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Preface

Health systems in advanced economies are facing a set of major issues: first, a rapid population aging, which raises long-term expenditure and chronic care needs, coupled with a severe de-growth in fertility trend; second, the existence resource constraints that challenge the sustainability of care. In order to tackle such challenges, rigorous empirical evidence is required on both sides of the healthcare market, the demand and supply one. The present dissertation attempts at fitting into the empirical health economics debate, by centering on both dimensions: first on the *demand*-side —how individuals adjust health and reproductive behaviors when macro shocks challenge their budget constraints and/or alter their ordinary behaviors—, then on the *supply* one — as in, how institutions produce and diffuse medical knowledge under diverse funding and incentive schemes.

Chapter 1 studies the demand side by examining how the COVID-19 job-suspension and furloughing policy, which suddenly shifted time allocation and both social and economic security in 2020, affected the abortion patterns of Italian women. By leveraging cross-municipal variation in the share of workers suspended (and furloughed) from “non-essential” activities within a difference-in-differences framework, the chapter makes use of administrative data to document that municipalities in the upper tail of the suspension distribution experienced a short-lived *increase* in abortion rates of roughly one-tenth of the average quarterly rate, despite an overall lockdown-induced decline. The effect is primarily driven by areas with higher industrial furloughing and by married women, out of the labor force, who have already mothered one or two children; by contrast, pregnancies culminating in live births nine months later show no differential change. These patterns hint to within-couple channels and economic uncertainty rather than supply restrictions of services as the main drivers.

Chapter 2 turns to the supply side and asks how different research incentive regimes shape medical knowledge production within a major private Italian hospital. Exploiting a number of various difference-in-differences strategies around two sequential policy changes—the introduction of a performance-based Management-By-Objectives (MBO) bonus scheme, explicitly rewarding publications, and the designation of the entity as a research-accredited institute, thus unlocking dedicated public research funding—the chapter shows that the MBO, on its own, did not raise output among

non-academic clinicians, whereas IRCCS recognition substantially boosted the publication rates of academic, more research-oriented physicians; meaningful gains appear for non-academics only when both policies unfold for them. The analysis also displays the absence of effects on cross-group co-authorship, with heterogeneous consequences on citations and research quality. This highlights that broad, stable research funding outperforms narrow performance pay in this context, although less-academic oriented researchers may value quality over quantity in collaborations and publications.

Taken together, the two essays show how macro shocks and institutional design jointly shape reproductive health outcomes and the production of medical innovation: understanding unintended fertility responses is essential for policy makers in the context of demographic change, while getting research incentives and funding mechanisms right is paramount to better understand the productivity and diffusion of effective (and efficient) healthcare. In societies which are growing older, and demand burdens in the face of reduced fiscal space and varying labor market supply, the evidence collected here may highlight the relevance of policies able to both anticipate behavioral responses on the patient side in case of unexpected health shocks requiring sudden labor-market interventions, and ensure a sustained and mission-oriented funding strand for health providers/researchers.

Chapter 1

Furloughed workers, abortions and fertility

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Abstract. We investigate the effect of a COVID-related policy that mandated temporary work suspensions in specific industries on Voluntary Pregnancy Terminations (VPTs) and fertility decisions within households. We exploit the variability in the share of furloughed workers across Italian municipalities to estimate in a DiD framework the impact of closures on abortion rates and pregnancy rates. Relying on administrative data on VPTs and births, we find that—although social distancing caused an overall drop in abortions—municipalities in the 4th quartile of the furloughed workers’ distribution experienced a positive differential in quarterly ARs (10-13% relative to the mean prepandemic rate), compared to those in the lower tail of the distribution. The effect is mostly driven by married, non-working women in households with 1-2 children. We also find that the result holds for furloughed workers in the industrial sector. This suggests that economic insecurity during the first months of the pandemic in households with children, where the sole earner is the man, is the likely driver of the effect on VPTs. In line with this interpretation, we do not find any effects on births.

Keywords: Furloughed workers, Abortion, Fertility, Covid-19.

JEL Classification: I12, I18, J13, J16.

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The empirical analysis has been carried out at ISTAT's Microdata Analysis Laboratory (ADELE) and complies with the relevant legislation on the protection of statistical confidentiality and protection of personal data. The results and opinions expressed are the authors' sole responsibility and do not involve official statistical officers. The analysis makes use of administrative data directly referring to the recorded universe of the variables under question, thus they need not be weighted by the coefficients of carry from surveys to the universe

1.1 Introduction

The positive cross-country correlation between GDP per capita and fertility, recorded in recent decades—mostly explained by improved family planning, women’s empowerment and control of their fertility¹—suggests that households’ (and women) economic conditions matter for fertility decisions. In turn, fertility decisions have significant economic and policy implications (Becker, 1960), affecting, e.g., family and child-care interventions, public and reproductive health, gender equality, and, to a greater extent, welfare systems and economic growth. The issue is crucial in the context of the sharp demographic decline experienced in developed countries (Lesthaeghe, 2010).

Voluntary Pregnancy Termination (VPT) is one of the mechanisms that have eased family planning choices and empowered women (e.g., Clarke, 2023). VPT is a surgical procedure that allows for the termination of an unwanted pregnancy and can carry psychological consequences for women. Global statistics indicate that about 35% of European and North American pregnancies between 2015 and 2019 were unintended (Bearak et al., 2020), and Buckles et al., 2019 estimate that the decline in American fertility between 2007 and 2016 ought to be credited to a 35% reduction in originally unplanned pregnancies.

In this paper, we aim to study how household (and, in particular, women’s) economic insecurity impacts the demand for VPTs. As an exogenous shock to insecurity, we exploit the discontinuity induced by the COVID-19 pandemic, which caused an unexpected, remarkable breakthrough in several aspects of daily life. In particular, we leverage a policy implemented by the Italian government aimed at counteracting the spread of the virus in the immediate outburst of the crisis, which mandated temporary work suspensions in specific industries considered as “non-essential” (e.g., manufacturing of non-essential goods like furniture and cars, clothing and shoe stores, theaters, museums, and personal care services) between the end of March and June 2020. Italy has been one of the first countries in Europe to be hit by the virus, and one of the most affected. The analysis is based on quarterly data at the municipal level on abortion rates (AR) from 2018 to 2021, computed from the administrative archive of the universe of abortions collected by the Ministry of Health and managed by the Italian National Institute of Statistics (ISTAT). To estimate the impact of insecurity, we use a TWFE Difference-in-Differences approach and define the “treated” units by leveraging the distribution of furloughed workers.² While stay-at-home orders, homogeneously imposed across the whole country, have likely reduced non-committed sexual encounters, probably triggering an overall drop in unwanted pregnancies (hence, abortions)³, the impact of furloughed workers has been heterogeneous across areas and increased time

¹See, e.g., Doepke et al., 2022. Relevant factors for these changes include the easier access to female contraceptives (Goldin and Katz, 2002, Bailey, 2010) and abortions (Myers, 2017), education and technology-driven “social sterility” (Baudin et al., 2015), shifts in parental times’ usage due to the ‘marketization’ of childcare (Del Boca, 2002, Bar et al., 2018), improvements in women’s bargaining power within couples (Gobbi, 2018, Doepke and Kindermann, 2019), and changes in family size norms (de Silva and Tenreiro, 2020).

²This strategy has already been used to analyze mortality and mobility (Borri et al., 2021), the spread of contagion (Di Porto et al., 2022), and the impact of economic insecurity on electoral outcomes (Bordignon et al., 2023) in Italy.

³For instance, Trommlerová and González, 2024 observe a relevant drop in abortion rates in Spain when the lockdown was implemented. They explain the observed drop with the overall reduction of social interactions.

for sexual activity between partners. In this case, the impact on unwanted pregnancies and abortions can either increase or decrease. On the one hand, increased time within couples may raise the exposure to intimate partner violence, which can result in unplanned pregnancies and an increased demand for VPT. On the other hand, given the insecurity created by the virus, women may be more cautious about avoiding unplanned pregnancies, hence reducing the demand for VPT.

Our results suggest a positive differential in abortions where the share of furloughed workers was higher. Compared to municipalities in the lower tail of the distribution of furloughed workers, we find that municipalities hit more by the pandemic (i.e., the 4th quartile) experienced a positive difference in quarterly ARs of resident women by slightly more than 10 p.p. (corresponding to more than 10% of the average quarterly municipal AR in our data). However, the effect is short-lived, and the differential impact disappears after 6 months.

A major concern to our identification is its integration to the general context of the pandemic: similar non-pharmaceutical public policies and the overall trend of contagion and deaths may have contributed to affect the supply of abortion services, due to mobility restrictions, hospital workload and other limitations in reproductive assistance. In this regard, the study is provided with several robustness checks to rule out the channel of supply restrictions. Overall, evidence seems to show the absence of an impact of workers' suspension on the average provision of abortion services. We integrate the abortion analysis in further different directions, by employing the same DiD empirical design, but changing outcomes: we apply the same analysis on the municipal rates of pregnancies culminating, 9 months later, into live births, for which we have administrative data on birth registrations. The aim is to check whether the sudden suspension shock also affected more thoughtful and time-elapsing aspects of family planning. We find no effect on pregnancies resulting into live births. Second, we perform a thorough heterogeneity analysis by making use of detailed demographic and socio-economic information on aborting women, and by differentiating the identification between the suspended share of service workers and that of the industry. We retrieve that results are mostly driven by married women who are not working nor seeking for a job (out of the labor force, *OLF*). Concerning the treatment, the effect seems to be brought about by the suspension of industrial workers, while the economic branch of activity of women in the labor force does not seem to matter. These results point out to the direction that the policy might affect women mediated by their partner's job trajectories, rather than a direct socio-economic impact of the pandemic insecurity in their field of employment. Third, and for the first time in this strand of literature (in our knowledge at least), we explore the information on previous pregnancies of aborting women.. We retrieve that the effect of the work suspension is significant mostly for women that have already 1 or 2 kids, and for those who had aborted at most once in the past. Such outcomes may leave room to the hypothesis of socio-economic insecurity being part of the general explanation of the findings, and as a plausible argument in favor of female vulnerability or lack of independence with respect to male breadwinners. This also provides with relevant insights on how Italian couple may respond to exogenous shocks by adjusting their *ex-post* decisions concerning involuntary fertility.

The contribution of the present paper is threefold. First, it can be encompassed within the literature analyzing how socio-economic shocks affect fertility patterns broadly speaking, by adding relatively unexplored elements other than the mere hypothesis of resource constraint being caused by income or job loss: more time availability due to unemployment/job suspension; economic uncertainty meant in a broader sense; past reproductive behavior. Concerning unemployment shocks, Currie and Schwandt, 2014 found out that young women experiencing higher unemployment rates are associated to fewer live births; Bardits et al., 2023 and Cavallini, 2024 show instead that fertility rates are affected by job displacement and local unemployment; they both conclude that live births respond negatively. Some papers tackle the topic in the light of a work-motherhood trade-off: Del Bono et al., 2012, 2015, find out that job displacement negatively impacts fertility, mostly for high-skilled women working in career-oriented jobs⁴. Local negative income shocks also have a negative effect on fertility; real estate market price increases may play a detrimental role on fertility rates (Dettling and Kearney, 2014), as well as local income fluctuations (Schaller, 2016; Schaller et al., 2020). Demographic works found an association between the general socio-economic uncertainty and bleak prospect brought about by economic crises (such as the Great Recession) and low fertility rates too (Modena et al., 2014, Comolli, 2017, Caltabiano et al., 2017, Fahlén and Oláh, 2018).

Second, the present work aims at contributing to the quite novel economic literature on the determinants of abortion patterns. The so-called “economics of abortion policy” (see Clarke, 2023) has actually produced numerous contributions over the last three decades, mostly addressing the effect of the access to abortion (as in, abortion liberalization) on several social outcomes, improving female welfare and empowerment. Regarding the determinants of abortion patterns, mainly birth control practices have been tackled by research. A substantial strand of applied economic literature, mostly focusing on the Anglo-Saxon countries, assessed how abortion rates are impacted by reproductive health factors, such as contraceptive diffusion (Girma and Paton, 2006, 2011, Bailey, 2010, Ananat and Hungerman, 2012, Bentancor and Clarke, 2017, Cintina, 2017), and access restriction due to closure of abortion clinics (Colman et al., 2013, Fischer et al., 2018, Lindo et al., 2020, Venator and Fletcher, 2021). By contrast, along the past few years, a line of empirical research on the socio-economic determinants of the abortion demand has soared, mostly concerning the relationship between income/labor market opportunities and fertility: Herbst, 2011, for instance, displays how increases in income tax-credits lowered abortions in the U.S. Regarding family policies, González, 2013; González and Trommlerová, 2023 analyzed how the introduction (and cancellation) of child benefits influenced Spanish abortion rates. Some studies on abortions inquire both those and childbirth rates: for instance, Bardits et al., 2023 investigate the positive effect of mass layoffs on pregnancy termination choices amongst Hungarian women, while Cavallini, 2024, uses a Bartik IV strategy to find a positive association between provincial unemployment and abortion. Few recent studies assess even further determinants: González et al., 2023

⁴Such evidence is consistent with later literature on the so-called “motherhood penalty” (Adda et al., 2017), according to which the child penalties usually associated to parenthood bring about lower earnings trajectories for women, in doing this contributing to widen the gender gap (Goldin, 2014, Angelov et al., 2016, Kleven et al., 2019, Lucifora et al., 2021, Zandberg, 2021, Core, 2024).

investigate the role of Spanish political partisanship on abortion rates, while the findings of Pieroni, Rossellò Roig, et al., 2023 highlight that granting legal status to immigrant women in Italy substantially reduced their abortion rates. Abortion being legal notwithstanding, in some countries (like Italy or Spain) gynecologists are granted the right of conscientious objection for cultural or religious reasons, and more than half of them resort to such device. This seems to impact the exercise of the right to abort for the women seeking for it (Bo et al., 2015, Autorino et al., 2020, Muratori, 2023). The interaction between a legal right and a restricted supply can even bring about relevant long-term repercussions. S. Miller et al., 2023 and Londoño-Vélez and Saravia, 2025 surveyed indeed the long-run worsening of health and labor outcomes of women (and their children) to whom abortion is denied, albeit for gestational limits.

Eventually, our work aims at extending the knowledge about the consequences of Covid-19, in terms of fertility and gender disparities. Among the other things, fertility has been studied in relation to the initial pandemic phase-related birth trends. Early demographic predictions by Aassve et al., 2021, displayed a baby bust in most advanced countries in the immediate aftermath of the pandemic, with Sobotka et al., 2023, retrieving that the downturn observed just after the outbreak was mostly short-lived. A relevant economic appraisal in such heading is the one by González and Trommlerová, 2024, who assess the impact of the pandemic’s restriction policy on Spanish fertility rates, by retrieving a pattern similar to the mentioned ones. In Italy, one of the countries hit the earliest and the hardest, demographic studies underlined the negative impact of insecurity on family planning decisions (Luppi et al., 2020, Rosina et al., 2022). To this extent, fertility choices are a key matter in the context of the pandemic crisis, which has been shown to be affecting women harder than men, compared to previous crises. For the latter reason, many scholars have re-branded the pandemic crisis as a “She-cession”. The various policies and social distancing had indeed a sectoral-specific economic impact (Dingel and Neiman, 2020), whereas the affected sectors were characterised, on average, by larger female employment share. Therefore, the extent of the She-cession features job loss in general, more significant for women than men (Adams-Prassl et al., 2020, Alon et al., 2021, Crossley et al., 2021, Montenovio et al., 2022, Bluedorn et al., 2023), the positive shifts of female labor supply from market to childcare and home care (Alon et al., 2020, Del Boca et al., 2020, Hupkau and Petrongolo, 2020, Zamarro and Prados, 2021, Deryugina et al., 2021), and a detrimental impact on female mental health (Galasso et al., 2020, Etheridge and Spantig, 2022, Barili et al., 2024). Regarding the relationship between fertility and Covid-related remote working (which has the faculty to notably affect households’ time consumption patterns), Kurowska et al., 2023 find out how highly tele-workable tasks are negatively associated with fertility intentions, due to the increase in the blurriness of the limits between work and free time once most activities in both contexts were to be performed at home; their claims are not causal though.

There is also some cross-county evidence on the impact of Covid-19 on abortion access, although they focus mostly on the supply of pregnancy interruption services rather than on the demand side (Bojovic et al., 2021, Ong et al., 2023), while the medical paper by Thacher et al., 2024 uses Swedish data to assess the impact of the pandemic on deliveries and induced abortions through the age channel. In terms of the

covered subject, the work by Trommlerová and González, 2024 is the closest existing contribution, as it studies the negative impact of the lock-down on abortion patterns, observed in Spain due to the decrease in social interactions, although their methodology relies on a temporal discontinuity which does not account for other forms of territorial heterogeneity. To a certain extent, Cavallini, 2024’s work is also very alike to ours, since it studies the role of local unemployment in driving up (down) abortion (fertility) rates. In addition, by focusing on Italy, she exploits the very same dataset which we use, although her levels of frequency and aggregation are the provincial and annual ones, unlike our municipal, quarterly disaggregation⁵. Plus, we try to, if not uncover at least underline the potential existence of asymmetric channels operating through the job trajectories of different genders within couples.

The work is structured as follows: after the introduction, which includes a literature review (Section 1.1), we present the institutional framework on abortion laws in Italy and on how the government responded to the COVID-19 outbreak (Section 1.2). In Section 1.3 we describe the data, whereas Section 1.4 introduces the identification strategy and the empirical model, whose results are reported in Section 1.5. Section 1.6 is dedicated to robustness checks, to internally validate the estimates. In Section 1.7 we perform heterogeneity analyses to better dwell into our findings, while in (Section 1.8), the same is done through some ancillary evidence explored via other data sources (live births, domestic violence, contraception). After a brief discussion on feasible research implications, concluding remarks are reported in Section 1.9.

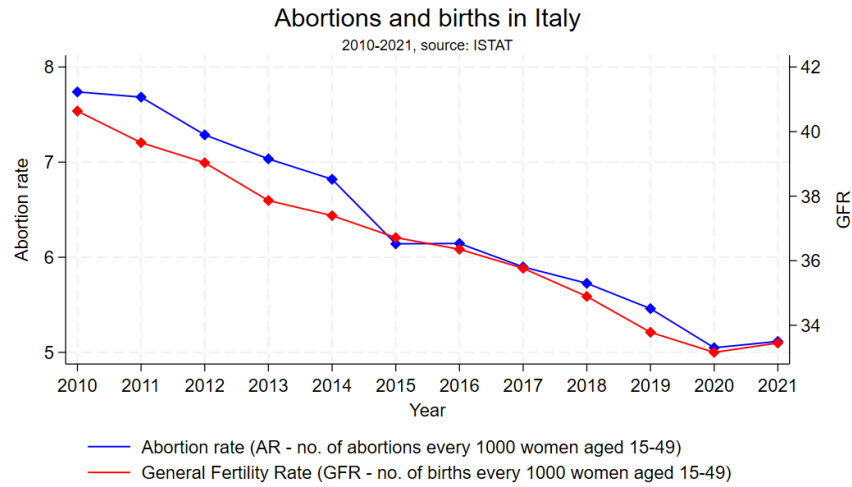
1.2 Institutional Background

1.2.1 Abortion in Italy

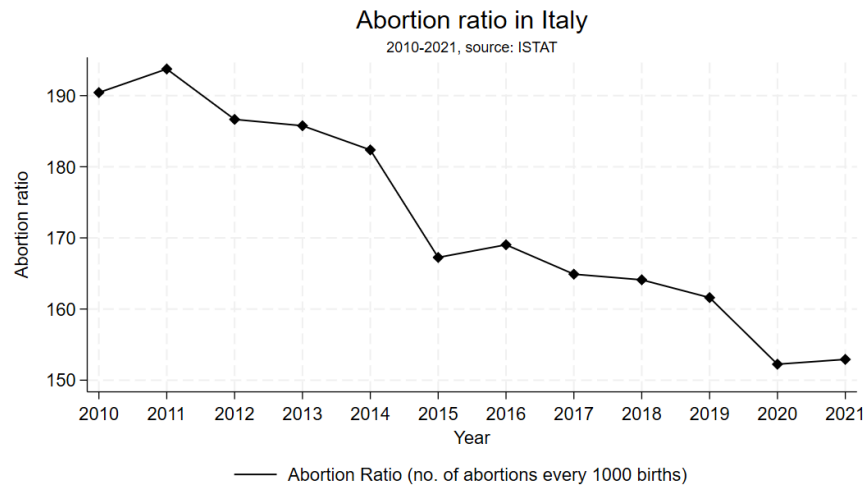
The Italian National Health Service (*Servizio Sanitario Nazionale* in Italian; *NHS* from now on) is a public, tax-funded system which offers universal coverage for a broad set of services. Voluntary Pregnancy Terminations (VPT, in Italian *Interruzione Volontaria di Gravidanza*, IVG) are provided by the NHS free of charge since 1978, as it was made legal by L. no. 174 1978. A woman can undertake a VPT within 90 days from the conception, provided that pregnancy, childbirth, or maternity may compromise her health, either psychic or physic, taking into consideration her health, economic, social or family characteristics, the occurrence during which the pregnancy happened, or the possibility that the newborn will suffer from malformations or anomalies. The looseness of such conditionalities enables any woman who demands the abortion service within the specified terms to interrupt her pregnancy, setting abortion as a woman’s legal right. After 90 days, a pregnancy can be interrupted only when the mother’s mental and/or physical health is in danger. The service can be performed to any woman, irrespective of her citizenship, her residence, and her potential partner’s consent. To do so, a gynaecologist must first certify the occurred pregnancy and indicate a

⁵The other relevant works utilizing the same ISTAT source on abortions are Bo et al., 2015, Autorino et al., 2020, Pieroni, Rossellò Roig, et al., 2023, Muratori, 2023

presumed date of conception; then, unless the situation requires urgency, the woman has to wait a week of reflection before undergoing the treatment.



a)



b)

Figure 1.1: Trends followed by major fertility-related variable between 2010 and 2021. Panel a) reports the pattern of the Abortion Rate and that of the General Fertility Rate; Panel B) the abortion ratio's pattern. Source: ISTAT

The VPT, whether it is a surgical or medical one, must be performed inside a hospital, and the anonymity of the aborting woman shall be guaranteed. In case of a minor, undertaking the VPT requires the consent of her parent or tutor. Art. 9 of Law 194 also regulates the right for gynaecologists and other health professionals to exercise conscientious objection, i.e. to avoid administering a VPT to a woman, unless she is in danger for her life. In Italy, conscientious objection is widespread among doctors and health professionals, mostly for religious, moral or career advancement reasons (de la Fuente Fonnest et al., 2000, Muratori, 2023). The hospitalization requirement is fundamental to monitor the women's health after the abortion; in this regard,

the National Institute of Health (*Istituto Superiore di Sanità*, ISS) has maintained a surveillance system on Italian VPTs since 1978 (*Sistema di sorveglianza epidemiologica delle IVG*), based on communication with health authorities, the Ministry of Health, and the National Institute of Statistics (ISTAT).

