally, in columns (5)-(8) are reported the estimates using 90 days before the lockdown as the pre-treatment period and 150 to 240 days after the lockdown as the post-treatment periods. In these cases, overall gender differences in production are equal to those observed in the other specifications, whereas the interaction terms, despite being negative and shrinking in magnitude as gradually you move away from the event, they are never statistically significant.

Overall, these estimates support the view that the lockdown policies have somehow affected women more than men and the persistence of this effect, on average, seems to last up to 120 days (about 4 months) after the lockdown and then dissipating during/after the mild lift of social distancing measures occurred during summer in almost every country.

3.4 Possible mechanisms

So far, my estimates show that during the first acute phase of the pandemic, that obliged governments to put in place strong mobility restrictions, female economists' productivity dropped considerably. This evidence, also, seems to be consistent not only with an extensive literature that investigates gender productivity gaps in academia but also with the literature reporting the negative side-effects implied by the lockdown measures. However, while I have limited information on researchers personal data (such as parental status) that prevent me to directly test the mechanism underlying the observed empirical patterns, I can take advantage of the gradual school re-openings to test the role of childcare demand and of previous and recent researches that shed light on other possible mechanisms underlying gender gaps in productivity that further exacerbated during the pandemic.

Finally, it has to be said that even though my estimates are specific to a particular academic field, there is evidence that negative developments in academic productivity gaps are being observed also for other disciplines, especially those where gender balance was the lowest among all the academic fields (see for instance King and Frederickson, 2020 for STEM disciplines).

Childcare and household chores. Since the imposition of lockdown and shelter-in-place measures as well as the consequent school closures, many parents have been responsible for caring and homeschooling, while they were expected to work simultaneously from home. The economic literature has always documented a gender gap in households' chores, especially borne by women, and during the current health crisis, this gap is expected to considerably wide. Indeed, the extraordinary childcare burden implied by the COVID-19 is consistent with the evidence regarding a lower productivity level of female economists.

Previous evidence shows that while women in academia, all the stages of the professional ladder, are less likely to have children if compared to men (see the National Science Foundation, 2019), academic women do over half of the childcare in their family (men on about a third), with very little variation over different generations (Schiebinger and Gilmartin, 2010). In addition, faculty mothers of young children dedicate less time to research activities compared to men (for the same amount of weekly time they dedicate to paid work) because of care and household chores (Misra, Lundquist, and Templer, 2012) and they report, more often than men, that work and family interfere with each other (Fox, Fonseca, and Bao, 2011).

As a result of the shelter-at-home policies due to the health crisis, more than one-quarter of mothers report a substantial increase in childcare and housework (Carlson, Petts, and Pepin, 2020) and it seems that also the *ivory tower* is not immune from this threat. Indeed, according to recent survey of US and European principal investigators carried out during the pandemic, academics with young dependents reported a 17% reduction in research time compared to colleagues without children, with an additional 5% reduction for female academics (Myers et al., 2020).

To assess whether the drop in female economists' productivity is due to an extraordinary increase in childcare demand, I collected data on worldwide school re-openings from the COVID-19 Impact on Education data managed by Unesco. This dataset, updated every week, tracks the evolution, timing and duration of school closures and the number of affected students since mid-February 2020. For the US, instead, I collected data on state-district-level school re-openings from EducationWeek, an independent organization specialized in providing information on K-12 education[2]. Based on this information, I assign to each author the respective school re-opening date mandate in her country. Symmetrically to what I have done to define the specific lockdown date for each paper (as explained in Section 3.2), I compute the latest school re-opening date among those of each author for each working paper. However, if one paper has among its authors a female economist (or more) I always consider as the latest date that of the female economist whose country last mandated school re-openings. Figures 3.2a and 3.2b report school re-opening dates of authors' country (of affiliation) at the daily and monthly level. Overall, 6 countries have never closed school because of the COVID pandemic[3], whereas at the time of writing 34 countries[4] still have not opened them, meaning that almost half of the countries set up plans or had the epidemic conditions to allow schools to go back to business as usual. School re-openings occurred mainly with the starting of the 2020/2021 school year, that is during August and September when 46 countries allowed for in-person learning. Few countries, instead, opened them by the end of 2019/2020 school year (about 36 countries, 27 percent of the sample). Finally, the last openings occurred during October (9) and November (1). In order to test whether childcare demand contributed to the productivity deterioration observed for female economists, I augment equation (3.1) with a variable that captures the staggered school re-openings and its interaction with the gender variable. The empirical model I estimate reads as follows:

[2]In the US, despite many states mandated school closures in the aftermath of the COVID-19 outbreak, they left school re-openings decisions to local education districts and public health authorities. Each school-district, hence, in accordance with the local health authority decided whether to re-open schools with in-person lessons or to continue with distance-learning (or also to adopt a flexible and hybrid way of learning, that is a mix of in-person and distance learning). Given the high degree of fragmentation within each state, I adopted the following rule: if more than half of the state-specific school-population was allowed to go back in class I consider schools in that state as opened, otherwise not.

[3]These countries are: Australia, Iceland, New Caledonia, Russia, Sweden and Taiwan.

[4]Countries where distance learning is still going on are: Alabama, Arizona, California, Colorado, Delaware, District of Columbia, Georgia, Idaho, Illinois, Kentucky, Maryland, Massachusetts, Michigan, Nevada, New Jersey, North Carolina, Ohio, Oklahoma, Oregon, Pennsylvania, Texas, Virginia, Washington and Wisconsin (within US states); Bangladesh, Bolivia, Costa Rica, Ecuador, Kuwait, Philippines, Saudi Arabia, Venezuela.

$$no.papers_{it} = \alpha + \beta F_i + \theta F_i \times LD_t + \phi SO_{it} + \delta F_i \times SO_{it} + \lambda_t + \eta_{it} \tag{3.2}$$

and I expect, if childcare demand has played a role, that in the aftermath of school re-opening δ to be close to 0 or at least not statistically significant. To be consistent with the previous evidence, I estimate Eq. (3.2) using the same bandwidths as before with Eq. (3.1), but in this case, I am forced to drop the tightest bandwidth (that is ± 30 days) given that during the first 30 days in the aftermath of the lockdown no school was opened yet.

The results of this further analysis are available in Table 3.3. In line with the previous evidence, discussed in Section 3.3, overall production is lower for female and the persistence of the negative effects implied by the lockdown lasts up to 120 days later. During the school re-opening, however, I do observe a strong and statistically significant decline in overall production in line also with the descriptive evidence of Figure 3.7b, whose magnitude declines as the bandwidth size increases. Interestingly, the coefficient associated with the interaction of the school re-opening period and the female dummy is never statistically significant and its magnitude is very close to 0. Therefore, this seems to suggest that indeed childcare demand has played a role in determining productivity deterioration for female economist during the lockdown period.

Teaching duties. A second potential mechanism that may explain the observed reduction in female economists' productivity regards an increase in teaching duties during the pandemic.

Past research, indeed, showed that women have academic positions that generally require higher teaching and service commitments (Misra et al., 2011; American Association of University Professors, 2001). Indeed, women with tenure-track as well as full professor positions report spending more time than their male colleagues in academic services, and this difference remains even after controlling for rank, race and discipline (Guarino and Borden, 2017). Furthermore, female academics report spending more time on teaching, mentoring and service with respect male colleagues (Misra, Lundquist, and Templer, 2012). The COVID-19 crisis and the subsequent university closures urged rearranging academic plans, and this huge administrative effort has been dealt, very often, by the creation of working groups that included faculty members, the majority of them being women.

Furthermore, lockdown measures imposed instructors a quick transition to online teaching. Recent evidence, indeed, suggests that online teaching takes more time, especially when initially creating a class, than in-person teaching (Myers et al., 2020), thus creating greater demand on the time of faculty members with larger teaching activities. Given that these activities seems to fall disproportionately on women, the worsening of the productivity gap in the aftermath of lockdown measures may be due, at least in part, to increased teaching demand.

Psychological and mental well-being. A third potential mechanisms that may contribute to understanding the worsening of the productivity gap among female economists can be attributed to mental well-being deterioration. Indeed, lockdowns and social-distancing measures have limited individuals' possibility to meet and socialize with others. This significant disruption in daily-life and routine may have contributed to a deterioration of people's mental health on top of other negative consequences of the pandemic. Mental health and subjective well-being, moreover, influence and drive several individual choices and behaviors that may end to influence also economic outcomes, such as productivity levels.

Early indicators from COVID-19-specific survey have already shown lower levels of subjective well-being and higher depression and anxiety levels than those observed in the last quarter of 2019. Several studies, exploiting these sources, have shown a significant deterioration of subjective well-being during the lockdown periods establishing, hence, large effects of the pandemic on mental health by showing, also, that these effects are larger for women than for men (see for instance Armbruster and Klotzbücher, 2020; Arpino, Bordone, and Pasqualini, 2020; Banks and Xu, 2020; Brülhart and R., 2020; Tubadji, Boy, and Webber, 2020; Yamamura and Tsutsui, 2020). Hence, as women subjective well-being has decreased the most during the pandemic, it is reasonable to think that increase in depression and anxiety levels may lower individual productivity and then this may be part of the likely channels that explain the widening of the productivity gap.

3.5 The role of tenure

According to my results, there is evidence that the gender production gap has widened during the lockdown period but during the same period for authors living in countries that allowed children to go back in class such decline in production has vanished. However, the previous analysis, at the working paper-level, does not allow me to explore the individual author dimension of the data, that is tenure.

First of all, tenured and untenured scholars may have different incentives in research production given that the requirements for promotion are clear and measurable, usually assessed through research output, but also because skipping even a single year of publications may significantly reduce the likelihood of promotion (Ioannidis, Boyack, and Klavans, 2014), and this is true especially for not-tenured researchers[5]. Secondly, as descriptive evidence suggests, while women in academia are less likely to have children with respect to men, among women this likelihood is also increasing with tenure (National Science Foundation, 2019). Hence, the current disruption implied by the COVID-19 epidemic may have affected gender production differently not only according to the tenure-level but also in light of the likelihood of being mother within tenure-levels.

[5]Nonetheless, Universities are recognizing the strain that the COVID-19 pandemic is having on non-tenured faculty. According to a researchers-made list, about 261 Universities are reevaluating tenure and promotion processes to account for the disruption caused by COVID-19. While it might be true that tenure extension and promotion timelines may have a direct impact on the quality of a given working paper and subsequently on its publication journal, it is very unlikely that it will influence the decision to start circulating it as a working paper, also in light of the time that elapses between the working paper circulation and its publication within the economics field.

To test whether such heterogeneities correlate with the pandemic and school re-openings exposure and female economists' productivity, I estimate Equation (3.2) separately for tenured and not-tenured scholars (as defined in Section 3.2) and using as dependent variable the number of daily authors by gender and tenure. In other words, I perform a sample-split Difference-in-Differences and I expect that the production drop observed at the working paper-level comes from a strong reduction in productivity by tenured female economists, that in principle should represent the most affected group because of the previous arguments. However, it has to be said that I expect also a reduction in the number of daily untenured female authors although with a lower magnitude, given that I do not observe parental status.

The results of these tests are available in Table 3.4, where the first panel is devoted to the estimates relative to the tenured sub-sample, whereas the second to those not-tenured. With regards to the first sub-group, consistent with the descriptive evidence of Figure 3.8 the number of female economists is lower than that of male colleagues. During the lockdown period and up to 180 days after the begin of restrictions, the daily number of women has decreased in the range of -22 and -49 percentage points, relative to men. With school re-opening, even though the average number of authors, independently of gender, has decreased, the interaction term (F*SO) is positive and always highly statistically significant ranging between 7.9 and 9.9, translating in an average increase of about $47-59\%$, depending on the bandwidth size estimated. On the other hand, for what concerns the sub-sample of not-tenured I find similar overall effects although, as I expected, with a smaller magnitude. However, the relevant interaction terms when evaluated at the sample mean suggest that the drop in the number of daily not-tenured female authors has been not alike as that one observed for tenured women, relative to men, of about -28 to -72 p.p. (depending on the bandwidth size) without fully recovering during the school re-openings period ($+29-49\%$) differently for what observed for the sub-sample of tenured women.

In Table 3.5 I repeat the same exercise of Table 3.4 but considering only economist working within academia. In particular, I choose to focus only on academics given that by the nature of their job and the institution they work for they enjoy a greater degree of flexibility if compared to economists working for institutions different from universities. The assumption behind this further test is the following: economists working for research institutions or other kinds of institutions different from academia (*i.e.* central banks) may be subject to more stringent working requirements, time schedules and deadlines with respect academics. Hence, including this sub-group of individuals may result in an attenuation bias in my results. Interestingly, by looking at the coefficients reported in Table 3.5 at first glance they are smaller, in terms of magnitude, than those reported in Table 3.4 not supporting my initial guess. However, despite a lower persistence in the negative effects implied by the lockdown on female production (up to 150 days (180 days in Table 3.4) later for tenured and up to 180 days (210 days in Table 3.4) later for not-tenured scholars), all the interaction terms when evaluated at the sample average reveal that, relative to men, the decrease in the daily number of tenured (untenured) female authors in the aftermath of the lockdown is about 32-57 (35-72) percentage points, whereas during the school re-opening period an increase of 54-62 (30-57) p.p. for tenured (not-tenured).

While for not-tenured female economists, being an academic or not, does not make such a difference in terms of relative decrease (increase) during lockdown (school re-openings) period, for those tenured this distinction seems to matter more given that women in academia are those who lost more ground with the imposition of mobility restrictions but are also those who gained the most with school re-openings, providing support, at least partially, to the arguments at the begin of this Section.

Finally, the evidence presented here seems to be consistent with the dynamics of the gender production gap discussed in Section 3.3: when the production gap has widened so as the number of daily female authors decreased, whereas when the production gap has disappeared so as the daily number of women increased.

3.6 Falsification tests

To show that my estimated effects are not driven by a seasonality effect, I rely on two different tests aiming at alleviating this concern.

As a first exercise, I test whether such a decline in female productivity, as well as the daily number of authors also existed during the first 90 days before the enactment of lockdown measures, a period during which the COVID-19 started to compare almost in every country but no particular actions were taken. In particular, considering only the first 90 days of my sample, I design a placebo lockdown date that split the 90 days period in two symmetric windows of 45 days each. Then, I repeat the same analysis specified in Equation (3.1) for this placebo time window. If my results simply capture seasonality, I should find significant effects after the enactment of the placebo lockdown. Table 3.6 reports the falsification test results. In all cases the placebo estimates are insignificant, implying that women's productivity, as well as their daily numerousness, did not decline significantly during the 90 days before the lockdown began, then suggesting that the seasonality concern seems not motivated.

To further reinforce the above argument, I perform a second test using working paper data uploaded on SSRN between January and November (17^{th}) 2019[6]. In particular, I estimate Eq. (3.2) using the daily number of papers and authors as dependent variables and anticipating the lockdown and school re-openings dates by one year, that is in 2019. Again, if the evidence observed for 2020 is simply the result of a seasonality effect, then I should observe declines in production level and authors numerousness also in 2019. The results of this further tests are available in Tables 3.7, 3.8 and 3.9 that present the estimated (*anticipated*) effects for the production gap and the daily number of authors, respectively. With regards the first most relevant piece of evidence of this paper, that is the production drop observed for female economist, Table 3.7 shows that in 2019 there is no evidence of a widening of the production gap during the lockdown period nor a reduction of this gap during the school re-opening phase

[6] A detailed description of the 2019 data is available in Appendix B.

(all the relevant interaction terms, F*LD and F*SO, are very close to 0 and never statistically significant). This result, hence, provides strong support for the evidence presented in Sections 3.3 and 3.4. Concerning, instead, the evidences for the daily number of authors by gender and tenure, Tables 3.8 and 3.9 (considering only academics) show that, differently for what observed in 2020, the average number of female tenured authors (working or not in academia) increases in the aftermath of the *anticipated* lockdown, whereas for untenured women (working or not in academia) the F*LD coefficient is almost equal to 0. On the other hand, the estimated coefficients associated with the school re-opening period are always positive independently of the sub-sample considered, pointing to, however, the positive effect of schooling on the labour market performance of women, which is a well established result in the economic literature.

Overall, these tests provide strong evidence in support to the robustness of the estimated causal effects of the lockdown on gender production suggesting that these results are very unlikely driven by a seasonality effect.

3.7 Quantity vs. Quality?

Having ascertained the decrease in productivity of female economists during the lockdown period and not after school re-openings, one might wonder whether due to the lockdown researchers had trade-off quantity (in terms of the number of pre-prints) for quality. If this is the case, then I should find an increase in the quality of working papers in the aftermath of the lockdown period.

In order to answer this question, I gathered data on two proxies that are used by SSRN to rank working papers that are the number of abstract views and the number of downloads. I test this possibility estimating Equation (3.2) using as dependent variables the number of views and downloads on individual working paper-level data. While I am fully aware of that the typical metric to evaluate research quality is usually given by journal ranking, at this stage I can only use these proxies bearing in mind, however, all the limitations they are subject to.

The results of these tests are available in Table 3.10. According to the estimates, independently of the bandwidth chosen, none of the treatment effects (F*LD and F*S0) is significant, with the only exception for the F*LD coefficients using the no. of downloads as dependent variable and a bandwidth of 120 and 150 days which are positive and statistically significant. Overall, these results seem to suggest that in the aftermath of the lockdown measures research quality did not change significantly, supporting the idea that my findings are unlikely to be driven by the shifts in research quality at the expense of quantity.

3.8 Conclusions

This paper contributes to investigate the short-run economic (labour market) implications implied by the recent COVID-19 pandemic. In particular, the research question aims at answering whether academic economists' productivity has been negatively impacted by lockdown measures adopted at the international level, following a gender perspective.

Indeed, the current health crisis and measures enacted in an effort to curb down the virus diffusion have caused not only disproportionate declines in economic activity but also worsened further iniquities in home production – given that women contribute, more often than men, in childcare and housework.

According to my estimates, since the lockdown began, the number of working papers written by a female economist, alone or jointly with other researchers, and uploaded on SSRN declined of about 20 percentage points, a persistent negative effect that lasts up to 4 months later the imposition of confinements. Such a decline in female production, however, disappears once schools are allowed to re-open suggesting that indeed childcare demand has played a salient role in determining productivity deterioration for female economists.

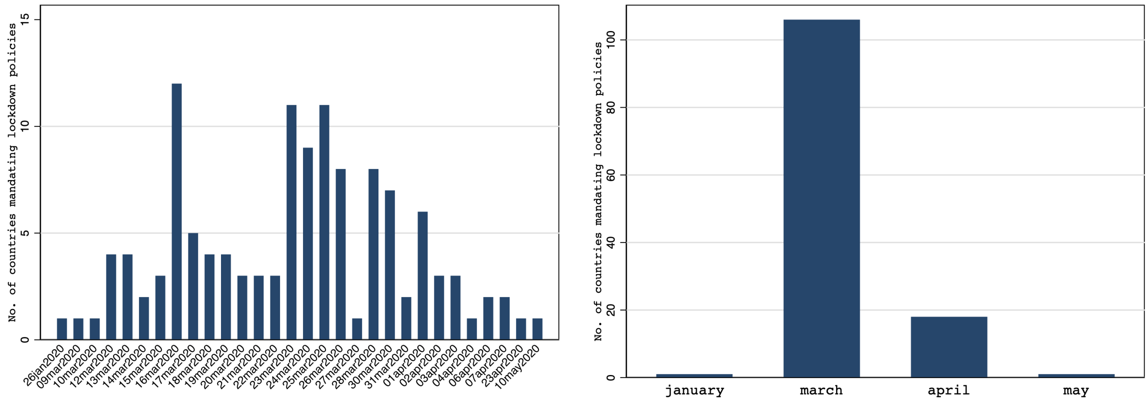
Hence, the evidence yields consistent results with the idea that lockdown policies have increased household chores borne by women that in turn affect negatively their labour market performance. Alternatively, I discuss two additional potential mechanisms, teaching duties and mental well-being, that may further contribute to explain the observed empirical patterns.

By exploring the individual author-level dimension of my data, I show that the daily authors' numerousness followed, more or less, the dynamics of the gender production gap. Furthermore, I show that while tenured and not-tenured women productivity is severely hit during the lockdown, recovering instead during the school re-opening period, tenured economists working within academia are those who lost more ground during the epidemic crisis. Finally, I show that declines in productivity were not associated with increases in pre-prints' quality.

Taken together these results seem to suggest strong short-run negative effects on female economists' productivity, that however seems to vanish in the medium-term, at least according to my data. On the policy side, whether or not these short-run impacts would translate in long-run impacts will depend exclusively on the actions Universities will undertake to mitigate the further unequal consequences this crisis has added on pre-existing inequalities within academia. While on the one hand, many Universities are recognizing the strain this crisis has brought on junior faculties, introducing automatic or opt-in (one-year) contract renewals, they should not ignore these issues while devising career advancement processes for tenured economists to equally mitigating the negative consequences emerged during this crisis. Finally, also Governments should take into account with more emphasis gender inequality issues especially when planning strategies aimed at curbing down the virus diffusion. If, on the one hand, as the record suggests there is not an optimal and unique strategy to effectively tackle such epidemics, on the other hand, the clear policy message that emerges from this study is that lockdown policies should balance the need of lowering the infection spread and the need of not further amplify gender inequalities, at least by lightening and mitigating the negative effects implied by the surge in childcare demand.

Figures and Tables

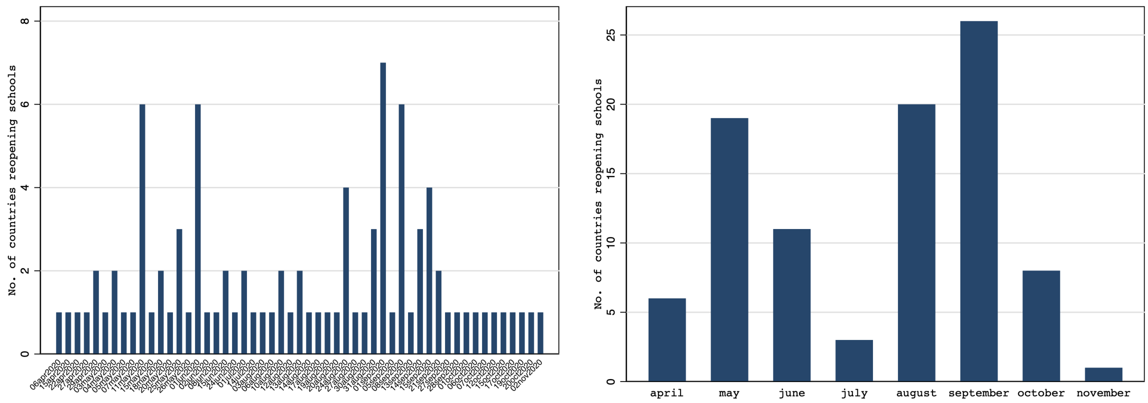
FIGURE 3.1: Lockdown dates



(A) Lockdown dates at daily level (B) Lockdown dates at monthly level

Source: My own calculation based on newspapers and official statements.

FIGURE 3.2: School re-openings dates



(A) School re-openings dates at daily level (B) School re-openings dates at monthly level

Source: My own calculation based on COVID-19 Impact on Education and EducationWeek.

FIGURE 3.3: Number of working papers by month

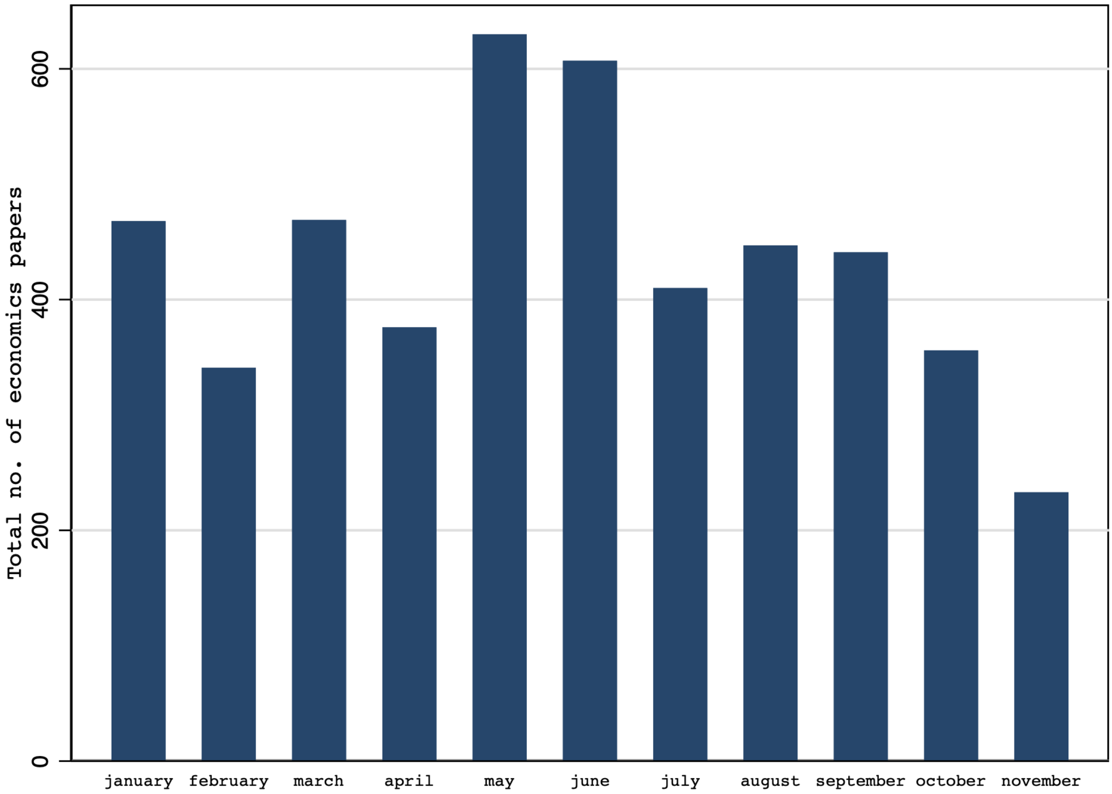
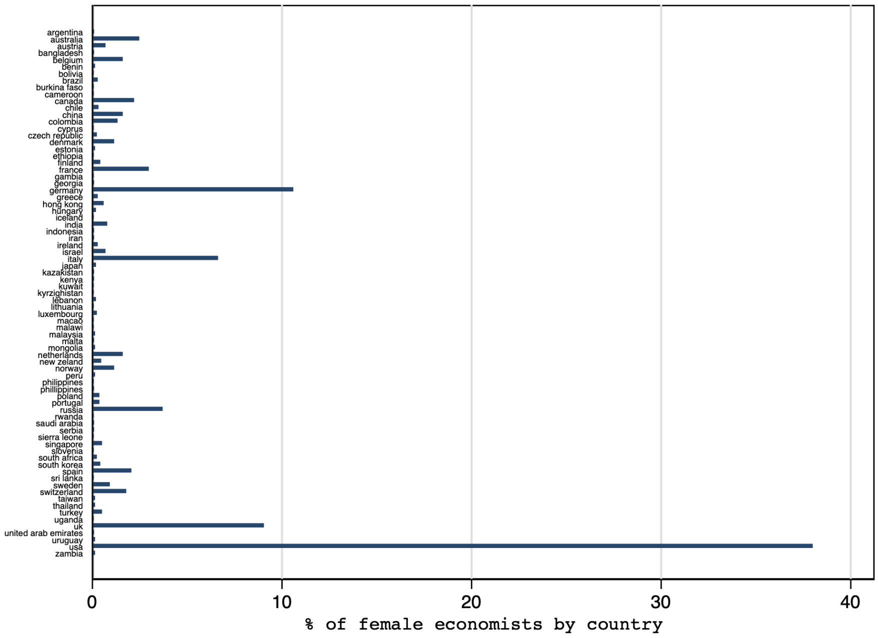