Official data (Figure 1.1), show a persistent decreasing trend in the abortion rate (AR - i.e. the number of VPTs every 1000 women aged between 15 and 49 years) over the last decades, and in the General Fertility Rate (GFR, number of live births every 1000 women in fertile age) too. In 2021, both variables seem to have faced a little rebound after the 2020's trough, possibly related to the Covid-19 outbreak aftermath (Rosina et al., 2022). The official 2021 report made by the Ministry of Health (MoH, 2022) confirmed that the trend (63.653 VPTs in 2021) was maintaining its long-lasting decline since 1983. According to the report, the major drivers of the fall in abortion rates are the legalization of abortion itself, the increase in utilization of contraceptives and information about them, and the support provided by professionals in health centers and reproductive care facilities.

Geographically speaking, the pattern for abortions results quite heterogeneous (see Figure A1 in Appendix 1.10). In such regards, conscientious objection is credited as a major setback to abortion supply. Some facts about that are worth to mention indeed. The Ministry of Health reported that conscientious objection, in 2021, involved 63.4% (in 2020 they were 64.6%) of Italian gynaecologists, a rate characterized by strong regional heterogeneities, ranging from 17% (autonomous province of Trento) to 85% (Sicily). In general, conscientious objection is mostly spread in the Southern and North-Eastern regions, as it is shown by Figure A2 in the Appendix 1.10. These percentages are not, however, deemed as a substantial obstacle to the right to abort by the Ministry of Health. Recent contributions tried to challenge the latter conclusion. According to Bo et al., 2015, conscientious objection significantly increases the workload of non-objectors and amplifies the waiting times to undertake an abortion. Autorino et al., 2020 find instead a positive correlation between the regional rates of objection, waiting times and the flow of women aborting outside of their region of residence. Muratori, 2023 even challenges the accuracy of the data collected by the Ministry: after collecting data on objection at provincial level, she retrieves a detrimental positive correlation between objection and illegal terminations of pregnancy, "hidden" by miscarriage data.

1.2.2 The Covid-19 Pandemic and the Italian Economy

As of the beginning of 2024, Italy results as the 8th Country in the world for total cases of Covid-19 (see Worldometer); in fact, in January 2nd 2022, with 6.442 million cases and 137,000 deaths, it was one of the countries hit the most by the pandemic. After discovering the virus in China in December 2019, the first Italian case of Covid-19 was registered in Lombardy in February 2020; then, the disease rapidly spread across the region and the whole nation with severe impacts on mortality and public health, as documented by early studies (Buonanno et al., 2020, Depalo, 2021, Cerqua et al., 2021). The health consequences persisted all over 2020 and 2021, as excess mortality, measured as mortality in such biennium net of the mean of mortality

in the years between 2015 and 2019, saw a significant increase (see Figure D1 in Appendix 1.13).

Being the first European country having to deal with such a fast spread of contagions, Italy was also the first one to undertake strict measures; after applying some mobility restrictions to the early affected areas, the government declared a national lockdown on 9th March 2020, paralyzing the whole Country. On 11th March 2020, a further decree of the President of the Council of Minister (DPCM) imposed the suspension of common retail and schooling activities, the closing of restaurants and the ban on meetings in public spaces. Such restrictions were exacerbated with a DPCM on 22nd March 2020, which provided with a distinction between “essential” and “non-essential” activities, based on their ATECO-5 digit classification. Non-essential activities were suspended, and all the related enterprises and affiliated workers with them. In the meantime, the government undertook massive action to deal with the harmful economic repercussions stemming from the health crisis. On the 17th March, the legislative decree *Cura Italia* (*Cure Italy*) was issued. Through the latter, an amount of €4 billion was allocated to the *exceptional fund for supplement earnings* (*Cassa Integrazione*, or CIG), which was exploited by private employers forced to reduce or suspend their activities during the pandemic to pay wages and compensations. The allowance was a resort to employers who did benefit from the standard pre-pandemic CIG as well as to those who did not. Wage subsidies requirements were also loosened, and several simplification measures and exemptions were guaranteed to employers with respect to social security contributions. The government also introduced a €600 indemnity for free-lancers, traders, farmers and seasonal workers, an “emergency income” ranging from €400 to €800 for households in hardship, and several lump-sum bonuses to specific vulnerable categories (MEF, 2020). The decree also suspended dismissals for companies already resorting to supplement earnings (*firing freeze*), forbade house ejectments (*eviction freeze*), and imposed a loan standstill for SMEs and free-lance professionals (*moratorium*).

Such measures were implemented as a response to the economic problems caused by the lockdown and job suspension orders, and aimed at protecting the most vulnerable citizens and workers in a period of high economic uncertainty. Although planned as momentary interventions meant to last for few months, they ended up being periodically renewed and prolonged all over 2020 and up until 2021; at the end of 2020 alone, the government had outlaid €35 billion circa to support work (MEF, 2020). On the other hand, the national lock-down lasted until the 4th May 2020 only, while economic closures of non-essential activities started being loosened gradually; the process lasted as far as the summer, when most businesses had re-opened. After an almost complete return to normality over the summer, Fall 2020 saw a resurgence of cases and deaths. Starting from 6th November 2020, and until late June 2021, a national curfew was declared all over the country, while different degrees of restrictions were imposed at regional (in some cases provincial) level, according to the severity of the contagion. Regions started to be classified weekly in four categories marked by specific colors, depending on contagion diffusion. Details on the mobility and economic restrictions imposed during the “coloring” times are described in Conteduca and Borin, 2022. Notwithstanding some commercial and labor restrictions imposed to enterprises (like opening curfews and workers’ safeguards), the performance

of all economic activities would never be shut down again. After some further restrictions between the end of 2021 and the beginning of 2022 due to a new wave of cases, the emergency state, initially declared on 31st January 2020, ceased on 22nd April 2022.

1.3 Data

The present work requires the matching of various sources of data. Concerning the outcomes, ISTAT collects the annual cross-sections of all Italian abortions, as envisaged by the epidemiological surveillance system on Italian VPTs. These surveys are available upon request at the ISTAT Laboratories for the analysis of microdata, ADELE. The present study makes use of the universe of VPTs performed in Italy between 2018 and 2021, available at monthly frequency and collapsed quarterly for the purpose of the present work⁶. The choice of the time-span stems from such years being the ones around the Covid outbreak, for which also the aborting women’s municipalities of residence have been collected (such information is not made available by ISTAT for previous or subsequent years indeed). The total number of VPTs during these years amounts to 276,760 abortions. We remove about 8,500 observations with relevant missing information or concerning women residing abroad. Our final sample is composed by 268,054 abortions, undertaken by women using the legal VPT service.

The annual cross-sections are then aggregated into a municipal quarterly panel of 126,448 observations. The dataset also contains relevant information about women’s demographics (year and province of birth, macro-area, region, province and municipality of residence, citizenship, marital status, age class, whether they are minor), socio-economic status (level of education, professional condition, professional position, economic branch of activity), health-related data concerning previous pregnancies and the current one (number of previous VPTs, miscarriages, pregnancies, live births and stillbirths, gestational age, fetal malformation and number of weeks of amenorrhea). There are also details on the VPT procedure (urgency, analgesia, type of intervention, hospitalization regime, length of stay) and the identification code of the institute where the medical treatment is performed. The main outcome of interest is the Abortion Rate (AR) measured for the municipality of residence of the aborting women and defined by following the official ISTAT definition. As in, the total number of VPTs undertaken by women of a given municipality, divided by the municipal female population in fertile age (15-49 years old).

It is worth noting that our baseline analysis focuses mostly on an observed abortion outcome (abortion

⁶Variables are collapsed quarterly for the following reasons: 1) being abortion trends strongly seasonal (they follow conception patterns), the quarterly aggregation helps removing the bulk of said seasonality; 2) measuring monthly outcomes for almost 8000 municipalities yields an extremely high number of zeros, placing heavy weight on the extensive margin, which is partly offset by aggregating quarterly; 3) the policy was introduced on the 22nd March 2020, at the end of 2020Q1. Accounting for the “week of reflection” before VPTS, the consequences of the policies could only start unfolding in April (except, of course, for the VPTs performed in the weeks between the end of March and the beginning of April, conceived before the closures). Thereby, we can consider the effect to deploy since 2020Q2. To corroborate the design though, we perform the same analysis at different frequencies, including the monthly one (Section 1.7).

rate), rather than a fully-fledged fertility decision framework. As a matter of fact, it would be meaningful to opt for the abortion ratio - i.e., the number of abortions over total pregnancies - as main outcome variable. However, the pandemic might have plausibly affected both the probability of becoming pregnant and that of terminating a pregnancy, but likely not with the same timing and with differently sized effects. Short-run variations in pregnancies may thus enter mechanically in our framework, either deflating or inflating the denominator of the abortion ratio. To avoid this noise in our main analysis, we separately study abortion rates and pregnancies resulting into live birth 9 months later; then, we focus on the abortion ratio in a later section of the paper as an additional analysis.

We also match information about medical facilities reported in our data with the official dataset on healthcare institutions recovered from the open data of the Italian Ministry of Health to obtain additional information about the municipalities, provinces, local health authorities and regions where the VPTs are undergone. Since hospitals are not present in all municipalities, the matched units in terms municipalities-of-hospital/quarters are 5,492, way fewer than the total of the units in the main residence-based panel.

Data on the official annual municipal population per age, which is used both as the denominator of the main outcomes and for weighting reasons in some of the complementary analyses, are recovered from the demographic section of the ISTAT website for the years 2019-2021. For 2018, the inter census reconstruction, made available by the same institution, is used.⁷

We also make use of ISTAT data on municipal registration of childbirths from 2018 and 2021, to have information about the pregnancies which are not terminated. The frequency of birth data collection is daily, and we can recover all 1,664,972 births registered in Italy between 2018 and 2021, with information on the birth date of the registered children, their municipality of residence and that of birth; plus, we know their parents' marital status and the number of minor kids in the household of the head of the family. We are interested in analyzing the impact of the policy on the occurrence of pregnancies which are carried on. Therefore, we look only at births registered 9 months after the periods measured in the sample⁸. We then aggregate at quarterly level to match with our dataset about abortions⁹. Since we do not have the birth registrations in 2022, we cannot calculate conceptions happening between April and December 2021, thus losing the last nine months of the sample. We use information about births to define a further outcome measure, i.e. the pregnancy rate, constructed as the ratio of (estimated) pregnancies carried over and registered 9 months after the supposed conception, over the total population of women in fertile age (15-49).

⁷As the time frequency of the analysis is quarterly, we would better make use of quarterly population statistics as denominator. However, we focus on women aged between 15-49 in a relatively short time frame (4 years): therefore, the quarterly population changes ought to constitute just a negligible noise in our estimates. While birthrates ought not to affect our denominator at all, as female newborns do not go in the denominator, we assume quarterly municipal mortality to be negligible for women aged 15-49 even in presence of the pandemic, as Covid mortality mostly hit the elderly.

⁸Being pregnancies registered on a daily basis, we shift backwards the day of birth of the children by 270 days (9 months), to get a rough estimate of the daily conceptions occurring in Italy in the period under consideration and resulting into live births

⁹Note that disaggregating at more granular frequencies does not affect our estimates on pregnancies.

In addition, we recovered the share of furloughed workers (or *temporarily inactive*) across Italian municipalities during the national lock-down for the year 2020 from ISTAT. The municipal information on the inactive share of workers was disclosed by ISTAT on May 2020. This information was already provided by the Institute in a structured dataset, and does not require researchers to integrate further calculations to obtain the furloughing metrics at the municipal level. The dataset makes use of the Statistical Register on the economic results of Italian enterprises, the *Frame SBS Territoriale (Frame of the territorial Structural Business Register)*. The register contains information on the Italian firms active in the private sector (4.4 million at the considered time), regarding their revenues, value added, workers and employees, and the ATECO-5 digit sector within which they perform their activity. ISTAT thus matched the municipal-sectoral data on local units with the information on the suspended sectors (disaggregated at 5 digits) during the lock-down. By knowing both which 5-digit sector was suspended, and the sectoral weight of each sector on the municipal local units' structure of Italian municipalities, the National Institute enabled a detailed estimate of the number of inactive workers at the local level after the economic closures, and their share over total working population¹⁰.

The sectoral share of workers refers to the local units in 2017 (the most recent one before 2020), confirming that the distribution of the working population was orthogonal to the pandemic outbreak¹¹. As ISTAT provided only the information on suspended units and workers percentages overall, the share of active and suspended workers is not directly available to the researchers for each sector at the municipal level. However, the dataset discloses information about the macro-disaggregation between the industry and the service sector¹². Data involve all 7978 Italian municipalities¹³, which can be aggregated into 610 Local Labor Market Areas¹⁴. Both the municipal and LMA distribution of the suspended workers' share is reported in Figure 1.2.

Eventually, starting from November 2020, Italian areas began adopting heterogeneous restrictions according to the number of contagions registered within their borders (RT index): they were being also classified as “colored zones” (in ascending order of severity: white, yellow, orange and red). To account for that, we add up to the model some covariates, which proxy for the quarterly days of colored regions and for the stringency of the restrictions imposed on given areas, together with the excess quarterly mortality, by making use of the indicators devised by Conteduca and Borin, 2022, better explained in Appendix 1.13. Together with this set of policy covariates, we add data on municipal excess mortality, aggregated at quarterly frequency, as

¹⁰Estimates of workers stem from calculations on job positions and worked hours.

¹¹This may constitute a problem in case of relevant structural and labor market changes occurring in Italy between 2017 and 2020. However, as also highlighted by Borri et al., 2021, this is not the case.

¹²The dataset does not have information on people active in agriculture, hunting and fishing, public administration, finance and insurance, household and self-production activities and international organizations

¹³Since a number of small municipalities merged over the time-frame, we hold the boundaries of said municipalities as it they were always unique merged municipalities. In fact, it is more straightforward to aggregate the population of merged municipalities *before* the merge occurs rather than subdividing it *after*.

¹⁴In the wording of ISTAT, LMAs are “610 sub-regional areas where the bulk of the labour force lives and works, where establishments find the main part of the labour force (...), defined on a functional basis, the key criterion being the proportion of commuters who cross the LMA boundary on to work.”.

provided by ISTAT with the official and publicly available dataset about causes of mortality in Italy¹⁵

Dataset	Content	Main processing for this paper
ISTAT-ADELE (Abortion microdata)	Universe of Italian voluntary pregnancy terminations	* Municipal $\times$ quarter aggregation; * Outcome: Abortion Rate (AR)
	2018–2021	$AR = \frac{VPTs}{women\ 15-49}$
ISTAT-ADELE (Birth registrations)	Universe of birth records (daily)	* Births shifted 9 months back * No 2021 pregnancies from Apr2021 * Outcome: Qterly Preg. Rate (PR)
	2018–2021	$PR = \frac{Pregs.\ turned\ births}{women\ 15-49}$
demo.istat.it (Demographic data)	Municipal population by age and gender	* Denominators for AR and PR
	2018–2021	
ISTAT suspended workers (May 2020)	Municipal share of workers in suspended ATECO 5-digit sectors (No info on single sectors)	* Municipal share present * Treatment intensity (quantiles) * Disaggregated by industry/services
Ministry of Health Open data Facilities registry	Hospital IDs and locations	* Provider-level panel building Quarter $\times$ municipality * For Robustness checks
Conteduca et al. (2022) (IEJ)	Covid restriction indicators	* Controls for heterog. restrictions * Quarterly days of Red Area * Quarterly days of Orange Area * Quarterly days of Yellow Area * Quarterly stringency index
	2020–2021	
ISTAT mortality data	Municipal excess mortality	* Quarterly control for severity. * Covariate: Excess Mortality (EM)
	2020–2021	$EM = \frac{2020\ deaths - avg\ deaths\ (2015/19)}{pop.}$

Table 1.1: Data sources and processing.

Given that the dataset is built by aggregating a vast source of information, we report the sources for

¹⁵This will be made clearer in the next section, but these regressors do not really condition for unobservable confounders; indeed, as these are all post-treatment variables, time-varying omitted heterogeneities related to the pandemic policies may not be captured. This notwithstanding, we add them up to the specification to assess whether the outcome somehow follows the epidemiological pattern of the pandemic anyway, similarly to Franzoni et al., 2024.

each relevant part of the data in Table 1.1. Descriptive information about the abortion dataset, the main under study, are instead presented in Table 1.2.

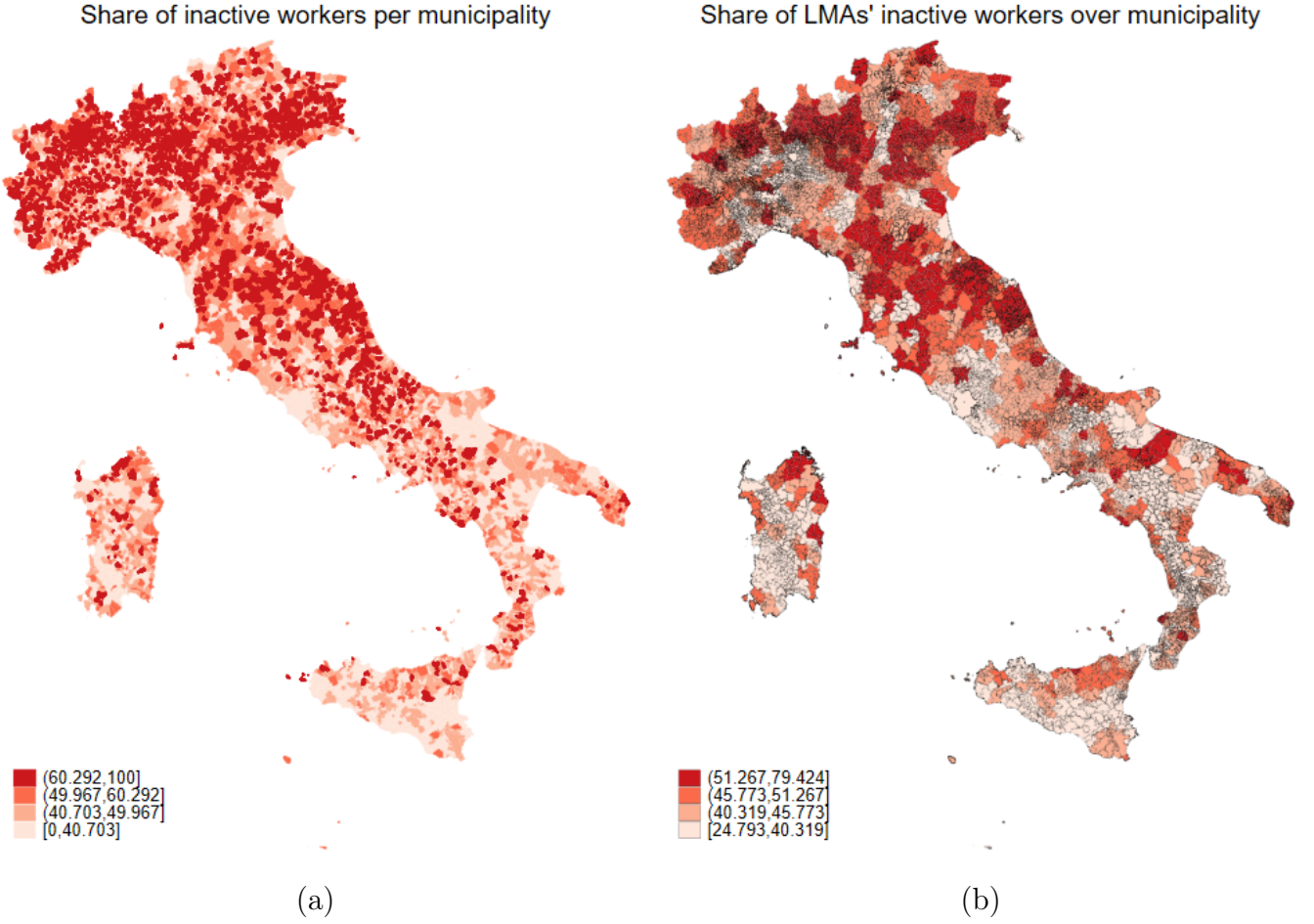


Figure 1.2: Municipal share of suspended, non-essential workers during the Italian national lock-down. The (a) map shows the municipal share of inactive workers during the period of suspension of non-essential activities, i.e. from 22nd March 2020 to 4th May 2020. The (b) map shows the LMA share of inactive workers, albeit highlighting Italian municipality borders. Both maps are shaded according to the position of the unit of reference in given quartiles of the inactive share's distributions.

While the aim is to try and assess whether work suspensions stemming from the governmental policy have a differential impact on abortion decisions, we reckon how, after the implementation of the policy, abortion rates substantially dropped all over the sample, although heterogeneously across the suspended workers' share distribution. In Section 1.8, we argue and try to empirically validate the finding that the observed difference between the municipalities in the upper portion of the distribution and the other ones is not driven by reasons linked to the healthcare supply contraction; however, it is worth to understand why a decrease in VPTs is seen in the available data, in the period involving the lock-down.

	Mean	SD	Min	Max
<i>Abortion Rate (by municipality of residence)</i>	1.092	2.550	0	142.857
<i>Abortion Rate (by hospital of abortion)</i>	6.284	16.947	0	433.962
<i>Pregnancy rate (by municipality of residence)</i>	7.813	6.882	0	333.333
<i>Inactive share</i>	50.254	16.502	0	100
<i>Inactive share (service)</i>	24.739	12.946	0	100
<i>Inactive share (industry)</i>	25.515	17.183	0	100
Covid policy covariates				
<i>Mean quarterly stringency index (since 2020)</i>	52.642	14.400	31.925	77.192
<i>Quarterly days of red area (since 2020)</i>	6.083	10.035	0	55
<i>Quarterly days of orange area (since 2020)</i>	8.576	13.188	0	77
<i>Quarterly days of yellow area (since 2020)</i>	13.417	18.635	0	56
<i>Quarterly days of white area (since 2020)</i>	24.533	37.904	0	92
<i>Municipal excess mortality</i>	0.864	6.483	-145.667	684.933
Observations	126448			

Table 1.2: Descriptive statistics of the main variables used in the present work.

1.4 Empirical Strategy

When, on March 2020, the government declared some sectors as “essential” based on their Ateco code, a substantial share of Italian workers occupied in non-essential activities were suspended from their job. Such suspension lasted from March to middle June (for some activities even later), with the end of the lock-down happening on May 4. The exploitation of the heterogeneity of such share as means of identification is not novel in the Italian economic literature (Borri et al., 2021, Di Porto et al., 2022). The suspended workers’ share functions as a compelling exogenous shift in the general economic conditions (Bordignon et al., 2023), as the distinction between essential and non-essential activities was something never thought about before the already unexpected event of the pandemic, and it was legally imposed by the government; there was no way to anticipate the closures and their impact on the labor market¹⁶. The temporary inactivity of workers caused by the pandemic may represent a reasonable proxy of the unexpected perceived vulnerability for the future embedded by the shock, being able to capture municipal-level dynamics not embedded into provincial employment statistics. Bordignon et al., 2023 utilize survey data to observe how the condition of temporary inactivity was able indeed to shift the vote preferences of the sampled citizens, due to uncertainty for the future; this shall hold for the postponement of fertility. The identification allows for the possibility of an increase in sexual intercourse within cohabiting couples too, as higher prevalence of non-essential workers staying home with their partners might be linked to more unplanned pregnancies. The latter could be linked indeed to the positive shift in time available together, irrespective of economic insecurity.

¹⁶It is relevant to stress the difference between inactive share and unemployment. As notorious, the unemployment share is higher in Southern Italian areas, while the inactive share during the lock-down was mostly greater in the Centre-Northern parts of the Country, due to sectoral heterogeneity motifs. We can observe how (Figure C1 in Appendix 1.12), in terms of mere correlation, the suspended workers’ share across Local Market Areas, was actually negatively associated to their unemployment rate in 2020. Such North-South sectoral asymmetry in suspensions due to the pandemic and unemployment turns out to be consistent with previous literature on Covid-19 and the labor market (Cerqua and Letta, 2022).

With respect to the time dimension, the latter may also affect non-voluntary pregnancies due to non-intended intercourse tied to abusive within-couple dynamics, leading women not to be in control of their own fertility, which potentially involves psychological or sexual IPV (*Intimate Partner Violence*). Data report indeed a peak in the calls to the Italian hot-line for domestic violence after the closure of economic activities in 2020 (Figure B1 in Appendix 1.11)¹⁷.

Holding the latter considerations as fair, it is worth to note that economic uncertainty may also make less affordable the unplanned fertility stemming from the time shock (i.e., there are more unexpected pregnancies, but they are carried over at a lower rate). Eventually, mobility restrictions may have caused a setback to people moving across areas seeking for healthcare, while clogged institutes due to the treating of large numbers of infected people may have been delayed in the undertaking of VPTs (Bojovic et al., 2021), especially those with many objectors; longer waiting times might also lead women to overcome the eligibility time threshold. In such case, we would observe a supply-driven decrease in abortion rates in areas hit harder by the pandemic in epidemiological terms¹⁸: this is tackled in Section 1.5.

We illustrate the identification strategy for our main outcome (the VPTs) by means of two Dyrected Acyclic Graphs (DAG), reported in Appendix 1.12. In Figure C2, we observe the phenomena described insofar, without including the identifying device. We also add a dotted arrow between socio-economic insecurity and a possible, unknown shift in consensual sex, mediated by some unobservable U . As the economic closures were announced two weeks after the national lockdown, the consequences of the two policies deployed almost concurrently (the time dimension is thus neglected), causing a compound effect. In the DAG, we stress how they may have reasonably affected the same channels in the same direction before reverberating on the VPTs; as a matter of fact, the outcome of both policies was the same, i.e. to stay at home. In addition, the lockdown was homogeneous across the whole country, hence it is not possible to disentangle its impact by identifying municipalities hit more severely, as all of them were affected to the same extent. By contrast, the distribution of the suspended workers was strongly heterogeneous. The second DAG (Figure C3) displays what changes in the identification of the effects after introducing such empirical device (indicated as “inactive share” in the picture). By means of the inactive share, we place into the framework a further dimension of variability, which adds up to the homogeneous shift caused by the national lockdown. The paths originating from the latter can thus be closed (as marked by the X). Note that the basic identification strategy, which gives shape to our baseline estimates, only allows to address the “raw” impact of the economic closures on the VPTs, with no discernment across the various channels. Plus, it must be also clear that the inactive share may open an unknown backdoor path related to the link between local economies’ characteristics and VPT trends. As long as these unobservables are not time-varying, we

¹⁷Early estimates displayed a similar pattern in the U.S. after the introduction of shelter-in-home policies (Leslie and Wilson, 2020). However, A. R. Miller et al., 2022, 2023 found no robust evidence of increases in American IPV consistent with the exposure theory. They conclude that the concerns raised by policy-makers news outlet about the possible surge of IPV episodes during lockdowns were the triggering determinant in the increase of calls, which were not linked to a raise in actual violence. Similar results were found in Italy by Colagrossi et al., 2022.

¹⁸Women may also be discouraged to access hospitals in areas where it was more likely to contract the virus.

can close such path by means of fixed effects, while including interacted province by time fixed effects allows to a better degree of control to such extent.

To assess the effect of the work suspensions on Italian VPTs, we estimate via OLS a TWFE Difference-in-Differences model, defined as follows:

$$AR_{mpt} = \beta_1 + \sum_{k=2}^4 \beta_k Post_t \times \mathbb{1}(Inactive\ Share_{q2/2020} \in Q_k) + X'_{mt}\beta + \tau_m + \gamma_t + \varepsilon_{mt} \quad (1.1)$$

The outcome of interest is AR_{mpt} , the abortion rate in municipality m (municipality of residence), in province p , in quarter-year t . $Post_t$ is a dummy taking value 1 when $t \geq Q2/2020$, 0 otherwise. $\mathbb{1}(InactiveShare_{q2/2020} \in Q_k)$ is a binary variable which equals 1 when m belongs to the k^{th} quartile Q_k of the inactive share distribution during the lockdown, 0 otherwise. X'_{mt} is the set of municipal-level, quarterly covariates for restrictions' severity and viral diffusion: specifically, we include the average quarterly stringency index and the total number of red, orange and yellow area days, being white area the omitted category. We also include quarterly, municipal excess mortality, to proxy for the seriousness of the viral impact of the pandemic on referred areas, which further accounts for both health providers' excessive workload and for a higher perception of epidemiological risk (see Franzoni et al., 2024); such post-treatment covariates allow to track whether the phenomenon follows the epidemiological evolution of the pandemic. However, they cannot really condition for other time-varying unobservables related to the infection, as they all are post-treatment variables, reasonably related to other phenomena captured by the error term ε_{mt} . τ_m are the municipal FEs, while γ_t the quarter-year FEs. Provided with the baseline model, we run further estimates by including $\delta_{p,t}$ in the equation; the latter is an interaction between the province of residence and quarter-year dummies, which accounts for the presence of unobservable time-varying heterogeneity that may be affecting VPTs at an aggregate level by following a persistent pattern. The chosen level of aggregation of the interaction is provincial, as the heterogeneity evolution (mirroring the “secular” decline in VPTs) may be possibly linked to the factors underlined by the ISS and the Italian Ministry of Health, such as the evolution of birth control facilities, the presence of reproductive care assistance on the territory or other healthcare-related motifs. Although Italian healthcare is administered at regional level, some of its functions are further de-centralized to Local Health Authorities, so it is meaningful to include an intermediate level of aggregation to capture the time-varying interaction between factors strictly related to healthcare and other provincial socio-demographic changing determinants. The coefficient of major interest is β_4 . It represents the ITT (Intention-to-Treat) of suspending higher shares of workers in given municipalities, on the resulting abortion rate. Since the distinction between treated and controlled units is not neat, as also lower parts of the distribution are affected by the policy, what we do is to make discrete an actually continuous treatment (Callaway et al., 2021). The reference point, as in the omitted category, is the 1st quartile of the distribution.

Additionally, since the unit of observation is municipal, taken at quarterly frequency, the majority of observations (small Italian municipalities with few inhabitants) presents a null rate. Given such high preva-

lence of zeros, and provided for the count data nature of the Abortion Rate, we benchmark our OLS results by estimating the non-linear model in Equation 1.2, as in a Pseudo-Poisson Maximum Likelihood (PPML) panel regression with the same inputs, in the fashion of both Lindo et al., 2020 and Muratori, 2025.

$$\begin{aligned} E(AR_{mpt}|Post_t, Inactive Share_{q2/2020}, X'_{mt}, \tau_i, \gamma_t, \delta_{p,t}) = \\ = \exp(\beta_1 + \sum_{k=2}^4 \beta_k Post_t \times \mathbb{1}(Inactive Share_{q2/2020} \in Q_k) + X'_{mt}\beta + \tau_i + \gamma_t + \delta_{p,t}) \end{aligned} \quad (1.2)$$

The statistics of interest of the latter are the marginal effects of the estimated coefficients β_4 , β_3 and β_2 .

Parallel trend assumption. Since the design is based on a Difference-in-Differences methodology, in order for the estimates to hold robustly, the parallel trend assumption needs to be met. That means, in absence of the treatment (i.e. the repercussions triggered by Covid-related closures), the “treated” (4th quartile) and “non-treated” units (other quartiles) would evolve following their pre-existing path. To robustly control for this, we assume that the counterfactual in absence of treatment for the treated units was the same as before the treatment; then, we compare the evolution of abortion rates in treated units before the treatment, to look at what would have happened had the work suspension never occurred. In order to perform such robustness check, we estimate an event-study equation as follows:

$$AR_{mpt} = \beta_0 + \sum_{j=2}^J \beta_j (Lag\ j)_{mt} + \sum_{k=1}^K \beta_k (Lead\ k)_{mt} + X'_{mt}\beta + \tau_m + \gamma_t + \delta_{p,t} + \varepsilon_{mt} \quad (1.3)$$

The equation above shall be interpreted on the basis of the event of interest, which is $Event_t := 2020Q2$. As in Equation 1.2, m is the municipality of residence of the women. $(Lag\ j)_{mt}$ are the leads, as in the pre-treatment period dummies equal to 1 if associated to the treated units (i.e., belonging to the 4th quartile) and 0 otherwise (other quartiles), with $(Lag\ j)_{mt} = \mathbb{1}[t = Event_t - j]$ for $j \in \{1, \dots, J\}$. On the other hand, the lags are $(Lead\ k)_{mt}$, i.e. the post-treatment period dummies equal to 1 if associated to the treated units again, with $(Lag\ k)_{mt} = \mathbb{1}[t = Event_t + k]$ for $k \in \{1, \dots, K\}$. $(Lead\ 1)_{mt}$ is set equal to 0 as baseline, to avoid multicollinearity. The specification involves, as in the baseline, policy covariates, time and municipality FEs; in a further model we include the interaction dummies for Province x Quarter and Year FEs as well. The coefficients of the event study are estimated both via OLS (Figure 1.3), and Poisson (the latter mirroring Equation 1.3; its MEs are reported in Figure E1 in Appendix 1.14).

1.5 Baseline Results

The overall drop in induced abortions notwithstanding, a visible difference in the patterns of ARs is observed across the suspended workers' share distribution, suggesting it might affect said pattern. By comparing outcomes (VPTs, births) before and after 2020Q2, over the distribution of inactive workers across Italian municipalities (or Local Market Areas), we track the effect of the local economic change on aggregate abortion

decisions, by ruling out the explanations due to lock-down, as the whole country was under such homogeneous restrictive measure for a some time. Work suspensions also acted as a further trigger of the social and economic insecurity brought about by the general bleak economic situation (Bordignon et al., 2023). This mattered especially for women, as the pandemic mostly resulted in the increase in unemployment in given service sectors (Bluedorn et al., 2023), where the share of employed women was higher (Casarico and Lattanzio, 2022).

OLS estimates of Equation 1.1 and PPML estimates of Equation 1.2 are reported in Table 1.3 and Table 1.4, respectively. Results clearly show that belonging to the higher tails of the inactive share distribution has a positive impact on the municipal AR. However, only the dummy for the 4th quartile is statistically significant in both specifications. Column (1) of both tables reports the baseline model results without policy restriction covariates. In Column (3) covariates are added,; in Column (5) the model is integrated by provincial interactions. Columns (2), (4) and (6) of both tables display how the models are consistent when coefficients are estimated through (log-) municipal population weighing¹⁹. The coefficient on the 4th quartile is significant at 1% level in all specifications, with Column (5) of Table 1.3 being significant at 5% level (i.e., the unweighted OLS specification with provincial interactions). The magnitude of the coefficients are very similar across models (Table 1.4 reports Marginal Effects of the non-linear estimates indeed), suggesting that the municipalities belonging to the 4th quartile of the suspended share’s distribution during the lock-down experimented an AR between 11 and 14.6 p.p. higher than municipalities in the 1st one. Such values, quite low in absolute terms, appear more relevant when compared to the mean value of the municipal AR of treated units pre-treatment, as they range between 10% and 13% of the number of abortions every 1000 women in fertile age residing in Italy.

¹⁹Following the same approach used by Pieroni, Rosselló Roig, and Salmasi, 2023.

Impact of work suspension on municipal ARs, linear estimates						
	(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$	0.13134 ***	0.11340 ***	0.12879 ***	0.11133 ***	0.13168 **	0.11087 ***
	[0.04565]	[0.03563]	[0.4587]	[0.03585]	[0.05185]	[0.04061]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$	0.05711 [0.04266]	0.04825 [0.03300]	0.05570 [0.04276]	0.04698 [0.03311]	0.06342 [0.04501]	0.05119 [0.03488]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$	0.03486 [0.03950]	0.02961 [0.03058]	0.03505 [0.03953]	0.02949 [0.03062]	0.03353 [0.02949]	0.02714 [0.03353]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10043	0.10529	0.10046	0.10531	0.11223	0.11621
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. $\times$ Quarterly FE					✓	✓
Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103

Table 1.3: The coefficients in Columns (1) to (6) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs. The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on municipal ARs, non-linear estimates						
	(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR
	(ME)	(ME)	(ME)	(ME)	(ME)	(ME)
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$	0.13978 ***	0.11324 ***	0.13676 ***	0.11082 ***	0.14560 ***	0.11515 ***
	[0.05105]	[0.03904]	[0.05119]	[0.03919]	[0.05580]	[0.04335]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$	0.06016 [0.04669]	0.04614 [0.03527]	0.05855 [0.04697]	0.04471 [0.03537]	0.07307 [0.04834]	0.05239 [0.03685]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$	0.03709 [0.04346]	0.02896 [0.03258]	0.03769 [0.04350]	0.02919 [0.03205]	0.03967 [0.04300]	0.02714 [0.03279]
Observations	116,192	116,192	116,192	116,192	116,192	116,192
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. $\times$ Quarterly FE					✓	✓
Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103

Table 1.4: The coefficients in Columns (1) to (6) represent the Marginal Effects of the municipal Poisson Pseudo-Poisson Maximum Likelihood (PPML) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs. The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Estimates from the event-studies show the absence of significant pre-trends at both 10% and 5% levels, although some level of seasonality (to be possibly credited to a cyclical pattern of pregnancies) seems not having been fully removed from the quarterly-frequency aggregation. In general, during the first two quarters of the quasi-experiment, abortion rates seemed to be significantly higher in treated municipalities over the 6 months after March 2020, (i.e., from April to September), before turning back to be statistically similar to zero upon the last quarter of 2020, when coloring areas started being implemented. The observed pattern suggests that the bulk of the effect we observe in the DiD baseline estimates was concentrated in the months immediately after economic closures, which were the ones involving the actual suspension of concerned workers (April, May, part of June), the whole Summer, and September. During the latter months, the significance of the effect might have been driven by a slow reintegration of suspended workers to their usual tasks, possibly tied to the presence of informal/precarious contracts and remote working, coupled with the period of summer holidays. At the beginning of 2021, the effect seemed to be already fully re-absorbed. To support the credibility of the parallel trend assumption, we need to address the feasibility of having unobservable time-varying confounders (which may be leading to non-parallel patterns). Accounting for a hypothetical violation of common trends, we follow the so-called “honest” approach developed by Rambachan and Roth, 2023, which enable us to perform inference and sensitivity even in presence of (small) deviation from parallel trends²⁰. Their methodology is based on bounds on relative magnitude: we replicate it by estimating the entity of the deviations from the common trend after the treatment that would make our outcomes non-valid according to the pre-trend assumption, relative to the maximum pre-treatment violation.