Figure 3.4: Distribution of authors by gender and country of affiliation

(a) Only female authors



(b) Only male authors

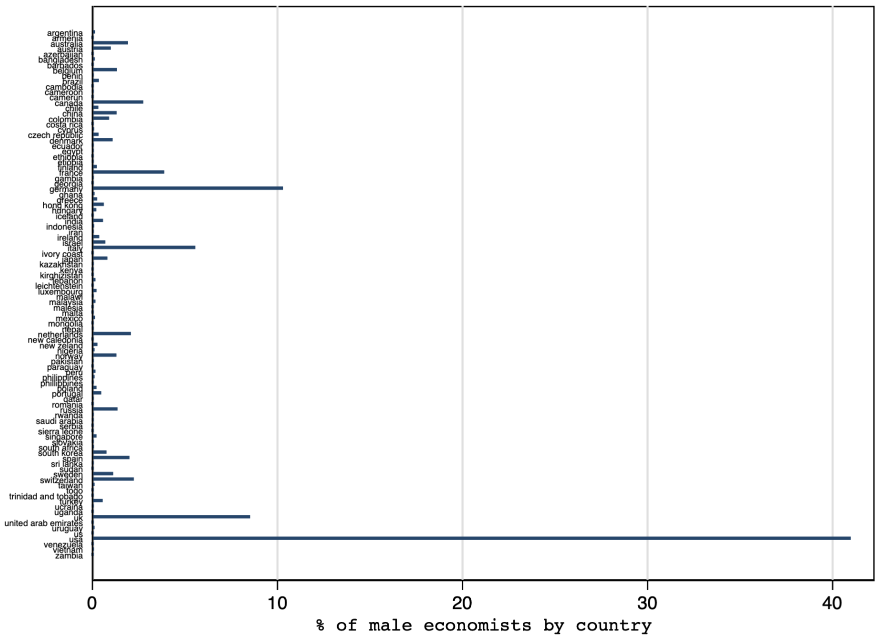
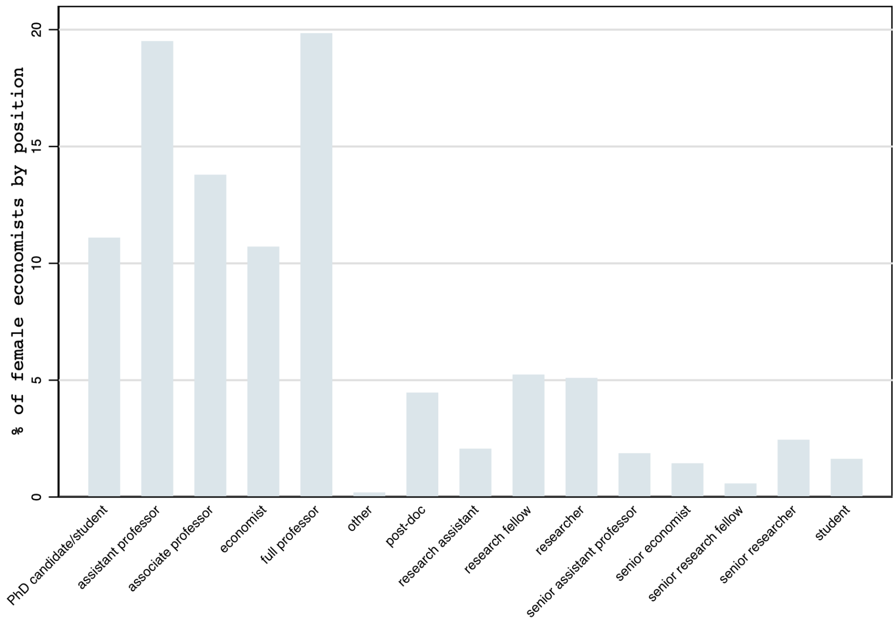


FIGURE 3.5: Distribution of authors by gender and position

(A) Only female authors



(B) Only male authors

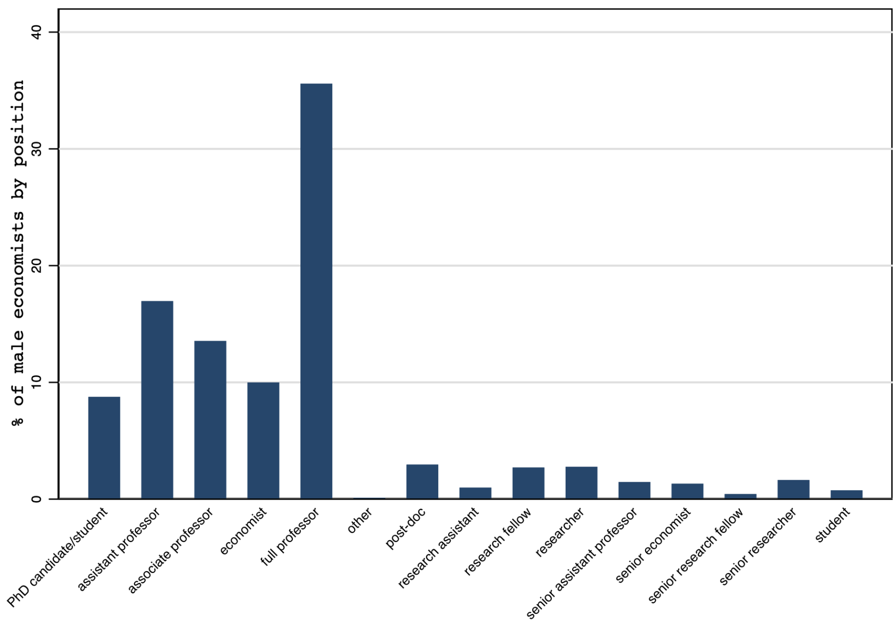
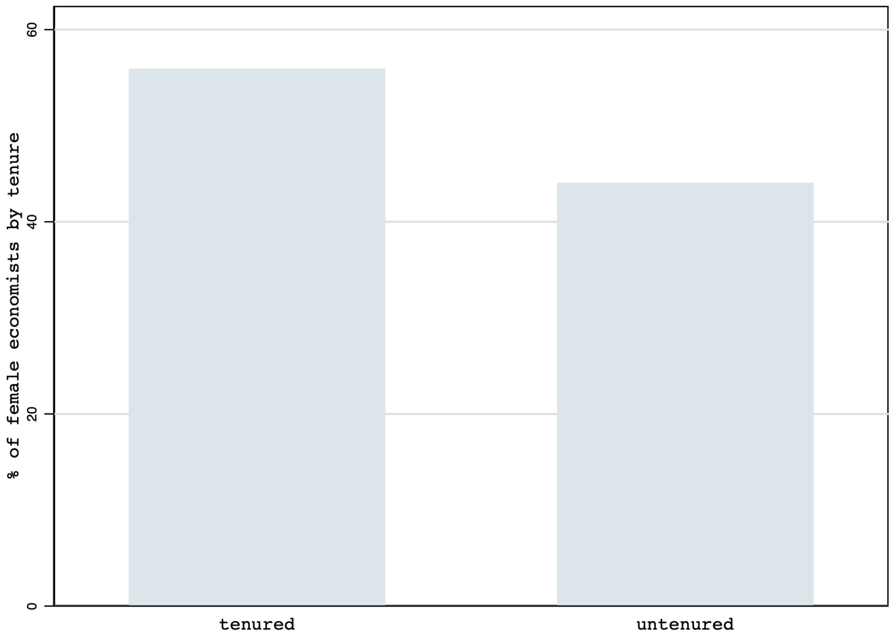


FIGURE 3.6: Distribution of authors by gender and tenure

(A) Only female authors



(B) Only male authors

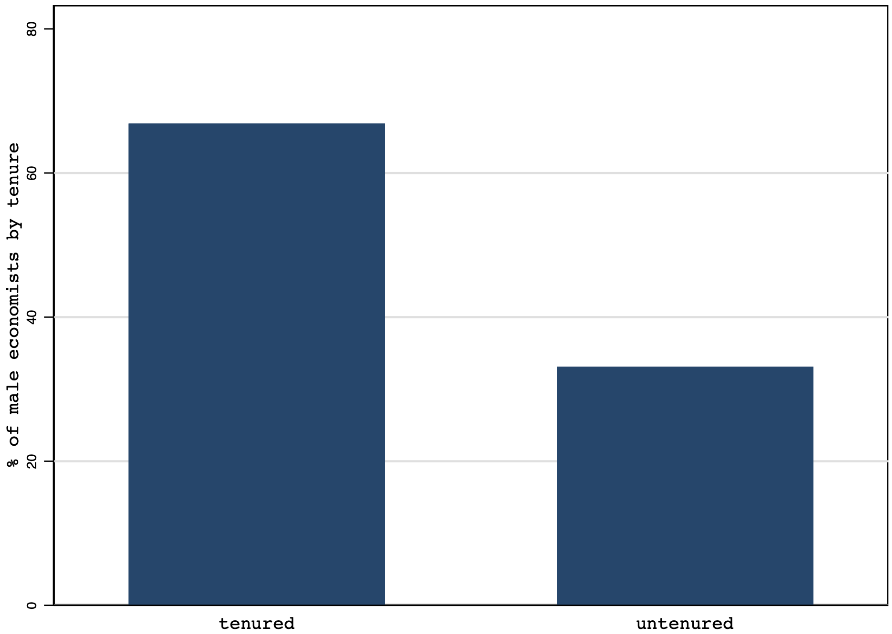
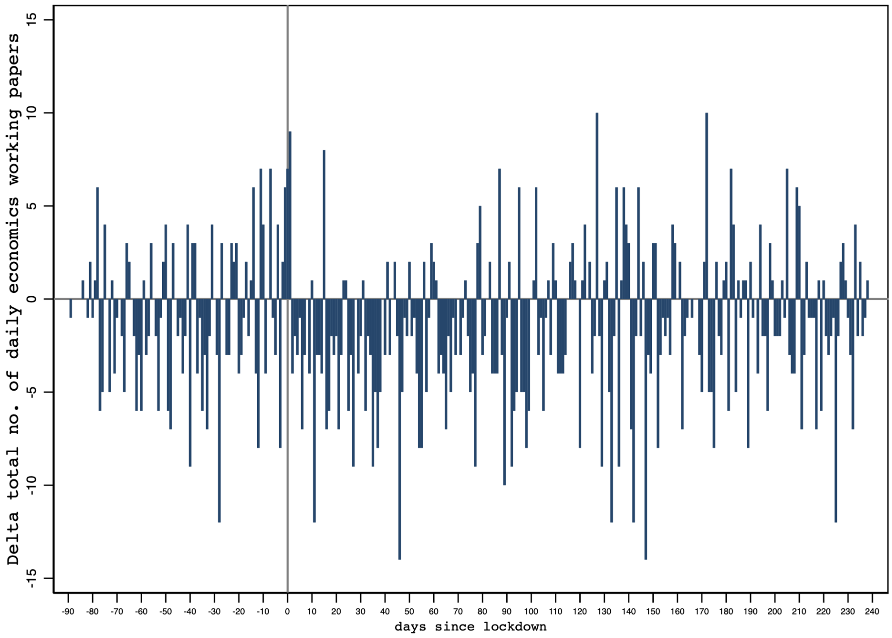


FIGURE 3.7: Aggregate number of papers by gender

(A) Production gap



(B) Average daily production

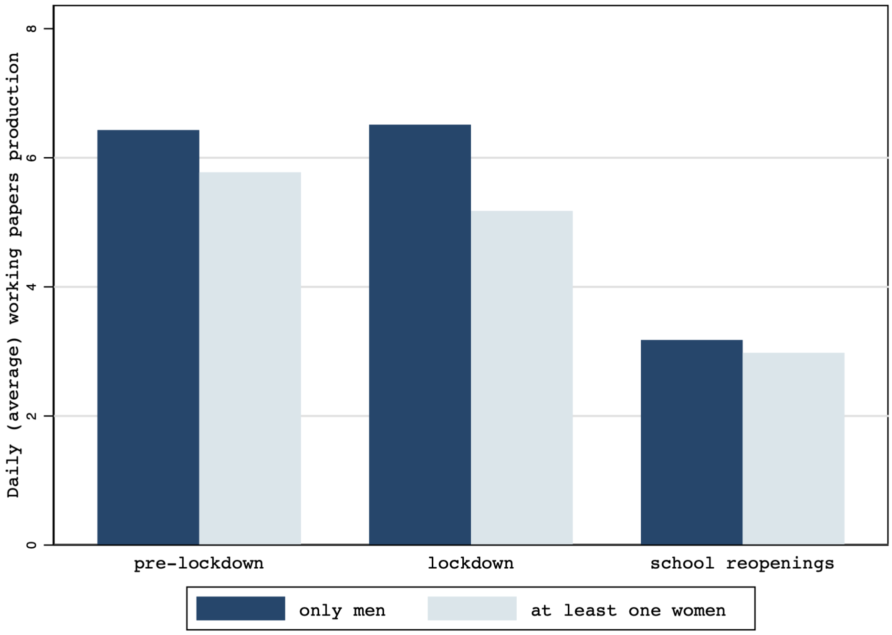


FIGURE 3.8: Average no. of authors by period, gender and tenure

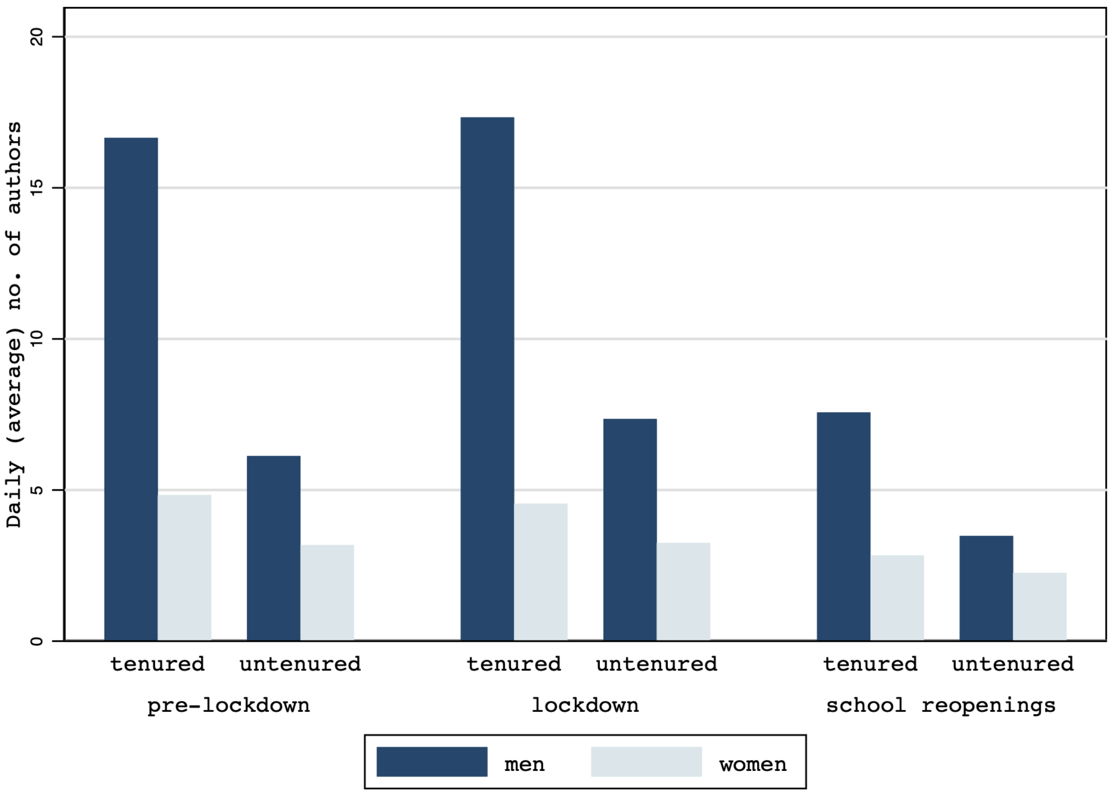


Table 3.1: Economics working papers summary statistics

	Mean	**SD**	**min.**	**Max.**	**Obs.**
All observations:					
Tot. authors	2.738	1.366	1	18	4,778
No. of downloads	19.502	78.726	0	3,615	4,778
No. of views	111.341	458.425	1	28,396	4,752
No. of pages	47.186	23.789	2	564	4,745
Female author	0.442	0.497	0	1	4,778
Total no. of female	0.625	0.852	0	7	4,778
Intensity female	0.22	0.297	0	1	4,778
At least one female author:					
Tot. authors	3.199	1.549	1	18	2,112
No. of downloads	16.143	56.201	0	1,545	2,112
No. of views	103.014	228.24	1	8,121	2,098
No. of pages	48.073	21.5	4	250	2,098
Total no. of female	1.415	0.725	1	7	2112
Intensity female	0.498	0.247	0.071	1	2112
Only male authors:					
Tot. authors	2.374	1.07	1	9	2,666
No. of downloads	22.163	92.691	0	3,615	2,666
No. of views	117.923	578.851	1	28,396	2,654
No. of pages	46.483	25.438	2	564	2,647

Notes: Sample period goes from January to November (17^{th}) 2020.

Table 3.2: Impact of lockdown on gender production

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BW:	±30	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
F	-0.4643	-0.8929	-0.8974^{+}	-0.8974^{*}	-0.8974^{+}	-0.8974^{+}	-0.8974^{+}	-0.8974^{*}
	(0.8674)	(0.5711)	(0.4582)	(0.4501)	(0.4715)	(0.4596)	(0.4570)	(0.4466)
F*LD	-1.8690	-1.5478^{+}	-1.3071^{*}	-1.1291^{+}	-0.9476	-0.7634	-0.5282	-0.4971
	(1.2060)	(0.7973)	(0.6293)	(0.5851)	(0.5868)	(0.5546)	(0.5407)	(0.5204)
Days FE	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	116	230	332	382	440	498	546	592
R^2	0.2349	0.4719	0.4970	0.5082	0.4766	0.4862	0.4835	0.5034

Notes: Difference-in-Differences estimates on aggregate daily economics working papers based on different bandwidths, measured in days, around the enactment of lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.3: Impact of lockdown and school re-openings on gender production

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
F	-0.8929	-0.8974^{+}	-0.8974^{+}	-0.8974^{+}	-0.8974^{+}	-0.8974^{+}	-0.8974^{+}
	(0.6017)	(0.4790)	(0.4712)	(0.4849)	(0.4785)	(0.4811)	(0.4812)
F*LD	-1.5478^{+}	-1.2624^{+}	-1.1290^{+}	-0.9639	-0.7486	-0.4704	-0.4511
	(0.8400)	(0.6573)	(0.6119)	(0.6029)	(0.5767)	(0.5680)	(0.5591)
SO	-9.2372^{***}	-6.4873^{***}	-5.8632^{***}	-5.4167^{***}	-4.4943^{***}	-3.7537^{***}	-3.1366^{***}
	(1.2475)	(0.6534)	(0.5297)	(0.4557)	(0.3956)	(0.3554)	(0.3266)
F*SO	-0.5888	0.0696	0.1271	0.3525	0.3820	0.5345	0.4894
	(1.5732)	(0.8914)	(0.7603)	(0.7026)	(0.6524)	(0.6218)	(0.5986)
Days FE	yes	yes	yes	yes	yes	yes	yes
Obs.	252	402	489	595	697	803	909
R^2	0.4432	0.4769	0.4790	0.4601	0.4367	0.3942	0.3669

Notes: Difference-in-Differences estimates on aggregate daily economics working papers based on different bandwidths, measured in days, around the enactment of lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.4: Impact of lockdown and school re-openings on gender production by tenure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
				Tenured:			
F	-13.4643***	-12.8553***	-12.8553***	-12.8553***	-12.8553***	-12.8553***	-12.8553***
	(1.3518)	(1.1284)	(1.1403)	(1.1652)	(1.1403)	(1.1460)	(1.1547)
F*LD	-5.1392**	-4.7010**	-3.8589**	-3.3123*	-2.3072+	-1.4809	-0.6299
	(1.8951)	(1.5492)	(1.4769)	(1.4433)	(1.3730)	(1.3521)	(1.3407)
SO	-26.9446***	-18.9611***	-17.8135***	-15.9379***	-13.6780***	-11.5667***	-9.7381***
	(2.4337)	(1.4470)	(1.2051)	(1.0429)	(0.9102)	(0.8215)	(0.7610)
F*SO	8.2262*	9.9220***	9.6618***	9.3303***	8.8231***	8.2283***	7.8812***
	(3.9539)	(2.2514)	(1.9527)	(1.7700)	(1.6132)	(1.5238)	(1.4719)
Obs.	248	387	472	574	668	770	869
R^2	0.6238	0.6049	0.5875	0.5672	0.5504	0.5152	0.4814
				Untenured:			
F	-3.5385***	-3.4627***	-3.4627***	-3.4627***	-3.4627***	-3.4627***	-3.4627***
	(0.7257)	(0.6048)	(0.5853)	(0.5764)	(0.5633)	(0.5636)	(0.5593)
F*LD	-3.1282**	-2.5331**	-2.3980**	-2.0367**	-1.5308*	-1.1854+	-0.8951
	(1.0035)	(0.8110)	(0.7480)	(0.7066)	(0.6722)	(0.6607)	(0.6463)
SO	-11.2053***	-7.7262***	-6.9591***	-6.4456***	-5.4061***	-4.6897***	-3.9675***
	(1.4573)	(0.8010)	(0.6236)	(0.5090)	(0.4447)	(0.4030)	(0.3660)
F*SO	3.4633+	2.5661*	2.2724*	2.2065**	2.0342**	2.1325**	2.1043**
	(1.8344)	(1.1204)	(0.9419)	(0.8385)	(0.7677)	(0.7320)	(0.6976)
Obs.	239	363	440	537	627	714	808
R^2	0.4302	0.4307	0.4529	0.4610	0.4353	0.3933	0.3674
Days FE	yes	yes	yes	yes	yes	yes	yes

Notes: Difference-in-Differences estimates on the aggregate daily number of authors by tenure based on different bandwidths, measured in days, around the enactment of lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: $^+ p < 0.10$, $^* p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

TABLE 3.5: Impact of lockdown and school re-openings on gender production by tenure, only academics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
				Tenured:			
F	-11.0577***	-11.1159***	-11.1159***	-11.1159***	-11.1159***	-11.1159***	-11.1159***
	(1.2349)	(1.0417)	(1.0418)	(1.0420)	(1.0327)	(1.0360)	(1.0336)
F*LD	-4.8370**	-3.9748**	-3.2610*	-2.7236*	-1.8506	-1.1733	-0.5515
	(1.7077)	(1.4059)	(1.3348)	(1.2768)	(1.2313)	(1.2129)	(1.1933)
SO	-21.9452***	-15.5738***	-14.8460***	-13.1898***	-11.2039***	-9.5284***	-8.2193***
	(2.2113)	(1.3029)	(1.0740)	(0.9032)	(0.7956)	(0.7150)	(0.6579)
F*SO	7.4712*	8.4323***	8.5301***	8.0742***	7.6896***	7.1690***	6.7433***
	(3.5829)	(2.1524)	(1.8386)	(1.5936)	(1.4634)	(1.3842)	(1.3259)
Obs.	237	364	441	543	635	730	819
R^2	0.5939	0.5735	0.5639	0.5507	0.5223	0.4864	0.4608
				Untenured:			
F	-2.7872***	-2.8167***	-2.8167***	-2.8167***	-2.8167***	-2.8167***	-2.8167***
	(0.6356)	(0.5170)	(0.5046)	(0.4965)	(0.4783)	(0.4775)	(0.4721)
F*LD	-2.3914**	-1.8232**	-1.6260*	-1.2837*	-0.9420+	-0.6592	-0.4086
	(0.8722)	(0.6837)	(0.6384)	(0.6027)	(0.5660)	(0.5561)	(0.5436)
SO	-8.3581***	-5.8054***	-4.8132***	-4.5461***	-3.8981***	-3.3293***	-2.8135***
	(1.2751)	(0.6990)	(0.5488)	(0.4382)	(0.3766)	(0.3404)	(0.3073)
F*SO	3.0761+	2.3025*	1.8481*	1.7402*	1.6482*	1.7420**	1.7088**
	(1.7055)	(0.9790)	(0.8353)	(0.7392)	(0.6640)	(0.6301)	(0.5966)
Obs.	219	334	401	493	575	652	734
R^2	0.3786	0.4108	0.4084	0.4112	0.4006	0.3590	0.3388
Days FE	yes	yes	yes	yes	yes	yes	yes

Notes: Difference-in-Differences estimates on the aggregate number of authors by tenure, considering only economist with typical academic job positions, based on different bandwidths, measured in days, around the enactment of lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 3.6: Falsification tests

	No. papers	No. authors			
		All economist		Only academics	
		Tenured	Untenured	Tenured	Untenured
	(1)	(2)	(3)	(4)	(5)
F	-0.9444	-12.2059***	-3.5926***	-11.0645***	-3.0000***
	(0.6609)	(1.5067)	(0.6926)	(1.2912)	(0.6832)
F*PL	0.0873	-1.1751	0.2176	-0.0934	0.3056
	(0.9006)	(2.0268)	(0.8964)	(1.7399)	(0.8819)
Placebo days FE	yes	yes	yes	yes	yes
Obs.	156	152	134	138	120
R^2	0.3912	0.5968	0.4648	0.5886	0.3891

Notes: Difference-in-Differences estimates based on a bandwidth of ±45 days centered around the enactment of placebo lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: + $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.7: Falsification test: Eq. (3.2) using 2019 data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
F	-0.6724	-0.7632^{+}	-0.7632^{+}	-0.7632^{+}	-0.7632^{+}	-0.7632^{+}	-0.7632^{+}
	(0.5010)	(0.4274)	(0.4062)	(0.4153)	(0.3993)	(0.4550)	(0.4431)
F*LD	-0.3276	-0.1192	-0.0243	0.0482	0.0634	0.1610	0.1596
	(0.7181)	(0.5882)	(0.5250)	(0.5138)	(0.4799)	(0.5351)	(0.5119)
SO	-3.4788**	-3.7739***	-3.5323***	-2.6631***	-2.2311***	-1.5840***	-1.2626***
	(1.1811)	(0.6379)	(0.4870)	(0.4112)	(0.3511)	(0.3541)	(0.3126)
F*SO	-0.5181	-0.4138	0.2692	-0.1100	-0.0563	-0.3510	-0.2132
	(1.5267)	(0.8897)	(0.7046)	(0.6363)	(0.5680)	(0.6064)	(0.5624)
Days FE	yes	yes	yes	yes	yes	yes	yes
Obs.	242	373	463	564	660	764	876
R^2	0.7580	0.7054	0.6969	0.6462	0.6381	0.5493	0.5333

Notes: Difference-in-Differences estimates on aggregate daily economics working papers based on different bandwidths, measured in days, around the enactment of a placebo lockdown (day 0) in 2019. Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.8: Falsification test: *anticipated* lockdown and school re-openings on gender production by tenure in 2019

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
				Tenured:			
F	-14.0175***	-12.3699***	-12.3699***	-12.3699***	-12.3699***	-12.3699***	-12.3699***
	(1.6681)	(1.2907)	(1.2006)	(1.1578)	(1.1010)	(1.1373)	(1.0874)
F*LD	4.1119^{+}	2.0973	2.6486^{+}	2.7334^{+}	3.3494*	3.5571**	3.7506**
	(2.4032)	(1.7768)	(1.5532)	(1.4341)	(1.3244)	(1.3396)	(1.2586)
SO	-10.5639**	-11.2136***	-10.3680***	-8.6527***	-7.8347***	-6.6142***	-5.7173***
	(3.7370)	(1.8481)	(1.3497)	(1.0746)	(0.9096)	(0.8435)	(0.7306)
F*SO	7.5916	7.3415*	8.6500***	7.5771***	8.4715***	7.4962***	7.4816***
	(5.9238)	(2.8655)	(2.2115)	(1.8513)	(1.5946)	(1.5348)	(1.3940)
Obs.	233	356	441	537	631	729	835
R^2	0.5145	0.5327	0.5433	0.5302	0.5285	0.4933	0.4934
				Untenured:			
F	-3.2041***	-2.8387***	-2.8387***	-2.8387***	-2.8387***	-2.8387***	-2.8387***
	(0.5746)	(0.4799)	(0.4584)	(0.4527)	(0.4334)	(0.4853)	(0.4883)
F*LD	0.7651	0.2207	0.1989	0.3062	0.3771	0.5150	0.6741
	(0.8203)	(0.6580)	(0.5954)	(0.5614)	(0.5222)	(0.5709)	(0.5638)
SO	-3.0459^{+}	-3.9664***	-3.2316***	-2.9651***	-2.6681***	-2.0800***	-1.4164***
	(1.7360)	(0.7555)	(0.5771)	(0.4727)	(0.3832)	(0.3769)	(0.3402)
F*SO	1.5144	1.6968	1.7082*	1.7798*	1.9606**	1.8305**	1.4707*
	(2.2084)	(1.1248)	(0.8629)	(0.7343)	(0.6422)	(0.6714)	(0.6354)
Obs.	202	303	368	450	530	619	715
R^2	0.7091	0.6685	0.6565	0.6260	0.6195	0.5241	0.4851
Days FE	yes	yes	yes	yes	yes	yes	yes

Notes: Difference-in-Differences estimates on the aggregate daily number of authors by tenure based on different bandwidths, measured in days, around the enactment of a placebo lockdown (day 0) in 2019. Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 3.9: Falsification test: *anticipated* lockdown and school re-openings on gender production by tenure in 2019, only academics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
				Tenured:			
F	-13.3462***	-11.4478***	-11.4478***	-11.4478***	-11.4478***	-11.4478***	-11.4478***
	(1.5895)	(1.2359)	(1.1532)	(1.1068)	(1.0537)	(1.0755)	(1.0316)
F*LD	5.4890*	3.0052+	3.5922*	3.5556**	4.0055**	4.1753**	4.2921***
	(2.2821)	(1.6925)	(1.4858)	(1.3661)	(1.2659)	(1.2670)	(1.1939)
SO	-8.0397*	-9.2466***	-8.3408***	-6.7932***	-6.2408***	-5.1511***	-4.4447***
	(3.4086)	(1.7245)	(1.2571)	(0.9994)	(0.8506)	(0.7772)	(0.6734)
F*SO	7.4202	7.2145**	7.4827***	6.7210***	7.6111***	6.8548***	6.9297***
	(5.4120)	(2.7345)	(2.2065)	(1.8469)	(1.5883)	(1.5002)	(1.3560)
Obs.	215	332	410	498	579	666	766
R^2	0.4805	0.4834	0.4904	0.4781	0.4790	0.4467	0.4438
				Untenured:			
F	-2.6585***	-2.4038***	-2.4038***	-2.4038***	-2.4038***	-2.4038***	-2.4038***
	(0.6077)	(0.4866)	(0.4610)	(0.4497)	(0.4309)	(0.4463)	(0.4364)
F*LD	0.7547	0.3228	0.2991	0.3932	0.4376	0.5843	0.6641
	(0.8638)	(0.6624)	(0.5984)	(0.5565)	(0.5179)	(0.5233)	(0.5030)
SO	-3.2011+	-2.8491***	-2.6068***	-2.3743***	-2.1180***	-1.5810***	-1.1985***
	(1.6959)	(0.7488)	(0.5642)	(0.4503)	(0.3787)	(0.3493)	(0.3048)
F*SO	0.1670	1.3259	1.5932+	1.5634*	1.7143*	1.5920*	1.3287*
	(2.5361)	(1.2324)	(0.9251)	(0.7494)	(0.6631)	(0.6339)	(0.5758)
Obs.	170	259	311	384	451	528	611
R^2	0.6417	0.6188	0.6140	0.5860	0.5776	0.5244	0.5061
Days FE	yes	yes	yes	yes	yes	yes	yes