²⁰One of the major reasons for which they introduced such methodology of checking involves the (usually) low power of basic pre-trend testings due to few observations. However, in the present case, this does not constitute an issue, given the length and frequency of the timespan.

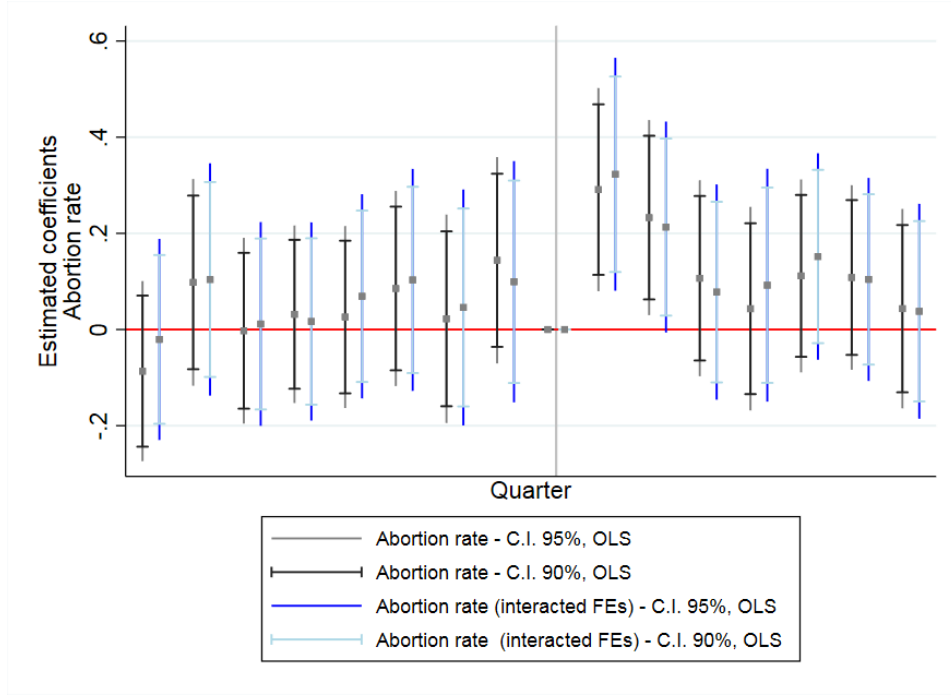


Figure 1.3: OLS Event-study estimates. The figure reports the coefficient on temporal units and their confidence intervals, both for the baseline specification and the one with interaction FEs between province dummies and quarterly dummies. Confidence intervals are reported at both 90% and 95% The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical line is set on quarter 9, which corresponds to the first quarter of 2020, the first lead before the treatment, occurring on 2020Q2.

We assess the sensitivity of our event-study results to assumed violations of parallel trends using the relative magnitude approach, by computing the break point parameter M (within a range going from 0 to 1, with 1 meaning that the hypothesis is to face a 100% deviation after treatment relative to the pre-treatment trend) which would make our results inconsistent. Graphs (a) and (b) in Figure E2 and in Appendix 1.14 show the results of such estimates: the red vertical line represents the 90% and 95% confidence interval for the coefficient on the treatment in 2020Q2; the blue lines represent, instead, the confidence intervals for the coefficients calculated by allowing a some degree relative magnitude deviation from the common trend (going from 0%, i.e. the baseline estimate, to 100%). We observe how, at 90% confidence level, we could admit a maximum 40% violation of the parallel trend assumption in our baseline specification, with the parameter dropping at 20% if we estimate the confidence sets at the 95% level.

1.6 Robustness

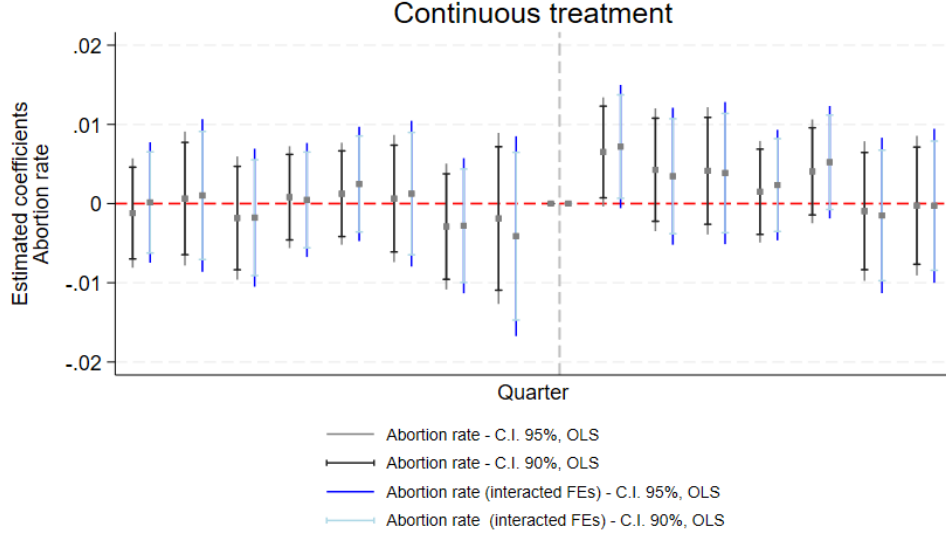


Figure 1.4: OLS Event-study estimates, as in dynamic specification of Equation 1.3 with the continuous treatment specification. The figure reports the coefficient on temporal units and their confidence intervals, both for the baseline specification and the one with interaction FEs between province dummies and quarterly dummies. Confidence intervals are reported at both 90% and 95% The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical line is set on quarter 9, which corresponds to the first quarter of 2020, the first lead before the treatment, occurring on Q2 2020.

In this section we perform several robustness checks. First and foremost, we implement a sensitivity analysis to show that the significance of the effect is not driven by the differentiation into quartiles, the time frequency aggregation, neither is it random. We then perform a time placebo to check whether the impact stems from seasonal patterns, while in the subsequent section we address possible issues in the SUTVA (*Stable Unit Treatment Value Assumption*) by aggregating the unit of treatment at the Local Market Area level, to account for potential spillovers across municipalities caused by commuters.

1.6.1 Sensitivity analysis

To assess the validity of the research design in different time frameworks, we collapse at both yearly, semestral and monthly frequency, respectively restricting and increasing the granularity of data. We adopt the same specifications as the one in Equation 1.1, with slight variations.

For the annual and semestral models, we consider the whole 2020 ($Post_t = 1$ if $t \geq 2020$) and the first semester of 2020 as the time discontinuities ($Post_t = 1$ if $t \geq S1/2020$); as for the monthly specification, the event time refers to the month of April 2020 (one week after the job suspension, $Post_t = 1$ if $t \geq April/2020$), while the policy covariates are included in the latter regression lagged by one period, to account for the unfolding of their heterogeneous impact on the abortion decisions with some delay (X'_{mt-1}). All OLS and PPML estimates at the annual, semestral, and monthly frequency are reported in Appendix 1.14. Results are consistent with the baseline model. Coherently with expectations, in absolute values the monthly coefficients on the 4th quartile are lower in magnitude than the quarterly ones (ranging between 3.5 and 4.4 p.p); however,

relative to the mean, the monthly results are slightly lower than our benchmark estimates, amounting to 9.5%-11.9% of the mean AR circa (Figure E2, Tables (a) and (b)). Consistently, the coefficients for the annual (Figure E1) and semestral specifications (Figure E3) are magnified in absolute sizes, although their relative values are alike. In both cases though, the 3rd quartile of the distribution is significant in some of the adopted specifications (at 10%). After that, to guarantee that the results are not driven by an arbitrarily selected cut-off, we perform further sensitivity checks by considering different ways to capture the suspended workers' share distribution. We assess units below/above the median, and those belonging to the 2nd and the 3rd tercile (the 1st tercile being the reference category). We also estimate a DiD with continuous treatment, following Di Porto et al., 2022 and Bordinon et al., 2023. The estimates in Tables (a) and (b) of Figure F1 in Appendix 1.15 are consistent with the baseline results. Hence, we are able to claim the differentiation into quartiles is not leading the results. The sensitivity analysis embodies a loss of one degree of statistical significance in the model with the continuous specification and in the one with the above-median framework. We make use of one of the sensitivity estimations to provide with further robustness for the hypothesis of absent pre-trends. As the "raw" treatment is given by the continuous specification of the suspended workers' share, we report the event-study accounting for continuous treatment, as specified by Equation 1.3. However, we do not identify the treated units through quartiles, but we apply the continuous treatment specification, which cannot be subject to arbitrary threshold manipulations (Figure 1.4). In such specification, the coefficient on the second quarter after the treatment loses significance, possibly due to the smaller aggregate effect of the treatment expressed in such granular fashion, even though this loss occurs for the OLS coefficients only, not for the PPML ones (Figure F1). However, the first lag after 2020Q1 still results significant at both 90% and 95% level. In addition to that, the coefficient dynamics and their confidence intervals display an evident divergent pattern of the difference in the effect between "more treated" and "less treated" units in correspondence to the treatment threshold, which hints for the lack of pre-trends viable to be biasing the results, for both the estimated event-studies. We also perform the estimations introduced in the baseline models on three sub-samples, to further validate the sensitivity of our results: 1) as abortions cannot be undertaken after the gestational limit (90 days) unless the woman's health is endangered, we discard all VPTs performed after 90 days, as they shall be credited to health-related necessities rather than to voluntary fertility; 2) as it appears clear from Table 1.2, the treated municipalities are on average less populated than control ones, as bigger cities are less likely to record high shares of suspended workers. In this regard, the big cities might be driving down the estimates on abortion rates for the control units. To account for that, we re-perform the estimates removing from the municipal panel dataset all the chief towns of the 15 Italian Metropolitan Cities administrative units, which include Italian biggest cities²¹. Eventually, we further restrict the sub-sample in point 1) by excluding all abortion interventions that are featured by the flag "urgency", which implies the observed woman aborted via an urgent treatment. In doing this, we make

²¹The metropolitan cities are, in alphabetical order, Bari, Bologna, Cagliari, Catania, Florence, Genoa, Messina, Milan, Naples, Palermo, Reggio Calabria, Rome, Turin, Venice.

the very conservative and narrow assumption that all treatment performed with urgency are to be credited to health-related concerns, even when performed before the gestational limits; therefore, we hypothesize they are not the subject of a fertility decision process.

The results of these three estimations are reported, respectively, in the tables of Figures F3, F4, and F5. While the specifications estimated in the first two sub-samples are strongly consistent with the baseline, in the tables of Figure F5 the magnitude of the coefficients is quite lowered, together with the significance in the most restrictive specification (which is null in the model with interacted provincial FEs); however, this shall not be troubling the internal validity of our research design, as it stems from a very narrow and quite unrealistic restriction of the sample. Eventually, we perform a further sensitivity check to assess whether our results are randomly driven. Specifically, we undertake a randomization inference procedure with 1000 permutations which randomly assign the treatment to units of the sample, in doing this estimating a random density function which, if overlapping our actual baseline estimate, would hint towards the randomness of our results. We report the kernel density plot in Graphs (a) and (b) of Figure F6. The graphs prove for the non-randomness of our findings, as our estimated coefficient (the red vertical line at the extreme right of the graph) lies way rightwards of the sampled distribution from the randomization inference procedure, in both specifications with and without interacted provincial FEs.

1.6.2 Time placebo

As the event-study graphs hint for the absence of pre-trends, we recognize two main aspects in our estimates: first, there is an evident presence of cyclicity, which is not fully removed from our quarterly aggregation. Second, the identification of the effect is visibly re-absorbed after two quarters, which suggests that seasonality might be contributing in driving our estimates; which is not a problem per se, as long as such seasonality is independent upon the distribution of the suspended workers share. To account for that, we perform time placebo regressions, and separately estimate the same models in various time-spans, always around the seasonal temporal discontinuity of the second quarter, although in different years²². Estimates are performed by employing both the OLS and the PPML specifications (Appendix 1.15, Tables F1 and F2). The considered treatment is always a dummy equal to 1 if the municipality belongs to the 4th quartile of the inactive share distribution, 0 otherwise. The time placebo in Table F1 displays that the interaction between the inactive share distribution and the time in correspondence to which the pandemic outbreak occurred is significant in explaining the behavior of abortion patterns. The estimates hint that the effect on ARs in municipalities

²²The restricted samples, with their relative treatment discontinuity, are the following: 1) from 2018 to 2021 excluding 2020, with Q2 2018 as treatment threshold; 2) 2018 only, with Q2 2018 as threshold; 3) 2018-2019 only, with Q2 2018 as threshold; 4) 2018-2019 only, with Q2 2019 as threshold; 5) from 2018 to 2021 excluding 2020, with Q2 2019 as threshold; 6) 2019 only, with Q2 2019 as threshold; 7) 2019-2020 only, with Q2 2019 as threshold; 8) from 2018 to 2020, with Q2 2020 as threshold; 9) 2019-2020 only, with Q2 2020 as threshold; 10) 2020 only, with Q2 2020 as treatment threshold; 11) 2020-2021 only, with Q2 2020 as threshold; 12) from 2018 to 2021 excluding 2020, with Q2 2021 as threshold; 13) 2021 only, with Q2 2021 as threshold; 14) 2020-2021 only, with Q2 2021 as threshold; 15) from 2018 to 2021, with Q2 2021 as threshold

with higher share of suspended workers is plausibly driven by the Covid-related repercussions, and not by some seasonal pattern due to a relation with sexual behavior between April and September and the municipal sectoral composition. The non-significant coefficients on March 2021 estimates (among which one is even negative, Column (14)), are a feasible suggestion to the re-absorption of the effects occurring in 2021, as displayed by the event-plots already. In addition to that, the fact that the impact of the inactive share is statistically significant starting from the second quarter of 2020 only, even when we use year 2020 alone as the sample of our model, corroborates our conclusion.

1.6.3 Local Market Areas

A major problem involving data collection refers to the possibility that women may not indicate their actual residence in their official generalities. If part of this issue can be overcome by performing the robustness check with the employment of the right-hand side LMA aggregation (explained later in the section), the problem stays as an intrinsic bias of the collected data. It must be noted that the municipal level of aggregation of the inactive workers' share may result restrictive for the present impact evaluation: indeed, people living in a given municipality may be commuters, and thus being affected by work suspension of bordering or close municipalities, as in the one where they are actually employed. In such case, we may be violating the stable unit value assumption (SUTVA). We therefore undertake two sets of estimations: 1) a robust clustering of the standard errors at the LMA level rather than municipal, both for OLS and PPML estimates, to account for the possibility of VPT rates to be independently distributed within local labor markets due to workers' cross-municipal commuting; 2) secondly, we directly apply the treatment at the LMA level, hypothesizing that the overall shock to the industry area of the municipality may matter as well as the actual shock to the municipality itself. We would expect a similar, positive significant effect on municipal ARs when extending the treatment area. Clustering is, again, at LMA level. The aggregation of the outcome remains the municipality. In both hypotheses, the yielded estimates are consistent with the ones obtained at the municipal level; they are all reported in Table G1 of Appendix 1.16. In the case of clustering only, the values do not change at all, as only SEs do, and not enough to compromise significance. When the treatment is considered at LMA level, coefficients on the 4th quartile of the inactive share shift down on average by few p.p. compared to the baseline, although their magnitude is clearly comparable; the only exceptions are the estimates involving provincial trends. All coefficients are significant at 1% confidence level. In relative terms, values are still oscillating around 10% of the mean of the treated pre-treatment, with the higher unweighted estimates reaching 14-16% of the average value when including province-specific trends indeed.

1.6.4 Geographical gradient

A doubt about the consistency of the results might arise by accounting for the fact that, in the analysis, we are analyzing thousands of very diverse municipalities, which may be hardly compared across Regions,

especially accounting for the major differences existing between Northern and Southern Italy.

Impact of work suspension on municipal ARs, differentiated by Italian macro-area						
	(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR
	(North)	(North)	(Center)	(Center)	(South)	(South)
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.19098 *** [0.06749]	0.16360 *** [0.05401]	0.00555 [0.17073]	0.03342 [0.12425]	0.03087 [0.07455]	0.01954 [0.06002]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.14382 ** [0.05869]	0.12009 *** [0.04581]	0.18416 [0.14595]	0.14645 [0.10643]	0.03211 [0.06254]	0.02817 [0.05065]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.00007 [0.05744]	0.00375 [0.04472]	0.03399 [0.13372]	0.02947 [0.09751]	0.07342 [0.06590]	0.06292 [0.05204]
Observations	70,128	70,128	15,488	15,488	40,832	40,832
R-squared	0.11421	0.11609	0.10919	0.11644	0.10718	0.11519
Policy covariates	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province $\times$ Quarter-year FE	✓	✓	✓	✓	✓	✓
Weight		✓		✓		✓
Mean	1.083	1.083	1.240	1.240	1.102	1.102

Table 1.5: The coefficients in Columns (1) to (6) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, differentiated by Italian macro-areas. Columns 1 and 2 refer to the Northern municipalities' sub-sample. Columns 3 and 4 to the Central one. Columns 5 and 6 to the Southern one. The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Although municipal fixed effects ought to remove such unobservable heterogeneities, we still differentiate the dataset into 3 different macro area-related sub-samples, to check whether the observed effect is to be credited to a North vs South-type of socio-economic underlying asymmetry. If we indeed look at Figure 1.4, a geographical stylized fact becomes immediately apparent: the inactive share, during the 2020's lock-down, was notably higher in Central and Northern Italian municipalities compared to those of the South. While the cause may reasonably be led back to the sectoral composition of the different parts of the country, it seems that the Centre-North is driving up our results however. In this robustness analysis, we subset the sample into North, Center, and South, to establish that the outcomes are brought forward by Northern towns having higher inactive share on average, and not by structural national asymmetries. Table 1.5 shows how the results are clearly driven by the Northern municipalities, as the suspended workers share has a significant impact in the Northern Italian sub-sample only. However, the internal consistency of the design within the Northern macro-area hints for the fact that the impact is led indeed by Northern Italy, but not by a mere North vs. South heterogeneity. As a matter of fact, in the Northern sub-sample it looks like that belonging to the 3rd quartile of the distribution of the inactive share involves a significant increase in the AR as well, with coefficients ranging from 12 to 14 p.p., according to the specification. What emerges from the latter estimates is that the industrial and labor market composition of Northern Italian municipalities allowed for

a abortion response to the inactive share at a lower “sensitivity” threshold. The aforementioned pattern may be due to cultural reasons, as abortions in Northern Italy are less hindered by cultural barriers. Therefore, the ARs might have been more “responsive” to the job suspension. No significant effect seems to loom within the Southern and Central areas’ sub-samples.

1.7 A plausible explanation: the male partner’s job trajectory

In the present section we try to understand which is the driver of the observed effect, by converging towards the anticipated explanation that the shift in abortion patterns are related to the partners’ job trajectories rather than those concerning the actual activity of aborting women.

1.7.1 Sectoral heterogeneity

In Table 1.6 we differentiate the distribution between that of suspended workers in the industrial sector and that of suspended workers in the service one. We reckon how only suspensions in the industrial sectors matter in explaining our results. As the industrial workers’ suspended share is more correlated with male employment, it is reasonable to believe that the effect on abortions that we observe depends more upon the job trajectories (in this case, temporary inactivity) of males than that women. Since the effect is more significant on married women, as it shown later in the paper, that would consistently mean that the suspension of plausibly male partners is driving the effect.

We already highlighted in Section 1.4 how a trigger in economic insecurity does not necessarily overlap with the shift in unemployment recovered from official statistics and early studies on the pandemic, as higher rates of suspension did not coincide with higher levels of unemployment (Figure C1, see also Cerqua and Letta, 2022). However, if the economic insecurity reasoning held in a straightforward fashion (i.e., directly affecting women’s profession), we would be led to believe that a higher share of inactive workers in the service sectors, featured by a higher prevalence of women (Casarico and Lattanzio, 2022), would be the determinant factor in explaining the response in ARs. In addition, if a direct link between women’s work suspension and abortion existed, we would credibly observe the abortion rates of women active in the service sector to be more “responsive” in municipalities with higher shares of inactive workers in the service sectors, and the same would hold with respect to the industry sector²³.