Notes: Difference-in-Differences estimates on the aggregate number of authors by tenure, considering only economist with typical academic job positions, based on different bandwidths, measured in days, around the enactment of a placebo lockdown (day 0) in 2019. Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.10: Quantity vs. Quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BW:	±60	±90	-90,+120	-90,+150	-90,+180	-90,+210	-90,+240
				No. of abstract views			
F	-20.3565*	-16.8934*	-16.8934	-16.8934	-16.8934	-16.8934	-16.8934
	(10.3412)	(8.2433)	(11.7902)	(11.8163)	(34.0277)	(32.4696)	(31.2512)
F*LD	18.9826	13.4288	13.7892	11.8353	-3.4149	-4.8512	-4.8601
	(13.8169)	(10.8113)	(14.8635)	(14.3658)	(40.6140)	(38.2606)	(36.5031)
SO	13.3143	7.4306	-20.0042	-17.3695	-54.2111	-42.0975	-37.8504
	(30.1371)	(15.1074)	(17.1660)	(13.9815)	(35.6794)	(30.1949)	(26.6631)
F*SO	10.2765	11.0193	28.6878	13.4856	-1.7638	-8.5203	-1.3294
	(43.0945)	(21.9449)	(25.8868)	(21.6636)	(56.5204)	(49.1835)	(44.4585)
Obs.	1735	2487	2883	3415	3806	4216	4580
R^2	0.0397	0.0651	0.0503	0.0410	0.0183	0.0182	0.0180
				No. of downloads			
F	-17.7607**	-14.3489**	-14.3489***	-14.3489***	-14.3489*	-14.3489**	-14.3489**
	(6.0210)	(4.4886)	(4.3221)	(4.0954)	(5.7775)	(5.5434)	(5.3329)
F*LD	9.0604	7.7095	9.6104+	10.0929*	9.4804	9.2851	9.3089
	(8.0467)	(5.8879)	(5.4482)	(4.9795)	(6.8962)	(6.5327)	(6.2296)
SO	-8.2468	-4.1042	-4.2123	-3.9210	-9.1922	-7.7112	-6.8707
	(17.5984)	(8.2437)	(6.3024)	(4.8551)	(6.0669)	(5.1636)	(4.5557)
F*SO	19.8543	12.7012	13.6724	11.8854	10.2290	9.3387	10.4645
	(25.1618)	(11.9759)	(9.4922)	(7.5150)	(9.5999)	(8.3939)	(7.5866)
Obs.	1,744	2,499	2,899	3,431	3,824	4,236	4,601
R^2	0.0275	0.0391	0.0392	0.0406	0.0247	0.0249	0.0257
Days FE	yes	yes	yes	yes	yes	yes	yes

Notes: Difference-in-Differences estimates on individual daily economics working papers based on different bandwidths, measured in days, around the enactment of lockdown policies (day 0). Robust standard errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 4

Investment Subsidies Effectiveness: Evidence from a Regional Program

4.1 Introduction

A long-standing result in industrial organization is the sub-optimality of firms' *R&D* expenditures and innovation activities with respect the socially desirable outcome. Under the neoclassical theory of innovation, the main rationale behind the need for public policies to support private *R&D* and innovation is a market failure argument (Arrow, 1962b; Schumpeter, 1934). Firms cannot completely internalize the benefits of *R&D* investments because knowledge has the characteristics of a public good: it is non-excludable and non-rivalrous (Arrow, 1962b; Stiglitz, 1988). The presence of positive externalities means that the social return on innovation spending is greater than the return gained by firms, so the level of private *R&D* expenditure tends to be lower than the optimal social level. A negative effect is also associated with the imperfections of capital markets (Griliches, 1997; Hall, 1992; Guiso, 1998) and information asymmetries (Hall, 1993; Bond and Reenen, 2007; Hall and Lerner, 2010) leading private firms to discard or defer socially valuable *R&D* projects[1]. In these circumstances, economic

[1]These asymmetries may be further amplified if firms are reluctant to reveal their innovative ideas to intermediaries because of the substantial costs they would bear in case of leaks of knowledge to competitors. This reluctance coupled with the leaks threat also imply a reduction in the quality of the signal a firm can send to the marketplace about a potential project. Therefore, the marketplace for financing the development of innovative ideas looks like the "lemons" market modelled by Akerlof, 1970.

theory calls for government intervention through fiscal incentives (*i.e.* subsidies, tax credits) to compensate for the gap between private and social returns to research and development so as to guarantee the social optimal supply of innovation efforts by the private sector (Wallsten, 2000; Almus and Czarnitzki, 2003).

Government policy measures (most notably) in EU Member States (MS) and OECD countries have always tried to induce the private sector (*i.e.* firms) to increase research and development activities by providing them large sum of public funds. During the 2000 to 2013 period, for example, government financial support instruments to promote *R&D* have accounted for nearly 70% of all *R&D* costs performed in OECD countries (Appelt et al., 2016). In Europe, 25 MS are using *R&D* incentives in an effort to boost innovation investment, increase productivity, economic growth, consumers and businesses welfare with an average of more than 12 cents for every euro invested in *R&D* coming from public incentives provided by MS (Commission, 2017).

Governments, hence, have a prominent role in trying to stimulate private sector innovative activities by reducing innovation's marginal costs and inducing firms to increase *R&D* spending. Also economists and policy makers agree on the sustained need to stimulate innovation. However, there is still an open debate concerning to what extent public incentives are able to induce additional spending in *R&D* from private firms, and likely it will be renewed, especially in Europe, with the implementation of the NextGeneration EU investment projects. In this paper, I provide novel empirical evidence aimed at answering the following questions: "Do public incentives induce firms in increasing their spending in innovative activities? Does public funding complement or displace private innovation expenditures?"

Therefore, the main purpose of this paper is to assess the effectiveness, in terms of input-additionality, of public funding for innovation activities in enhancing firms' investment expenditures by verifying empirically if public funds substitute or complement private investment in innovation. In order to do so, I exploit an investment subsidy program taken place in southern Italy in 2013 targeted to small and medium enterprises (SMEs) operating in the region Campania. In particular, firms were invited to submit proposals for new innovative projects and only those that scored above a certain threshold received a subsidy, covering up to the 50% of the spending in innovative intangible and tangible assets according to the selection criteria set by the region.

Indeed, this regional program qualifies itself as an interesting case study for at least three reasons. First, the program rules require that firms that are willing to be financed in developing innovative activities must operate (and be located) within the region boundaries. Hence, the policy's local dimension allows me to remove much of the unobserved heterogeneity among enterprises that, instead, characterizes nationwide programs by comparing a sample of more homogeneous firms (subsidy-recipients and non-subsidy-recipients) based and operating in the same region and thus exposed (reasonably) to the same set of business rules and local shocks. Then, an additional program requirement obliged participating firms to request funding only for brand new investment projects and to develop them only with regional support (subsidy), that is the program rules forbidden firms in combining several public incentives. In this way, I am more confident of estimating a clean causal effect (if any) that comes only from the effect of regional subsidies on the level of innovative investment. Finally, the regional government pledged a sizeable amount of funds to foster private firms' investments. Indeed, about €22 millions of public resources have been distributed to firms in order to induce them to increase innovative expenditures.

In order to recover the causal effect of interest, I exploit detailed data on the regional program matched with balance sheet data from the AIDA database (managed by Bureau Van Dijk) for treatment and control groups and I employ a Difference-in-Differences approach by comparing the average expenditures of public incentive *recipients* and *non-recipients* firms, identified by taking advantage of the assignment scheme of the subsidy, before and after the program implementation.

Overall, the Difference-in-Differences estimates over the period 2010-2015, suggests that no crowding-out, but neither crowding-in has occurred. Indeed, the *average treatment effect of the treated* (ATT) amounts to about 0.025 and it is statistically different from zero in all the specifications considered, with and without the inclusion of control variables. Because of the subsidy, treated firms spent 33% more in innovative investment projects with respect to their level of spending in 2013. Furthermore, by relying on a back-of-the-envelope calculation, I find that the *R&D* cost elasticity amounts to about 0.8 - like those documented in the literature. In addition, I show large heterogeneity in the firms response. By investigating to what extent firms' size may have played a role in explaining the effect of the subsidy on innovative expenditures, I find that only micro- and small-sized firms exhibit a positive response (average increases of about 47% and 30% and implied elasticities ranging between 0.9 and 1.2 and about 0.7), consistent with the main findings in the literature. Then, I investigated the response to the subsidy by looking at the sectorial technological intensity (high and low tech sectors) showing that only for firms belonging to high technology intensity sectors the public incentive increased significantly spending.

Moreover, I disentangled the role of the sector technology intensity in shaping the effect of the public incentive by distinguishing between two macro-sectors: the *traditional* one, that is the manufacturing sector and the service sector - thus, improving the existing empirical evidence that has mainly focused on the manufacturing sector only. The estimated coefficients point out that the increase in innovative expenditures seems to be exhibited by high tech firms belonging to the service sector (with an estimated causal effect of about 0.03 and an *R&D* cost elasticity above 1), while there is evidence of no effect at all for low tech service firms and the whole manufacturing sector. Despite the large heterogeneity in firms response, the increase of investment because of the subsidy, in the aftermath of the program, is not compatible with the input-additionality hypothesis. In fact, I cannot rule out that spending was (lower) equal than the size of the public funding received. Finally, I document that the program had spillover effects on firms' labour demand but not improvements in productivity.

This paper has close ties to three main strands of the literature. The first, and most related one, regards the empirical micro-evidence on the input-additionality hypothesis, according to which public incentives should act as a source of complimentary funding in fostering private innovation efforts by reducing the (marginal) cost of capital and increasing the expected investment profitability.

According to theory, public programs in maximizing their effectiveness, in the context of imperfect capital markets and information asymmetries[2], should target and finance projects that without the grant (subsidy) would not be undertaken, that is *marginal* projects. Indeed, a public subsidy has the effect of shifting the marginal cost schedule by decreasing the cost of funds. This reduction should allow firms to put in place "marginal" projects with the effect of raising the equilibrium level of investment (see Figure 4.1). In this case crowding-in occurs. If, instead, *infra-marginal* projects are targeted by the subsidy the resulting investment equilibrium level remains unaffected since these projects were already worth to carry on. As a result, firms will substitute completely private for public funding (crowding-out hypothesis) to take advantage of the lower cost of capital (Wallsten, 2000).

The empirical micro-evidence on the effectiveness of public programs on innovation activities is not conclusive. The results are mixed and vary with the context (time period, country, industry; Klette, Møen, and Griliches, 2000), empirical approach (Cerulli, 2010), outcome variables and level of government responsible for the policy program (David, Hall, and Toole, 2000; Zúñiga-Vicente et al., 2014). The examination of the main findings corroborates the existence of a great diversity of results: many studies conclude that public *R&D* subsidies tend to stimulate additional company-financed *R&D* ("additionality" or "crowding-in" hypothesis; Busom, 2000; Almus and Czarnitzki, 2003; Bronzini and Blasio, 2006; González and Pazó, 2008; Czarnitzki and Lopes-Bento, 2013). Other studies, on the contrary, find evidence public *R&D* subsidies offset private *R&D* ("crowding-out" hypothesis; Wallsten, 2000; Bronzini et al., 2008; Bronzini and Iachini, 2014) or that they do not crowd-out but neither crowd-in privately financed *R&D* (Klette, Møen, and Griliches, 2000; Bronzini and Iachini,

[2]In a simple static partial equilibrium model with perfect capital markets the supply of capital is perfectly elastic so that internal (private capital for innovation efforts) and external (*i.e* pubic subsidies) funds are perfectly substitute and public incentives are not suitable in order to increase private efforts in *R&D* spending.

2014).

Studies have found that public subsidies have a positive impact also on firms' sales and employment (Lerner, 2000). Most of the empirical studies, furthermore, argue that the crowding-in mechanism crucially depends on the size of the firm, the volume of the public support (Görg and Strobl, 2007) and on the technological intensity of the sectors in which firms operate. The cases where public incentives tend to exhibit more input-additionality are relative to small firms (Lach, 2002; González and Pazó, 2008) operating in relatively low technological sectors (mainly the manufacturing sector; González and Pazó, 2008; Becker and Hall, 2013; see also Acconcia and Cantabene, 2018 that show also a prominent role for liquidity) and firms located in less advantaged regions (Bronzini and Blasio, 2006).

The second strand of the literature, instead, concerns with the (growing) empirical evidence on the relationship between *R&D* and its price. On the theoretical ground, the price for *R&D* investment is given by the implicit rental rate, or user cost, after taxes. By reducing the price of an input (*R&D* for instance), tax incentives allow to estimate the elasticity of such input to its price[3]. Several studies provide evidence on the positive effects of tax incentives and subsidies on the *R&D* cost elasticity estimated around 1 (Acconcia and Cantabene, 2018; Guceri and Liu, 2019; Agrawal, Rosell, and Simcoe, 2020), persistent over time (Cummins et al., 1994; Bloom, Griffith, and Van Reenen, 2002), in some instances heterogeneous and linked to the business cycle (Parisi and Sembenelli, 2003). However, there are also studies showing the negligible role of tax incentives on the cost elasticity of innovation efforts (Wilson, 2009). In general, these estimates are broadly consistent with the conclusion in Hall and Van Reenen, 2000 that asserted: "*A tax price elasticity of around unity is still a good ballpark figure, although there is a good deal of variation around this from different studies as one would*

[3] The literature on *R&D* tax incentives identified two broad empirical strategies in order to obtain a measure able to quantify the economic magnitude of the response of the level of investment in innovation to shifts in tax rates. One approach is to estimate a reduced form *R&D* demand equation that includes a shift parameter to measure the impact of changes in the *R&D* tax credit. A second approach is to regress *R&D* spending on the after-tax cost (*i.e.*, user cost) of *R&D* to obtain a scale-free estimate of the cost elasticity of *R&D* spending.

expect".

Finally, the last strand of the literature at which this paper is related to regards the management and allocation of public resources. Indeed, this study permits to shed light on the effects of place-based policies managed by local governments, that, however, have always had scant attention from the impact evaluation literature, despite the prominent role of local governments in shaping the local economic conditions and the relatively great bulk of public resources that the private sector absorbs from the public sector (Kline, 2010).

The remainder of the paper is structured as follows. In section 4.2, I illustrate the features and characteristics of the investment subsidy program, by also describing qualitatively the technological nature of the subsidized projects. Section 4.3 describes the data and the empirical strategy used to recover the causal effect of interest. In section 4.4, the main results are discussed. In Section 4.5, the estimates relative to the heterogeneity analysis are presented. Section 4.6 presents some robustness checks. In Section 4.7, I explore whether the program had spillover effects on other firms' outcomes. Finally, Section 4.8 concludes.

4.2 The Regional Program

In 2009, the Government of Campania published the "Regional SMEs Support Program *de Minimis* for Organizational, Process and Products Innovation" (ex Reg. (CE) No. 1998/2006), a regional program, with an endowment of more than 20 millions of Euro, intended to sustain through public monetary support, in the form of direct subsidies, private brand new innovative investment developed by requesting small and medium firms with particular regards to those connected with information technology.

The priority was to favour the implementation of innovative investment programs, through the use of new technologies (ICT in particular), by improving the competitiveness of the local business fabric and increasing the productivity of the same firms, also from a management and product innovation point of view.

Specifically, firms were invited to submit proposals for new projects and the regional government subsidized the innovative investment expenditures of eligible firms through a direct grant (subsidy). According to the program, the grant may cover up to 50% of the costs for intangible and tangible innovative assets and 10% for expenses connected to the projects development. In any case, the maximum grant per project cannot exceed the sum of 200 thousands of Euro, so to not break the European State Aid Legislation. Eligible firms, including temporary associations or consortia, were those firms that had an operative main office located in the region and mostly important intended to implement the project within the regional boundaries.

The subsidy covered the following investment outlays: (a) research and development expenditures, (b) start-up and expansion costs, (c) patents, (d) licenses *et similar* rights, (e) plant and machineries and (f) industrial and commercial equipments.

Even though the program's call ended in 2009, its implementation took place only in 2013 and eligible firms were to put in place investments during a two years window (2014-2015), while subsidies were materially transferred to eligible firms throughout 2014 (see Figure 4.2).

One important characteristic of the program was that firms could not receive other types of public subsidies for the same project. This helps the evaluating process given that the impact of the regional program cannot be confused with that of other public subsidies. In addition, all the projects must be brand new since no eligibility was granted for projects that involved the completion of investments begun prior the submission of the proposal to the region.

The grants were assigned after a process of projects' assessment carried out by a Technical Commission appointed by the Regional Government. The commission examined the projects and assigned a score for each of the following criteria: (a) Project Quality and Innovation (*max* 60 pts), (b) Competitiveness and Impact on

Product/Service (*max* 30 pts) and (c) Youth and Female SMEs (*max* 10 pts)[4]. Only innovative projects that obtained a total score equal or greater than 60*pts* received the grant (*max score* 100 pts). For the evaluation process, the Technical Commission must comply with the general principles for the research evaluation specified by the Ministry of Education, University and Research of the Italian Government and the general principles of the European Commission[5].

At the end of 2009, when the program application deadline expired, 2,174 firms requested to have access to the public grant. However, in 2013 when the program results became available of these 2,174 enterprises only 396 became eligible, whereas 424 non-recipient and about 1,354 were excluded from the program[6]. Overall, the region has granted to eligible firms about €22 millions, meaning that it has committed to finance, on average, about 43% of total spending in innovative investments. This in turn translates in about €79,900 received by the average treated firm (see Figure C.2 for the average subsidy awarded by score and Table 4.2) and it represented roughly half, but slightly lower, of the total expenditures made by the average SME during the two years after the program implementation. Indeed, by comparing the average level of investment for *recipient*-firms in 2015 with that of 2013, the difference, possibly induced by the program, is about €167 thousands roughly a bit more than double of the average value of the public measure granted to treated firms. In addition, the subsidy value amounted for the 8 percent of total assets in 2013 (see Table 4.2), while during the same year the share of innovative investment over Total Assets was about, on average, 6%.

As I have already discussed in the previous lines and showed in Figure 4.2, the program was published on the Regional Journal (Bollettino Ufficiale della Regione

[4]For a detailed overview of the criteria set out by the Region for the eligibility status consult Section C.1 of the Appendix.

[5]For a detailed overview of the investment program criteria see section C.1 of the Appendix.

[6]The exclusion of firms from having access to the public support measure regarded the non-compliance of them with all the documentation requested by the region to be attached to the submission files.

Campania, BURC[7]) in April 2009, but its real enactment only occurred during 2014-2015, after the publication of the final raking list of winners by the regional government in late 2013.

Given the 5 years time span between program publication and implementation, one may argue that projects that could have been considered as *innovative* in 2009, in 2014 they may result out-dated since technological advancements and improvements have occurred (given, also, the dramatic shortening of the technology life-cycle). So, from my point of view, apart from the program evaluation exercise to assess the additionality effect of the subsidy on the level of innovative investment, it appears very useful to discuss the technological nature of the projects conducted by *recipient*-firms. In order to accomplish this task, I compare (at random) some of the firms' projects that obtained the lowest possible score to be declared eligible in obtaining the public support ($score: 60-61$) with that of firms that, instead, scored the highest ($score > 71$).

According to the official documents and projects released by the Campania region (available only for *recipient*-firms), low-scoring firms presented innovative projects involving the adoption of *Enterprise Resource Planning* (ERP) systems along with the creation of websites to manage service provision with clients and *e-commerce* platforms[8]. Nowadays, ERP systems are widely diffused among enterprises, still very useful in order to manage globally business processes and its adoption still contribute to improvements and reinforcement in the competitive advantage of a firm. Furthermore in the era of Internet and social media, the creation of websites and *e-commerce* platforms are crucial instruments available to businesses to increase their chance of broadening the base of customers, becoming more visible on the market and hopefully to encourage them to shape and adopt business strategies oriented towards the penetration of international markets given that Internet allowed for the breaking down of

[7]The BURC is the information press service that advertises the laws, regulations and acts of the Region, assuming a role similar to that performed by the Official Gazette (Gazzetta Ufficiale). In the legal field it is considered as one of the official sources of legal knowledge.

[8]An ERP system is the integrated management of core business processes, often in real-time and mediated by software and technology that provides the infrastructure to manage information and coordinate activities within the firm to develop more efficient operations and to take advantage of benefits in terms of cost and time saving, routines and information exchange within the firm.

physical boundaries.

On the other hand, high-scoring firms proposed projects that along with the adoption (or renovations) of ERP systems as well as the creation of *e-commerce* platforms included also innovative activities aimed at improving substantially, especially from a technological point of view, the product/service they were offering on the market. For instance, one of the two high-scoring firms I chose in order to write this paragraph declared that a consistent share of the subsidy, if awarded, would have been spent on the installation of digital sensors on plants that would have allowed to know in real time the production progress and able to communicate with other firm's plants and business sectors in order to optimize the overall production process. Or again, for example, the latter enterprise in order to improve its position on the international marketplace, it was planning to develop IT programs able to standardize the software development process in order to reduce its costs and implementation time and being competitive with foreign companies, while still offering a superior quality and highly specialized service to its customers.

To sum up, the lag between the program publication and its implementation did not seem to have played a role in reducing the technological improvements that the program aimed to. On the contrary, apart from the heterogeneity on the level of technological advancements proposed by firms that is also able to explain the differences in reported scores (at least in part), all of them submitted projects involving the adoption of technologies that were not meant to become obsolete within a few years.

4.3 Data and Empirical Strategy

Sample and descriptive statistics. The empirical analysis is based on an original and novel dataset combining two sources of information. Data related to the investment subsidy program is retrivied from the Campania Region website. The Final Ranking Dataset covers many precious information in order to distinguish *recipient* and *non-recipient* firms, such as: name, tax code number, score received, planned investment (however, available only for treated firms), grant assigned, subsidies revoked and renunciations. Then, I combined these data with balance sheet data covering several firm's dimensions, for subsidized and non-subsidized enterprises over the period 2008-2016. The main source of firm-level data is AIDA, a database produced by Bureau van Dijk that collects balance sheet information on all Italian firms that are required to file a balance sheet; the requirement applies to corporations but not to partnerships[9].

By matching the the firms' tax code number, I am able to retrieve from AIDA 232 out of 299 *recipient* enterprises and 313 out of 424 control firms[10]. However, as summarized in table 4.3 the number of observations shrinks in size because of reasons unrelated to the program, and the majority of them belong to the control group. Hence, to avoid attrition concerns, I decided to employ in the empirical analysis only those firms for which data are available up to 2016. In addition, I restrict the time horizon of the analysis from 2010 to 2015, 3 years for the *pre*-treatment period and 2 years for the treatment period (since firms were allowed to put in place investment starting from January 2014 up to December 2015). The choice of excluding the first two years of data mainly regards to the advent of the financial crisis. Indeed, a simple comparison along many firms' dimensions between 2008 and 2009, revealed that firms included in treated and control groups were severely hit by the the financial downturn.

The final dataset, then, results in an almost perfectly balanced sample between the two groups and it is composed of 182 firms belonging to the treatment group (*recipient firms*) and 186 enterprises for the control group (*non-recipient firms*) per year.

The outcome variable I chose in order to conduct the empirical analysis reflects the overall yearly sum of the intangible and tangible innovative investments for which the region assigned the subsidy, scaled by contemporaneous Total Asset; that is:

$$y_{it} \equiv \frac{K_{it}}{TA_{it}} = \frac{SUEC_{it} + RD_{it} + Patents_{it} + Licenses_{it} + PM_{it} + ICE_{it}}{TA_{it}}$$

[9] For a comprehensive view of the overall data retrieved from AIDA see table 4.3.

[10] The AIDA database does not collect information on individual companies and that is why I am not able to retrieve the total number of *recipient* and *non-recipient* firms.

where $SUEC_{it}$ stands for Start-up and Expansion Costs for firm i at time t, RD_{it} for Research and Development ($R\&D$) expenditures, $Patents_{it}$ and $Licenses_{it}$ the book value of Patents, Licenses *et similar* rights for firm i at time t, PM_{it} and ICE_{it} the accounting value of Plant and Machineries and Industrial and Commercial Equipments, respectively. Hence, the dependent variable tries to summarize the book value of all the tangible and intangible innovative assets that might be subsidized, given that treated firms may spend in tangible innovative assets only, in intangible innovative assets only or in both.

In Figure 4.5, I compare the evolution of the outcome variable during 2010-2016 for treated and control groups. During the 3 pre-treatment years, although the level of spending in innovative investments has been slightly higher for control rather than for treated firms, the evolution of the outcome variable seems to be quite parallel, and it seems to be more credible by not taking into account the period 2010-2011. When the treatment kicks in, that is from 2014 onward according to the program rules, treated firms increased their level of spending peaking its maximum in 2015, hopefully because of the subsidy, while those of control groups declined.

During the pre-treatment period, firms did not show statistically different investment patterns (see Table 4.5). The pre-treatment level of investment, averaged over the 3 years span, was slightly higher for *non-recipient* (about 7.3% of Total Assets) rather than *recipient* (about 6.8 percentage points of Total Assets) firms, that is half of a percentage point difference among the two groups. However, (and with respect the 2008 figures; see Table 4.4) some heterogeneity emerged while comparing groups in key variables that may affect investment decisions, such as cash flows and sales. The pre-treatment analysis revealed that treated firms were more profitable with respect to control firms. On the contrary, for what concern borrowing costs and level of indebtedness no statistically significant differences emerged between groups.

Empirical Strategy. The objective of the empirical analysis is to assess the causal effect of public subsidies on the outcome of interest, that is the level of innovative spending (scaled by total assets).

In the language of the program evaluation, this is a *counterfactual* question. What I would like to know is how *recipient* firms would have behaved in absence of the treatment, that is the public subsidy. However, this is not an easy question since at the same time it is impossible to observe for firm i its outcome when treated and when it did not. Hence, I had to rely on a control group that should be able to approximate as best as possible how treated firms would have behaved in the absence of the treatment. A natural way to solve this issue was to take advantage of the assignment scheme of the public subsidy, so that to identify treated and control groups.

In particular, following the assignment scheme of the public grant, I defined treated firms all those firms that received a score greater or equal to 60 points. On the other hand, I defined as control group all those firms that received a score lower than 60 points.

However, as it has been long discussed in the literature about public subsidies (see for instance David, Hall, and Toole, 2000; Klette, Møen, and Griliches, 2000; Cerulli, 2010 and Becker, 2015)[11], very often $R\&D$ subsidies are not randomly assigned to firms. Because of this non-random assignment and self-selection of firms into programs, estimation of the causal effect is biased. Indeed, the regional program herein studied provides subsidies to requesting firms only for those investment that, after the evaluation process carried out by the Technical Commission, obtained a minimum score of at least 60 points. Clearly, the treatment assignment is based on a deterministic rule that, as I have already explained in Section 4.2 and in Table 4.1, firms were not able to manipulate in any way and based on a competitive projects' ranking that rewarded for a 90% of the overall score project quality and competitiveness and for a 10% observed firms' characteristics measured in 2008 (such as: age, prevalent sex and

[11] In particular, David, Hall, and Toole, 2000 criticised the econometric methods of nearly all research performed until the end of the 1990s, based on OLS estimation of linear regression models, for largely ignoring endogeneity problems. However, the potential sources of endogeneity might lead to inconsistent estimates of the causal effect of subsidies on private $R\&D$ decisions. To address endogeneity problems in such a way as to obtain appropriate estimates of this causal effect, several approaches have been used, which can be summarised as follows: (1) difference-in-differences estimators; (2) sample selection models; (3) instrumental variables and (4) non-parametric matching methods.

economic sustainability intended as how much of the project costs are covered by sales volume). The endogeneity of the treatment status may pose some doubts on the choice of employing the sample of firms that failed to obtain the grant (since their score was lower than 60 pts.) as control group in order to perform the impact evaluation of the program.

However, to sustain that the sample of non-*recipient* firms constitutes a valid control group that is able to approximate as best as possible how treated firms would have behaved in the absence of the subsidy, I provide some supportive evidence that this is the case. First, I estimated a *naive* participation equation, by means of a Probit model, where I regressed the treatment status variable (known by firms only in 2013) on some firm-level variables measured in 2008[12] that may explain, at least partially, the obtained scores (and thus their eligibility status). The reduced form equation reads:

$$P(Treatment_{i2013} = 1) = \Phi(\beta' X_{i2008} + \varepsilon_{i2008}) \tag{4.1}$$

where X_{i2008} is a vector of firms characteristics (included separately or jointly in the specification) composed of: K_{i2008}/TA_{i2008}, as proxy for the observable quality of proposed projects, $Sales_{i2008}/TA_{i2008}$, CF_{i2008}/TA_{i2008} and $Liquidity_{i2008}/TA_{i2008}$, as proxies for the economic sustainability of the project[13]. Results, available in Table 4.7, show that none of the covariates (separately or jointly) are able to statistically predict treatment status in 2013.

Furthermore, to strengthen my argument I performed also a balance covariate test between treatment and control firms in 2008. As the test shows (Table 4.4), in the year before the application to the regional program the two groups of firms were perfectly

[12] In order to conduct this test I chose the year 2008 given that in the application form firms were required to attach the last available balance sheet. Moreover, the economic sustainability of the projects was measured also in terms of 2008 sales volume. Having said this, results are unaffected also considering 2009 instead of 2008.

[13] Unfortunately, I cannot include in the *naive* participation equation the information regarding the sex and age profiles composition of the shareholders, given that I only observe these for treated firms. However, as suggested by Figure C.1 and Table C.1 these did not weight so much since the high percentage of male shareholders and the lower share of young shareholders admitted to public funding.

comparable given that none of the analyzed firm-level characteristics significantly differs one from the other, with the only exception for ROA which is significant at the 5 percent level.

These evidences seem to suggest that the channel explaining differences in reported scores is provided by the (unobserved) quality of proposed investment projects, given that as of 2008 the two groups seemed to be equally likely to obtain public funding. Thus, if one believes that part of the the self-selection mechanism works though the unobserved ability of firms in proposing higher quality projects, and if this unobserved ability remained more or less constant through the sample period, then the Difference-in-Differences approach represents a credible estimation procedure in order to recover the causal of effect of interest. Indeed, the Difference-in-Differences removes biases in post-intervention period comparisons between the treatment and control group that could be the result from permanent differences between those groups (such as ability, age and sex), as well as biases from comparisons over time in the treatment group that could be the result of trends due to other causes of the outcome, notwithstanding with the fact that this methodology is also an approach robust to selection and self-selection bias. The causal interpretation of the Difference-in-Differences estimate as the effect of the subsidy on the investment level rests on the identifying assumption that firms who were declared subsidy-*recipient* in 2013 were on parallel trend with respect to firms that obtained a score lower than 60 points in the pre-treatment period. As I have already discussed, according to Figure 4.5 it seems to be the case but in Section 4.4 I provide two additional tests that confirm the fulfillment of the common trend assumption.

Therefore, in order to recover the causal effect of interest, that is the effect of the subsidy on the level of spending in innovative investments, I employed a Difference-in-Differences approach by comparing the average expenditures of treatment and control groups, identified by taking advantage of the eligibility status (subsidy-*recipient* or not) determined from assignment scheme of the regional subsidy in 2013. However given the heterogeneity in some firms' covariates emerged during 2010-2013 and the endogeneity of the subsidy allocation scheme (implying that subsidies were not allocated randomly to firms), I can strengthen the identification strategy and balance as best as possible the two groups by relying on the Conditional-Independence assumption. That is, I assume that the treatment is orthogonal to the error term by including in the specification an appropriate vector of controls. This vector is composed of a set of variables averaged during the pre-treatment period and their interaction with the dummy $Post_t$. The choice of not controlling for time-varying firms' variables lies in the fact that during/after the program implementation these variables may respond to the treatment, and their inclusion may contribute in biasing the coefficient of the causal effect, β_3. In particular, the vector of controls include pre-treatment averages by firms of Cash Flows and Sales[14] (which are highly correlated with reported scores, see Table 4.8) and their interaction with the post-treatment identifier, as well as a set of dummies regarding firms' sectors, legal forms, years and provinces to ameliorate any concern about an omitted variable bias[15].

In particular, I estimated the following reduced form model that reads:

$$y_{it} = \beta_0 + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i * Post_t + \beta_4 X_i + \varepsilon_{it} \tag{4.2}$$

where $y_{it} = K_{it}/TA_{it}$ is the outcome of interest for firm i at time t, that is the level of innovative expenditures (over Total Assets); $Treat_i$ is a dummy that takes value of 1 if firm i is included in the treatment group, that is it received the subsidy or in other words it received a score of at least 60 points; $Post_t$ is a dummy that takes values of 1 during the period of implementation of the program (2014-2015), and 0 otherwise. The coefficient of interest measuring the causal effect of the subsidy on the level of innovative expenditures is given by β_3.

[14]One specification uses as controls ROA and EBITDA (over Total Assets) averaged over the pre-treatment period, but it should be interpreted with very caution since those two controls are highly correlated during 2010-2013.

[15]In addition to the standard Difference-in-Differences approach, I estimated also a generalized two-way fixed effects Difference-in-Differences with firms and year fixed effects. The model is not discussed in this section but its results are available in Section C.2 of the Appendix (see Tables C.2, C.3, C.4, C.5, C.6 and C.7). For the sake of completeness, even estimating the model with FEs this do not change the magnitude of the results. In some cases statistical significance of the estimated coefficients changes depending on which standard errors (heteroskedastic robust or clustered at the firm level) are used.

Having stated the estimated model, I would like to discuss very briefly how to interpret the coefficient of the causal effect of interest. A coefficient $\beta_3 > 0$ can be interpreted as signal of crowding-in, that is treated firms invested additionally with respect the control group because of the subsidy. On the contrary, a coefficient $\beta_3 \leq 0$ signals crowding-out, that is treated firms substitute private funds with public capital. However, a simple $\beta > 0$ statistically different from zero does not guarantee that the input-additionality holds, although the positive response of the investment level. To check if the hypothesis holds, I perform a simple hypothesis testing where the null (H_0) is given by $\hat{\beta}^{DD} \leq$ threshold (that is there is no evidence of input-additionality), whereas the alternative (H_1) is $\hat{\beta}^{DD} >$ threshold (input-additionality hypothesis holds); where threshold is given by the average value of the ratio between the subsidy (obtained by treated firms, only) and Total Assets over the period 2014-2015, assuming that firms spent half of the public incentive in each year, that is half in 2014 and half in 2015 (to be also consistent with the investment program).

4.4 Main Results

This section provides the main findings of the impact evaluation analysis. In particular, I will discuss the baseline estimates of the Difference-in-Differences model based on Equation 4.2 as well as some interpretations, in terms of economic significance, of the results and I will provide two formal tests on the validity of the chosen reduced form empirical model.

Baseline Estimates. The results of the empirical analysis are available in Table 4.9. The sample period goes from 2010 to 2015, where the period 2010-2013 is the pre-treatment period. The estimation results did not show any statistically significant difference between groups nor in the post-treatment period. The coefficients of $Treat_i$ and $Post_t$ are never statistically significant in almost all the specifications (columns (1) to (5)). Instead, the coefficient relative to the causal effect of interest, that is the effect of the subsidy on the average level of expenditures in innovative investment, is always positive and statistically different from zero. The ATT amounts to 0.025, meaning that treated firms increased their level of investment, on average, by 33% more with respect their level of spending in 2013. These results seem to rule in favour of the effectiveness of this program in rising, at least, the level of innovative expenditures for subsidy-*recipient* firms.

Even controlling for Cash flows and Sales over Total Assets (separately or jointly, see columns (3), (4) and (5) of Table 4.9), averaged during the pre-treatment period (and interacted with the post-treatment identifier), did not affect the magnitude of the estimated casual effects. Given that the estimates are pretty much similar in the baseline and augmented specifications, this indicates that my results are unlikely to be driven by an omitted variable bias.

In addition to obtain a measure of the economic magnitude of these results, I estimated also an elasticity of the investment to its price by relying on a back-of-the-envelope calculation. The implied elasticity amounted to -0.8, a value that turns out economically meaningful and being pretty much close to elasticities estimated in similar programs documented in the economic literature (see for instance Acconcia and Cantabene, 2018, Agrawal, Rosell, and Simcoe, 2020).

In order to test whether the input-additionality holds or not, I performed the hypothesis testing described in Section 4.3. For this sample of treated firms the threshold, given by the average values of the subsidy scaled by Total Assets assuming that the public funding is spent equally across the two post-treatment years, is about 0.05. The *p-values* of the associated t-tests are higher than 0.98 in all the cases (see Table 4.23, Panel A), implying that I am not able to reject the null hypothesis that the Difference-in-Differences estimated coefficients are lower or equal than the threshold values that would have confirmed the input-additionality hypothesis, that is given the subsidy value investment increased more than the value of the public funding.

Overall, the empirical evidence shows that treated firms responded positively to the regional program by increasing their level of spending in the period 2014-2015.

However, I cannot rule out that subsidy-*recipient* firms increased their investments by approximately the amount of the subsidy they received, thus providing no support for the input-additionality hypothesis.

Parallel Trend Assumption. The key identification assumption of the DID strategy is that, nevertheless differences in level, trends in outcomes would be the same in both groups in the absence of treatment that is the well known *common trend assumption* or *parallel trend assumption*. As I have already discussed in Section 4.3, by looking at Figure 4.5 there seems to be evidence ruling in favour of the fulfillment of *common trend assumption*, and more credibly at least starting from 2012. Although the average outcome for control group is higher than that one the *recipient* firms, starting from 2012 they seemed to move fairly parallel. Only with the implementation of the program, started in 2014, treated firms increased their level of spending, while that of one control group started to decline. This non perfectly parallel movement by groups during the first year of the pre-treatment period may cast some doubts about the validity of the *parallel trend assumption*. Hence, to dissolve any doubts about its fulfilment, I estimated the baseline model by interacting the coefficient of the causal effect with year-dummies (from 2010 to 2015) while omitting the year 2013 as reference category. That is, I estimate the following equation which consists in an event-study that estimates the baseline regression with different treatment years:

$$y_{it} = \sum_{\tau=2010}^{2012} \gamma_\tau Treat_i \mathbf{1}(t=\tau) + \sum_{\tau=2014}^{2015} \gamma_\tau Treat_i \mathbf{1}(t=\tau) + \lambda_t + f_i + \epsilon_{it} \tag{4.3}$$

Equation 4.3 includes interactions between the treatment indicator variable ($Treat_i$) and year dummies for every year excluded 2013. Under the assumption of parallel trends $\gamma_\tau = 0$ for $\tau < 2013$. Figure 4.6 reports the point estimates for γ_τ in equation 4.3 and 95% confidence intervals. Given the long time span occurred between the issuing of the program and its final conclusion with the publication of the list of subsidy *recipients*, it may have been the case that some of the planned investments were conducted well before the timing set by the Region (01/01/2014-31/12/2015) and so if it is the case the *common trend assumption* is not satisfied. However, the Figure provides evidence of the absence of anticipation effects of the subsidy program. Furthermore, besides the lack of anticipation effects before year 2013 (year in which firms knew their final status) there is no evidence of statistically significant differences in outcome between control and treated groups, in other words there is no evidence of pre-trends, as the point estimates in the pre-period are close to zero and not statistically different from zero. Even though the 2010's coefficient is above zero, it is not statistical significant as shown by the confidence interval. The point estimates of γ_τ for $\tau > 2013$ show the dynamics of the effect of the program. Statistical significance of the interaction terms starts from 2014 onward, as desired by the program, highlighting also a heterogeneous response over time in the level of investment.

Placebo. To check the robustness of the baseline estimation results, I arrange a placebo analysis covering only the pre-treatment period, that is the years from 2010 to 2013. In particular, I design a fake treatment following the program rules of the regional subsidy program. More specifically given the assignment of treated and control firms according to their score, I define as pre-treatment period the years 2010-2011 and as (placebo) treatment period the years 2012-2013, hence allowing always for a two years window investment (as in the original program).

The results of the placebo exercise are available in Table 4.10. According to the estimated coefficients, none of them exhibit statistical significance in all the specification considered, meaning that prior the effective begin of the program there were no differences in investment spending between treated and control groups as well as, despite the long time horizon separating application and implementation of the program, no anticipation has occurred. Hence, also this further test confirms the interpretation I gave on the estimated coefficients regarding the event study equation and provides further evidence of validity of the identification and estimation strategy adopted to recover the causal effect of interest.

4.5 Heterogeneity

The results of the baseline regression showed a positive response of innovative investment spending because of the subsidy. Expenditure in innovation for treated firms increased more than that of firms that did not received public funding, suggesting evidence for a positive effect of the public contribution to the level of spending but not higher than the assigned subsidy ruling out the input-additionality mechanism. However, these results may mask some degree of heterogeneity in the investment responses by firms.

In these subsections I argue that indeed there may be some differences in the response of firms' expenditures in innovation following the regional subsidy and these heterogeneities may be due to firm size and the technology level of the sector in which firms operate. In order to test for these hypothesis, I run four sample splitting exercises always relying on the Difference-in-Difference approach.

Accounting for firms' size. The first sample split exercise regards how firms of different sizes changed their level of innovative spending following the public funding. Hence, I classified the sample of firms included in control and treatment groups according to their size based on the classification given by the European Commission in order to test whether there exist a differential response to subsidy. In particular, I consider micro-sized firms those up to 10 workers on average during the pre-*treatment* period (2010-2013), small-sized those between 11 and 50 workers on average, medium/large-sized[16] those with more than 51 workers on average.

The sample, then, is composed of for a 90% of micro- and small-sized enterprises whereas for the remaining 10% of medium- and large-sized firms (see Table 4.6). For what concerns the pre-treatment characteristics of the three sub-samples, micro-sized

[16] I merge these two subgroups of firms given the scant number of large-sized firms (2 firms)

enterprises show a high degree of heterogeneity with particular regard to financial variables that likely may influence firms' investment decisions. Instead, small and medium/large sized companies during the pre-treatment period seem to be characterized by the same degree of heterogeneity, *i.e* ROA, EBITDA and Cash Flows. However, these differences are not alike than those that are observed in Table 4.5 without classifying them in terms of size.

As shown by the previous literature review, the empirical evidence suggest that these kind of programs may be more effective for small-sized firms. On the contrary, medium- and large-sized firms are those that most likely substitute private with public capital, and hence their level of spending, despite having received the subsidy, remains unchanged. Furthermore, the literature on capital market imperfections argues that small-sized firms are those that may have worse access to capital markets due to information asymmetries. Generally, small firms are more reliant on external finance, for which they also pay a higher premium when compared to large firms. Hence, to the extent that small firms cannot easily tap external finance for *R&D* investments, such fiscal policy instruments that cover some portion of a firm's innovations expenditures in cash should have a larger impact on small-firm expenditures than for large firms.

In order to test the relationship between firms' size, subsidy response and innovative spending, I re-estimated the baseline specification (that is Equation 4.2) separately for each of the three size groups I defined above.

The results (Tables 4.12, 4.13, 4.14), in line with previous evidence, show that for micro-sized enterprises the effect of being subsidized was the highest. In addition, also for small-sized firms the subsidy has been effective in inducing an increase in spending, but the coefficient of interest is estimated with less precision in terms of statistical significance. On the other hand, the estimated coefficient measuring the causal effect of interest for medium/large-sized firms is not statistically different from zero or statistically significant but negative, according to the specification considered. In this case, the evidence for this sub-group of firms seems to support the idea that these firms took advantage of the subsidy without increasing the level of investment.

In particular, the estimates imply that because of the subsidy micro and small firms increase their expenditures by about 47% and 30%, respectively, with respect to their level of investment in 2013. For these firms, I also estimated an implied elasticity that turned out economically meaningful and around the value estimated without taking into account firms' size. The elasticity of investment to its price amounted to .9 and .7 for micro- and small-sized enterprises respectively, meaning that for each euro granted spending increased of about 90 cents and 70 cents of Euro for micro and small enterprises, respectively.

However, as showed by Table 4.23 (Panels B and C), I cannot rule out, that also for these sub-sample of firms, the increase of spending did not go above the level of the public funding the region granted them.

High vs. Low Tech: The role of Sector Technology Intensity. Hall and Lerner, 2010 widely discussed that usually High Tech companies are characterized by a sluggish response of *R&D* to change in its cost, since they also tend to have more stable year flows of *R&D* expenditures - if compared to *traditional* firms. This argument has been discussed, also, recently by Acconcia and Cantabene, 2018, where they found that indeed firms may respond heterogeneously to public incentive programs according to the technological intensity of the sectors in which they operate.

To verify if it was the case also with the program that I analyzed, I distinguished firms in high and low tech according to ATECO2007-NACE2 classification (derived from Eurostat classification) of sectors' technology intensity (see Tables 4.15 and 4.16). As showed by Table 4.17, the sample of firms is almost equally split among high and low tech, albeit there is a slightly preponderance of the latter (54% low tech vs 46% high tech).

In particular, high tech firms are those that, in principle, should smooth investment in innovation continuously over time because of the strong market competition they face, and for which I expect that the subsidy has a lower impact.

Prior to conduct the empirical analysis, I investigated if during the pre-treatment period there existed differences in investment patterns and other key variables among treated and control groups in high and low tech sectors. The results, available in Table 4.18, show that treated and control firms in high tech sector were the more heterogeneous in terms of profitability but not in investment patterns[17]. On the contrary, treated and control low tech firms were the more homogeneous. Regarding this second group of firms no meaningful statistical differences during the pre-treatment period emerged, except for their size (measured in terms number of workers, on average treated firms are larger).

Thus, to analyze the impact of the subsidy on the outcome of interest, differentiating by technology intensity, I augmented the baseline specification by distinguishing between firms belonging to high or low technology intensity sectors, and estimating the models separately for the two groups.

In Table 4.19 and 4.20, I report the results for high tech companies and low tech enterprises, respectively. By comparing the two tables, the impact of the public incentive has induced a positive effect only for firms belonging to high tech sectors. The coefficient of the causal effect for this group of firms is always positive and statistically significant. A 50% subsidy, or to put it differently a 50% reduction in the cost of investment, implied an increase in innovative spending by about 45% with the respect of the level of spending of high tech firms in 2013, but again there is no evidence of input-additionality effect (see Table 4.23, Panel D). In addition, the implied elasticity interpreted as elasticity of investment to its price is about 1, meaning that the subsidy has been very effective in lowering the investment price allowing *recipient*-firms to increase their expenditures in innovation activities.

Hence differently from the large micro-evidence supporting the view that only firms operating in *traditional* sectors may benefit from public incentives, the evidence supported by the previous empirical estimates suggest that also high tech firms are able

[17] Such differences were not alike from those detected in Table 4.5 between *recipient* and *non-recipient* firms independently of the technology intensity of sectors in which they operate.

to reap the benefits of fiscal policies aimed at sustaining innovative investments, differently from what the literature has sustained so far.

High vs. Low Tech: Manufacturing vs. Service Sectors. The majority of the empirical works investigating the role of public incentives on private firms investment in research and development has mostly focused on the manufacturing sector. In general, manufacturing firms are characterized by a higher share of tangibles than intangibles, whereas the contrary is true for service firms. A *priori*, one can argue that programs that aim to sustain and increase the level of innovation and technology intensity should be more effective for manufacturing firms because of their typical asset composition and reliance.

However, the services sector has had an increasing and prominent weight in most developed countries. It is therefore important to analyse innovative investment in this sector, to understand how public subsidies affect it, and to compare the results with those for the manufacturing sector (Zúñiga-Vicente et al., 2014). The next two paragraphs tries to fill this gap and in which I compare the causal effect of the investment subsidy program between High and Low Tech companies distinguishing whether they belong to the manufacturing sector or to the service sector.

Manufacturing Sector Only. By considering only the manufacturing sector in the comparison between high vs low tech sectors, I did not find any additional effect on spending implied by the public incentive (see Table 4.21). Indeed, the causal effect estimate is never statistically different from zero. Hence, these estimates for the whole manufacturing sectors do not suggest any positive effect on investment in innovation, differently from the wide number of positive results available in the literature.

Service Sector Only. The empirical results for this sample of firms reveals an opposite conclusion with regard the general wisdom that public subsidies are more effective only for *traditional* firms. The evidence, reported in Table 4.22, suggests that the grant induced to increase spending only to firms belonging to high technology service sector. For these firms, the impact of the subsidy implied an increase in innovative expenditures of about 56 percentage points, with respect to the 2013 level, and an implied cost elasticity above 1. These results, despite being positive and statistically different from zero, do not suggest that the additionality effect holds for high tech firms given that I cannot reject the hypothesis that innovation expenditures increased by about the size of the subsidy (see Table 4.23, Panel G).

4.6 Robustness

In this Section, I provide some robustness checks of the previous estimated models. In particular, I will borrow from the literature on the Regression Discontinuity Design where the identification strategy relies on the continuity assumption. According to that, if the treatment depends on whether a (forcing) variable (in my case the score) exceeds a known threshold and agents cannot control precisely the forcing variable, the continuity assumption is satisfied since the variation in treatment around the cutoff is randomized, as if the agents had been randomly drawn just below or just above the cut-off[18]. However, despite not relying on a RD design, I can take advantage of this insight in checking whether the previous estimated causal coefficients, still within a Difference-in-Differences framework, remain stable even shrinking the size of sample around the cut-off, and at the same time alleviating any concern about the treatment status endogeneity. If this the case, this means that the effect is not driven by firms with higher reported scores (and thus presumably higher quality), providing in turn further validity to the empirical strategy adopted.

First, I discuss that indeed firms had no room to manipulate the forcing variable, as well there is no evidence that Technical Commission accommodated more firms in

[18]It follows that it is possible to assess the validity of the design by verifying whether differences in treated and control firms' observables become negligible close to the cut-off point. However, this is not my case since differences even in a closer bandwidth around the threshold ($score = 60$) are not alike from those that characterize the full sample of firms. That is why I do not rely on a sharp RDD for the robustness checks.

obtaining the public funds, by for instance assigning them the lowest score possible to get access to the subsidy. These conclusions come from the visual inspection of the density function of the sample by score (Figure 4.4). In fact, at the threshold no evident excess of mass is located neither at the right nor at the left of it. This goes, somewhat, in favour of ruling out any possible sorting behaviour from both firms and the panel of experts that evaluated the investment projects.

In order to perform the robustness exercise, I redo the previous estimations of Sections 4.4 and 4.5 by selecting three different tighter windows around the treatment threshold: [53-66], [56-64] and [58-62] windows around the *score* = 60 that indicates the eligibility status.

Table 4.24 reports the estimates relative to the three windows for the full sample of firms, that is regardless of their size and technology intensity, showing that these are very close to the estimated causal effect (about 0.025) reported in Table 4.4. No matter the chosen window, the estimated causal effects remain stable, in the range of 0.025 for the wider window (statistically significant at the 5 percent level) and about 0.024-0.025 for the two tighter window ([56-64] and [58-62]), despite shrinking sample size lowers statistical significance of the estimated coefficient. By the same token, there is evidence that the estimated causal effect are pretty much the same when distinguishing firms by their size. Table 4.25 reports the three set of estimates relative to micro-sized firms, whereas Table 4.26 for small firms[19] (despite the estimates are only statistically different from zero in the widest and tightest window.).

The robustness check confirms the stability of the reported coefficients in Section 4.5 also for firms operating in high-tech sectors as well as for low-tech industries. For the whole high-tech sector the estimated causal effect was about 0.03 (see Table 4.19), whereas it was about 0.02 (albeit not strongly statistically different from zero) for low-tech enterprises (see Table 4.20). The three estimates in Table 4.27, relative to windows [53-66], [56-64] and [58-62] for high-tech firms, are about 0.03 (for the

[19] However, I am not able to conduct the robustness check for medium-large sized firms given their extremely small sample size that is distributed along all the score distribution (about 1 firm for each point-score).

wider one) and about 0.05 for the others two, an higher magnitude with respect to the baseline but not extremely different. Instead, for low-tech firms the reported coefficients of the robustness check (Table 4.28) are never statistically significant, even though the magnitude of the estimated betas reduce as the sample size shrinks around the cut-off.

The final two robustness exercises regard the technology sector intensity in both the manufacturing and service sector. While for the manufacturing high-tech sector the baseline estimate is about 0.055 (not statistically significant), I find only an equal estimate in tighter bandwidth around the threshold (it is about 0.05). In the medium window ([56-64]) a slightly higher one (about 0.07), whereas in the wider one it is about 0.02 but not statistically different from zero (see Table 4.29). For this sample of firms, restricting the sample size around the threshold seems to suggest the existence of a positive effect of the subsidy on the level of investment (given the statistical significance of the estimates), which however was ruled out in the full sample. On the other hand, low-tech traditional firms display coefficients that are perfectly in line with their baseline counterparts (Table 4.21), also with respect the statistical significance of the estimated causal effect. However, in the window [58-62] I find a positive coefficient, of similar magnitude (0.03), which is also statistically significant (see Table 4.30, column (6)). Finally, also for the service sector (both high-tech, Table 4.31, and low-tech, Table 4.32), I find that the robustness exercise provides evidence for the validity of the estimated coefficients of Table 4.22.

4.7 Spillover effects

So far my estimates show that the subsidy program has induced a sizeable increase in innovation expenditures, despite not fulfilling the common additionality criteria studied and advocated by the literature. Then, it is reasonable to inquire whether the program had spillovers effects on treated firms.

The first spillover effect I investigate is firms' labour demand. According to official figures, retrieved from the subsidy-*recipients*' project forms, the 55 percent of treated firms in 2009 would have been willing to hire new employees following the innovative investment. For the sake of clarity, however, the subsidy program did not cover wage expenditures nor aimed at inducing firms in increasing employment to get access to public funds.

Hence, by exploiting Eq. (4.2) and also the previous heterogeneities discussed in Section 4.5, I test whether public funds awarded firms increased employment in the aftermath of the program, relative to control firms. Unfortunately, I do not have access to matched employer-employees administrative data and I am forced to carry out the analysis using the aggregate workforce reported at the end of the year in financial statements.

However, one of the limitation of this employment measure is that it is expressed in terms of full time equivalent units. To overcome this issue, I use as additional dependent variable the *per* worker wage bill that may be more sensitive to changes in the overall worker composition[20].

Tables 4.34 and 4.35 report the results for labour demand and the *per capita* wage bill. For the sake of brevity, I only report specifications in which the relevant coefficient of interest, that is Treat*Post, is statistically significant. Consequently, if a sub-sample is missing in the table is to be interpreted as a null effect on the relevant outcome.

According to Table 4.34, I find that in the aftermath of the program treated firms, relative to controls, increased workforce of about 59 percent (see column (1)) with an implied elasticity of employment to the subsidy of about 1.2. In addition, I find marginally statically significant coefficients at 10% level for micro-sized firms (an increase of 17 p.p.) and for high tech enterprises (+75%). For medium and large companies the relevant estimated coefficient equals about 176 (suggesting an increase in labour demand of about 78 percent) and for high tech firms operating in the service sector an estimate of 33.3 (+92 p.p.), both statistically significant at the 5 percent level.

[20]Indeed, it may be the case that firms may substitute expensive workforce with cheaper ones by relying on flexible job contracts and keeping constant the overall number of employees.

For what concerns the other employment margin, all the reported coefficients have a confidence level of 10% and suggest a reduction in the wage bill *per* worker. Hence, this may be a sign that somehow small-sized and low tech manufacturing firms hired cheap labour keeping almost constant the overall number of employees. For medium and large companies the observed increase in labour demand (see column (3) of Table 4.34) seems to come only from cheaper (and presumably more precarious) labour.

Table 4.36, finally, analyzes, as second potential spillover channel, the impact of the subsidy on two productivity measures common in the accounting literature, that are added value *per* worker and added value over total asset. In the majority of the cases no positive effects are exerted by the subsidy on firms' productivity. The results of Table 4.36, furthermore, depending on the outcome are either positive or negative not being very clear on the true effect.

4.8 Conclusions

This paper analyzed the effectiveness of a subsidy investment program implemented by the Regional Government of Campania (south Italy) during 2014-2015. The program provided direct monetary support, in the form of grants (*i.e.* subsidies), for private SMEs in sustaining innovative investments with particular regards to those connected with information technology.

The main research question addressed by the evaluation analysis was to assess whether public regional subsidies had input additionality effects, and whether these effects were heterogeneous in terms of firms' size and technological intensity of the sectors in which they operate. The main conclusion is that in none of the analyzed sub-samples, despite the a sizeable response of innovative expenditures in the aftermath of the program, there is evidence that subsidy-*recipient* firms increased additionally their level of spending. In other words, it seems to be the case that treated firms increased investment by about the amount of the subsidy they received.

Overall, the program considering the whole sample of firms, regardless of their size or technology intensity, has induced them to increase in innovative investments. The *average treatment effect of the treated* was about 0.025, meaning that treated firms increased their level of expenditures about 33% more with respect their level of spending in 2013. The program was also effective in reducing the price of investment. Indeed, the implied elasticity, obtained by relying on a back-of-the-envelope calculation, is about 0.8, meaning that for each extra euro of subsidy firms expenditures increased contemporaneously by €0.8.

By shedding light on the response of treated firms according to their size, I found that only micro- and small-sized companies, in the aftermath of the program, increased spending, thus not in the sense of input-additionality. For what concerns medium-sized companies and large enterprises, the empirical exercises revealed that they substituted private funds with public capital, and so their level of spending remained the same. These findings are pretty much in line with previous empirical evidence, reinforcing the argument for which these kind of programs are more effective if directed to small enterprises that more likely are subject to liquidity constraints and asymmetric information on financial markets.

In addition, I have investigated the role of how the sector technological intensity (High Tech vs. Low Tech) impacted on the response of firms to the subsidy. The results show that only high tech firms, and in particular those belonging to the high tech service sector, are those that responded positively to the program, but again I was not able to reject the hypothesis that spending increased by about the amount of the monetary subsidy the region assigned to them.

I provided also some robustness checks of the estimated effects of the regional program on the level of investment. Given the concerns regarding the endogeneity of the treatment status, I showed that my results are very robust also when I looked for a causal effect in tighter windows of the score distribution around the cut-off where reasonably the treatment eligibility can be thought as if randomized, alleviating, thus, any concern that the evidence presented here may have been driven by omitted variable bias or by firms that where located on the upper tail of the score distribution.

Finally, I show that in the aftermath of the subsidy program treated firms expanded labour demand, although with large heterogeneity and very likely with cheaper and precarious workforce. On the contrary, firms' productivity did not improve significantly.

On the policy side, taken together these results seem to support the view that public funding programs aimed at increase spending in innovation activities should be supported by a quick a strong administrative capacity in order to ensure that such policies can be conceived as effective instruments in stimulating investment demand, especially during downturn business cycle periods.