A greater proportion of suspended workers in one’s own sector of affiliation could be leading to terminate an unplanned pregnancy because of fear of losing an already suspended job due to its “non-essentiality”, a downward shift in income in such non-essential profession average (or individual) wages, or an overall bleak prospect for future development in that given sector. The sectorally-differentiated inactive share distribution

²³Of course, this would hold as well in the case of non-single women who are partnered to an individual whose job is more likely to have been suspended. More on this in the following pages.

affects differently the abortion rates of women who are, in order, not in professional condition, or, if active, occupied in the private service sector, in industry or in public administration. The latter ones, in particular, shall not be answering to our treatment by hypothesis, as the suspended workers' share is based on statistics which exclude civil servants. We exclude women active in the primary sector (not used by ISTAT to compute the inactive share), and those on whom we do not have professional information from the analysis.

Impact of suspension on municipal ARs, differentiating the treatment distribution across economic sectors						
	Industry Share			Service Share		
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * I.S._{q2/2020} \in Q_4 (Industry)$	0.11880 *** [0.04568]	0.11528 ** [0.04623]	0.12829 ** [0.05384]			
$Post * I.S._{q2/2020} \in Q_3 (Industry)$	0.02693 [0.04599]	0.02427 [0.04634]	0.03758 [0.05003]			
$Post * I.S._{q2/2020} \in Q_2 (Industry)$	0.01450 [0.04355]	0.01393 [0.04364]	0.01966 [0.04408]			
$Post * I.S._{q2/2020} \in Q_4 (Services)$				-0.01773 [0.04775]	-0.01388 [0.04807]	-0.01645 [0.05077]
$Post * I.S._{q2/2020} \in Q_3 (Services)$				-0.04640 [0.04026]	-0.04235 [0.04041]	-0.03867 [0.04178]
$Post * I.S._{q2/2020} \in Q_2 (Services)$				-0.05198 [0.03983]	-0.04960 [0.03989]	-0.05771 [0.04034]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10042	0.10045	0.11223	0.10036	0.10039	0.11218
Policy Controls		✓	✓		✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter_year FE	✓	✓	✓	✓	✓	✓
Province $\times$ Quarter_year FE			✓			✓
Mean	1.087	1.087	1.087	1.172	1.172	1.172

Table 1.6: The coefficients in Columns (1) to (6) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the treatment distribution differentiated across sectors. Columns 1-3 consider show the effect of the industrial inactive share. Columns 4-6 the service share. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The estimates in Table 1.7 seem not to be any sensitive to the occupational sector of the aborting women, thus suggesting the branch of activity not to be influencing the VPT decision. Actually, the only significant estimates for the abortion rates' heterogeneity by branch of activity, are the coefficients on the 4th quartile of the distribution (at 5%) for women not in professional condition, whose AR is greater by 15.5% the mean if residing in a municipality of the 4th quartile. As expected, civil servants' municipal ARs do not respond to treatment. In addition, although the coefficient is never significant, it seems like the abortion rate of service women is negatively correlated to higher quartiles of the inactive share in the service sector itself (I1

in Appendix 1.18). Such considerations may highlight a plausible higher responsiveness in terms of abortion decisions by women in a pre-existing condition of vulnerability within couple arrangements, if really the effect on ARs of women out of the labor force (OLF) was driven by their partners' suspension²⁴. The low significance of the analysis heterogeneized by branch of activity cannot lead us to unambiguous conclusions. However, the slightly significant role played by the inactive share of the industry sector for women who are out of the labor force (OLF), may suggest abortion decisions not to be directly influenced by a shift in economic insecurity related to women's career.

Impacts of work suspension on ARs, across sectors (linear estimates)				
	(1) AR not in prof. condition	(2) AR services	(3) AR industry	(4) AR P.A.
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.06984 ** [0.03532]	0.04511 [0.02936]	0.01454 [0.01293]	0.01346 [0.01030]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.05002 [0.03094]	0.02213 [0.02465]	0.00006 [0.01083]	-0.00022 [0.00891]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.03253 [0.02852]	0.00178 [0.02204]	0.00934 [0.00968]	0.00574 [0.00806]
Observations	126,448	126,448	126,448	126,448
R-squared	0.010409	0.08890	0.07934	0.07679
Policy covariates	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓
Mean	0.508	0.376	0.0682	0.0350

Table 1.7: The coefficients in Columns (1) to (4) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized by the economic branch of activity where the aborting women are active. Column 1 reports the effect on the ARs of women out of the labor force. Column 2 that on those active in the services. Column 3 that on industrial workers. Column 4 that on civil servants. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

1.7.2 Marital Status

Irrespective of the mechanisms, to corroborate further the male partner's job trajectory hypothesis, we heterogeneize the outcome by women's marital status, and assess how it is impacted by the treatment. As the effect observed is concentrated along the quarters immediately after the lock-down, we would expect a higher effect amongst married/cohabiting couples, as unplanned pregnancies were way more unlikely to

²⁴The estimates for the AR of women differentiated by professional branch of activity with respect to the different sectors of the inactive share are reported in the Appendix 1.18 (Table II).

occur among non-cohabiting partners (actually being almost impossible during the lock-down, which is the reason credited by Trommlerová and González, 2024 to the drop in Spanish abortions during the lock-down), given the restrictions on mobility and social gatherings. Restrictions were being gradually loosened during the post-lockdown period, but they were still in place until June.

As expected, Table 1.8²⁵ shows that the coefficients on the 4th quartile of the distribution for married women are significant (at 5% level), both for OLS and PPML estimates (Columns (2) and (6)). The estimates range between 7 to 8.5 p.p. (more or less 15% of the respective AR). The treatment also impacts single women though, even if at 10% level of significance only; note that the latter result may sensibly be driven by cohabiting but unmarried couples. Such results are consistent with the fact that, differentiating by age class (Table I5, the effect is significant among older women (more likely to cohabit with their partner). A quite counterintuitive result is observed when heterogeneizing by educational attainment (Table I6). As a matter of fact, significant coefficients are found for women with university degree, which is indeed consistent with the finding concerning age and marital status (being graduates older on average, and thus more likely to be married), although colliding with the results observed with regards to women OLF. One would expect indeed that, once observed the findings about professional condition, VPTs would display a differential shift mostly for women with a lower degree.

1.7.3 Fertility: past pregnancies

We exploit a further source of heterogeneity by looking at abortion rates across women who either had or had not previous pregnancies (of any kind) before their recorded abortion. In this regard, we already acknowledged how the effect is significant and positive for women not in professional condition and for married women.

We can distinguish between live births, stillbirths, miscarriages and previous voluntary abortions. We differentiate municipal abortion rates across women with no previous pregnancies, those with *at least one* previous pregnancy (including live births, stillbirths, miscarriages, and VPTs), those with *at least one* delivery (live births and stillbirths), those with *at least one* previous abortion (miscarriages and VPTs) (Columns (1)-(4) of Table 1.9. Then, we differentiate the outcome according to the extensive margin of each type of previous pregnancy (Columns (5)-(8)). We therefore flag women with dummy indicators equal to 1 if they report the mentioned of such characteristics and 0 otherwise, without accounting for the actual number of pregnancies²⁶.

²⁵Estimates differentiated by industrial and service share are reported in Appendix 1.18, Tables I3 and I4

²⁶PPML estimates and OLS estimates differentiated across industrial and service sectors are reported, respectively, in Tables J1, J2, and J3 in Appendix 1.19.

Impact of work suspension on municipal ARs across different marital status (linear and non-linear estimates)								
	(1) AR single	(2) AR married	(3) AR separated	(4) AR widowed	(5) AR single	(6) AR married	(7) AR separated	(8) AR widowed
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.07200 * [0.04115]	0.06963 ** [0.02993]	-0.00023 [0.00511]	-0.00282 [0.00799]	0.08556 * [0.04597]	0.08473 ** [0.03642]	0.00051 [0.05367]	-0.01011 [0.02625]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.03583 [0.038446]	0.03278 [0.02634]	-0.00147 [0.00432]	-0.00378 [0.00546]	0.04031 [0.03935]	0.04763 [0.03020]	0.02850 [0.04788]	-0.01840 [0.01976]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.00633 [0.03005]	0.02771 [0.02468]	-0.00166 [0.00367]	-0.0089 [0.00469]	0.00477 [0.03399]	0.04359 [0.02888]	-0.03472 [0.03992]	-0.00135 [0.01950]
Observations	126,448	126,448	126,448	126,448	108,608	98,096	17,105	29,934
R-squared	0.10016	0.09405	0.08802	0.07705				
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. $\times$ Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.631	0.379	0.0284	0.0259	0.631	0.379	0.0284	0.0259

Table 1.8: The coefficients in Columns (1) to (8) represent the municipal OLS and PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized across the marital status of the aborting women. Coefficients in Columns 1-4 are estimated via OLS. Coefficients in Columns 5-8 represents the Marginal Effects of Pseudo-Poisson Maximum Likelihood estimates (PPML). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The findings of Table 1.9 show that the coefficients on ARs for women with previous pregnancies and previous deliveries (which is a subset of pregnancies) are positive for municipalities in the upper three quartiles, relative to the reference category, (Col. (1) and (3)). In this case, the differences between the upper quartile, which we assumed to be the treatment in the baseline DiD specification, and the other two are not so clear-cut, and a valid different size is plausibly disproved by the SEs. This notwithstanding, the statistical significance is definitely increasing in the inactive share portions of the municipal distribution, at least for AR of women with any kind of previous pregnancy. On the other side, we observe no effect for women with no previous pregnancies before the current abortion (Col. (2)) or with previous abortions as a whole, i.e. including both miscarriages and VPTs (Col. (4)). The latter imprecise estimation is plausibly led by the negative, albeit insignificant, effect of women with previous involuntary abortions, with the PPML estimates displaying even a slightly significant 13% positive effect of being a municipality in the 4th quartile of the inactive share of the distribution and the abortion rate of women with any type of previous abortion (Table J1). With regards to the heterogeneity by type, coefficients on AR of women who delivered alive newborns in the past are significant at 5% in both quartiles of the right tail, amounting to 9.7 p.p. at the 4th (15% of the mean, Col. (1)), and 6.6 p.p at the 3rd (10%, Col. (5)). Coefficients on stillbirths and miscarriages are non significant, except for the 2^o quartile of the estimates in Col. (6), which does not

point towards consistent interpretations. Overall, they contribute to suggest that mothers with kids already are the ones more likely to abort, conditional on being gotten pregnant already in the past. Concerning abortions, we observe that only previous VPTs matter in significantly influencing upwards the abortion rates with respect to the inactive share. Being a municipality in the treatment quartile means having a differential effect in the AR of women who had voluntarily aborted in the past by 5.4 p.p. (OLS) at the 5% level, about 25% of the average pre-pandemic rate. The findings shed some light on the categories of women led to either terminate or not their pregnancies. It appears that women with previous deliveries (many of them mothers already, possibly) aborted more in treated municipalities in response to the shock. The reason might be twofold: 1) women with previous deliveries are more likely to be mothers already, possibly cohabiting with their partner. Thus, the positive coefficient may be pushed upwards by the larger amount of time spent home by partners during the job suspension, which boosted intercourse; 2) women who become unwillingly pregnant during the pandemic may find fertility less affordable if they have kids already, and they may prefer to pause their fertility in such hard times due to socio-economic considerations, to parity of sexual activity. One fact does not exclude the other; 1) women with no previous pregnancies are more likely not to be in a stable relationship or married, thus VPTs may fall due to a decrease in sexual activity during lock-downs and closures (see Trommlerová and González, 2024; 2) women in a relationship with no kids are less economically constrained (or they perceive to be less economically constrained in a context of overall economic insecurity) if they get pregnant, so they may find less costly to carry over the pregnancy. Since we cannot distinguish between an increase in sexual activity and the insecurity-related socio-economic considerations, we further explore the heterogeneous results according to the actual number of children or VPTs performed prior to the current abortion, and the overall response of pregnancies resulting into live births in the time-span under consideration.

1.7.4 Fertility: number of pregnancies

Women may be deciding to abort depending on the number of children they have already (intensive margin) and not only on whether they had children or not, and this might hold for the VPTs they undertook in the past too. Those with at least one live birth and thus, plausibly, a current kid, are more likely to be in a stable relationship than those with no previous live births. In case of women who undertake VPTs and that had more than one previous live birth, the likelihood of an increase in sexual activity being the driver of the abortion decision is intuitively lower: being at home a prolonged time with children (in times of lock-downs and school closures) means a greater time devoted to childcare, thus making less plausible a positive shift in sexual activity. The increased burden on childcare and housework for Italian parents during the pandemic has been empirically documented by several studies, for both mothers and fathers as well (Del Boca et al., 2020, Biroli et al., 2021, Brini et al., 2021, Mangiavacchi et al., 2021).

Impact of work suspension on municipal ARs according to past reproductive behavior (extensive margin)								
	(1) AR with prev. pregnancies	(2) AR with no prev. pregnancies	(3) AR with prev. deliveries	(4) AR with prev. abortions	(5) AR with prev. live births	(6) AR with prev. stillbirths	(7) AR with prev. miscarriages	(8) AR with prev. VPTs
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.10948 *** [0.04144]	0.02238 [0.03154]	0.09800 ** [0.03974]	0.04712 [0.03186]	0.09660 ** [0.03957]	0.00297 [0.00321]	-0.01574 [0.02222]	0.05414 ** [0.02446]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.06753 ** [0.03442]	-0.00302 [0.02778]	0.06796 ** [0.03243]	0.01226 [0.02585]	0.06636 ** [0.03236]	0.00354 [0.00250]	-0.01006 [0.01667]	0.01331 [0.02181]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.05178 * [0.03132]	-0.01648 [0.02474]	0.04370 ** [0.02997]	0.02988 [0.02366]	0.04171 [0.02990]	0.00376 * [0.00218]	-0.01272 [0.01554]	0.03215 [0.01979]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.11128	0.07972	0.10592	0.10431	0.10601	0.07487	0.08131	0.09652
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.729	0.366	0.650	0.357	0.649	0.00672	0.166	0.227

Table 1.9: The coefficients in Columns (1) to (8) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to whether or not the aborting women had previous pregnancies, depending on the type of pregnancy. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In addition to that, Del Boca et al., 2022 also find out, by means of two waves of pandemic-related survey data, that amongst the sampled women those out of the labor force were way more significantly likely to be burdened by the positive shift in childcare and housework-related further workload triggered by the staying-at-home orders: note that these are the ones who display higher ARs in our estimates on average. On the other hand, a higher number of children can be reasonably tied to larger economic constraints, which would lead mothers of many to resort to abortion in cases of vulnerable socio-economic condition. We build a number of dummy indicators which are set equal to 1 if women had delivered already 1, 2, 3, 4 or at least 5 live births in the past, and 0 otherwise. In a subsequent analysis, whose results are shown in the same table, we employ a similar set of indicators for the VPTs. In our OLS estimates (Table 1.10)²⁷, we observe that the abortion rate for women with 1 previous live birth is significant at 10% at all quartiles of the distribution with respect to the first (amounting to more than 15% of the mean in all cases), while those with 2 previous live births display ARs positive and significant at the 4th quartile at 5 % (21%), and at 10% at the 3rd. There is no statistically and positive significant effect on the abortion rate of women with no children, consistently with the results on women without previous pregnancies in Table 1.9. Concerning VPTs, we reckon that the Italian Ministry of Health highly stressed how not only the overall rates of VPTs have been decreasing over

²⁷PPML estimates in Table J4 in Appendix 1.19.

the years, but even more those of women who had already aborted once or multiple times in life, in doing so highlighting the importance of reproductive care facilities’, and the enhancement of healthy sexual behaviors and awareness (MoH, 2022). Findings in the Cols. (7-11) somehow confirming the Ministry claims, showing that the effect is positive and slightly significant indeed for women with no previous VPTs, who constitute the bulk of the data; the effect amount to about 8.8% of the mean (Col. (7)). However, the impact of being in the upper quartile of the work suspension’s distribution on women who had aborted once already is more significant and way greater in magnitude, as it amounts to 26% of the pre-treatment mean (Col. (8)). This notwithstanding, we observe a significant effect of being in the 2nd quartile of the distribution. Such fact validates the significant difference from municipalities with the lowest rate of suspension, but it is not fully consistent with our baseline results, which show the highest portion to be mostly mattering. If anything, the results corroborate the hypothesis that abortion rates increase with growing shares of suspended workers. When the outcome considered is the AR for women more than one previous VPTs, findings are all negligible taken singularly. The results seem to suggest that there could be an average disutility from the first abortion that may be dropping once women have already aborted, being more “prepared” to the occurrence and thus less resistant to undergo a VPT in an altered conditions. We cannot assess whether the non-significant coefficients for higher numbers of previous abortions could be driven by a hypothetical decrease in the drop in disutility of abortion after already having had one, or by the low power of the estimates due to lack of enough data for such women.

1.8 Alternative Explanations

1.8.1 Access restriction

A main threat to the validity of our explanations would be the belief that the shift in abortions is to be causally linked to supply-side channels, like supply restrictions or other consequences of lack of hospital assistance in Covid times. Graph (a) of Figure 1.5 shows how the Covid-19 outbreak is not just related to a differential shift in abortion rates according to the suspended workers’ distribution, but it triggered an overall decrease in VPTs.

Impact of work suspension on municipal ARs, differentiating the outcome according to the number of women's previous kids or abortions											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR
	with no prev. live births	with 1 prev. live birth	with 2 prev. live births	with 3 prev. live births	with 4 prev. live births	with at least 5 live births	with no prev. VPTs	with 1 prev. VPT	with 2 prev. VPTs	with 3 prev. VPTs	with at least 4 VPTs
$Post * \mathbb{1}(I, S_{-q2/20} \in Q_4)$	0.03527 [0.03425]	0.03973 [0.02407]	0.06118 [0.02716]	-0.00059 [0.01439]	-0.00570 [0.00536]	0.00179 [0.00430]	0.07773 [0.04403]	0.04618 [0.02013]	0.00601 [0.01618]	0.00162 [0.00405]	0.00014 [0.00415]
$Post * \mathbb{1}(I, S_{-q2/20} \in Q_3)$	-0.00184 [0.02990]	0.03932 [0.02111]	0.03557 [0.02097]	-0.00574 [0.01158]	-0.00244 [0.00462]	-0.00146 [0.00417]	0.05121 [0.03427]	0.00927 [0.01458]	0.00404 [0.01296]	0.00001 [0.00361]	-0.00110 [0.00329]
$Post * \mathbb{1}(I, S_{-q2/20} \in Q_2)$	-0.00640 [0.02644]	0.03297 [0.01818]	0.02566 [0.01986]	-0.00948 [0.01122]	-0.00648 [0.00484]	-0.00274 [0.00349]	0.00315 [0.03427]	0.02862 [0.01458]	0.00375 [0.01296]	-0.00173 [0.00361]	-0.00027 [0.00329]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.08540	0.09021	0.10048	0.08365	0.07765	0.12733	0.09458	0.09009	0.07543	0.07370	0.13794
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.445	0.247	0.287	0.0901	0.0199	0.0140	0.867	0.178	0.0380	0.00733	0.0132

Table 1.10: The coefficients in Columns (1) to (11) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to the number of previous children (Columns 1-6) or abortions (Column 7-11). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Trommlerová and González, 2024 highlight how Spain, a country similar to Italy under the economic, demographic and cultural point of view, faced a decrease in abortions due to a drop in social encounters, rather than to issues tied to supply restrictions. Graph (b) of Figure 1.5 shows how this could be sensibly consistent with the Italian case, as movements towards the most relevant places of aggregation followed a decreasing pattern during the lock-down, before recovering in Summer 2020. With respect to the possibility of a VPT reduction due to access restriction, according to the Italian ISS the public supply of abortion services was not hindered by the pandemic (INIH, 2022). However, both credible anecdotal press evidence (Muratori and Di Tommaso, 2020, Torrisi, 2021) and some healthcare research studies (Ong et al., 2023) contradicted such claims, due to hospital excess workload and epidemiological concerns. The latter brought however a relaxation of usual abortion constraints (Bojovic et al., 2021). In any case, we address the issue in the present section.

Given that the pandemic crisis not only required the implementation of shelter-in-place policies, but also brought about relevant pressure on hospitals, this might have led to major difficulties, for women, to undertake a VPT; this would cause, in turn, a potential increase of mobility to seek for the service (Autorino et al., 2020). If such phenomenon was correlated to our “treatment”, there could be important bias in our estimates, as the effect could be almost entirely driven by the reduction in abortion supply in areas less affected by the treatment, but more affected by the epidemic (as shown in Borri et al., 2021 and Di Porto et al., 2022), which would dampen the credibility of our results. First, we plot some descriptive evidence (Figure H3, Appendix 1.17), to observe how mean excess mortality (computed as percentage change in quarterly municipal mortality in 2020-2021 compared to the 2015-2019 average of the same variable) and the cumulated sum of provincial covid-cases, normalized by provincial population, were not substantially differing across the inactive share distribution in the quarters interested by the effect.