Figures and Tables

Figure 4.1: Investment Costs and Returns with Imperfect Capital Markets

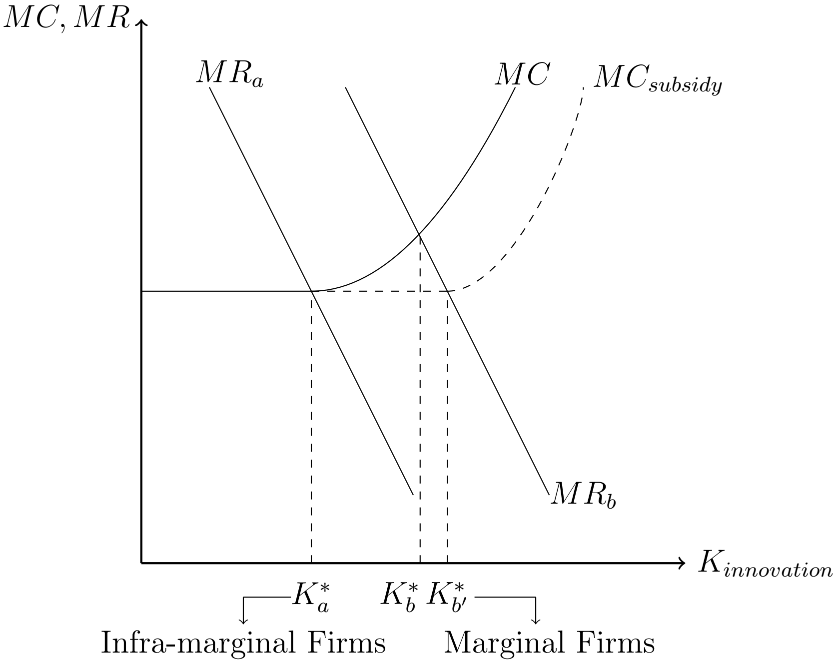


Figure 4.2: Regional Program Timeline

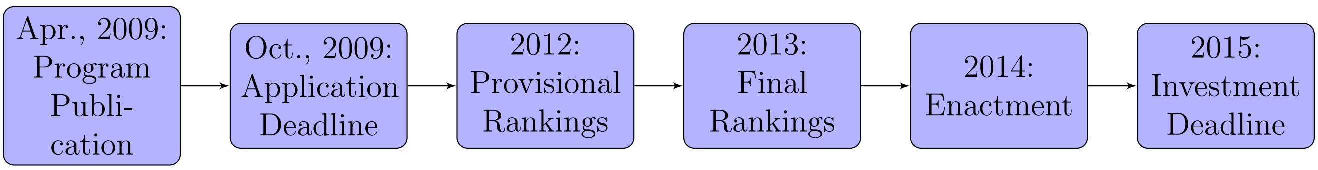


Figure 4.3: Treatment Probability as function of Score

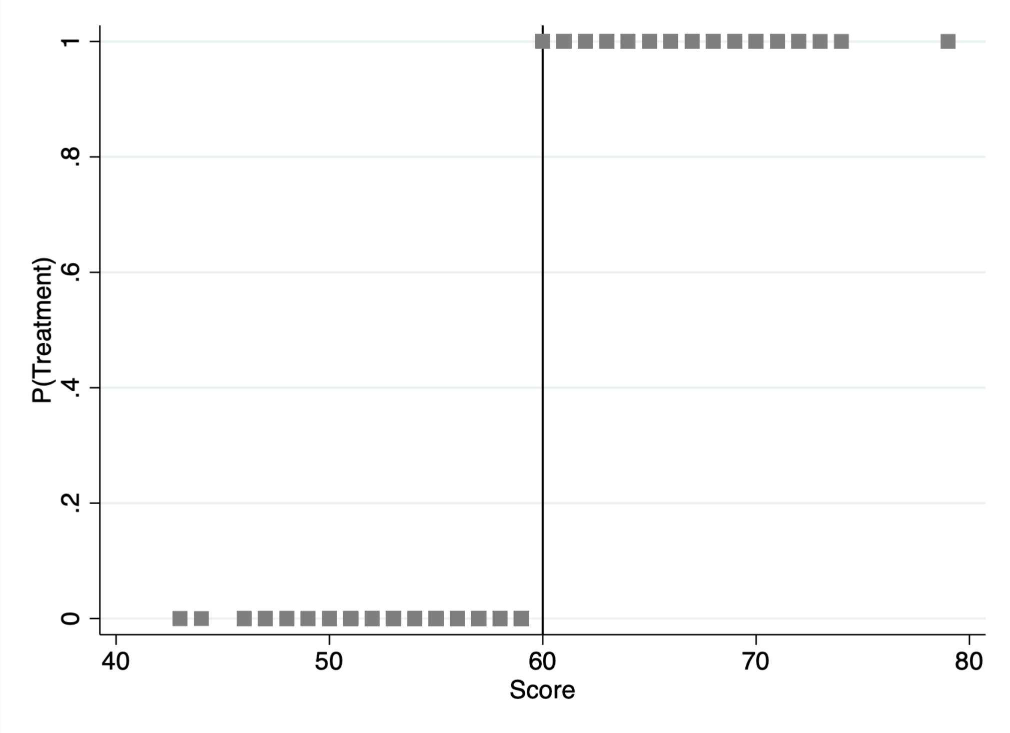


Figure 4.4: Firms' Density Distribution by Score

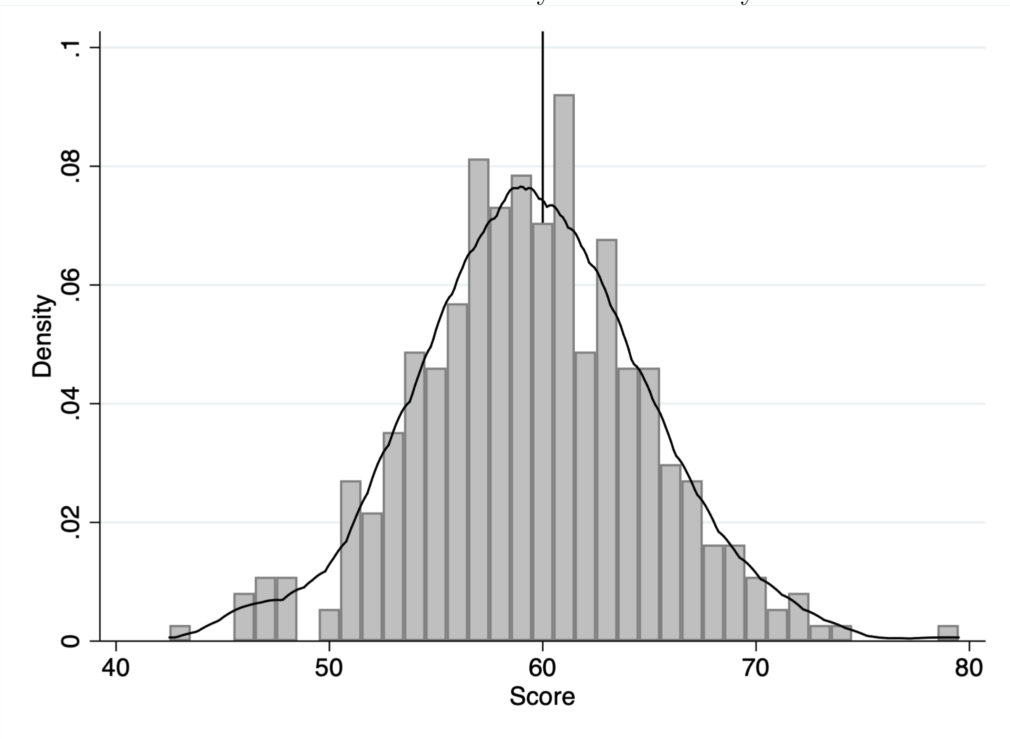


Figure 4.5: Firms' Investment (scaled by TA)

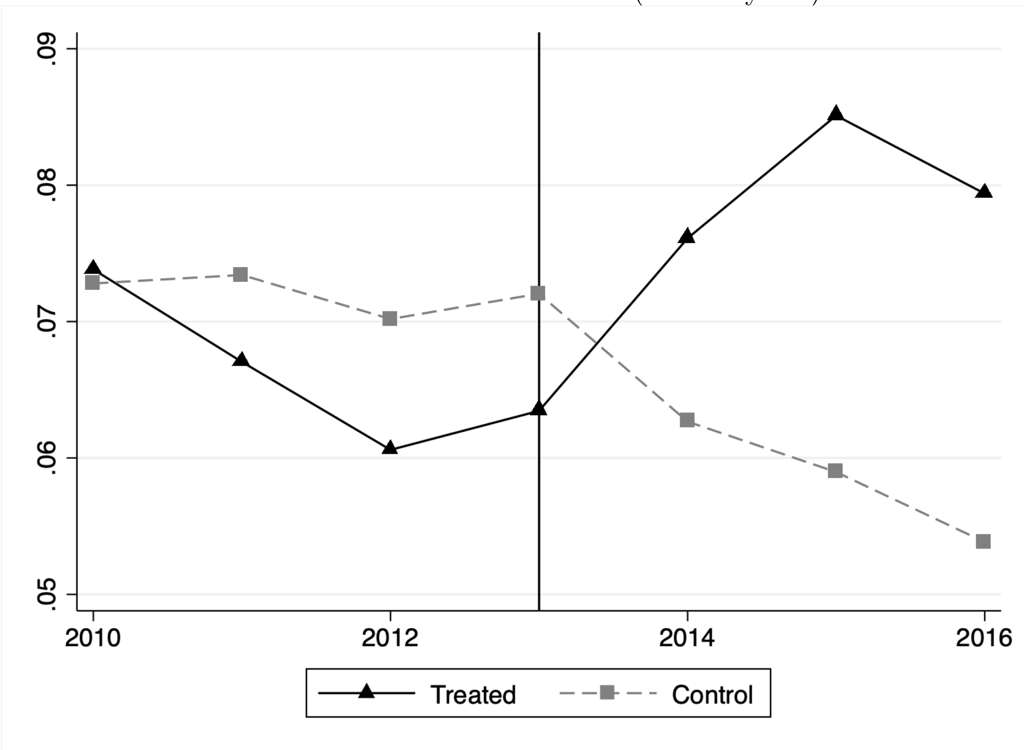


Figure 4.6: Event Study Estimates (Year Relative to 2013)

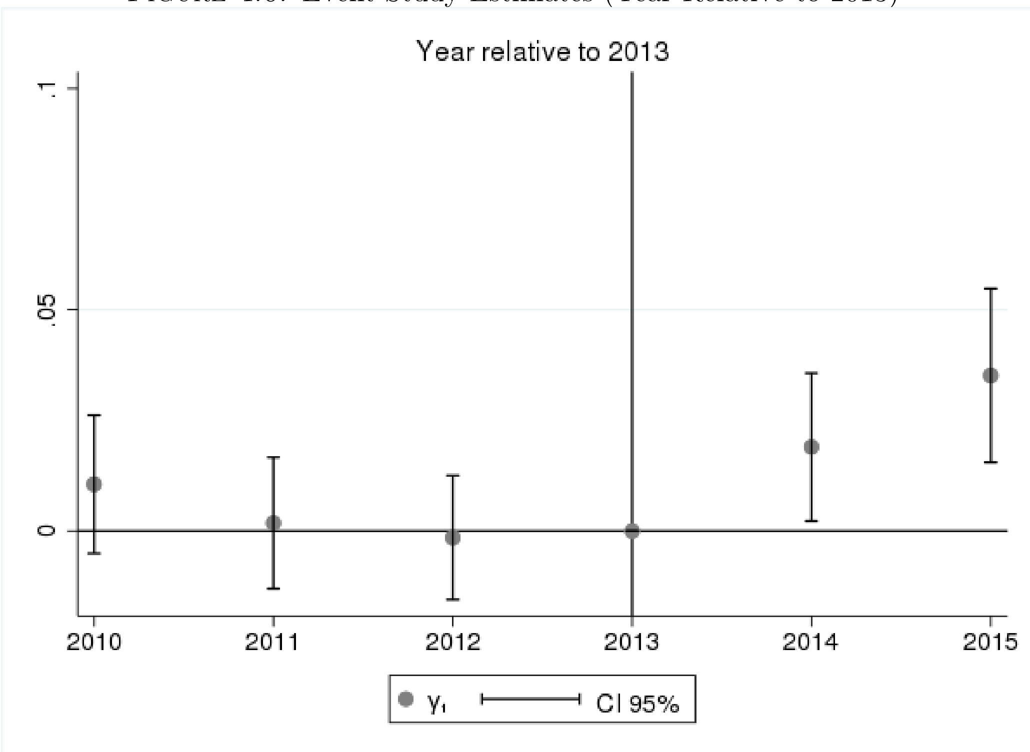


Table 4.1: Investment Programs Evaluation and Criteria

Criteria	**Score**
Project Quality and Innovation	*max* 60 pts
Competitiveness and Impact on Product/Service	*max* 30 pts
Youth and Female SMEs	*max* 10 pts
Total	100/100
min Score ($\rightarrow$ subsidy)	60/100
Final Ranking Results:	
Treated Firms ($score \geq 60$)	299
Control Firms ($score < 60$)	424

Notes: Assessment of innovative projects are carried out by a Technical Commission appointed by the Regional Government according to the general principles for the research evaluation specified by the Ministry of Education, University and Research of the Italian Government and the general principles of the European Commission

Table 4.2: Subsidy Summary Statistics

	Mean	**SD**	**min**	**Max**
$Subsidy_{i2014}$ (%)	43	6.78	20.8	50
$Subsidy_{i2014}$	79979.8	50282.87	6871.5	200000
$Subsidy_{i2014}/TA_{i2013}$ (%)	8.47	15.87	.147	125.6
$K_{i2015} - K_{i2013}$	107663	566072.6	-2242584	4891820
$K^{treat}_{i2015} - K^{treat}_{i2013}$	167552.5	630400.5	-1065815	4891820
$\bar{K}^{treat}_{i2014,2015} - K^{treat}_{i2013}$	123039.9	436392.1	-775036	3210333

Notes: all the figures are expressed in units of Euro, except those with the symbol %.

TABLE 4.3: Data Retrieved from AIDA DB

	Not Admitted to Evaluation (N=1354)			**Admitted to Evaluation** (N=820)								
				Non Recipients *score* < 60 (N=424)			*Recipients:* *score* ≥ 60 (N=299)			*Renunciations:* *score* ≥ 60 (N=97)		
	(**A**) Recorded in AIDA	(**B**) Bankruptcy Liquidation, Dissolution	(**C**) No Longer recorded in AIDA	(**A**) Recorded in AIDA	(**B**) Bankruptcy, Liquidation, Dissolution	(**C**) No Longer recorded in AIDA	(**A**) Recorded in AIDA	(**B**) Bankruptcy Liquidation, Dissolution	(**C**) No Longer recorded in AIDA	(**A**) Recorded in AIDA	(**B**) Bankruptcy Liquidation, Dissolution	(**C**) No Longer recorded in AIDA
31/12/2009	838	13	11	313	1	2	232	0	0	74	0	0
31/12/2013	683	108	47	251	45	17	220	4	7	46	23	5
31/12/2016	521	84	78	188	35	28	185	19	17	24	13	9

Notes: At 31/12/2009, 2174 applications were received for participation in the call for tenders announced by the *Campania* Region for the allocation of capital grants (subsidies) for the implementation of innovative investments by local **SMEs**. Of these, 1354 applications were excluded from the subsequent phase of project evaluation, as these companies were non-compliant with program's regulation due to the lack of presentation of some preliminary documents for the participation of the tender. 820, instead, were the proposals admitted to the evaluation phase, which took place between 2011 and 2013. The values included in the table refer to the availability in the AIDA database of the last deposited balance sheet: at the conclusion of the submissions of the applications for participation, at the time of publication of the final ranking of participants (*Eligible assignees and Eligible non-assignees*), one year from the deadline for the realization of investments. In particular, starting from the year 31/12/2013 and summing the columns (**A**, Recorded in AIDA), (**B**, Bankruptcy, Liquidation, Dissolution) and (**C**, No longer registered in AIDA) we obtain the number of enterprises *Recorded in AIDA* (column "**A**") referring to the previous period indicated in the table. For example, for ***Not admitted to evaluation*** as of 31/12/2009 these are 838; adding the line referring to 31/12/2013, i.e. $683 + 108 + 47$ we obtain the number of companies registered in AIDA at 31/12/2009 for the relative *sub-group*. The number of reported BS is reduced over time due to the modification of the legal *status* of the participating companies (*declaration of bankruptcy, dissolution, entry into liquidation*) or, although still active, no longer recorded by AIDA.

Table 4.4: Summary Statistics: 2008

	Mean of Control	**Obs.**	**Mean of Treatment**	**Obs.**	Δ
K_{it}/TA_{it}	0.07	171	0.07	171	0
$Leverage_{it}$	8.24	171	9.28	171	-1.04
Cost of $Debt_{it}$	8.41	77	8.95	82	-0.54
ROA	4.90	171	7.75	171	-2.85*
$Sales_{it}/TA_{it}$	1.10	171	1.26	171	-0.16
$EBITDA_{it}/TA_{it}$	0.09	171	0.12	171	-0.02
CF_{it}/TA_{it}	0.05	171	0.07	171	-0.02
$Liquidity_{it}/TA_{it}$	0.11	171	0.11	171	0
No. of Workers	32.44	142	20.96	141	11.48

Notes: Statistical Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.5: Summary Statistics: 2010-2013

	Mean of Control	**Obs.**	**Mean of Treatment**	**Obs.**	Δ
K_{it}/TA_{it}	0.073	727	0.068	731	0.005
$Leverage_{it}$	8.199	724	7.092	728	1.107
Cost of $Debt_{it}$	7.805	399	7.683	442	0.122
ROA	3.961	727	5.440	728	-1.479**
$Sales_{it}/TA_{it}$	0.929	727	1.123	731	-0.193***
$EBITDA_{it}/TA_{it}$	0.078	724	0.090	728	-0.013*
CF_{it}/TA_{it}	0.045	724	0.055	728	-0.010*
$Liquidity_{it}/TA_{it}$	0.078	724	0.087	731	-0.008
No. of Workers	26.758	683	23.318	688	3.440

Notes: Statistical Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.6: Firms Composition by Size per Year

	Freq.	%	**Cum.** %	**Treated**	**Control**
Micro	203	55.31	55.31	90	113
Small	128	34.88	90.19	68	60
Medium-Large	36	9.8	100	23	13
Total	367	100		181	186

Notes: Firms size decomposition follows the definition given by the EU Commission. In particular, I consider micro-sized firms those up to 10 workers on average during the pre-*treatment* period (2010-2013), small-sized those between 11 and 50 workers on average, medium-sized those between 51 and 250 and large-sized those with more than 250 workers on average.

Table 4.7: Naive Participation Eq.: Treatment Status (2013) and Observable Charact. (2008)

	(1)	(2)	(3)	(4)	(5)
K_{i2008}/TA_{i2008}	0.0078				-0.0299
	(0.6249)				(0.6309)
$Sales_{i2008}/TA_{i2008}$		0.0980			0.0916
		(0.0673)			(0.0685)
CF_{i2008}/TA_{i2008}			1.0796		0.9758
			(0.7110)		(0.7223)
$Liquidity_{i2008}/TA_{i2008}$				-0.0799	-0.2880
				(0.4671)	(0.4703)
Constant	-0.0005	-0.1155	-0.0681	0.0085	-0.1368
	(0.0798)	(0.1037)	(0.0804)	(0.0842)	(0.1247)
Obs.	342	342	342	342	342
Pseudo R^2	0.0000	0.0045	0.0050	0.0001	0.0089

Notes: Probit estimates of Treatment Status ($Treat_i$; known in 2013) on investment and some liquidity measures observed in 2008 (as in the program economic sustainability requirement). Robust standard errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.8: Correlation Score (2013) and Observables (2008)

	(1)	(2)	(3)	(4)	(5)
K_{i2008}/TA_{i2008}	-1.5697				-1.9381
	(2.7637)				(2.7558)
$Sales_{i2008}/TA_{i2008}$		0.6688^{*}			0.6599^{*}
		(0.2987)			(0.2682)
CF_{i2008}/TA_{i2008}			6.3115^{*}		6.0158^{+}
			(3.1496)		(3.1350)
$Liquidity_{i2008}/TA_{i2008}$				-2.5720	-4.0220^{+}
				(2.4371)	(2.3667)
Constant	59.7137^{***}	58.8174^{***}	59.2139^{***}	59.8820^{***}	59.0105^{***}
	(0.3312)	(0.4653)	(0.3341)	(0.3844)	(0.5408)
Obs.	342	342	342	342	342
R^2	0.0010	0.0169	0.0134	0.0048	0.0382

Notes: The dependent variable is given by the Score obtained after the projects' technical evaluation in 2013. Score > 60 implies that the firm is entitled to obtain the subsidy. Robust standard errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.9: Subsidy Impact on K_{it}/TA_{it} during 2010-2015 using Eq. (4.2)

	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	-0.00587	0.00068	-0.00304	0.00207	-0.00143	0.00609
	(0.00659)	(0.00660)	(0.00629)	(0.00658)	(0.00628)	(0.00603)
$Post_t$	-0.01130	-0.01389	-0.00172	-0.02521*	-0.01606	0.01190
	(0.00697)	(0.00920)	(0.00963)	(0.01219)	(0.01237)	(0.01108)
$Treat_i * Post_t$	0.02565*	0.02565**	0.02814**	0.02315*	0.02494*	0.02135*
	(0.01039)	(0.00969)	(0.00960)	(0.00978)	(0.00969)	(0.00919)
CF_{it}/TA_{it} (2010-2013)			0.38795***		0.46283***	
			(0.07379)		(0.07405)	
CF_{it}/TA_{it} (2010-2013) * $Post_t$			-0.26712**		-0.31729***	
			(0.09023)		(0.08983)	
$Sales_{it}/TA_{it}$ (2010-2013)				-0.01305**	-0.02378***	
				(0.00496)	(0.00505)	
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$				0.01218	0.01786*	
				(0.00821)	(0.00847)	
ROA_{it} (2010-2013)						-1.77309***
						(0.17148)
ROA_{it} (2010-2013) * $Post_t$						0.96832***
						(0.22139)
$EBITDA_{it}/TA_{it}$ (2010-2013)						1.51332***
						(0.14702)
$EBITDA_{it}/TA_{it}$ (2010-2013) * $Post_t$						-0.81135***
						(0.19572)
Constant	0.07211***	0.14444***	0.13094***	0.15150***	0.14037***	0.07320***
	(0.00479)	(0.02227)	(0.02156)	(0.02207)	(0.02099)	(0.02047)
Year Dummies	No	Yes	Yes	Yes	Yes	Yes
NACE 2 Dummies	No	Yes	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes	Yes
Obs.	2,166	2,166	2,166	2166	2,166	2,166
R^2	0.00273	0.19455	0.21374	0.19758	0.22284	0.28824
$K_{2013}^{Treated}/TA_{2013} =$			.0634549			
Estimated Elasticity (s=0.5)	-.8	-.8	-.9	-.8	-.8	-.7
Estimated Elasticity ($\bar{s} = 0.43$)	-.9	-.9	-1	-.9	-.9	-.8

Notes: The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. Furthermore, in each period, control and treated groups are perfectly balanced with a sample of 181 firms by group. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD} * \bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.10: Placebo Subsidy Impact on K_{it}/TA_{it} during 2010-2013

	(1)	(2)	(3)
$Treat_i$	-0.0027	0.0070	0.0052
	(0.0094)	(0.0092)	(0.0090)
$Placebo_t$	-0.0020	-0.0015	-0.0054
	(0.0096)	(0.0106)	(0.0137)
$Treat_i$*$Placebo_t$	-0.0064	-0.0071	-0.0075
	(0.0132)	(0.0118)	(0.0116)
$Sales_{it}/TA_{it}$ (2010-2011)			-0.0223***
			(0.0052)
$Sales_{it}/TA_{it}$ (2010-2013)* $Placebo_t$			0.0085
			(0.0075)
CF_{it}/TA_{it} (2010-2011)			0.3006***
			(0.0741)
CF_{it}/TA_{it} (2010-2011) * $Placebo_t$			-0.0908
			(0.0868)
Constant	0.0731***	0.1357***	0.1306***
	(0.0067)	(0.0281)	(0.0273)
Year Dummies	No	Yes	Yes
NACE 2 Dummies	No	Yes	Yes
Province Dummies	No	Yes	Yes
Legal Form Dummies	No	Yes	Yes
Obs.	1,438	1,438	1,438
R^2	0.0012	0.2413	0.2625

Notes: The *placebo* analysis covers the pre-treatment period only. In particular, to check the robustness of the main specification I design a fake treatment that goes in place during 2012-2013, allowing for a 2 years window investment as in the real program. Robust Standard Errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.11: Summary Statistics by Firms Size, 2010-2013

	Mean of Control	**Obs.**	**Mean of Treatment**	**Obs.**	Δ
			(A): **Micro-sized Firms**		
K_{it}/TA_{it}	0.08	433	0.07	361	0.00
$Cost\ of\ Debt_{it}$	8.39	192	9.08	166	-0.69
$Leverage_{it}$	8.05	430	6.48	361	1.56
ROA	3.18	433	6.21	361	-3.03***
$Sales_{it}/TA_{it}$	0.80	433	1.16	361	-0.36***
$Ebitda_{it}/TA_{it}$	0.07	430	0.10	361	-0.03***
CF_{it}/TA_{it}	0.04	430	0.06	361	-0.02**
$Liquidity_{it}/TA_{it}$	0.09	430	0.12	361	-0.03**
No. of Workers	3.14	406	4.38	338	-1.24***
			(B): **Small-sized Firms**		
K_{it}/TA_{it}	0.07	242	0.06	278	0.01
$Cost\ of\ Debt_{it}$	7.27	170	6.82	208	0.46
$Leverage_{it}$	8.62	242	7.93	275	0.69
ROA	5.99	242	4.59	275	1.41*
$Sales_{it}/TA_{it}$	1.12	242	1.08	278	0.04
$Ebitda_{it}/TA_{it}$	0.10	242	0.08	275	0.02**
CF_{it}/TA_{it}	0.06	242	0.05	275	0.01*
$Liquidity_{it}/TA_{it}$	0.06	242	0.06	278	0.01
No. of Workers	25.06	227	23.45	261	1.61
			(C): **Medium/Large-sized Firms**		
K_{it}/TA_{it}	0.05	52	0.06	96	-0.01
$Cost\ of\ Debt_{it}$	7.20	37	6.97	72	0.23
$Leverage_{it}$	7.48	52	6.85	96	0.64
ROA	1.00	52	5.00	96	-3.99*
$Sales_{it}/TA_{it}$	1.10	52	1.07	96	0.03
$Ebitda_{it}/TA_{it}$	0.04	52	0.08	96	-0.05**
CF_{it}/TA_{it}	0.01	52	0.05	96	-0.04**
$Liquidity_{it}/TA_{it}$	0.07	52	0.06	96	0.01
No. of Workers	226.22	50	92.97	93	133.25

Notes: Statistical Significance denoted as follows: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.12: Subsidy Impact on Kit/TA_{it} for **Micro-Sized** Enterprises

	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	-0.00380	0.00230	0.00669	-0.00339	0.00529	0.00408
	(0.01048)	(0.01033)	(0.01044)	(0.00976)	(0.01042)	(0.00975)
$Post_t$	-0.02096*	-0.02470^{+}	-0.04399**	-0.00715	-0.02900*	-0.03460*
	(0.01004)	(0.01324)	(0.01698)	(0.01356)	(0.01445)	(0.01675)
$Treat_i * Post_t$	0.02980^{+}	0.02907^{+}	0.02049	0.03798*	0.02742^{+}	0.02614^{+}
	(0.01591)	(0.01503)	(0.01554)	(0.01488)	(0.01524)	(0.01515)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.02149**			-0.02609***
			(0.00698)			(0.00787)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.02381^{+}			0.03622**
			(0.01268)			(0.01328)
CF_{it}/TA_{it} (2010-2013)				0.51856***		0.65347***
				(0.10773)		(0.11002)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.44915***		-0.57772***
				(0.12697)		(0.13061)
$Liquidity_{it}/TA_{it}$ (2010-2013)					-0.16539***	-0.17286***
					(0.04195)	(0.04821)
$Liquidity_{it}/TA_{it}$ (2010-2013) * $Post_t$					0.05065	0.03761
					(0.05561)	(0.05821)
Constant	0.07649***	0.05707*	0.08803**	0.02880	0.05770*	0.05692^{+}
	(0.00730)	(0.02493)	(0.02796)	(0.02477)	(0.02569)	(0.03021)
Year Dummies	No	Yes	Yes	Yes	Yes	Yes
NACE2 Dummies	No	Yes	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes	Yes
Obs.	1,189	1,189	1,189	1,189	1,189	1,189
R^2	0.00378	0.21308	0.21988	0.23989	0.22517	0.26857
$\bar{K}_{2013}^{Micro-Treated}/TA_{2013} =$				.0629443		
Estimated Elasticity (s=0.5):	-.9	-.9	-	-1.2	-.9	-.9
Estimated Elasticity ($\bar{s} = 0.43$):	-1.1	-1.1	-	-1.4	-1	-1

Notes: The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.13: Subsidy Impact on Kit/TA_{it}: **Small-Sized** Enterprises

	(1)	(2)	(3)
$Treat_i$	-0.00828	-0.00237	-0.00149
	(0.00829)	(0.00876)	(0.00863)
$Post_t$	0.00086	0.00057	0.00009
	(0.00978)	(0.01242)	(0.01425)
$Treat_i * Post_t$	0.02251	0.02350^{+}	0.02346^{+}
	(0.01500)	(0.01235)	(0.01252)
CF_{it}/TA_{it} (2010-2013)			0.11889
			(0.11041)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			0.00923
			(0.12948)
Constant	0.06833^{***}	0.02389	0.02493
	(0.00549)	(0.02800)	(0.02825)
Year Dummies	No	Yes	Yes
NACE2 Dummies	No	Yes	Yes
Legal Form Dummies	No	Yes	Yes
Province Dummies	No	Yes	Yes
Obs.	765	765	765
R^2	0.00697	0.37885	0.38104
$\bar{K}_{2013}^{Small-Treated}/TA_{2013} =$		.0656127	
Estimated Elasticity (s=0.5):	-	-.7	-.7
Estimated Elasticity ($\bar{s} = 0.43$):	-	-.8	-.8

Notes: The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.14: Subsidy Impact on Kit/TA_{it} for **Medium/Large-Sized** Enterprises

	(1)	(2)	(3)
$Treat_i$	0.00497	0.02233	0.03166*
	(0.01185)	(0.01481)	(0.01434)
$Post_t$	0.01494	0.01227	0.01212
	(0.01837)	(0.01706)	(0.01543)
$Treat_i * Post_t$	-0.00243	-0.00243	-0.00311
	(0.02355)	(0.01626)	(0.01826)
CF_{it}/TA_{it} (2010-2013)			0.55813***
			(0.10771)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			0.01573
			(0.13042)
Constant	0.05259***	-0.05446+	-0.06739*
	(0.00922)	(0.03279)	(0.02805)
Year Dummies	No	Yes	Yes
NACE2 Dummies	No	Yes	Yes
Legal Form Dummies	No	Yes	Yes
Province Dummies	No	Yes	Yes
Obs.	216	216	216
R^2	0.00796	0.70049	0.73081
Estimated Elasticity (s=0.5):	-	-	-
Estimated Elasticity ($\bar{s} = 0.43$):	-	-	-

Notes: The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.15: High Tech Nace-Ateco2007 2-digits Sectors

2-digits code	Description	Type
26	Electronics and components	M
27	Electric devices	M
28	Machines and mechanical devices	M
29	Automotive	M
30	Means of transport	M
32	Medical devices	M
33	Maintenance of machines	M
35	Energy	M
58,62,63	Information and communication services	S
64,65	Financial services	S
66	SME auxiliary financial services	S
68-71	Consultancy	S
72	Research and development	S
73,74	Market research and other professional activities	S
77,78,80-82	Firms' services	S
85	Education	S

Notes: Type: M stands for Manufacturing sector; S for Service sector. Source: Firms technology intensity by sector based on ATECO 2007 2-digits classification according to technology intensity across economic sectors available at: https://www.istat.it/it/files//2017/08/GlossarioNotaMetodologica.pdf.