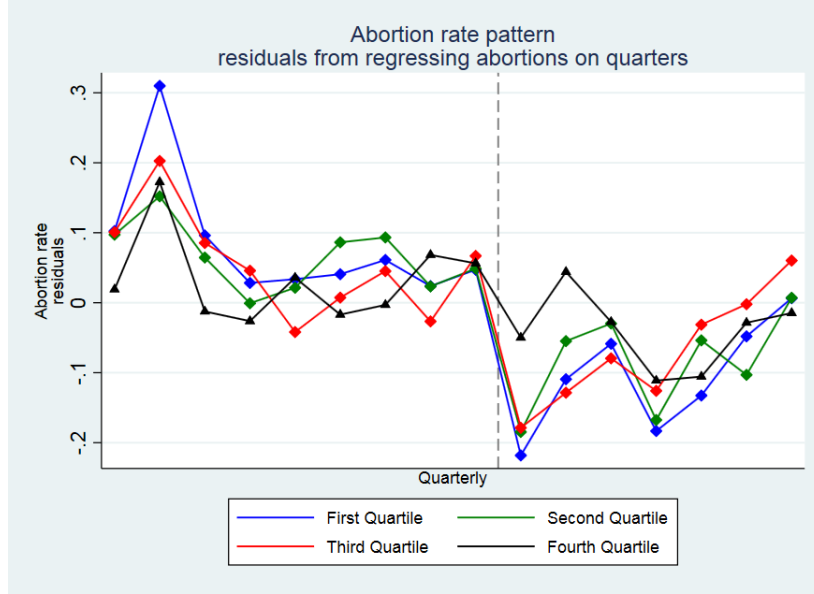
We further discuss the role of supply, by proceeding in two steps: first, we provide anecdotal evidence that a higher inactive share is not linked to a greater supply of VPTs, by aggregating the abortions at the hospital level and evaluating the treatment in correspondence of the municipality where the abortion is performed, rather than where women live; second (and most importantly), we estimate, by means of a Diff-in-Diff designed at the individual level, an OLS LPM equation using, as outcome, the likelihood of seeking for an abortion in areas which differ from that of one’s residence²⁸. The latter analysis is called upon by the fact that a supply-driven shift in VPTs hypothesis is plausible if we consider the times under exam: indeed, we are looking at the pandemic situation, characterized by a high number of mobility restrictions. Concerning the first check, the methodology slightly varies when we aggregate at the hospital level, as the datasets employed needs some modifications. Municipalities with hospitals where abortions are performed are indeed 308, and the matched municipal observations in the following analysis drop to less than 6,000.

²⁸By “area” we mean either a different municipality or LMA, as we estimate two separate models.

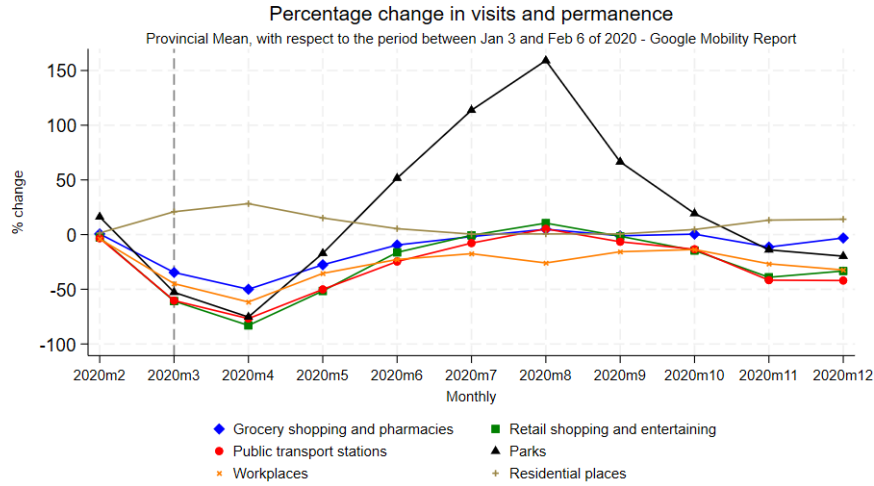
The empirical model is the following:

$$AR_{hmlt} = \beta_1 + \sum_{k=2}^4 \beta_k Post_t \times \mathbb{1}(I. S_{q2/20} \in Q_k) + X'_{mt}\beta + \rho_h + \gamma_t + \lambda_{l,t} + \varepsilon_{ht} \quad (1.4)$$

The outcome of interest is AR_{hmlt} , the VPT rate performed in hospital h within municipality m (irrespective of the municipalities of residence of the aborting women), managed by Local Health Authority l , in quarter-year t .



a)



b)

Figure 1.5: (a) Pattern of the de-seasonalized mean abortion patterns across the shares of the distribution of suspended workers. The unit of aggregation is the women's municipality of residence. The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical dashed line is set between Q1 and Q2 of 2020. (b) Mean percentage provincial monthly variation, with respect to the period between Jan 3 and Feb 6 of 2020, of visits to and permanence in different kind of places.

Note that the denominator of the outcome is the municipal population of women aged 15-49, which does not coincide with the population with such features in the hospital’s catchment area, for which data were not available; in addition, in a supply-based analysis there is really no credible way to disentangle the shifts in the demand; hence the anecdotal nature of the estimates, for which no robust causal claim can be drawn.

The inactive share-related treatment terms and the policy covariates in the RHS do not change with respect to the previous model, although referring to the hospital municipality as unit of measure. In turns, in addition to time FEs, we include hospital FEs (ρ_h) and an interaction between LHAs and a quarter-year FEs ($\lambda_{l,t}$), to account for evolving unobserved heterogeneities due to the territorial management of the healthcare supply²⁹. Results are reported in Tables (a) and (b) of Figure H1. All coefficients on the inactive share quartiles are non-significant. In addition, the direction of such non-significant effect is always negative when looking at the 4th quartile³⁰. Such descriptive results hints that the work suspension are affecting women’s demand rather than hospitals’ supply in explaining the observed effect.

A further, more robust validation, is yielded by our second estimation, as in the individual-level “abortion mobility” DiD. In such case, we keep the sample without municipalities where no abortion is performed. We do this as women residing in municipalities without performed VPTs have to move to across areas to abort nonetheless; their mobility is thus necessarily induced by supply constraints, irrespective of the treatment³¹. The aim is indeed to see whether women seek for a VPT in a different place than the one where they reside in, for reasons to be credited to the treatment. As a matter of fact, if the increase in abortion was driven by supply reductions in areas with lower inactive share, then we would plausibly observe a negative relationship between suspended workers and abortions performed in a different municipality (or LMA), since those women would be willing to travel across municipalities to seek for the VPT. In our restricted samples, we count 304 municipalities where VPTs are performed (3.8% of the total number of Italian municipalities), involving 118,351 individual observations; then, we count 251 LMAs where VPTs are performed, which amount to 41% of the Italian LMAs, for a total of 210,445 observations. Note that in this additional model we do not aggregate data by municipality, but just estimate the probability of aborting in another area (“abortion mobility”) for individual women, conditional on the fact they have aborted already. We follow the same approach used by Balia et al., 2020, for cross-regional patient mobility flows, as they estimate the probability of seeking for health care in a different region by means of a binary outcome likewise. Given that the analysis is at the woman’s level, it requires the use of a set of various individual controls for the female characteristics, which are provided by ISTAT in the VPT dataset. We report some descriptives about the relevant variables (together with the total share of abortion mobility) in Table H1, Appendix 1.17. We discard observations with missing information on the controls, so that we remain with 97,550 observations in the sample of inter-

²⁹The coefficients of interest are, again, β_2, β_3 and β_4 ; in addition, a specular PPML panel regression model is estimated with the same inputs.

³⁰There is one significant estimate at 10% (Column (2) of Table (a) in Figure H1), i.e. the OLS specification with policy covariates and weighted by population, of negative sign too.

³¹In the second situation, that of LMAs, we discard all municipalities that are part of LMAs which do not include any municipality where VPTs are performed

municipal VPTs, and 173,791 in the one of inter-LMA mobility. We estimate the following equation via OLS:

$$Y_{imt} = \beta_1 + \sum_{k=2}^4 \beta_k Post_t \times \mathbb{1}(I. S_{q2/20} \in Q_k) + X1'_{it}\beta + X2'_{it}\beta + X3'_{mt}\beta + \tau_m + \theta_{m(VPT)} + \eta_h + \gamma_t + \delta_{p,t} + \varepsilon_{imt} \quad (1.5)$$

The outcome is Y_{imt} , a dummy variable equaling 1 if woman i residing in municipality m undergoes a VPT in quarter-year t , in a municipality differing from m ($m \neq m(VPT)$), provided that in m there are facilities where VPTs are performed (i.e., abortions occur in such municipality). The dummies for the treatment are the same as in the baseline specification (Equation 1.1), always at the municipal level. $X1'_{it}$ is a vector including a set of health and fertility-related individual controls for the aborting woman, as in characteristics related to past reproductive behavior and health circumstances (number of previous live births, stillbirths, miscarriages and VPTs) and health-related features regarding the present VPT procedure (gestational age, weeks of amenorrhea, whether the intervention is urgent, whether there are complications or child malformations, whether the abortion is medical). Vector $X2'_{it}$ contains demographics instead (age, whether the woman is an Italian citizen, marital status) and categorical socio-economic regressors (educational attainment, professional condition and position, economic branch of professional activity). $X3'_{mt}$ are the above mentioned municipal characteristics, including population of females aged 15-49 as well. We add up FEs for the municipality of residence of the woman (τ_m), FEs for the municipality where the abortion is performed ($\theta_{m(VPT)}$), hospital FEs (η_h) and time FEs (γ_t). We also add, in a further specification, the above mentioned provincial-quarter-year interacted FEs ($\delta_{p,t}$). Then, we estimate a parallel equation which mirrors Equation 1.5, with few variations: the outcome becomes Y_{imst} , a dummy variable equalling 1 if woman i residing in municipality m , part of LMA s undergoes an abortion in quarter-year t in a LMA which is different from s ($m \in s \wedge m(VPT) \notin s$), which we call inter-LMA abortion mobility. The treatment is applied at the LMA level in this case³².

Tables 1.11 and 1.12 display that higher levels of inactive share increase the probability of seeking for a VPT in another area, thus confirming the initial conjecture that a potential restriction in supply is not undermining validity: we observe that, mobility restrictions notwithstanding, women were moving across areas to abort. Such women are the ones in municipalities more affected by economic closures, rather than those living in less affected municipalities, where hospital clogging was a feasibly more serious issue if suspension is negatively correlated with contagions. If we consider the inter-municipal mobility, we actually acknowledge both coefficients on the 3rd and 4th quartile of the distribution of the inactive share to be significant and positive. Estimates range between 5.3 and 8.7 p.p for the coefficients on the 3rd quartile, depending on the specification; such values amount to 12-22% of the mean of the treated pre-treatment. Concerning the 4th quartile, estimates range between 4.7 and 9.7 p.p. (the latter being 25% of the mean), at a lower degree of significance. Such estimates suggest that the relevant threshold in separating treated

³²In the first model, SEs are clustered at municipal level; in the second model, at the LMA level.

from non-treated municipalities, for the inter-municipal abortion mobility, ought to be the median. When we look at the greater sample of abortions occurring in LMAs with abortion facilities only, by assessing the impact of suspended workers on inter-LMA mobility, the situation resembles more to our baseline estimates. Only the 4th quartile matters, with values ranging between 3.1 and 4.4 p.p, 8-11% of the mean value of the treated units pre-treatment circa³³. The latter analyses seem to confirm that the inactive share does not play a role in shifting the supply of VPT services in directions which could potentially bias the baseline estimates. The results show that women seeking for abortion were able to move across municipalities or even LMAs to undergo a VPT anyway. This is consistent with our initial findings, which show quarterly abortion rates of municipalities hit the most by the workers' suspension greater by 10% circa³⁴

1.8.2 Remote Work

We also reckon, from existing literature (Kurowska et al., 2023), how teleworkable jobs' activities, whose use has been plenty made during the pandemic, might be held responsible in shifting potential parents' birth intentions. We use the 2019's *Labor Force Survey* provincial information to explore the issue. First, we recover the provincial distribution of male workers active in the industrial sector in 2019, and check whether there is at least a correlation with the provincial share of suspended workers during the lock-down. Second, following Barbieri et al., 2022, we built a provincial, sectoral *Working From Remote* (WFR) index. Such index has been developed by assigning a score from 1 to 100 to each Ateco 2-digit sector, according to the relative amount of tasks doable at home prior to the pandemic, as the reference base is the 2019 LFS. We weight such score at provincial level by the share of employees in the private sector occupied in the sector itself, and assess the overall distribution. Then, again, we look at potential correlation with the suspended workers' share, in overall terms and for males only. In this regard, we can even assess whether, in anecdotal terms, the possibility be able to work from remote before the pandemic might have played a role. As it is displayed by the maps (a) and (b) in Figure 1.7, the provincial distribution of the share of industrial workers relative to the total workers can be fairly overlapped to the distribution of industrial workers relative to male workers only, and both can be quite overlapped to the map of the provincial share of industrial male workers active in Italy in 2019 (Figure 1.7, map (c)), with areas reporting higher shares of industrial male workers located in Central-Northern Italy.

On the other side, the distribution of the *WFR* index computed over male workers looks more heterogeneous across provinces, even though the variability of such index is not quite substantial over the Italian territory (Map (a) of Figure I1, Appendix 1.18).³⁵ Such suggestions could be corroborated by looking at Table (a) in Figure I3. The latter displays the outcomes of some rough OLS regressions, to show unconditional

³³Although the highest coefficients are only significant at 10%

³⁴The event-study graphs of both inter-municipal and inter-LMA mobility are represented in Figure E1 in Appendix 1.14.

³⁵The maps on the provincial share of total industry workers and on the *WFR* computed on total workers, rather than males ones, are reported in Figure 1.18, in Appendix 1.18.

correlations between the suspended workers share (dependent variable), and a set of different regressors: the *WFR* index (scoring 1-100) (1), the *WFR* index computed on males only (2), the provincial share of industrial workers (3), the provincial share of male industrial workers over the total active individuals in the private sector (4), and the provincial share of male industrial workers over the total active males in the private sector (5). There exists a strongly significant correlation between the inactive share and the share of active industrial workers in 2019, whose coefficient gets slightly significantly higher when considering the males' share (4). Concerning the *WFR* index, the link, albeit positive, does not differ from zero in general terms (1), while it positively does for the men-only based measure (second column, at 5%). This holds similarly if we look at the evidence in Graph (b) of Figure I3 and Graphs (a) and (b) of Figure I4, summing up the issues discussed so far. These make up for simple correlational evidence, thus no causal claim can ever be drawn from the exercise. To conclude, we keep highlighting how the partner's job suspension trajectory is likely to play the most relevant role than that of the aborting woman in the finalization of the VPT decision itself, given the relevant link between male industrial 2019 workers' share and the inactive share in 2020. This seems to have brought about the effect led by industrial suspension mostly. By contrast, the role of remote working does not seem to play a major role in our framework, given the available data: this is consistent with our main results, i.e. a significant effect during the initial phase of the pandemic only (in correspondence of the actual job suspension, with the following social distancing policy measures not mattering at all), during which the faculty to perform teleworkable activities did not have any significance as long as one's job was legally suspended.³⁶

1.8.3 Live births

We now explore the impact on pregnancies resulting into live births 9 month later. In doing this, we refer to previous research.

For instance, Lindo et al., 2020, show that the decrease in abortions caused by the closure of abortion clinics is paralleled by an increase in newborns, even if it still does not offset the drop in VPTs³⁷. Similarly, when Cavallini, 2024, finds out that a SD increase in unemployment leads to a 0.25 SD increase in the abortion rate (IV estimates), the General Fertility Rate decreases by 0.95 SD. Was economic insecurity the main determinant, we would observe not only a rise in termination of unplanned pregnancies, but also a decrease in planned pregnancies (that turn to live births). On the other side, was it only time exposure, we would instead feasibly observe augmented fertility rates.

³⁶The *WFR* index does not indeed mirror, territorially, the industrial share distribution; yet it has higher values in the Center-North. As our results is ostensibly driven by the North, teleworkable jobs may actually intrinsically be a down-shifting force for the abortion demand, in terms of time availability. Jobs whose activities were already being performed remotely prior to Covid, the relative shift in time due to suspension might have a less significant impact on the possible intercourse during the lockdown, compared to pre-pandemic times. And yet Northern areas, with higher likely presence of telework, drive up the numbers, which may suggest that telework matters little in our context.

³⁷Their conclusion leads to suggest the existence of alternative channels to explain what happens to "missing pregnancies" which do not end up neither in abortion statistics nor live births; possibly, clandestine or self-induced abortions.

Impact of work suspension on the likelihood of undertaking a VPT outside of one's municipality of residence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.05254 ** [0.02120]	0.05083 ** [0.02130]	0.05595 *** [0.02158]	0.04880 ** [0.02120]	0.04717 ** [0.02127]	0.05222 ** [0.02154]	0.09667 ** [0.04253]	0.09477 ** [0.04406]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.05587 *** [0.01509]	0.05518 *** [0.01509]	0.06007 *** [0.01592]	0.05434 *** [0.01528]	0.05373 *** [0.01525]	0.05854 *** [0.01607]	0.08353 *** [0.03190]	0.08735 ** [0.03371]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.00225 [0.00652]	0.001144 [0.00667]	0.00554 [0.00730]	0.00223 [0.00589]	0.00117 [0.00605]	0.00550 [0.00668]	0.03064 [0.03169]	0.003457 [0.03305]
Observations	97,545	97,545	97,545	97,545	97,545	97,545	97,528	97,528
R-squared	0.55790	0.56319	0.56323	0.56454	0.56948	0.56952	0.57748	0.58296
Individual Controls		✓	✓		✓	✓	✓	✓
Policy covariates			✓			✓		✓
Other munic. covariates			✓			✓		✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Munic. of the VPT FE	✓	✓	✓	✓	✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE							✓	✓
Weight				✓	✓	✓		✓
Mean	0.387	0.387	0.387	0.387	0.387	0.387	0.387	0.387

Table 1.11: The coefficients in Columns (1) to (8) represent the individual OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the likelihood of aborting outside one's own municipality of residence. The reference category is the 1st quartile of the distribution. The individual controls include a large set of individual characteristics of the aborting woman, accounting for demographic, socio-economic and health and fertility-related characteristics, plus some clinical covariates concerning the current VPT procedure. Policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days, and municipal excess mortality. Other municipal covariates include the population of females aged 15-49. The sample consists of an aggregation of repeated cross-sections, with the inactive share distribution computed at the municipal level. SEs clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on the likelihood of undertaking a VPT outside of the LMA of residence								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs	Share of extra-m. VPTs
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.03187 *** [0.01026]	0.03224 *** [0.01037]	0.03476 *** [0.01074]	0.03099 *** [0.01015]	0.03120 *** [0.01027]	0.03429 *** [0.01056]	0.04420 * [0.02464]	0.04236 * [0.02468]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.00914 [0.00920]	0.00944 [0.00937]	0.01180 [0.00950]	0.00769 [0.00855]	0.00796 [0.00876]	0.01081 [0.00879]	0.03764 [0.02397]	0.03939 [0.02397]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	-0.00056 [0.00851]	-0.00032 [0.00878]	0.00222 [0.00926]	-0.00111 [0.00777]	-0.00096 [0.00806]	0.00211 [0.00851]	-0.02996 [0.03203]	-0.03278 [0.03201]
Observations	173,392	173,392	173,392	173,392	173,392	173,392	173,390	173,390
R-squared	0.56226	0.56692	0.56694	0.56853	0.57300	0.57302	0.57677	0.58263
Individual Controls		✓	✓		✓	✓	✓	✓
Policy covariates			✓			✓		✓
Other munic. covariates			✓			✓		✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Munic. of the VPT FE	✓	✓	✓	✓	✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE					✓	✓		
Weight				✓	✓	✓		✓
Mean	0.385	0.385	0.385	0.385	0.385	0.385	0.385	0.385

Table 1.12: The coefficients in Columns (1) to (8) represent the individual OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the likelihood of aborting in a municipality located outside other LMA including one's own municipality of residence. The reference category is the 1st quartile of the distribution. The individual controls include a large set of individual characteristics of the aborting woman, accounting for demographic, socio-economic and health and fertility-related characteristics, plus some clinical covariates concerning the current VPT procedure. Policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days, and municipal excess mortality. Other municipal covariates include the population of females aged 15-49. The sample consists of an aggregation of repeated cross-sections, with the inactive share distribution computed at the LMA level. SEs clustered at LMA level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1.