Table 4.16: Low Tech Nace-Ateco2007 2-digits Sectors

2-digits code	Description	Type
10,11	Foods, beverage and tobacco	M
14,15	Clothing and apparel	M
17	Paper products	M
18	Printed products	M
22	Articles in gum and plastic	M
23	Glass and concrete products	M
24	Iron and steel industry	M
25	Metallic constructions	M
38	Water and environment industry	M
41-43	Construction sector	M
45	Automotive commercial services	S
46	Wholesale services	S
47	Retailing services	S
49	Terrestrial transport services	S
52	Logistic services	S
55	Catering services	S
56	Hotel and tourism industry	S
79	Tourism services	S
86	Healthcare services	S
88	Non-residential social assistance	S
90	Entertainment services	S
93	Sport activities and services	S

Notes: Type: M stands for Manufacturing sector; S for Service sector. Source: Firms technology intensity by sector based on ATECO 2007 2-digits classification according to technology intensity across economic sectors available at: https://www.istat.it/it/files//2017/08/GlossarioNotaMetodologica.pdf.

Table 4.17: Firms Composition by Technology Intensity per Year

	Freq.	%	**Cum.** %	**Treated**	**Control**
High Tech.	169	45.8	45.8	76	93
Low Tech.	200	54.3	100	106	94
Total	369	100		182	187

Notes: High tech and low tech sectors are defined according Tables 4.15 and 4.16, respectively.

Table 4.18: Summary Stats by Technology Intensity during 2010-2013

	Mean of Control	**Obs.**	**Mean of Treatment**	**Obs.**	Δ	**Mean of Control**	**Obs.**	**Mean of Treatment**	**Obs.**	Δ
			(A): **High Tech.**					(B): **Low Tech.**		
K_{it}/TA_{it}	0.07	361	0.07	299	0	0.07	362	0.06	424	0.01
$Leverage_{it}$	8.01	358	6.97	299	1.03	8.41	362	7.21	424	1.20
$Cost\ of\ Debt_{it}$	8.28	180	8.37	140	-0.09	7.48	215	7.38	298	0.10
ROA	3.57	361	6.54	299	-2.97^{***}	4.35	362	4.65	424	-0.30
$Sales_{it}/TA_{it}$	0.75	361	1.02	299	-0.27^{***}	1.11	362	1.20	424	-0.09
$EBITDA_{it}/TA_{it}$	0.07	358	0.10	299	-0.03^{***}	0.08	362	0.08	424	0
CF_{it}/TA_{it}	0.04	358	0.06	299	-0.02^{**}	0.05	362	0.05	424	0
$Liquidity_{it}/TA_{it}$	0.08	358	0.11	299	-0.02^{*}	0.08	362	0.07	424	0.01
$No.\ of\ Workers$	31.91	337	11.33	273	20.57	21.43	342	30.71	410	-9.29^{**}

Notes: Statistical significance: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.19: Subsidy Impact on K_{it}/TA_{it} by High Technology Intensity

	High Intensity Tech.				
	(1)	(2)	(3)	(4)	(5)
$Treat_i$	-0.0040	-0.0031	-0.0012	-0.0084	-0.0051
	(0.0103)	(0.0107)	(0.0109)	(0.0103)	(0.0104)
$Post_t$	-0.0139	-0.0164	-0.0300^{+}	-0.0075	-0.0244
	(0.0103)	(0.0137)	(0.0178)	(0.0144)	(0.0180)
$Treat_i * Post_t$	0.0316^{+}	0.0329*	0.0282^{+}	0.0377*	0.0321^{+}
	(0.0170)	(0.0159)	(0.0166)	(0.0158)	(0.0164)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0074		-0.0220^{+}
			(0.0108)		(0.0123)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0179		0.0265^{+}
			(0.0145)		(0.0159)
CF_{it}/TA_{it} (2010-2013)				0.3533***	0.4163***
				(0.0921)	(0.1031)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.2169^{+}	-0.2953*
				(0.1238)	(0.1291)
Constant	0.0731***	0.3002***	0.3047***	0.2911***	0.2954***
	(0.0072)	(0.0634)	(0.0636)	(0.0652)	(0.0652)
Year Dummies	No	Yes	Yes	Yes	Yes
NACE2 Dummies	No	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes
Obs.	992	992	992	992	992
R^2	0.0040	0.1659	0.1676	0.1822	0.1878
$\bar{K}_{2013}^{HT-Treated}/TA_{2013} =$			.0658529		
Estim. Elasticity (s=0.5)	-1	-1	-.9	-1.1	-1
Estim. Elasticity ($\bar{s} = 0.43$)	-1.1	-1.1	-1	-1.3	-1.1

Notes: High tech firms are defined according to Table 4.15. Robust Standard Errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.20: Subsidy Impact on K_{it}/TA_{it} by Low Technology Intensity

	Low Intensity Tech.	
	(1)	(2)
$Treat_i$	-0.0069	0.0074
	(0.0085)	(0.0087)
$Post_t$	-0.0087	-0.0117
	(0.0094)	(0.0122)
$Treat_i * Post_t$	0.0207	0.0196^{+}
	(0.0088)	(0.0118)
Constant	0.0711^{***}	0.1148^{***}
	(0.0063)	(0.0194)
Year Dummies	No	Yes
NACE2 Dummies	No	Yes
Legal Form Dummies	No	Yes
Province Dummies	No	Yes
Obs.	1,174	1,174
R^2	0.002	0.2637
$\bar{K}_{2013}^{LT-Treated}/TA_{2013} =$	.0617256	
Estim. Elasticity (s=0.5)		-.6
Estim. Elasticity ($\bar{s} = 0.43$)		-.7

Notes: Low tech firms are defined according to Table 4.16. Robust Standard Errors in parenthesis. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.21: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity only Manufacturing Sector

	High Intensity Tech.		**Low Intensity Tech.**	
	(1)	(2)	(3)	(4)
$Treat_i$	0.0011	0.0612**	-0.0047	0.0039
	(0.0308)	(0.0221)	(0.0095)	(0.0081)
$Post_t$	-0.0374	-0.0336	0.0064	0.0051
	(0.0264)	(0.0325)	(0.0128)	(0.0161)
$Treat_i * Post_t$	0.0558	0.0558	0.0225	0.0235
	(0.0393)	(0.0362)	(0.0176)	(0.0146)
Constant	0.0759**	0.2487***	0.0613***	0.1152***
	(0.0245)	(0.0667)	(0.0072)	(0.0220)
Year Dummies	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes
Obs.	198	198	473	473
R^2	0.0121	0.3937	0.0129	0.3368

Notes: High-tech and Low-tech manufacturing firms are defined according Tables 4.15 and 4.16, respectively, with Type=M. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.22: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity only Service Sector

	High Intensity Tech.		**Low Intensity Tech.**	
	(1)	(2)	(3)	(4)
$Treat_i$	-0.0125	-0.0258*	-0.0091	0.0184
	(0.0103)	(0.0118)	(0.0130)	(0.0134)
$Post_t$	-0.0137	-0.0138	-0.0216	-0.0255
	(0.0109)	(0.0142)	(0.0136)	(0.0171)
$Treat_i * Post_t$	0.0295	0.0301^{+}	0.0231	0.0198
	(0.0181)	(0.0172)	(0.0188)	(0.0170)
Constant	0.0739***	0.2421***	0.0794***	-0.1107***
	(0.0070)	(0.0379)	(0.0098)	(0.0307)
Year Dummies	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes
Obs.	772	772	684	684
R^2	0.0037	0.1524	0.0029	0.2870
$\bar{K}^{Treated}_{2013}/TA_{2013}$:	.0529614		.0638714	
Estimated Elasticity (s=0.5)	-1.1			
Estimated Elasticity ($\bar{s} = 0.43$)	-1.3			

Notes: High-tech and Low-tech manufacturing firms are defined according Tables 4.15 and 4.16, respectively, with Type=S. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}^{Treated}_{2013}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.23: Input-Additionality HP: Test on Difference-in-Differences Estimates

$\hat{\beta}^{DD}$	SE($\hat{\beta}^{DD}$)	t_0	df	P(t>t_0)	IAH
A: Estimates Table 4.9; threshold=.04892; $\alpha = .05$					
.02565	.1039	-2.24	2162	.9874	No
.02565	.00969	-2.24	2090	.9874	No
.02814	.00960	-2.16	2088	.9946	No
.02315	.00978	-2.63	2088	.9957	No
.02494	.00969	-2.47	2086	.9932	No
.02135	.00919	-3	2086	.9886	No
B: Estimates Table 4.12; threshold=.08233; $\alpha = .05$					
.0298	.01591	-3.30	1185	.9995	No
.02907	.01503	-3.54	1153	.9998	No
.03798	.01488	-2.98	1133	.9985	No
.02742	.01524	-3.6	1133	.9998	No
.02614	.01515	-3.71	1129	.9998	No
C: Estimates Table 4.13; threshold=.01647; $\alpha = .05$					
.0235	.01235	.57	704	.2844	No
.02346	.01252	.56	702	.2878	No
D: Estimates Table 4.19; threshold=.05165; $\alpha = .05$					
.0316	.017	-1.18	988	.8809	No
.0329	.0159	-1.18	947	.8806	No
.0282	.0166	-1.41	945	.9206	No
.0377	.0158	-.88	945	.8105	No
.0321	.0164	-1.19	943	.8828	No
E:Estimates Table 4.20; threshold=.03937; $\alpha = .05$					
.0196	.0118	-1.68	1125	.9533	No
F1: Estimates Table 4.21; threshold=.02511; $\alpha = .05$					
.0558	.0336	.91	159	.1821	No
F2: Estimates Table 4.21; threshold=.01451; $\alpha = .05$					
.02	.0091	.6	387	.2744	No
G: Estimates Table 4.22; threshold=.065; $\alpha = .05$					
.0301	.0172	-2.03	736	.9786	No
.0301	.0131	-2.66	634	.996	No

Notes: One-sided t-tests where the null hypothesis (H_0) is $\hat{\beta}^{DD} \leq$ threshold (no additionality), whereas the alternative hypothesis (H_1) is $\hat{\beta}^{DD} >$ threshold (additionality holds). Threshold is given by the average value of the ratio between the subsidy (obtained by treated firms, only) and Total Assets over the period 2014-2015. In order to construct this ratio, I made the following assumption: I assumed that firms spent half of the public incentive in each year, that is half in 2014 and half in 2015 (to be also consistent with the investment program). The decision rule is the following: if P(t>t_0) < α, then reject H_0 at the given significance level. Only statistically significant coefficients are taken into account. IAH stands for Input-Additionality Hypothesis.

Table 4.24: Robustness: Whole Sample

	(1)	(2)	(3)	(4)	(5)
			Score: [53-66]		
$Treat_i$	-0.0048	-0.0027	-0.0023	-0.0079	-0.0078
	(0.0066)	(0.0069)	(0.0068)	(0.0064)	(0.0064)
$Post_t$	-0.0046	-0.0064	-0.0131	0.0053	-0.0044
	(0.0073)	(0.0099)	(0.0130)	(0.0101)	(0.0131)
$Treat_i * Post_t$	0.0249*	0.0250*	0.0239*	0.0278**	0.0263*
	(0.0111)	(0.0103)	(0.0104)	(0.0102)	(0.0103)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0049		-0.0149**
			(0.0052)		(0.0053)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0069		0.0113
			(0.0086)		(0.0088)
CF_{it}/TA_{it} (2010-2013)				0.3868***	0.4343***
				(0.0727)	(0.0728)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.2580**	-0.2860**
				(0.0933)	(0.0933)
Constant	0.0683***	0.1275***	0.1304***	0.1133***	0.1179***
	(0.0047)	(0.0245)	(0.0245)	(0.0236)	(0.0230)
Obs.	1804	1804	1804	1804	1804
R^2	0.0040	0.2103	0.2110	0.2326	0.2369
			Score: [56-64]		
$Treat_i$	0.0033	0.0035	0.0037	0.0011	0.0010
	(0.0071)	(0.0071)	(0.0071)	(0.0067)	(0.0067)
$Post_t$	-0.0027	-0.0005	-0.0040	0.0103	0.0036
	(0.0079)	(0.0101)	(0.0138)	(0.0103)	(0.0139)
$Treat_i * Post_t$	0.0245*	0.0245*	0.0238*	0.0262*	0.0250*
	(0.0124)	(0.0113)	(0.0114)	(0.0113)	(0.0113)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0063		-0.0146**
			(0.0054)		(0.0051)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0035		0.0075
			(0.0091)		(0.0092)
CF_{it}/TA_{it} (2010-2013)				0.2738***	0.3239***
				(0.0811)	(0.0781)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.2350*	-0.2534**
				(0.0948)	(0.0936)
Constant	0.0602***	0.1793***	0.1805***	0.1776***	0.1803***
	(0.0049)	(0.0289)	(0.0288)	(0.0275)	(0.0270)
Obs.	1335	1335	1335	1335	1335
R^2	0.0078	0.2498	0.2507	0.2625	0.2668
			Score: [58-62]		
$Treat_i$	0.0047	0.0076	0.0077	0.0009	0.0010
	(0.0100)	(0.0120)	(0.0118)	(0.0108)	(0.0108)
$Post_t$	-0.0130	-0.0105	-0.0136	0.0035	-0.0076
	(0.0101)	(0.0127)	(0.0159)	(0.0131)	(0.0156)
$Treat_i * Post_t$	0.0242	0.0241+	0.0238+	0.0266+	0.0258+
	(0.0155)	(0.0139)	(0.0141)	(0.0138)	(0.0139)
$Sales_{it}/TA_{it}$ (2010-2013)			0.0005		-0.0139
			(0.0100)		(0.0094)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0028		0.0115
			(0.0076)		(0.0079)
CF_{it}/TA_{it} (2010-2013)				0.4501***	0.4924***
				(0.1044)	(0.1011)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.3395**	-0.3707**
				(0.1141)	(0.1176)
Constant	0.0586***	0.1062*	0.1073*	0.1354**	0.1417***
	(0.0071)	(0.0468)	(0.0468)	(0.0423)	(0.0423)
Obs.	793	793	793	793	793
R^2	0.0058	0.3170	0.3171	0.3463	0.3490
Year Dummies	No	Yes	Yes	Yes	Yes
NACE2 Dummies	No	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.25: Robustness: Micro-sized firms

	(1)	(2)	(3)	(4)	(5)	(6)
			Score: [53-66]			
$Treat_i$	-0.0039	-0.0057	-0.0032	-0.0122	-0.0009	-0.0042
	(0.0104)	(0.0113)	(0.0113)	(0.0103)	(0.0114)	(0.0103)
$Post_t$	-0.0087	-0.0108	-0.0237	0.0075	-0.0128	-0.0122
	(0.0108)	(0.0144)	(0.0181)	(0.0146)	(0.0155)	(0.0178)
$Treat_i * Post_t$	0.0291^{+}	0.0282^{+}	0.0225	0.0375^{*}	0.0267	0.0283^{+}
	(0.0171)	(0.0162)	(0.0170)	(0.0161)	(0.0165)	(0.0167)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0116^{+}			-0.0155^{+}
			(0.0070)			(0.0079)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0154			0.0259^{+}
			(0.0131)			(0.0136)
CF_{it}/TA_{it} (2010-2013)				0.5306^{***}		0.6302^{***}
				(0.1100)		(0.1124)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.4630^{***}		-0.5417^{***}
				(0.1387)		(0.1409)
$Liquidity_{it}/TA_{it}$ (2010-2013)					-0.1688^{***}	-0.1900^{***}
					(0.0459)	(0.0552)
$Liquidity_{it}/TA_{it}$ (2010-2013) * $Post_t$					0.0303	0.0197
					(0.0623)	(0.0644)
Constant	0.0688^{***}	0.0515^{*}	0.0662^{*}	0.0179	0.0427^{+}	0.0194
	(0.0072)	(0.0250)	(0.0281)	(0.0236)	(0.0252)	(0.0290)
Obs.	971	971	971	971	971	971
R^2	0.0037	0.2339	0.2367	0.2665	0.2471	0.2918
			Score: [56-64]			
$Treat_i$	0.0073	-0.0048	-0.0034	-0.0083	-0.0017	-0.0034
	(0.0111)	(0.0102)	(0.0103)	(0.0096)	(0.0104)	(0.0097)
$Post_t$	-0.0118	-0.0122	-0.0193	0.0015	-0.0183	-0.0128
	(0.0109)	(0.0135)	(0.0187)	(0.0137)	(0.0146)	(0.0187)
$Treat_i * Post_t$	0.0373^{*}	0.0371^{*}	0.0331^{+}	0.0440^{*}	0.0337^{+}	0.0358^{+}
	(0.0186)	(0.0175)	(0.0196)	(0.0173)	(0.0182)	(0.0193)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0038			-0.0040
			(0.0069)			(0.0069)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0087			0.0132
			(0.0163)			(0.0168)
CF_{it}/TA_{it} (2010-2013)				0.3979^{***}		0.4362^{***}
				(0.1127)		(0.1108)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.3565^{**}		-0.3997^{**}
				(0.1348)		(0.1382)
$Liquidity_{it}/TA_{it}$ (2010-2013)					-0.1432^{***}	-0.1606^{***}
					(0.0369)	(0.0449)
$Liquidity_{it}/TA_{it}$ (2010-2013) * $Post_t$					0.0764	0.0670
					(0.0586)	(0.0515)
Constant	0.0578^{***}	0.0655	0.0681	0.0537	0.0597	0.0486
	(0.0074)	(0.0503)	(0.0503)	(0.0495)	(0.0508)	(0.0500)
Obs.	728	728	728	728	728	728
R^2	0.0125	0.2742	0.2748	0.2957	0.2821	0.3078
			Score: [58-62]			
$Treat_i$	0.0171	0.0165	0.0222	0.0097	0.0262^{+}	0.0147
	(0.0171)	(0.0164)	(0.0145)	(0.0136)	(0.0147)	(0.0134)
$Post_t$	-0.0219	-0.0235	-0.0279	-0.0096	-0.0323^{+}	-0.0269
	(0.0147)	(0.0195)	(0.0227)	(0.0164)	(0.0176)	(0.0224)
$Treat_i * Post_t$	0.0245	0.0242	0.0227	0.0360	0.0191	0.0291
	(0.0255)	(0.0251)	(0.0251)	(0.0219)	(0.0232)	(0.0238)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0068			0.0005
			(0.0152)			(0.0142)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0057			0.0105
			(0.0175)			(0.0182)
CF_{it}/TA_{it} (2010-2013)				0.4818^{**}		0.5073^{**}
				(0.1571)		(0.1543)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.4775^{**}		-0.4909^{**}
				(0.1771)		(0.1799)
$Liquidity_{it}/TA_{it}$ (2010-2013)					-0.3521^{***}	-0.3595^{***}
					(0.0652)	(0.0653)
CF_{it}/TA_{it} (2010-2013) * $Post_t$					0.1444^{*}	0.1256^{+}
					(0.0716)	(0.0663)
Constant	0.0601^{***}	-0.1841^{***}	-0.2021^{**}	-0.1810^{**}	-0.2315^{***}	-0.2045^{***}
	(0.0106)	(0.0499)	(0.0654)	(0.0638)	(0.0607)	(0.0600)
Obs.	414	414	414	414	414	414
R^2	0.0113	0.1023	0.4671	0.4901	0.4955	0.5206
Year Dummies	No	Yes	Yes	Yes	Yes	Yes
NACE2 Dummies	No	Yes	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.26: Robustness: Small-sized firms

	(1)	(2)	(3)
		Score: [53-66]	
$Treat_i$	-0.0137	-0.0230**	-0.0234**
	(0.0089)	(0.0082)	(0.0082)
$Post_t$	-0.0038	-0.0015	-0.0012
	(0.0108)	(0.0131)	(0.0143)
$Treat_i * Post_t$	0.0281+	0.0276*	0.0276*
	(0.0163)	(0.0125)	(0.0125)
CF_{it}/TA_{it} (2010-2013)			-0.0875
			(0.1118)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			-0.0042
			(0.1184)
Constant	0.0742***	-0.0036	-0.0091
	(0.0063)	(0.0316)	(0.0318)
Obs.	651	651	651
R^2	0.0079	0.4376	0.4387
		Score: [56-64]	
$Treat_i$	-0.0117	0.0046	0.0040
	(0.0097)	(0.0079)	(0.0080)
$Post_t$	0.0037	0.0128	0.0220
	(0.0130)	(0.0141)	(0.0137)
$Treat_i * Post_t$	0.0163	0.0156	0.0129
	(0.0181)	(0.0142)	(0.0138)
CF_{it}/TA_{it} (2010-2013)			-0.1398
			(0.1185)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			-0.1322
			(0.0971)
Constant	0.0680***	0.0554	0.0435
	(0.0071)	(0.0349)	(0.0350)
Obs.	467	467	467
R^2	0.0074	0.4571	0.4636
		Score: [58-62]	
$Treat_i$	-0.0123	-0.0033	-0.0006
	(0.0124)	(0.0169)	(0.0166)
$Post_t$	-0.0141	-0.0057	0.0033
	(0.0133)	(0.0129)	(0.0142)
$Treat_i * Post_t$	0.0306	0.0280*	0.0254+
	(0.0187)	(0.0138)	(0.0139)
CF_{it}/TA_{it} (2010-2013)			0.1016
			(0.0891)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			-0.1315+
			(0.0699)
Constant	0.0601***	0.0836	0.0615
	(0.0105)	(0.0515)	(0.0542)
Obs.	289	289	289
R^2	0.0093	0.5068	0.5098
Year Dummies	No	Yes	Yes
NACE2 Dummies	No	Yes	Yes
Legal Form Dummies	No	Yes	Yes
Province Dummies	No	Yes	Yes

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.27: Robustness: High-Tech firms

	(1)	(2)	(3)	(4)	(5)
			Score: [53-66]		
$Treat_i$	-0.0111	-0.0164$^+$	-0.0162	-0.0206*	-0.0196*
	(0.0087)	(0.0098)	(0.0099)	(0.0093)	(0.0094)
$Post_t$	-0.0002	-0.0033	-0.0112	0.0056	-0.0060
	(0.0105)	(0.0142)	(0.0184)	(0.0145)	(0.0183)
$Treat_i * Post_t$	0.0319$^+$	0.0329*	0.0311$^+$	0.0364*	0.0338*
	(0.0172)	(0.0163)	(0.0168)	(0.0164)	(0.0168)
$Sales_{it}/TA_{it}$ (2010-2013)			0.0075		-0.0047
			(0.0109)		(0.0126)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0097		0.0171
			(0.0144)		(0.0157)
CF_{it}/TA_{it} (2010-2013)				0.2967***	0.3098***
				(0.0739)	(0.0858)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.1993$^+$	-0.2514*
				(0.1207)	(0.1273)
Constant	0.0627***	0.0811**	0.0843**	0.0625*	0.0667*
	(0.0063)	(0.0272)	(0.0276)	(0.0264)	(0.0267)
Obs.	797	797	797	797	797
R^2	0.0091	0.1448	0.1480	0.1624	0.1648
			Score: [56-64]		
$Treat_i$	-0.0182$^+$	-0.0192$^+$	-0.0193$^+$	-0.0203$^+$	-0.0203$^+$
	(0.0100)	(0.0111)	(0.0110)	(0.0109)	(0.0108)
$Post_t$	-0.0142	-0.0148	-0.0135	-0.0080	-0.0103
	(0.0118)	(0.0154)	(0.0195)	(0.0154)	(0.0191)
$Treat_i * Post_t$	0.0498**	0.0501**	0.0502**	0.0515**	0.0507**
	(0.0187)	(0.0179)	(0.0182)	(0.0180)	(0.0183)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0061		-0.0154
			(0.0107)		(0.0132)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			-0.0013		0.0033
			(0.0112)		(0.0121)
CF_{it}/TA_{it} (2010-2013)				0.1451*	0.1926*
				(0.0671)	(0.0837)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.1510	-0.1578
				(0.1032)	(0.1095)
Constant	0.0658***	0.0562$^+$	0.0565$^+$	0.0472	0.0462
	(0.0081)	(0.0316)	(0.0315)	(0.0313)	(0.0305)
Obs.	584	584	584	584	584
R^2	0.0156	0.1743	0.1750	0.1793	0.1822
			Score: [58-62]		
$Treat_i$	-0.0165	-0.0258	-0.0255	-0.0263	-0.0262
	(0.0134)	(0.0171)	(0.0170)	(0.0170)	(0.0169)
$Post_t$	-0.0150	-0.0125	-0.0259	-0.0068	-0.0242
	(0.0146)	(0.0183)	(0.0225)	(0.0181)	(0.0222)
$Treat_i * Post_t$	0.0490*	0.0490*	0.0471*	0.0495*	0.0468*
	(0.0233)	(0.0218)	(0.0223)	(0.0220)	(0.0223)
$Sales_{it}/TA_{it}$ (2010-2013)			-0.0138		-0.0210
			(0.0181)		(0.0202)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0142		0.0202
			(0.0130)		(0.0144)
CF_{it}/TA_{it} (2010-2013)				0.1473	0.1936$^+$
				(0.0942)	(0.1083)
CF_{it}/TA_{it} (2010-2013) * $Post_t$				-0.1447	-0.1901
				(0.1087)	(0.1185)
Constant	0.0607***	-0.3094***	-0.2999***	-0.3089***	-0.2945***
	(0.0111)	(0.0801)	(0.0785)	(0.0791)	(0.0766)
Obs.	357	357	357	357	357
R^2	0.0156	0.2672	0.2701	0.2721	0.2779
Year Dummies	No	Yes	Yes	Yes	Yes
NACE2 Dummies	No	Yes	Yes	Yes	Yes
Legal Form Dummies	No	Yes	Yes	Yes	Yes
Province Dummies	No	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.28: Robustness: Low-Tech firms with Score:

	[53-66]		**[56-64]**		**[58-62]**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	-0.0049	0.0089	0.0159^{+}	0.0215*	0.0214	0.0427*
	(0.0095)	(0.0097)	(0.0095)	(0.0097)	(0.0143)	(0.0165)
$Post_t$	-0.0087	-0.0105	0.0069	0.0091	-0.0112	-0.0091
	(0.0104)	(0.0136)	(0.0105)	(0.0127)	(0.0141)	(0.0169)
$Treat_i$*$Post_t$	0.0208	0.0195	0.0051	0.0046	0.0048	0.0049
	(0.0147)	(0.0133)	(0.0162)	(0.0143)	(0.0208)	(0.0178)
Constant	0.0738***	0.1119***	0.0554***	0.1644***	0.0567***	0.1781***
	(0.0070)	(0.0213)	(0.0058)	(0.0226)	(0.0092)	(0.0411)
Year Dummies	No	Yes	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes	No	Yes
Obs.	995	995	745	745	436	436
R^2	0.0020	0.2732	0.0084	0.3213	0.0104	0.4053

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.29: Robustness: High-Tech Manufacturing firms with Score:

	[53-66]		**[56-64]**		**[58-62]**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	0.0247*	0.0339	0.0208^{+}	0.0441	-0.0050	0.0334
	(0.0114)	(0.0206)	(0.0123)	(0.0288)	(0.0075)	(0.0333)
$Post_t$	0.0136	0.0164	-0.0041	0.0028	-0.0034	0.0049
	(0.0133)	(0.0258)	(0.0082)	(0.0258)	(0.0098)	(0.0132)
$Treat_i$*$Post_t$	0.0198	0.0198	0.0699*	0.0699*	0.0518^{+}	0.0518*
	(0.0266)	(0.0268)	(0.0323)	(0.0310)	(0.0272)	(0.0211)
Constant	0.0205***	0.0274	0.0182**	0.0149	0.0210**	-0.1333^{+}
	(0.0039)	(0.0446)	(0.0054)	(0.0449)	(0.0065)	(0.0708)
Year Dummies	No	Yes	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes	No	Yes
Obs.	132	132	84	84	54	54
R^2	0.0681	0.3201	0.1724	0.4311	0.1876	0.5505

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.30: Robustness: Low-Tech Manufacturing firms with Score:

	[53-66]		**[56-64]**		**[58-62]**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	0.0146	0.0217*	0.0225*	0.0084	0.0402**	-0.0022
	(0.0106)	(0.0088)	(0.0099)	(0.0080)	(0.0122)	(0.0111)
$Post_t$	0.0115	0.0116	0.0130	0.0237^{+}	-0.0042	0.0135
	(0.0138)	(0.0178)	(0.0123)	(0.0127)	(0.0125)	(0.0146)
$Treat_i$*$Post_t$	0.0177	0.0192	0.0236	0.0216	0.0365	0.0336*
	(0.0197)	(0.0161)	(0.0208)	(0.0161)	(0.0250)	(0.0169)
Constant	0.0542***	0.1017***	0.0430***	0.1546***	0.0261**	0.1860***
	(0.0076)	(0.0213)	(0.0062)	(0.0201)	(0.0080)	(0.0388)
Year Dummies	No	Yes	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes	No	Yes
Obs.	395	395	258	258	140	140
R^2	0.0256	0.3649	0.0712	0.4848	0.1179	0.5437

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.31: Robustness: High-Tech Service firms with Score:

	[53-66]		**[56-64]**		**[58-62]**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	-0.0168^{+}	-0.0326**	-0.0182	-0.0231^{+}	-0.0189	-0.0230
	(0.0101)	(0.0107)	(0.0118)	(0.0122)	(0.0156)	(0.0175)
$Post_t$	-0.0077	-0.0050	-0.0153	-0.0120	-0.0172	-0.0156
	(0.0111)	(0.0152)	(0.0130)	(0.0170)	(0.0169)	(0.0214)
$Treat_i$*$Post_t$	0.0369^{+}	0.0373*	0.0476*	0.0477*	0.0487^{+}	0.0486^{+}
	(0.0192)	(0.0180)	(0.0217)	(0.0202)	(0.0269)	(0.0251)
Constant	0.0690***	0.1848***	0.0714***	0.1870***	0.0680***	-0.2874***
	(0.0070)	(0.0308)	(0.0089)	(0.0308)	(0.0129)	(0.0856)
Year Dummies	No	Yes	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes	No	Yes
Obs.	655	655	500	500	303	303
R^2	0.0079	0.1749	0.0115	0.2299	0.0128	0.2609

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.32: Robustness: Low-Tech Service firms with Score:

	[53-66]		**[56-64]**		**[58-62]**	
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i$	-0.0139	0.0101	0.0132	0.0374*	0.0232	0.0661**
	(0.0143)	(0.0152)	(0.0140)	(0.0146)	(0.0211)	(0.0234)
$Post_t$	-0.0221	-0.0224	0.0025	0.0004	-0.0126	-0.0185
	(0.0152)	(0.0185)	(0.0156)	(0.0186)	(0.0182)	(0.0226)
$Treat_i$*$Post_t$	0.0239	0.0205	-0.0030	-0.0033	-0.0191	-0.0176
	(0.0210)	(0.0189)	(0.0230)	(0.0205)	(0.0285)	(0.0249)
Constant	0.0847***	0.0072	0.0631***	-0.0051	0.0668***	-0.0934*
	(0.0109)	(0.0239)	(0.0087)	(0.0267)	(0.0117)	(0.0380)
Year Dummies	No	Yes	No	Yes	No	Yes
NACE2 Dummies	No	Yes	No	Yes	No	Yes
Legal Form Dummies	No	Yes	No	Yes	No	Yes
Province Dummies	No	Yes	No	Yes	No	Yes
Obs.	577	577	470	470	285	285
R^2	0.0035	0.3125	0.0024	0.3143	0.0116	0.4170

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.33: Intention to Hire (**ITH**), at 2009

	Mean	**SD**	**min**	**Max**
ITH	.55	.49	0	1

Notes: Intention to Hire data are available only for subsidy-*recipient* firms and are retrieved from the project forms where firms were asked if, following the innovative investment, they would have been willing to hire new employees. Just to remind, the program did not cover wage expenditures nor incentivized new hires

Table 4.34: Spillovers effect: Labour Demand

	All	Micro	Med.-Large	High Tech	Service HT
	(1)	(2)	(3)	(4)	(5)
$Treat_i$	-3.2127	1.0250***	-44.6292	-10.5468*	-15.0921**
	(2.4427)	(0.2524)	(37.1804)	(4.3782)	(5.5687)
$Post_t$	-6.4776	2.0332***	-144.9084	-23.9277	-32.4487
	(8.6721)	(0.4303)	(107.1761)	(17.9082)	(22.3015)
$Treat_i$*$Post_t$	15.8507*	0.7619+	176.0488*	23.5089+	33.3060*
	(6.4747)	(0.4268)	(84.5512)	(12.0167)	(15.4276)
Constant	21.2451**	2.1266	113.8837	12.1589	26.4216
	(7.6917)	(1.3520)	(81.8361)	(13.3591)	(16.5877)
Year Dummies	Yes	Yes	Yes	Yes	Yes
NACE2 Dummies	Yes	Yes	Yes	Yes	Yes
Legal Form Dummies	Yes	Yes	Yes	Yes	Yes
Province Dummies	Yes	Yes	Yes	Yes	Yes
Obs.	2,104	1,142	217	939	741
R^2	0.4935	0.3121	0.4955	0.5020	0.5005

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.35: Spillovers effect: Wage bill per worker

	Small (1)	Med.-Large (2)	Manu. LT (3)
$Treat_i$	-670.6554	9243.9320*	3571.0284*
	(1158.2779)	(3817.5464)	(1699.7333)
$Post_t$	2224.7077	5041.9615*	2875.7944
	(1701.3412)	(2504.3055)	(2548.3734)
$Treat_i$*$Post_t$	-3423.7825+	-4394.6743+	-4860.1514+
	(1837.4937)	(2568.5091)	(2621.1529)
Constant	46915.4036***	-4773.8954	10154.4847
	(5431.6721)	(8282.9994)	(7179.8524)
Year Dummies	Yes	Yes	Yes
NACE2 Dummies	Yes	Yes	Yes
Legal Form Dummies	Yes	Yes	Yes
Province Dummies	Yes	Yes	Yes
Obs.	703	205	433
R^2	0.3878	0.6703	0.2972

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE 4.36: Spillovers effect: Productivity measures

	Added value per worker Med.-Large (1)	Added value over TA Low Tech (2)
$Treat_i$	-971.3869	-0.0504***
	(11182.2527)	(0.0150)
$Post_t$	6309.3797	-0.0606*
	(6940.4156)	(0.0263)
$Treat_i$*$Post_t$	-17806.6342*	0.0437+
	(6934.6572)	(0.0260)
Constant	-47870.0198	-0.3187***
	(32260.1518)	(0.0429)
Year Dummies	Yes	Yes
NACE2 Dummies	Yes	Yes
Legal Form Dummies	Yes	Yes
Province Dummies	Yes	Yes
Obs.	204	1,186
R^2	0.5530	0.3599

Notes: Robust standard errors in parentheses. Statistical Significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix A

APPENDIX 1 - Chapter 2

A.1 Alternative treatment definition

In this Section, I re-estimate the findings discussed in Section 2.4, based on equation (2.1), changing the way I define the treatment variable. Specifically, the previous results were based on a time-invariant measure of exposure to the pension reform that mirrors the difference between the MRA in 2017 and 2011, that is the number of years of increase in the residual working life. Now, I change the treatment definition using as treatment variable a binary indicator that takes value of 1 if affected individual i has experienced more than 3 years of increase in her MRA (that is $shock > 3$), and 0 otherwise. Consequently, the interpretation of the Difference-in-Differences coefficient changes. Indeed, according to equation (2.1) the β coefficient, given by the interaction of the policy-induced shock measure and the post-reform dummy, measures the average human capital investment for each additional year increase in the MRA, exclusively depending on their degree of exposure to the policy, around its implementation. Instead, now it measures the average difference in human capital investment, in the aftermath of the reform, between those whore more exposed to the increase in MRA (those with shock greater than 3 years) relative to the control group, composed of individuals whose shock in the "residual" working life is lower or equal to three years.

TABLE A.1: *Forward-looking* effect on human capital participation activities

	All (1)	Men (2)	Women (3)
$S_i \times post2011$	0.0205*	0.0273*	0.0114
	(0.0086)	(0.0116)	(0.0132)
μ D.V. $S_i = 1$, $post2011 = 0$	0.3591	0.3591	
Coeff. rescaled	+5.7%	+7.6%	
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	no	no
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes
Obs.	53,977	28,478	25,499
R^2	0.1313	0.1042	0.1750
Adj. R^2	0.1299	0.1014	0.1722

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.2: *Forward-looking* effect on human capital participation activities by:

	Age class:		
	40-47	48-56	57-64
	(1)	(2)	(3)
		All:	
$S_i \times post2011$	0.0319^{+}	0.0282^{*}	-0.0317
	(0.0171)	(0.0115)	(0.0194)
μ D.V. $S_i = 1$, $post2011 = 0$	0.3368	0.3711	
Coeff. rescaled	+9.5%	+7.6%	
Obs.	13,600	27,289	13,088
R^2	0.1370	0.1353	0.1294
Adj. R^2	0.1330	0.1330	0.1246
		Men:	
$S_i \times post2011$	0.0290	0.0420^{**}	-0.0291
	(0.0246)	(0.0157)	(0.0249)
μ D.V. $S_i = 1$, $post2011 = 0$		0.3681	
Coeff. rescaled		+11.4%	
Obs.	6,103	14,703	7,672
R^2	0.1177	0.1071	0.1111
Adj. R^2	0.1084	0.1026	0.1027
		Women:	
$S_i \times post2011$	0.0243	0.0102	-0.0139
	(0.0259)	(0.0174)	(0.0328)
Obs.	7,497	12,586	5,416
R^2	0.1727	0.1831	0.1738
Adj. R^2	0.1657	0.1784	0.1629
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.3: *Forward-looking* effect on human capital participation activities by:

	Sector of employment:		
	Public	Private	Self-employed
	(1)	(2)	(3)
$S_i \times post2011$	0.0113	0.0104	0.0598*
	(0.0152)	(0.0112)	(0.0257)
μ D.V. $S_i = 1$, $post2011 = 0$			0.2989
Coeff. rescaled			+20%
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes
Obs.	21,113	24,831	8,033
R^2	0.0792	0.0729	0.0875
Adj. R^2	0.0754	0.0696	0.0775

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.4: *Forward-looking* effect on human capital participation activities by:

	Firm's economic sector:	
	Manufacturing	Service
	(1)	(2)
$S_i \times post2011$	0.0246	0.0247*
	(0.0197)	(0.0116)
μ D.V. $S_i = 1$, $post2011 = 0$		0.38
Coeff. rescaled		+6.5%
Obs.	8,059	24,805
R^2	0.0767	0.0860
Adj. R^2	0.0665	0.0828
Year FE	yes	yes
Shock FE	yes	yes
Gender FE	yes	yes
Age FE	yes	yes
Martial stat. FE	yes	yes
Region FE	yes	yes
Sector FE	yes	yes
Y. of contr. FE	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.5: *Forward-looking* effect on human capital participation activities by:

	Education level:		
	Low	Medium	High
	(1)	(2)	(3)
		All:	
$S_i \times post2011$	0.0035	-0.0079	0.0452*
	(0.0129)	(0.0119)	(0.0185)
μ D.V. $S_i = 1$, $post2011 = 0$			0.5678
Coeff. rescaled			+8%
Obs.	11,645	27,057	15,275
R^2	0.0726	0.1083	0.0767
Adj. R^2	0.0655	0.1054	0.0713
		Men:	
$S_i \times post2011$	0.0255	0.0172	0.0556*
	(0.0174)	(0.0159)	(0.0243)
μ D.V. $S_i = 1$, $post2011 = 0$			0.5518
Coeff. rescaled			+10%
Obs.	6,694	14,319	7,465
R^2	0.0723	0.0839	0.0768
Adj. R^2	0.0598	0.0782	0.0656
		Women:	
$S_i \times post2011$	-0.0178	-0.0358*	0.0540
	(0.0222)	(0.0179)	(0.0331)
μ D.V. $S_i = 1$, $post2011 = 0$		0.358	
Coeff. rescaled		−10%	
Obs.	4,951	12,738	7,810
R^2	0.1012	0.1547	0.0916
Adj. R^2	0.0849	0.1488	0.0812
Year FE	yes	yes	yes
Shock FE	yes	yes	yes
Gender FE	yes	yes	yes
Age FE	yes	yes	yes
Martial stat. FE	yes	yes	yes
Region FE	yes	yes	yes
Sector FE	yes	yes	yes
Y. of contr. FE	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: *Forward-looking* effect on human capital participation activities, women only

	Married		Not married	
	(1)	(2)	(3)	(4)
$S_i \times post2011$	0.0510*	0.0508*	-0.0047	-0.0044
	(0.0245)	(0.0244)	(0.0220)	(0.0219)
μ D.V. $S_i = 1$, $post2011 = 0$	0.3074			
Coeff. rescaled	+16.3%	+16.5%		
Year FE	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes
Age FE	yes	yes	yes	yes
Martial stat. FE	no	no	yes	yes
Region FE	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes
No. kids	no	yes	no	yes
HH size	no	yes	no	yes
Obs.	14,991	14,991	10,508	10,508
R^2	0.1633	0.1640	0.1990	0.1993
Adj. R^2	0.1586	0.1591	0.1924	0.1925

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7: *Forward-looking* effect on human capital participation activities, women only, by:

	Age class:					
	40-47		48-56		57-64	
	(1)	(2)	(3)	(4)	(5)	(6)
	Married:					
$S_i \times post2011$	0.0821^{+}	0.0825^{+}	0.0374	0.0376	0.0248	0.0246
	(0.0496)	(0.0495)	(0.0306)	(0.0304)	(0.0618)	(0.0614)
μ D.V. $S_i = 1$, $post2011 = 0$	0.2771					
Coeff. rescaled	+29%	+29%				
Obs.	4,610	4,610	7,127	7,127	3,254	3,254
R^2	0.1642	0.1643	0.1694	0.1703	0.1682	0.1708
Adj. R^2	0.1534	0.1531	0.1614	0.1621	0.1507	0.1528
	Not married:					
$S_i \times post2011$	0.0086	0.0090	-0.0051	-0.0053	-0.0234	-0.0275
	(0.0344)	(0.0344)	(0.0330)	(0.0330)	(0.0444)	(0.0443)
Obs.	2,887	2,887	5,459	5,459	2,162	2,162
R^2	0.2080	0.2088	0.2112	0.2114	0.1977	0.1992
Adj. R^2	0.1906	0.1908	0.2008	0.2007	0.1708	0.1716
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
No. kids	no	yes	no	yes	no	yes
HH size	no	yes	no	yes	no	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.8: *Forward-looking* effect on human capital participation activities, women only, by:

	Education level:					
	Low		Medium		High	
	(1)	(2)	(3)	(4)	(5)	(6)
	Married:					
$S_i \times post2011$	-0.0217	-0.0207	0.0121	0.0121	0.0944^{+}	0.0944^{+}
	(0.0331)	(0.0332)	(0.0325)	(0.0324)	(0.0505)	(0.0505)
μ D.V. $S_i = 1$, $post2011 = 0$					0.5268	
Coeff. rescaled					+17.9%	
Obs.	2731	2,731	7,585	7,585	4,639	4,639
R^2	0.1008	0.1011	0.1492	0.1508	0.0774	0.0774
Adj. R^2	0.0722	0.0719	0.1397	0.1411	0.0601	0.0601
	Not married:					
$S_i \times post2011$	-0.0212	-0.0219	-0.0501^{+}	-0.0500^{+}	0.0064	0.0064
	(0.0414)	(0.0414)	(0.0284)	(0.0284)	(0.0529)	(0.0529)
μ D.V. $S_i = 1$, $post2011 = 0$			0.3803			
Coeff. rescaled			−13%			
Obs.	2,220	2,220	5,153	5,153	2,453	2,453
R^2	0.1320	0.1332	0.1774	0.1775	0.1390	0.1390
Adj. R^2	0.0966	0.0969	0.1633	0.1630	0.1139	0.1139
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
No. kids	no	yes	no	yes	no	yes
HH size	no	yes	no	yes	no	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.9: *Forward-looking* effect on human capital participation activities by:

	Firm size:					
	1-9	10-15	16-25	26-49	50-249	>250
	(1)	(2)	(3)	(4)	(5)	(6)
$S_i \times post2011$	0.0562***	0.0144	-0.0158	0.0050	0.0206	-0.0209
	(0.0164)	(0.0304)	(0.0330)	(0.0389)	(0.0293)	(0.0237)
μ D.V. $S_i = 1$, $post2011 = 0$	0.2170					
Coeff. rescaled	+26%					
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes
Obs.	11,975	2,827	2,113	1,864	3,909	8,536
R^2	0.1006	0.0907	0.0944	0.1126	0.0768	0.0828
Adj. R^2	0.0939	0.0614	0.0550	0.0686	0.0556	0.0732

Notes: The estimates refer only to self-employed and private sector workers. Firm size refers to the number of employees, including the interviewed, working in the firm at the year of interview. Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10: *Forward-looking* effect on human capital participation activities by:

	Firm size:					
	1-9	10-15	16-25	26-49	50-249	>250
	(1)	(2)	(3)	(4)	(5)	(6)
	Manufacturing sector:					
$S_i \times post2011$	0.0266	-0.0414	0.1001	0.1259	0.0112	-0.0139
	(0.0321)	(0.0615)	(0.0886)	(0.0779)	(0.0513)	(0.0570)
Obs.	3,063	739	439	560	1,339	1,623
R^2	0.1215	0.1439	0.2294	0.2196	0.1208	0.1015
Adj. R^2	0.0955	0.0280	0.0384	0.0757	0.0589	0.0500
	Service sector:					
$S_i \times post2011$	0.0645***	0.0257	-0.0263	-0.0449	0.0359	-0.0321
	(0.0193)	(0.0352)	(0.0374)	(0.0463)	(0.0364)	(0.0270)
μ D.V. $S_i = 1$, $post2011 = 0$	0.2185					
Coeff. rescaled	+29.5%					
Obs.	8,912	2,088	1,674	1,304	2,570	6,913
R^2	0.1032	0.1078	0.1133	0.1435	0.0912	0.0937
Adj. R^2	0.0943	0.0685	0.0641	0.0814	0.0590	0.0820
Year FE	yes	yes	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes	yes	yes
Age FE	yes	yes	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes	yes	yes

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE A.11: *Forward-looking* effect on:

	Paid			Firm-sponsored
	All:	Wage above median:	Wage below median:	
	(1)	(2)	(3)	(4)
$S_i \times post2011$	0.0085	0.0125	0.0009	-0.0171
	(0.0124)	(0.0159)	(0.0194)	(0.0149)
Year FE	yes	yes	yes	yes
Shock FE	yes	yes	yes	yes
Gender FE	yes	yes	yes	yes
Age FE	yes	yes	yes	yes
Martial stat. FE	yes	yes	yes	yes
Region FE	yes	yes	yes	yes
Sector FE	yes	yes	yes	yes
Y. of contr. FE	yes	yes	yes	yes
Obs.	21,289	13,033	8,256	20,308
R^2	0.2081	0.2174	0.2036	0.0937
Adj. R^2	0.2048	0.2120	0.1949	0.0897

Notes: Robust standard errors, in parentheses, clustered at the age-sector of employment-gender-years of contribution level. Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix B

APPENDIX 2 - Chapter 3

B.1 2019 Data

2019 pre-prints data have always been scraped from the SSRN website and as for the 2020 data they refer to the *Economics Departments Research Papers* and *Economics Research Centers Papers* working paper series. The total number of papers extracted, uploaded for the first time during January and November 17, 2019, is equal to 3,867. Of these the 45.2% has been written by at least of female economist. Overall, the number of authors involved in the drafting of the working papers totals 7,189 distinct researchers (27.1% women and 72.9% men). Also for this sample of authors I hand-collected their employment status in 2019 recovering detailed information for the 96.3% of the sample. In addition, the comparison between the 2019 data and the 2020 sample seems to suggest that they are very similar along many dimensions (*i.e.* country of affiliation, distribution by position and tenure) as Table B.1 and the subsequent figures show.

Table B.1: Economics working papers summary statistics

	Mean	**SD**	**min.**	**Max.**	**Obs.**
All observations:					
Tot. authors	2.593	1.19	1	16	3,867
No. of downloads	38.725	130.928	0	4,659	3,867
No. of views	339.282	461.927	61	14,047	3,839
No. of pages	47.348	20.866	1	212	3,837
Female author	0.452	0.498	0	1	3,867
Tot. no. of female	0.637	0.855	0	8	3,867
Intensity female	0.235	0.307	0	1	3,867
At least one female author:					
Tot. authors	3.002	1.281	1	16	1,749
No. of downloads	36.71	140.127	0	4,659	1,749
No. of views	325.136	465.784	61	14,047	1,737
No. of pages	47.937	20.22	1	180	1,736
Tot. no. of female	1.408	0.727	1	8	1,749
Intensity female	0.519	0.246	0.125	1	1749
Only male authors:					
Tot. authors	2.255	0.99	1	7	2,118
No. of downloads	40.389	122.823	0	3,126	2,118
No. of views	350.972	458.497	65	9,376	2,102
No. of pages	46.861	21.378	4	212	2,101

Notes: Sample period goes from January to November (17^{th}) 2019.

FIGURE B.1: Number of working papers by month in 2019

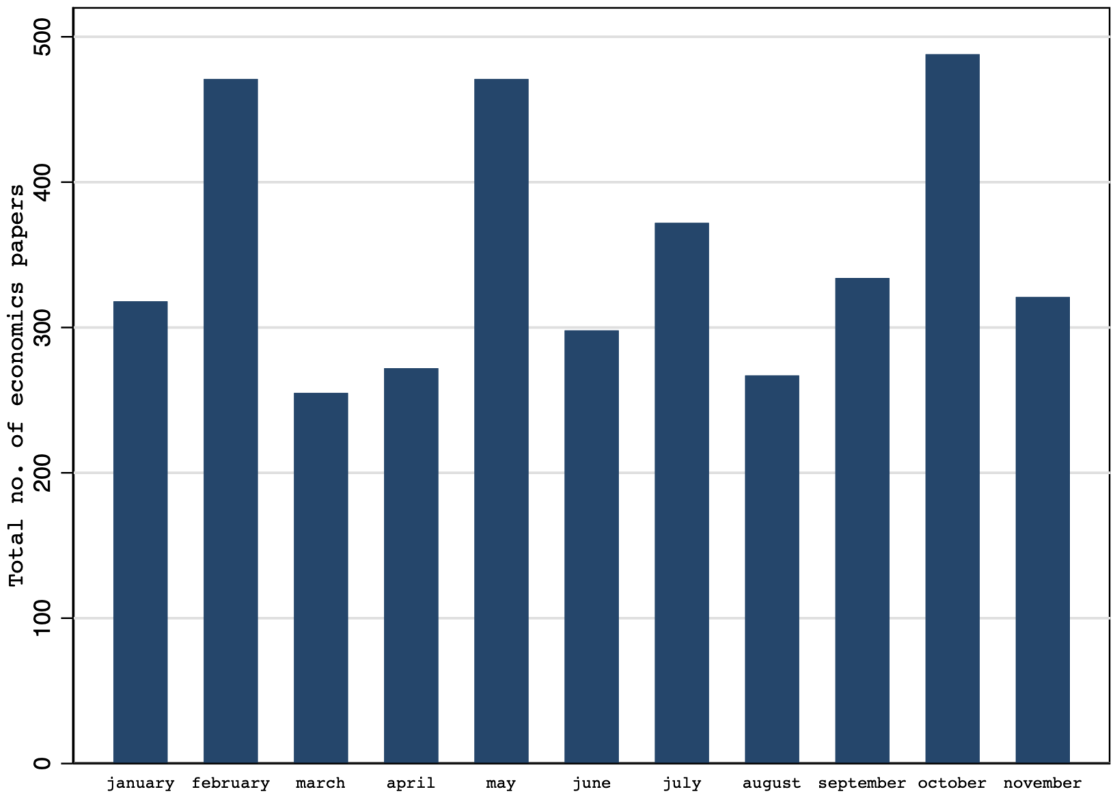
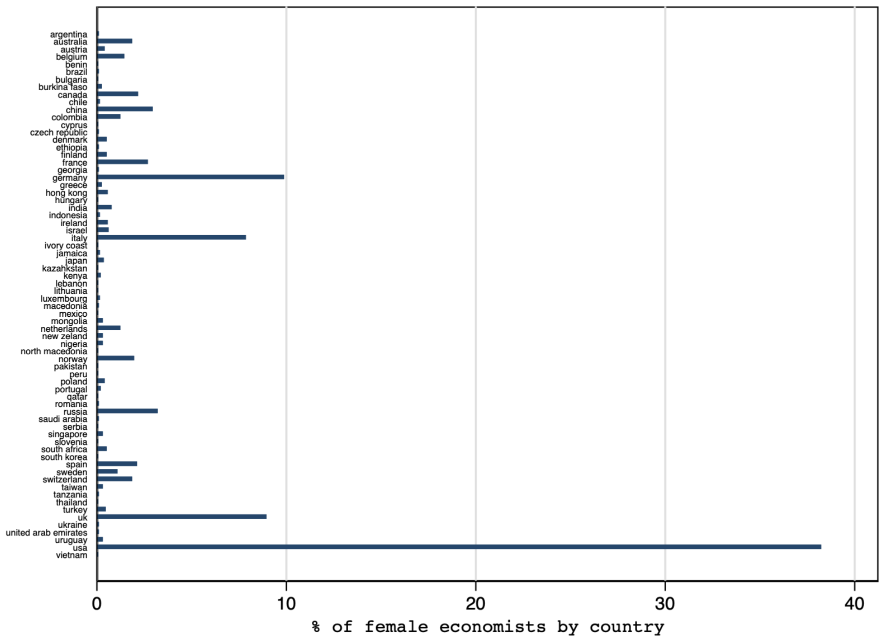


FIGURE B.2: Distribution of authors by gender and country of affiliation in 2019

(A) Only female authors



(B) Only male authors

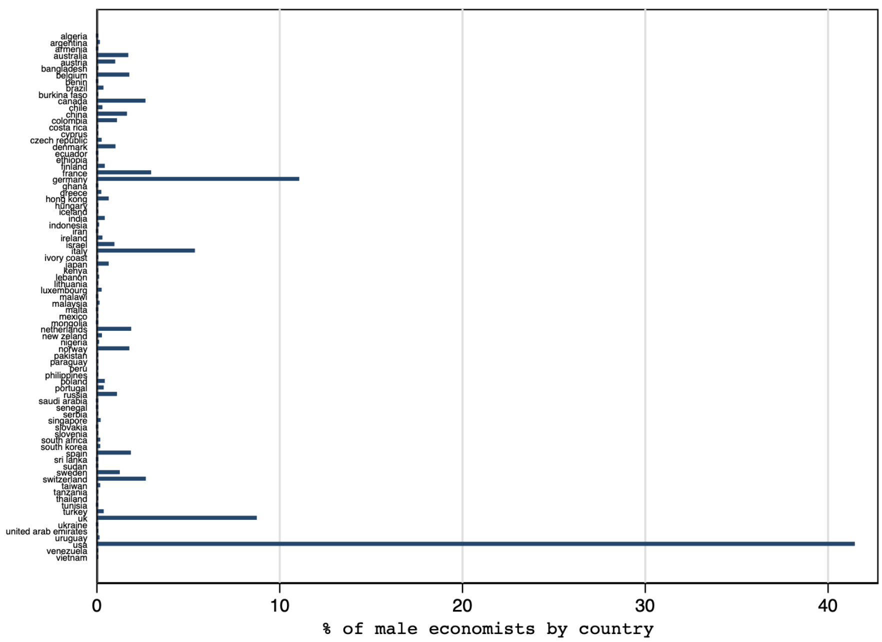
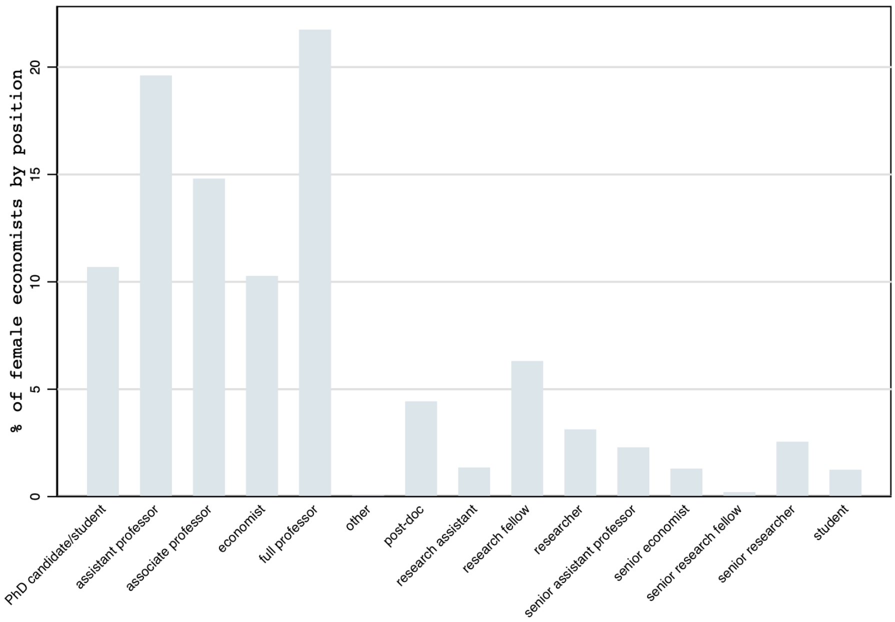