To recover the number of pregnancies in the time under question, we use the daily data on birth registrations from 2018 and 2021. Since we are interested in the pregnancies occurring in the available time-span, we shift backwards the day of birth of the children by 270 days (9 months), to obtain a rough estimate of the daily conceptions occurring in Italy in the period under consideration and resulting into live births. In doing this, we unfortunately lose the last nine months of our sample, as we do not have the birth registrations in 2022, therefore we cannot estimate conceptions happening from April to December 2021. We do not know the mother's municipality of residence, so we aggregate the births at the level of the municipality where the birth is registered. Aggregating by municipalities of birth would be meaningless, as almost all newborns are delivered in hospitals and healthcare institutions.

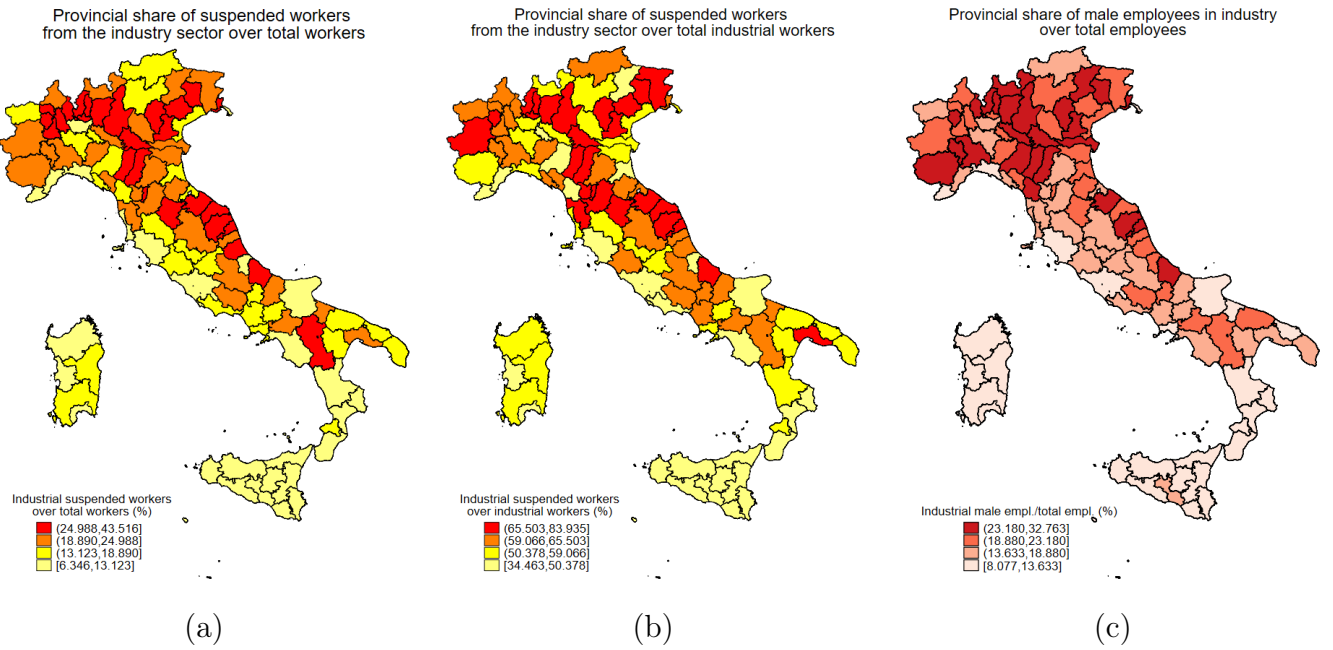


Figure 1.6: Maps (a) and (b) represent the provincial share of suspended workers from the industrial sector. Map (a) displays the share relative to the total number of private sector workers; map (b) displays the share relative to the number of industry-only private sector workers. Map (c) represents the provincial share of active male workers in the private industrial sector, by using data from the *LFS* 2019.

We aggregate data at quarterly frequency to be consistent with our estimates on abortions. The model of reference is the same as Equation 1.1, but we use the General Fertility Rate as outcome (number of live births every 1000 women aged 15-49), although in terms of pregnancies (hence, the number of pregnancies resulting into live births 9 months later every 1000 women aged 15-49). Results are all positive and never statistically different from zero (Table 1.13, PPML estimates in Appendix 1.20, Table K1). We further explore the pregnancy outcome by performing our analysis on a variety of temporal subsets, as we already did with the time placebo for abortion rates. The considered ranges are the same as those presented in Table K2, but without considering the march 2021 threshold. Such findings show however that the treatment has some slight impact on pregnancy rates, but is most likely random or seasonality-driven, as the dummy on post 2019 reports coefficient statistically different from zeros. The non-significance of these results does not help ruling out one explanation or the other: live births stemming from increased exposure may have been

offset by the unplanned ones due to economic insecurity/vulnerability. Given the ambiguity, we also perform estimates using the Abortivity Ratio as outcome (instead of the Abortion Rate), which is the ratio between quarterly municipal VPTs and 1000 registered live births. This allows to assess whether the magnitude of the differential impact on abortions is statistically significant compared to what non-significantly happens to live births. OLS and PPML results are reported, respectively, in Tables (a) and (b) of Figure K1 in the appendix. OLS estimates in Columns 1-4 show that the coefficient on the treatment for the ratio ranges from 9.8 to 10.3 p.p. (about 8.4% of the mean average pre-treatment), and it is significant at 5%. However, when the model is integrated by provincial trends, which is our most restrictive specification, the coefficients lose significance. PPML results are consistent, although the degree of statistical significance drop to 10%. Such estimations prove consistent with our baseline framework, although the most conservative ones seem to suggest that differential birth trends at the local level may be able to capture part of the investigated effects.

Impact of work suspension on municipal rates of pregnancies resulting into live births (linear estimates)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Pregnancy rate	Pregnancy rate	Pregnancy rate	Pregnancy rate	Pregnancy rate	Pregnancy rate
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.18441 [0.15293]	0.09482 [0.11474]	0.18762 [0.15364]	0.09508 [0.11531]	0.19109 [0.17377]	0.08248 [0.13211]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.07355 [0.12351]	0.05142 [0.09754]	0.06774 [0.12426]	0.04531 [0.09808]	0.04755 [0.13557]	0.02530 [0.10721]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.05488 [0.11799]	0.02759 [0.09259]	0.05190 [0.11817]	0.02512 [0.09278]	0.03168 [0.12208]	0.01163 [0.09572]
Observations	102,739	102,739	102,739	102,739	102,739	102,739
R-squared	0.12070	0.13008	0.12088	0.13024	0.13025	0.13877
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓
Prov. $\times$ Quarterly FE					✓	✓
Weight		✓		✓		✓
Mean	7.722	7.722	7.722	7.722	7.722	7.722

Table 1.13: The coefficients in Columns (1) to (6) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal rates of pregnancies turning into live births 9 months after, normalized by the municipal population in fertile age. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating births at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Live birth and marital status

Data on birth registrations contain some valuable information about the family of the newborn: we perform some heterogeneity estimates to see whether some phenomenon of interest emerges. To mirror the estimates

undertaken by using VPTs as outcome, we would need to know the mother's marital status. While we have the exact information on the maternal individual marital status, we do not know whether the mother had previous children exactly. We assume that the maternal municipality of residence coincides with the registration one³⁸; thus, individual marital status can work in the next heterogeneity analysis, reported in Table K4 in Appendix 1.20. As in the baseline specifications for pregnancies, heterogeneizing by marital status and keeping the overall inactive share distribution in the right-hand side of the identifying equation does not deliver any significant, unambiguous estimate (Table K3, Appendix 1.20³⁹). The pattern, although coefficients are all statistically close to zero, seems to mirror the one retrieved in the baseline. Contrarily to the results mentioned above, it seems like that there is a slight impact on the pregnancy rate of married women (or who are in a civil union) living in municipalities with the highest inactive share in the service sector. The coefficient is slightly positive and amounting to 27 p.p. (18.5% of the mean), statistically significant at 5% (OLS, Table K4, Col. (2)). However, the fact that OLS estimates are statistically significant may suggest that there was a slight trigger in planned pregnancies in municipalities with higher share of suspended workers from the service sectors for non-single parents (with respect to less affected municipalities), who possibly exploited the time available with their partners to adapt their fertility. The presence of an overall positive differential impact on pregnancies seems to be offset by municipalities with higher shares of suspended industrial workers, for which coefficients on most distributional dummies are non-significant (although this does not hold for divorced/separated women). It is worth noting, however, that the coefficients for pregnancies of single and married women, although not statistically different from zero, are negative on the upper quartiles relative to the industry sector suspended share. In this regard, especially considering the sectoral distribution of the impact on VPTs, it looks like that, while the shift in unplanned pregnancies (and therefore in abortion rates) is driven by municipalities with higher shares of suspended industrial workers, there was possibly a modest differential impact on the affordability of fertility (i.e. to carry over either intended or involuntary pregnancies) amongst married couples due to closures, which resulted in positive live births in municipalities with higher shares of service workers, counteracted in the overall pregnancy framework by including industrial workers to the specification. No evident pattern is retrieved by differentiating the outcome according to the number of minor children already present in the household of the head of the family (Table K5).

³⁸This assumption may bring about few issues: in fact, assuming the kid's municipality of registration as the one where the mother resides too, would be a more precise restriction if we considered children of mothers who are married with the fathers of the registered kids only, possibly living together in a unique household. However, we do not know the "joint" parental marital status, and we cannot conclude whether the husband of a married mother is the actual child's father and, thus, whether the municipality of residence of the newborn is the same as the mother's, in the case they live altogether. However, if both parents are individually married, it is more unlikely that the child has been conceived between two individuals not married with each other. In any case, even when children are born out of wedlock, they usually live with their mothers, especially along the first months of their life; this holds even more for single mothers. Thus, we can reasonably assume that the municipality where the child is recorded is the one where the mother lives as well.

³⁹The table actually shows a surprising positive effect on widowed and separated women, but this does not bring much to the table of our analysis.

1.8.4 Domestic violence

To deepen the role of unplanned pregnancies in explaining the results, we can inquire about whether the heterogeneous shift was led by forced rather than consensual intercourses, as in sexual violence by intimate partners. We acknowledge indeed how both social science and medical literature have established a link between domestic violence, unintended pregnancies and their consequent voluntary termination (Hall et al., 2014), not only in developing countries but even in a high income nation like Italy (Citernes et al., 2015). Therefore, we account for the sociological “exposure theory” (i.e., more time available together leading to episodes of domestic abuse, Dugan et al., 1999). Note that, beyond time, domestic sexual violence may also be triggered by the escalation of income or unemployment-related stressful situations (Card and Dahl, 2011, Diaz and Saldarriaga, 2023), or due to the worsening of female bargaining power given a detrimental shock to the labor market (Aizer, 2010).

This notwithstanding, the role of the topic’s salience might have played a major role in shifting numbers: Colagrossi et al., 2022 highlight indeed how the governmental domestic violence-related sensitisation policy put in place at the beginning of the pandemic was the driver of the increased reports, rather than assaults themselves. We also reckon how the phone calls to the national public hot-line for domestic violence (via the dedicated phone number 1522) faced a visible spike immediately after the economic closures (Figure B1). We aim at verifying whether a higher share of suspended workers brought about higher rates of calls to 1522 for Intimate Partner Violence, in a TWFE DiD specification which is almost the same as that we use as baseline for the abortions and live births. As many works underlined the role of salience in triggering reporting behaviors (A. R. Miller et al., 2022, 2023, Colagrossi et al., 2022, 2023), we try to overcome such bias by exploiting the provincial heterogeneity in the inactive share distribution. Indeed, the relevance of the domestic violence concern was highlighted at the national level by television campaigns, which were accessible by anyone staying home during the lock-down; Colagrossi et al., 2022 found heterogeneous effects depending on the exposure to the campaign itself, proxied by public television audience shares. We hence estimate the following model by OLS:

$$Call\ Rate_{pt} = \beta_1 + \sum_{k=2}^4 \beta_k Post_t \times \mathbb{1}(InactiveShare_{p12/2020} \in Q_k) + X'_{pt}\beta + \tau_p + \gamma_t + \delta_{r,t} + \varepsilon_{mt} \quad (1.6)$$

Where $Call\ Rate_{prt}$ is the total number of calls to 1522, either by users or victims (a subset of users), every 100,000 inhabitants, in province p and quarter-year t . The relevant independent variable is the distribution of suspended workers subdivided into quartiles, although here it is taken at provincial level. Covariates for the restrictions’ severity are included as well (X'_{pt}), aggregating the municipal value from Conteduca and Borin, 2022 at the province level (weighting by population). In addition to quarter-year (γ_t) and provincial FEs (τ_p) we also add up regional quarterly interaction dummies ($\delta_{r,t}$) to the baseline framework. We estimate a parallel Poisson model by means of PPML, adopting the same specification. In addition, since data on calls

to 1522 are collected weekly, we re-perform the same analysis but at weekly level. All results are displayed Appendix 1.11, the quarterly ones in Table B1 and Table B2, the weekly ones in Table B3 and Table B4. According to our estimates, in all specifications the inactive share distribution has no significant effect on reporting calls to 1522 for domestic violence. There are few specifications estimated both via OLS and PPML, which report significant coefficients at 10% level, and they do not highlight particular patterns. If anything, the direction of coefficient of upper quartiles is opposite to the one we would expect, as provinces which are part of the 4th quartile of the inactive share’s distribution face a differential, imprecisely estimated, negative effect in the 1522 call rate. Weekly frequency regressions lead not to really different conclusions, as shown in Tables B3 and B4. In the latter case, we observe how OLS estimates for calls by users are statistically significant and negative in two specifications (Col. (2) and (4), Table B2). This would suggest that the suspension of non-essential workers did not have a significant effect on *reported* domestic violence rates (and when it did, it was negative)⁴⁰. We could look at these results in the perspective of suspended workers being the perpetrators. If abusive partners, usually prone to violence, are suspended from the job and thus more exposed to sensitisation campaigns, they may become more cautious and reconsider their violent conduct, in order to avoid reporting and possible allegations⁴¹. Although understanding the determinants of such findings would have interesting policy implications, it goes beyond the scope of the present work, as long as we are able to conclude that the differential change in VPTs and unplanned pregnancies during the Covid-19 crisis was not driven by *reported domestic violence*; which appears to be the case. As a matter of fact, the hypothesis of power imbalances within a couple which may lead to abortions may not be so straightforwardly represented by official statistics on domestic violence.

1.8.5 Contraception

A further channel that may have credibly shifted unwanted conceptions in a heterogenous way, by leaving patterns of planned pregnancies unchanged, could be the access and usage of contraception methods. If there was some impact of the suspension of non-essential workers in the access or employment of birth control by the involved couples, then the observed results would mostly be driven by this factor. Unfortunately, to our knowledge, there is no available data source granular enough through which one can disentangle this. However, we recur to two different sources of data to provide with anecdotal evidence of the credible absence

⁴⁰This result would be quite counterintuitive, if significant; as a matter of fact, there are three most plausible channels that could feasibly work out in shifting the calls to domestic violence upwards. First, according to the exposure theory, suspended workers and their partners would spend more time together, and this shall exacerbate episodes of physical and sexual violence (Dugan et al., 1999); second, the economic insecurity channel could be a fostering effect in stress-related violence episodes or in abuses driven by the reconfiguration of bargaining power within couples Aizer, 2010). Third, the inactive share could actually foster the reporting bias highlighted by Colagrossi et al., 2022 already: by spending more time home, suspended workers could be more exposed to television campaigns and thus the rate of reporting ought to increase irrespective of actual violence occurring. Notwithstanding the latter form of upward bias to the estimates, we still retrieve non-significant, negative results.

⁴¹Or they may actively operate to physically prevent the victims to report, a more dramatic albeit less realistic hypothesis.

of correlation between work suspension and contraception. The Ministry of Health provided with yearly data on reproductive care facilities at regional level, (family planning centres, *consultori familiari* in Italian), while for years 2019 and 2022 they are also available at the municipal level; on top of that, we have information about the national consumption of emergency contraception (ECP) too, i.e. birth control drugs that can be assumed after an unprotected sexual intercourse. ISTAT platform Health For All collects instead regional data on contraceptive usage in Italy. Concerning the former, family planning centers are healthcare facilities designed to give assistance concerning the topics of family and motherhood. Among the other things, healthcare professionals of these centers offer psychological and social assistance to prepare couples to parenthood, give advice on the adequate tools to improve the likelihood to procreate or, on the other side, to prevent unwanted pregnancies, especially concerning the usage of birth control methods. The centers work with a twofold aim, in order to protect both the social value of motherhood and the assurance of the legal right of abortion, at the same time placing a great weight on the role of prevention. If there was a disruption in the services provided by family planning centers due to the pandemic, that was correlated with the inactive share, the observed increase in VPTs may be linked to a decrease in responsible prevention or to a lack of reproductive care facilities' assistance. We hence look at the number of yearly family planning centers at provincial level. In general, family planning centers are more spread in Central regions (especially Emilia Romagna and Umbria, Figure L1 in Appendix 1.21). Between 2019 and 2020, however, some Italian regions seem to have faced a reduction in the ratio of family planning centers to the number of women aged between 18 and 49; among them, there is Lombardy, one of the regions most hit by the pandemic, epidemiologically and economically. On the other side, two regions like Veneto and Piedmont, which had the greatest numbers of municipalities with a high share of suspended workers as well as Lombardy, actually faced an increase in family planning facilities. We also plot some merely correlational evidence at provincial level to assess whether it is the case to believe that the contraception channel deserves to be further investigated.

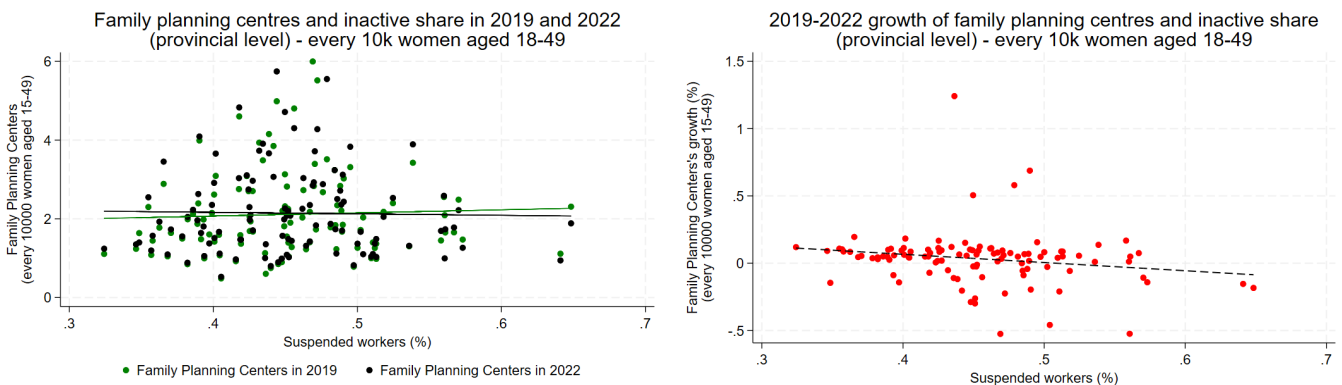


Figure 1.7: Annual growth of family planning centres in Italy and provincial inactive share. Left panel: rates of family planning centres every 10,000 women aged between 18 and 49 in 2019 and 2022. Right panel: 2018-2022 growth of family planning centres every 10,000 women aged between 18 and 49 in 2020. Source: Ministry of Health.

Albeit not statistically accurate enough, therefore insufficient to discard the hypothesis, we observe how,

in Figure 1.7, there seems to exist a very small negative correlation between the provincial suspended workers' share and the growth in reproductive care facilities.