FIGURE B.3: Distribution of authors by gender and position in 2019

(A) Only female authors



(B) Only male authors

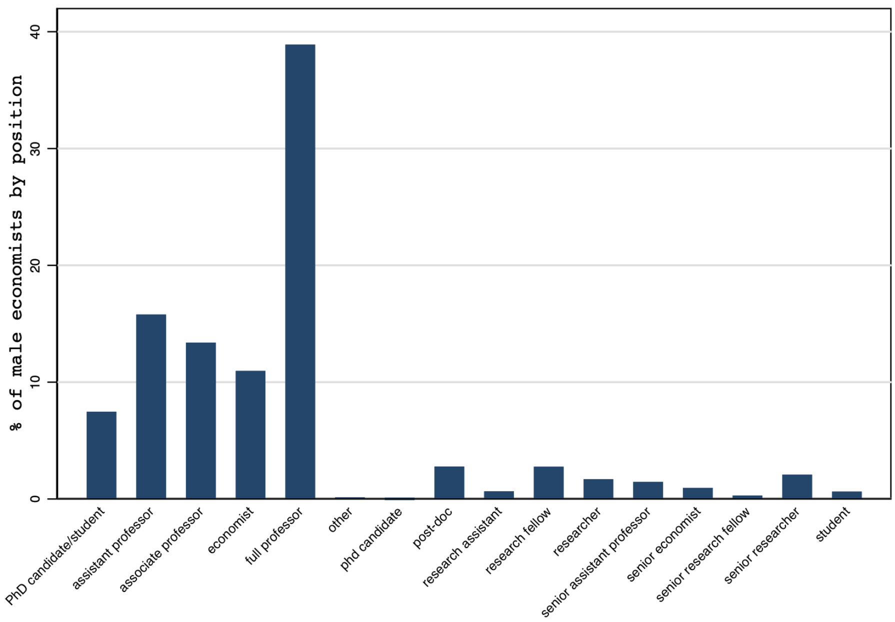
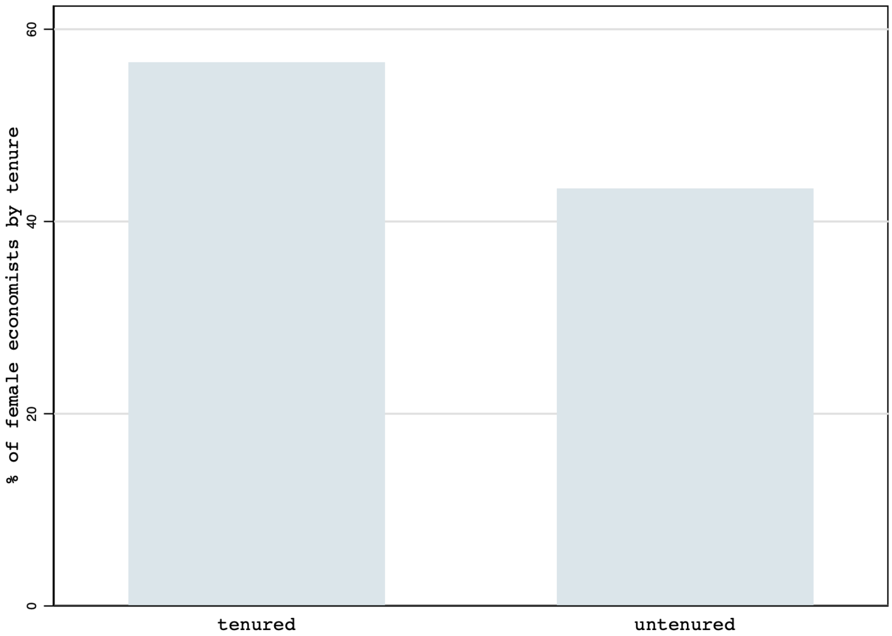


Figure B.4: Distribution of authors by gender and tenure in 2019

(a) Only female authors



(b) Only male authors

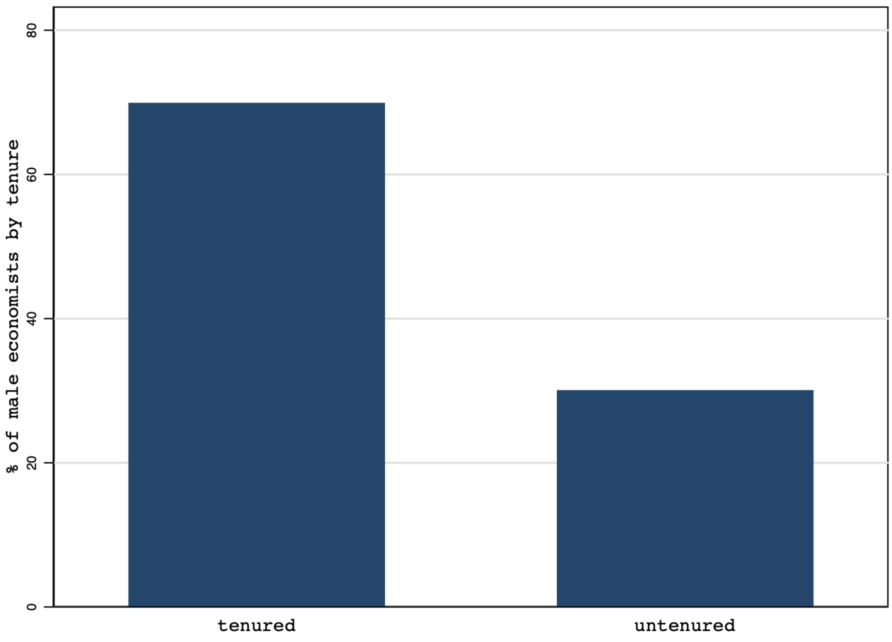
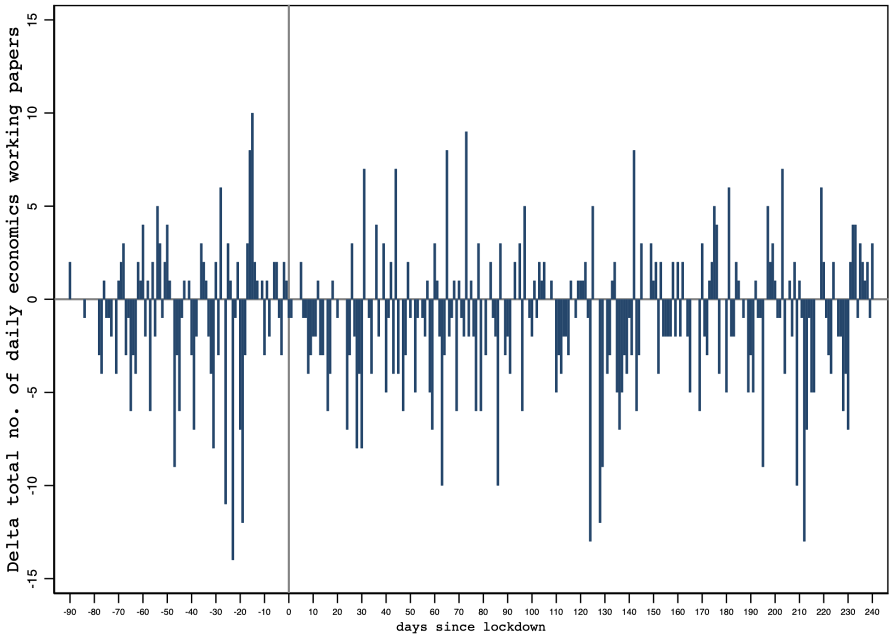


FIGURE B.5: Aggregate number of papers by gender in 2019

(A) Production gap



(B) Average daily production

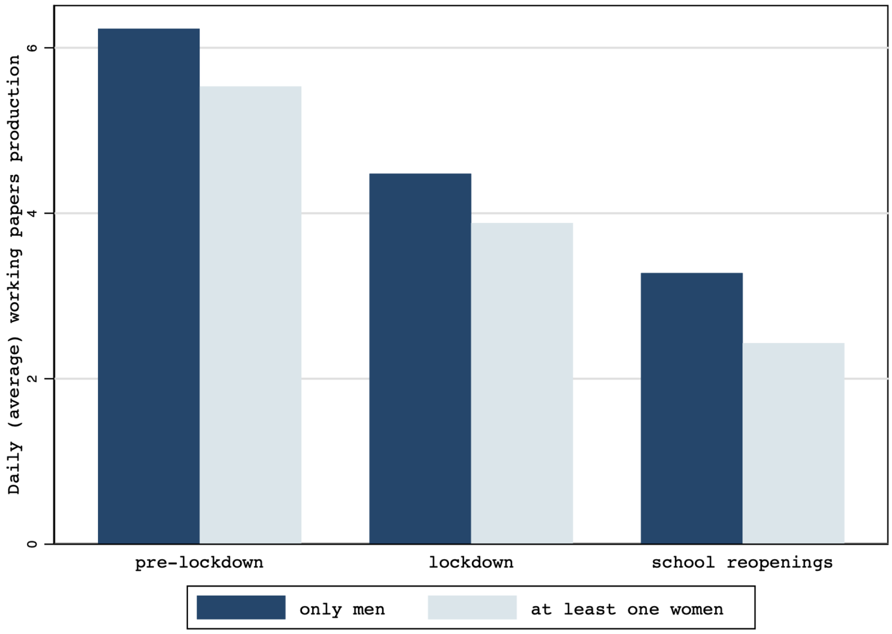
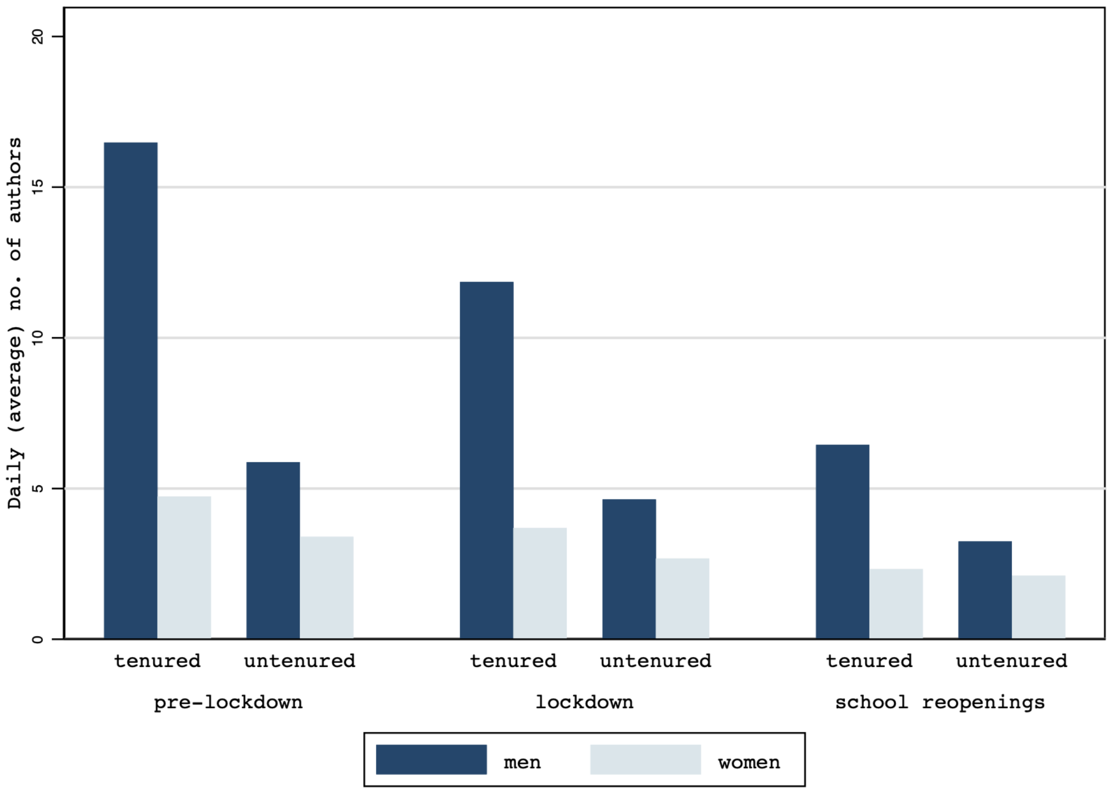


Figure B.6: Average no. of authors by period, gender and tenure in 2019



Appendix C

APPENDIX 3 - Chapter 4

C.1 The Regional Program: Additional Details

The maximum scores attributed to each *macro*-criteria summarized in Table 4.1 are defined as follows:

1. **Quality and innovation of the project, both for the purpose of increasing the efficiency of the management machine, and as a function of completing/upgrading existing ICT infrastructures** (*max* 60 pts):

   - Quality of the project in terms of precision and completeness in the identification of specific actions to be carried out, with particular regard to organizational and management procedures: *max* 20/60 pts
   - Innovation on the production organization: incidence of the interventions to be carried out on the strengthening of the production chain activity (transformation plants, company sales points, introduction and/or e-commerce development): *max* 20/60 pts
   - If the project is to complete/upgrade existing **ICT** infrastructures: *max* 10/60 pts
   - If the project involves the improvement of the company organization (reduction of the company underemployment, reconversion and / or increase in employment, ...) and of safety in the workplace: *max* 10/60 pts

2. **Impact on the qualification of the product/service with a relative increase of competitiveness on the market** (*max* 30 pts):

   - If the project involves the creation of new products and/or the diversification of some others and/or the quality certification of the company productions/services: *max* 10/30 pts
   - Percentage increase in the expected corporate added value with the measures co-financed when fully operational: *max* 10/30 pts
   - Economic sustainability, deductible from the relationship between total cost of the project and annual company sales volume: *max* 5/30 pts
   - Environmental sustainability, in the presence of interventions and/or machinery that reduce pollutant emissions or improve the management of company waste: *max* 5/30 pts

3. **Relevance of the juvenile and female components** (*max* 10 pts):

   - Age of the applicant (individual company), average age of members (partnership) of the Sole Administrator or average of members of the board of directors (limited liability company): $\leq$ 35 years 7/10 pts; $35 < x \leq 45$ years 5/10 pts; $45 < x \leq 55$ years 3/10 pts e $>$ 55 years 1/10 pts
   - Applicant sex (individual company), prevalent sex of members (partnership), of the sole director or predominantly of the members of the board of directors (joint-stock company): if female 3/10 pts

For the purpose of compiling the final merit ranking, the total score assigned to each project will be determined by the sum of the scores assigned for each of the evaluation parameters for a maximum of 100 pts. If the resulting sum will be less than 60 pts, the project will not be included in the final ranking.

FIGURE C.1: Number of Subsidized Project by Age Groups

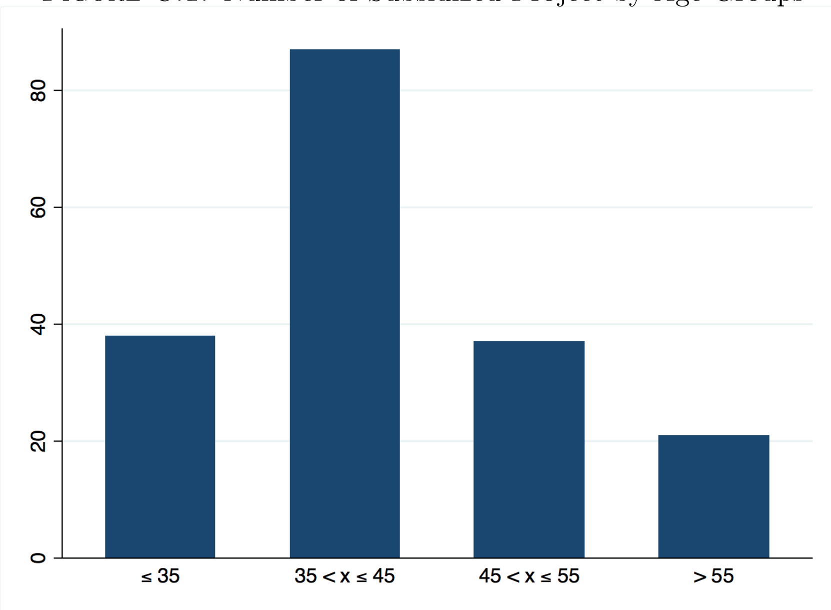
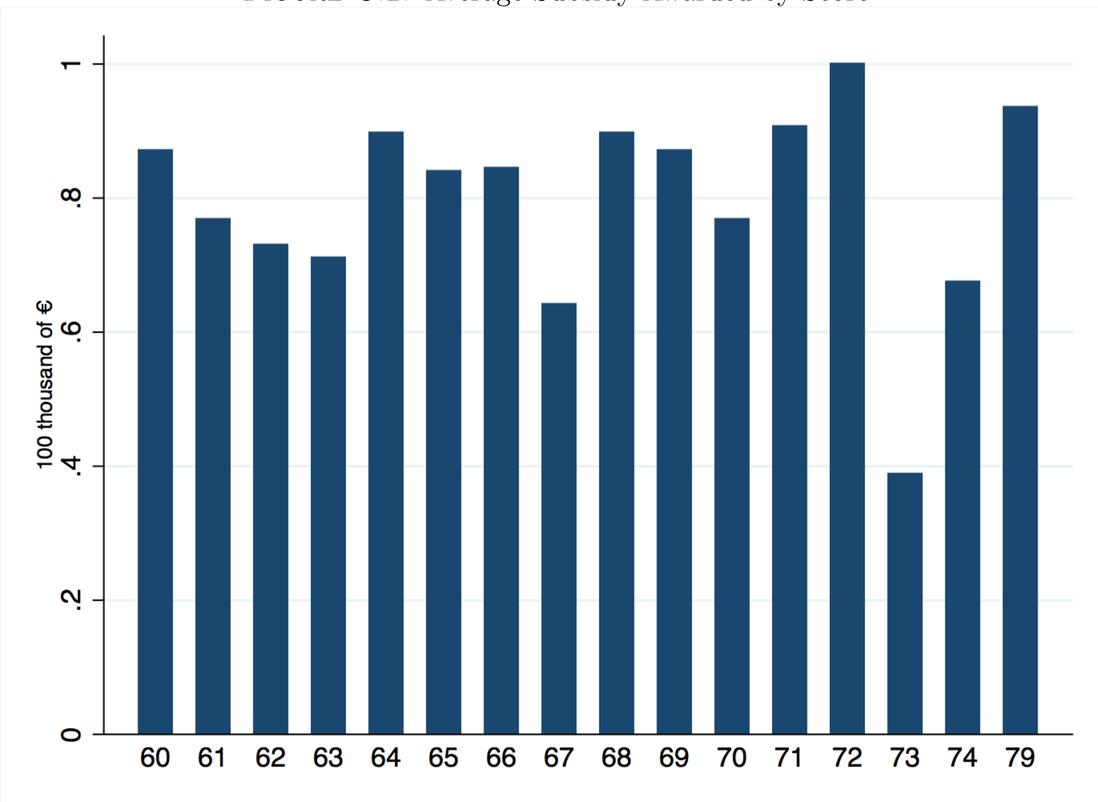


TABLE C.1: Summary Statistics by Age and Sex of Treated Firms' Stakeholders

	Obs.	**Mean**	**SD**	**min**	**Max**
Age	183	42.3	9.72	19	75
Prevalent Sex	183	.76	.43	0	1

Notes: Data about age and prevalent sex of firms' stakeholders are only available for treated firms. The variable Age is expressed in years, whereas the variable prevalent sex is a dummy variable that takes value of 1 if the average sex of the relevant stakeholder in *recipient* firm is male

Figure C.2: Average Subsidy Awarded by Score



Notes: the lowest score in order to be eligible for obtaining the subsidy is 60*pts*. The figure shows the average subsidy grant by score to eligible firms in thousands of Euro. The maximum amount of the subsidy that the Regional Government was allowed to award is set at 200000 €, so that it does not break the State Aid Legislation.

C.2 Additional Baseline Results: Generalized DID

$$y_{it} = \lambda_t + f_i + \delta Treat_i * Post_t + \theta X_i^{pre} * Post_t + \epsilon_{it} \tag{C.1}$$

where λ_t and f_i are time fixed effects and firm fixed effects; $Treat_i * Post_t$ is the interaction term indicating treatment gruop in the *post-treatment* period. The associated coefficient, δ, measures the causal effect of the subsidy program on the level of investment in innovative projects by eligible and awarded firms during the time span 2014-2015. $X_i^{pre} * Post_t$ is a set control variables averaged over the *pre-treatment* period in order to account for the heterogeneity between control and treated groups; ϵ_{it}: error terms that includes all the differences in the outcome not explained by the included regressors.

TABLE C.2: Subsidy Impact on K_{it}/TA_{it} during 2010-2015, Eq. (C.1)

	(1)	(2)	(3)	(4)
$Treat_i$ * $Post_t$	0.02435***	0.02686***	0.02199**	0.02380***
	(0.00736)	(0.00720)	(0.00725)	(0.00712)
CF_{it}/TA_{it} (2010-2013) * $Post_t$		-0.25630***		-0.30342***
		(0.07492)		(0.07507)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.01138+	0.01702**
			(0.00647)	(0.00653)
Constant	0.07278***	0.07278***	0.07283***	0.07285***
	(0.00362)	(0.00358)	(0.00361)	(0.00356)
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Obs.	2,166	2,166	2,166	2,166
Adjusted R^2	0.6455	0.6495	0.6465	0.6520
Estimated Elasticity (s=0.5)	-.8	-.9	-.7	-.7
Estimated Elasticity ($\bar{s} = 0.43$)	-.9	-1	-.8	-.9

Notes: The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. Furthermore, in each period, control and treated groups are perfectly balanced with a sample of 181 firms by group. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. Note that $\bar{K}_{2013}^{Treated}/TA_{2013} = .0634549$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses. Statistical significance denoted as follows: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

TABLE C.3: Subsidy Impact on K_{it}/TA_{it} during 2010-2015, Eq (C.1)

	(1)	(2)	(3)	(4)
$Treat_i$ * $Post_t$	0.0243*	0.0269*	0.0220+	0.0238*
	(0.0122)	(0.0118)	(0.0119)	(0.0116)
CF_{it}/TA_{it} (2010-2013) * $Post_t$		-0.2563+		-0.3034*
		(0.1332)		(0.1325)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$			0.0114	0.0170+
			(0.0102)	(0.0102)
Constant	0.0728***	0.0728***	0.0728***	0.0728***
	(0.0036)	(0.0036)	(0.0036)	(0.0036)
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Obs.	2,166	2,166	2,166	2,166
Adjusted R^2	0.6455	0.6495	0.6465	0.6520
Estimated Elasticity (s=0.5)	-.8	-.9	-.7	-.7
Estimated Elasticity ($\bar{s} = 0.43$)	-.9	-1	-.8	-.9

Notes: Clustered Standard Errors at the Individual Firm Level in parentheses. Statistical significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE C.4: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity: Eq. (C.1)

	High Intensity Tech.		**Low Intensity Tech.**	
	(1)	(2)	(3)	(4)
$Treat_i * Post_t$	0.0329**	0.0329	0.0168+	0.0168
	(0.0125)	(0.0214)	(0.0088)	(0.0136)
Constant	0.0750***	0.0750***	0.0709***	0.0709***
	(0.0058)	(0.0058)	(0.0046)	(0.0046)
Year FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes
Obs.	992	992	1174	1174
R^2	0.6557	0.6557	0.7637	0.7637
Adjusted R^2	0.5824	0.5824	0.7139	0.7139
Estimated Elasticity (s=0.5)	-1		-.5	
Estimated Elasticity ($\bar{s} = 0.43$)	-1.2		-.6	

Notes: Robust Standard Errors in parentheses (1) and (3). Clustered Standard Errors at the Individual Firm Level in parentheses (2) and (4). **High Tech** and **Low Tech** firms are defined according to the notes of table 4.19 and 4.20. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}^{Treated}_{2013}/TA_{2013}) * (1/s)$. Note that $\bar{K}^{High-Treated}_{2013}/TA_{2013} = .0658529$, whereas $\bar{K}^{Low-Treated}_{2013}/TA_{2013} = .0617256$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Statistical significance denoted as follows: $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE C.5: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity: Eq. (C.1) 'ctd

	High Intensity Tech.					
	(1)	(2)	(3)	(4)	(5)	(6)
$Treat_i * Post_t$	0.0282*	0.0282	0.0373**	0.0373+	0.0319*	0.0319
	(0.0128)	(0.0212)	(0.0123)	(0.0209)	(0.0125)	(0.0207)
$Sales_{it}/TA_{it}$ (2010-2013) * $Post_t$	0.0173	0.0173			0.0259*	0.0259
	(0.0117)	(0.0168)			(0.0120)	(0.0167)
CF_{it}/TA_{it} (2010-2013) * $Post_t$			-0.2050*	-0.2050	-0.2823**	-0.2823
			(0.1010)	(0.1835)	(0.1039)	(0.1864)
Constant	0.0751***	0.0751***	0.0749***	0.0749***	0.0750***	0.0750***
	(0.0058)	(0.0058)	(0.0058)	(0.0058)	(0.0057)	(0.0057)
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	992	992	992	992	992	992
R^2	0.6574	0.6574	0.6581	0.6581	0.6614	0.6614
Adjusted R^2	0.5839	0.5839	0.5847	0.5847	0.5883	0.5883
Estimated Elasticity (s=0.5)	-.9		-1.1	-1.1	-1	
Estimated Elasticity ($\bar{s} = 0.43$)	-1		-1.3	-1.3	-1.1	

Notes: Robust Standard Errors in parentheses (1), (3) and (5). Clusteterd Standard Errors at the Individual Firm Level in parentheses (2), (4) and (6). **High Tech** firms are defined according to the criteria showed in table 4.19. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. Note that $\bar{K}_{2013}^{High-Treated}/TA_{2013} = .0658529$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43% Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE C.6: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity only Manufacturing Sector: Eq. (C.1)

	High Intensity Tech.		Low Intensity Tech.	
	(1)	(2)	(3)	(4)
$Treat_i * Post_t$	0.0558+	0.0558	0.0200*	0.0200
	(0.0336)	(0.0650)	(0.0091)	(0.0137)
Constant	0.0737***	0.0737***	0.0640***	0.0640***
	(0.0118)	(0.0119)	(0.0077)	(0.0077)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Obs.	198	198	473	473
R^2	0.6840	0.6840	0.7753	0.7753
Adjusted R^2	0.6085	0.6085	0.7259	0.7259
$\bar{K}_{2013}^{Treated}/TA_{2013}$:	.0846592		.0597846	
Estimated Elasticity (s=0.5)	-1.3		-.7	
Estimated Elasticity ($\bar{s} = 0.43$)	-1.5		-.8	

Notes: High-tech and Low-tech manufacturing firms are defined according Tables 4.15 and 4.16, respectively, with Type=M. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses (1) and (2). Clustered Standard Errors at the Individual Firm Level in parentheses (2) and (4). Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

TABLE C.7: Subsidy Impact on K_{it}/TA_{it} by Technology Intensity only
Service Sector: Eq. (C.1)

	High Intensity Tech.		**Low Intensity Tech.**	
	(1)	(2)	(3)	(4)
$Treat_i * Post_t$	0.0301*	0.0301	0.0179	0.0179
	(0.0131)	(0.0215)	(0.0137)	(0.0212)
Constant	0.0703***	0.0703***	0.0768***	0.0768***
	(0.0065)	(0.0065)	(0.0058)	(0.0059)
Year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Obs.	772	772	684	684
R^2	0.6559	0.6559	0.7646	0.7646
Adjusted R^2	0.5815	0.5815	0.7139	0.7139
$\bar{K}_{2013}^{Treated}/TA_{2013}$:	.0529614		.0638714	
Estimated Elasticity (s=0.5)	-1.1			
Estimated Elasticity ($\bar{s} = 0.43$)	-1.3			

Notes: High-tech and Low-tech manufacturing firms are defined according Tables 4.15 and 4.16, respectively, with Type=S. The *pre-treatment* period goes from 2010 to 2013. Firms, in addition, according to the program rules were allowed to carry on investments up to 31/12/2015, starting in 01/01/2014. The implied elasticity is computed according to the following formula: $(\hat{\beta}^{DD}/\bar{K}_{2013}^{Treated}/TA_{2013}) * (1/s)$. The average subsidy awarded by Regione Campania to eligible firms amounts, on average, to 43%. Robust Standard Errors in parentheses (1) and (2). Clustered Standard Errors at the Individual Firm Level in parentheses (2) and (4). Statistical significance denoted as follows: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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