Another factor, usually deemed as fundamental in bringing down unwanted pregnancies, is the emergency contraceptive pill (Girma and Paton, 2006, 2011, Durrance, 2013, Cintina, 2017, Bentancor and Clarke, 2017, Core, 2024), or ECP, the so-called “day-after pill” (or “5-days after pill”). Emergency contraceptive pills enable birth control after the occurrence of an unprotected intercourse, conditional on assuming them as soon as possible; in any case, they do not constitute an interruption of pregnancy, as they work within a time-frame within which pregnancy has not emerged yet⁴². In Italy, emergency contraception is mostly available by means of two main pharmaceuticals: Levonorgestel (Norlevo), to be taken within 5 days from the intercourse; Ulipristal (EllaOne), to be taken within 72 hours from the intercourse. They were not available OTC until 2015, and required a medical prescription together with a certification of occurred pregnancy. In May 2015 AIFA (the Italian Medicine Agency) made them available OTC for all adult women, with no certification requirement whatsoever. In October 2020, AIFA made them available OTC for minor women too (MoH, 2023). We cannot recover the geographical disaggregation of ECP sales in Italy over the studied years, nor their quarterly evolution. However, we can acknowledge that, in the considered time framework, no evident disruption in the overall sales of the drug occurred, nor was it reported by health authorities. As a matter of fact, there has been no noticeable shortage in the supply of such kind of medicines, nor one that could be feasibly correlated to the non-essential workers' suspension. Their OTC availability made them easy to be acquired by any woman who requested them, as the access to pharmacies (which were never closed down, obviously) was always guaranteed all along the duration of the state of emergency; furthermore, the pharmacists' conscientious objection for these drugs is not legally allowed. A reason not to go to pharmacies to purchase them could be linked to the fear of getting the virus (and in fact Figure L2 shows how 2020 was the year with the fewest sales of ECPs along the last 4), but that shall not be any correlated to the increase of VPTs we observe in areas with more suspended workers. In addition, the only evident institutional discontinuity which specifically concerns ECPs is the fact they were made available OTC to minors in October 2020; in 2021 there is indeed an increase in ECP purchases by almost 10% compared to 2020, to be reasonably credited to such policy change. Also, an overturn in the market share of the two ECP medicines happens in that year. If such event had any effect on VPTs, it was certainly a reducing one. However, the change occurred in October 2020, two month after the estimated effect we observe loses its significance. Furthermore, it should have contributed to reduce unplanned pregnancies (and thus VPTs) for minor women. They are feasibly not the ones leading the results in the months beforehand, as in our framework the trigger was due to labor market reasons (work suspension), which possibly affected already married couples. However, to sensibly rule out such confounder, we run two TWFE DiD models (one estimated via OLS and the other via PPML), where the outcome is the AR for minor women only, whereas the treatment variable, as in the

⁴²Their effectiveness, however, is reduced at any additional use, and they do not protect from pregnancies if an intercourse happens after their assumption (MoH, 2023)

municipal share of suspended worker, is interacted with a temporal dummy which takes unitary value after the 3rd quartile of 2020 (i.e., after the AIFA issue on ECP), rather than the conventional pandemic discontinuity. Tables a) and b) in Figure L3 prove for the non-significance of such hypothesis in explaining our results.

1.9 Conclusions

To conclude, we discuss the potential policy implications of our findings. As a matter of fact, we have been able to establish a causal claim about the differential impact of what we identified as our treatment on the VPT municipal patterns of Italian women. However, one might be led to question whether such exercise is even sensible, as the outbreak of the pandemic clearly brought about, in its immediate aftermath, a visible drop in abortion rates (see Figure 1.1 and Figure 1.3). As also shown by Trommlerová and González, 2024, for the Spanish case, the drop shall be reasonably attributed to the social distancing policies put in place in 2020, which reduced the possibility of sexual intercourse. However, by looking at the heterogenous effect on different types of municipalities/subsets, we can draw relevant conclusions on how women (and, more generally, couples) adjust their fertility and reproductive behavior following an unexpected event. By exploiting the exogenous shock of Covid-related work suspensions, we can abstract from a relevant portion of the endogeneity embedded in the individual abortion decisions, to stress the most remarkable findings, and try to interpret them under the policy point of view. First of all, we are able to identify about a 11% significant differential effect on the mean ARs in treated municipalities after the policy closure, relative to the control ones. As the average AR pre-Covid amounts to 1.1 VPTs every 1000 women in fertile age, that means that in such municipalities there is one additional quarterly abortion every 9000 women. This does not add much valuable information to the economic setting of our study per se, especially accounting for the fact that treated municipalities are, on average, less populous than the untreated ones. However, it must be noted that such numbers refer to the average ITT of our estimates, although we observe that the bulk of the shift in ARs occur in the second and the 3rd quarters of 2020, which are the only ones shown by the Event-Study estimates as reporting significant coefficients (Figures 1.3 and 1.4). In 2020Q2, the effect was about 30 p.p., while in Q3/2020 about 20 p.p.: 27% and 18% of the average AR pre-treatment respectively. This means one additional VPT every 3700 and 5555 women in such periods, which shrinks the “quantitative” population threshold required to observe a meaningful change in the abortion rates. These periods coincide with the harsher restriction ones, and they are actually concomitant with the work suspension that we use as our treatment. Such values may result relatively important for women who reside in smaller municipalities where VPTs are less frequent, in particular in a time of uncertainty. In such regards, and with respect to the strict economic implications of our findings, we find no correlation between the distributional share of the two macro-sectors of work suspension and those in which aborting women result employed; the municipal ARs significantly responding to the exogenous shock are indeed mainly those of women who are not in

professional condition. Overall, the differential variation for women residing in treated municipalities and not in professional condition amounts to 7 p.p., 14% the pre-Covid mean, thus a slightly greater but similar proportion compared to the baseline results. The fact that, however, municipal ARs of women not in professional condition significantly respond to the industrial distribution treatment only, and live births do not, makes room for the hypothesis that the main reason why there is a differential impact on VPTs is driven by a heterogeneous effect on the unplanned pregnancies due to differential time patterns spent together with respective male partners. This mirrors quite consistently our results on the ARs relative to marital status, as our estimates display a significant difference by 7 p.p. (OLS, PPML amounts to 8.5 p.p.) for married women's ARs, which correspond to 18%-22%, i.e. one additional VPT amongst married women every 5000 females in fertile age. The set of evidence gathered so far does not actually prevent us from deeming economic insecurity as a potential channel; what is suggested is that, plausibly, such mechanism does not work directly through women, as most are not in professional condition. Nevertheless, due to the conclusions drawn from the analysis on marital status and seeing how the effect is driven from the industry sector, we acknowledge that the choice to undertake an abortion, for those who are locally affected by the policy, may be compellingly dependent upon the partner's work trajectories more than on their own jobs. And while this may be significantly consistent with the hypothesis of more exposure, nothing leads to conclude that economic insecurity may not be affecting fertility choices through the male partner's job suspension.

Finally, the two most interesting results of our study (not been tackled yet by Italian research on the same data), for which the magnitude of the differential impact may actually result quite substantial for a potential health policy-maker. First and foremost, we reckon how there is no differential impact distinguishable from zeros for women with no previous pregnancies (Table 1.12, Col. (2)), whereas there is a significant effect at all quartiles of the distribution relative to the first for those with a previous gestation, especially if carried over. However, it results still quite hard to draw any implication without differentiating for the type of pregnancies and deliveries carried over. In Col. (5) of the same table we observe how women with previous live births and VPTs mainly matter for our heterogeneous shift in ARs. It is however quite hard to quantify the effect for already-mothers, as the coefficients are clearly significant not only on the last quartile of the distribution. When looking at the intensive margin, the main non-ambiguous and significant result concerns live birth, from Table 1.13, tells us that there is positive differential impact on women residing in treated municipalities and with at least one or two children (proxied by previous live births). When looking at previous VPTs, women with either 0 or 1 previous voluntary abortion responding significantly to the treatment, which is pretty intuitive. Although these results alone cannot help us disentangling whether economic insecurity played a major role over time exposure, the fact that the past reproductive behavior is somehow able to predict abortion decisions, may be a hint towards the hypothesis that the budget constraint imposed by having children in a family already may trigger positively abortion decisions. The most conclusive summing up explanation is that, compared to municipalities in the lowest quartile of the suspended worker share distribution, those in the 4th display significantly higher ARs. Among such women, results are mainly

relevant for married women, who do not work and who had 1 or 2 children already. This means that the suspension most likely involves blue-collar husbands. By also referencing the literature on the impact of Covid on Italian households cited insofar (Del Boca et al., 2020 and Mangiavacchi et al., 2021 most prominently), our results is consistent with the literature showing that women, being more subject to bear housework and childcare duties, and plausibly with lower control on their own fertility, had become more afraid for the future, which was also highlighted by the early cross-country survey by Galasso et al., 2020.

Provided with the latter discussion, we try to appraise how our work could matter somehow in the steering of reproductive health and family policy strategies. The first theme concerns the abortion right strictly. Literature has already underlined how objections matter in shaping abortions patterns (Bo et al., 2015, Autorino et al., 2020, Muratori, 2023). Due to having identified the most plausible categories of women who are more likely to respond to the shock in terms of unintended pregnancies, it seems like they belong to groups of women to whom to find non-objecting gynaecologists across areas could result as a major setback. We refer to women whose mobility or budget possibilities may be reduced due to presence of children, due to lack of resources as not in professional condition, or due to stigma or psychological pressure associated to having already aborted once. In such regards, as long as to policy makers it results adequate to equally value both the right to objection and that to abort, which matters to the institutional context, the Italian NHS should deepen the way objection shapes abortion decisions, as although on overall terms the official reports seem not to show that objectors prevent women for seeking for VPTs, that may not be the case when looking at particularly vulnerable women. In such case, one should implement targeted policies in order to provide with preferential channels and adequate services to grant their right, which may be facing obstacles due to their fragile situation. In addition, the issue of preventing entire health care structure to undertake “institutional” objection, as highlighted by Muratori, 2023, should be addressed. The second issue to be addressed is the one of vulnerability, to a greater extent. An effect driven by the industrial sector on already mothers OLF, likely means that for many women the abortion choice is dependent upon the partner’s suspension. This implies the control over fertility for such women is reduced, as they could result more vulnerable in terms of the existing gender balances within couples. This may usher both a policy and research discussion about the issue on how less autonomous women are directly in charge of their own fertility choices contrarily to how such decisions may be relying on the (feasibly male) breadwinners’ needs.

Appendix

1.10 Aggregate data on Abortions

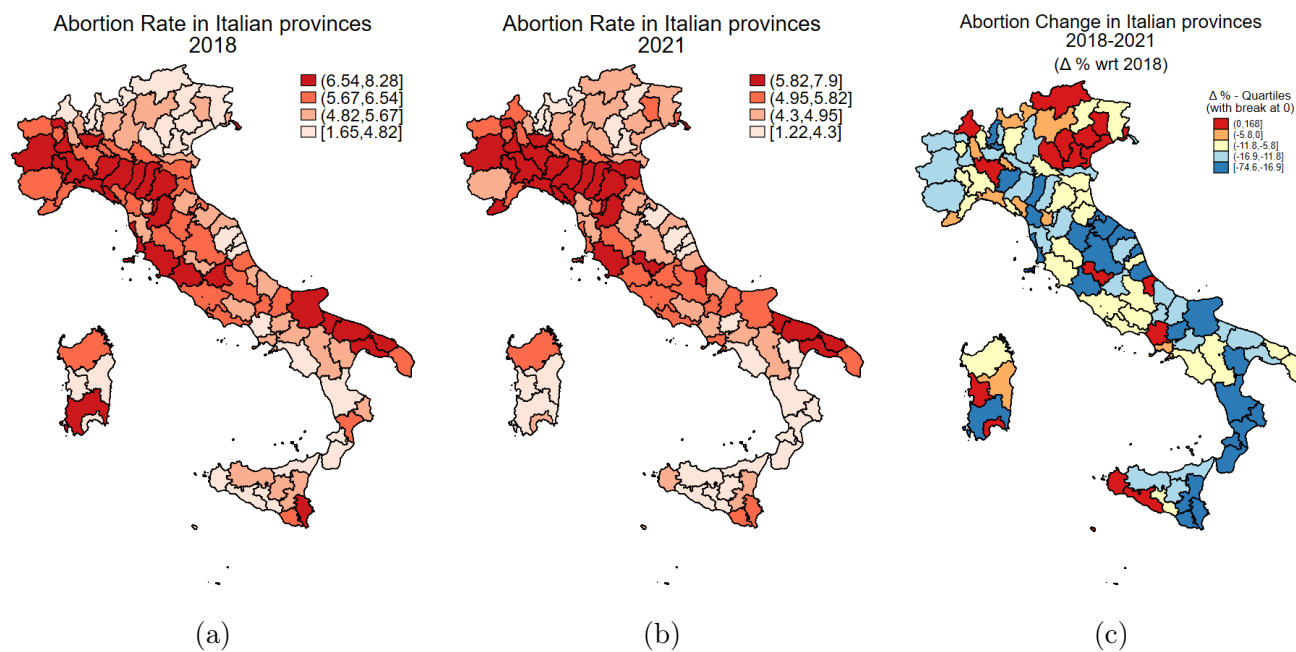


Figure A1: Provincial abortion rates: (a) 2018; (b) 2021; (c) $\Delta\%$ 2018-2021 wrt 2018 (source: ISTAT).

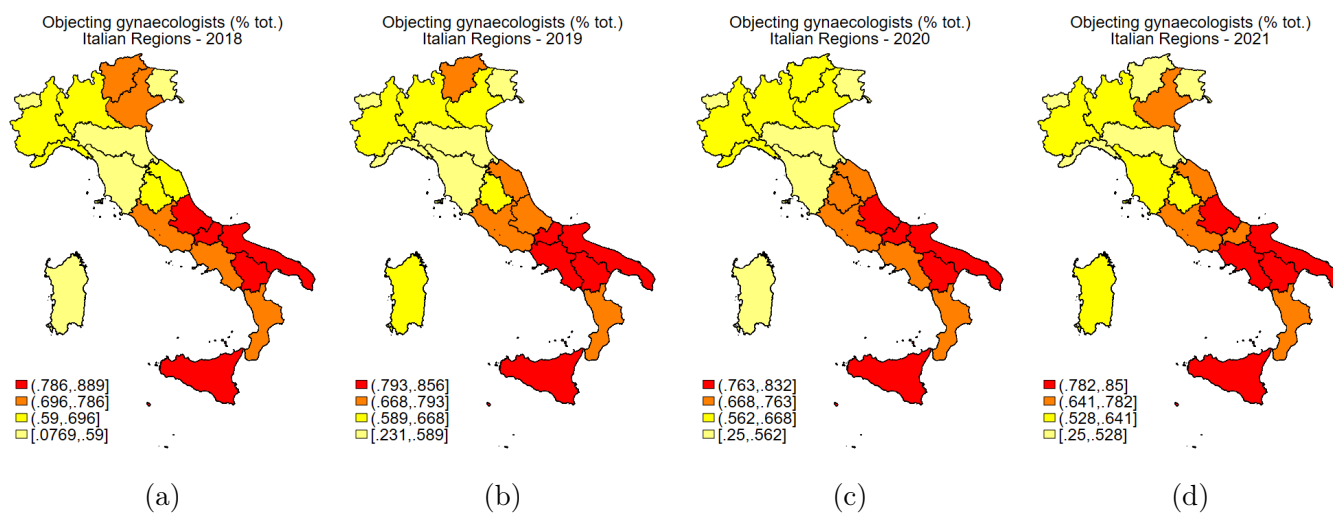


Figure A2: Objecting gynaecologists - % tot. gynaecologists - 2018-2021 (source: MoH)

1.11 Domestic Violence

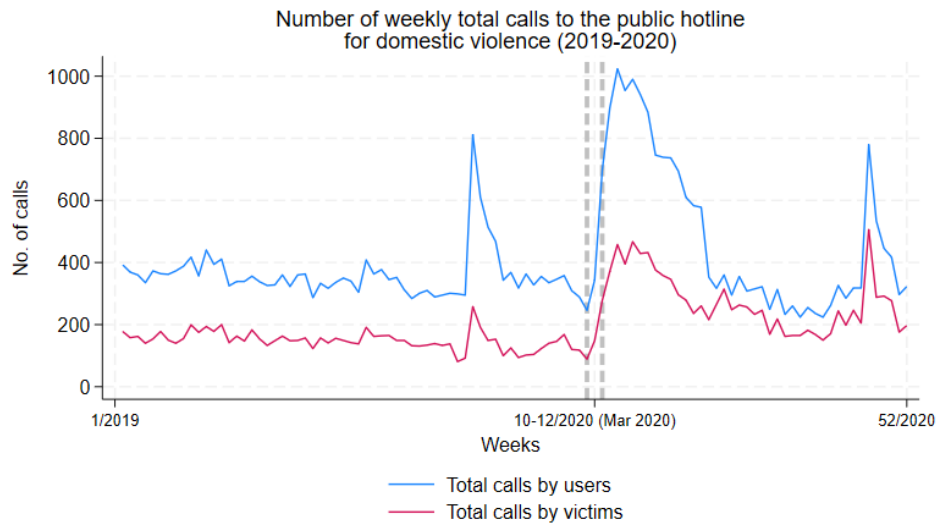


Figure B1: Weekly calls to the Italian public hotline for domestic violence (1522) in 2019 and 2020.

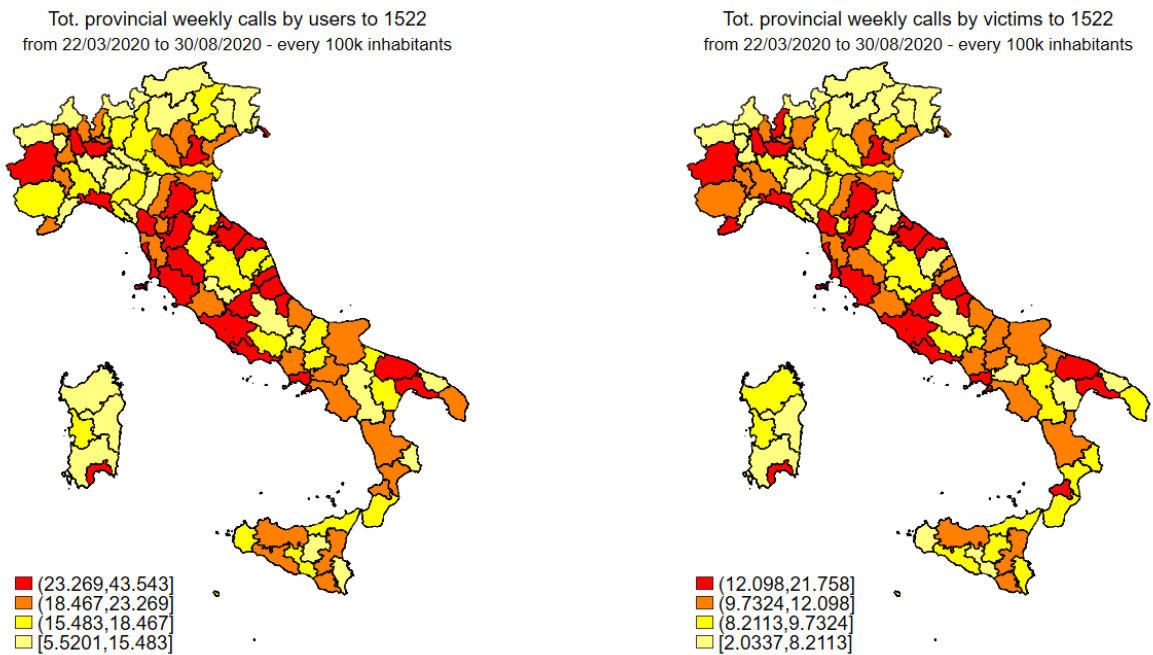


Figure B2: Total calls to 1522 over provincial population, from the week of closures in March 2020, to the last week of August 2020. Left panel reports the heat map for calls by all users, whereas the right panel reports the heat map for the calls by victims only. Calls with missing provenience are discarded.

Impacts of work suspension on reported calls by users to the hot-line for IPV (linear/non-linear quarterly estimates)								
	(1) Calls by users	(2) Calls by users	(3) Calls by users	(4) Calls by users	(5) Calls by users	(6) Calls by users	(7) Calls by users	(8) Calls by users
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$	-0.54188 [0.41952]	-0.64470 [0.56157]	-0.56786 [0.42165]	-0.70936 [0.56397]	-0.03438 [0.04324]	-0.01809 [0.05229]	-0.03459 [0.04267]	-0.02275 [0.05106]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$	0.17320 [0.49035]	0.22739 [0.59952]	0.14448 [0.48681]	0.16240 [0.60158]	0.04635 [0.05206]	0.09504 * [0.05514]	0.04518 [0.05083]	0.09024 * [0.05380]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$	-0.57632 [0.44929]	-0.82312 [0.54712]	-0.59057 [0.45280]	-0.83515 [0.54662]	-0.03282 [0.04858]	-0.03531 [0.05564]	-0.03301 [0.04784]	-0.03542 [0.05458]
Observations	1,712	1,664	1,712	1,664	1,712	1,664	1,712	1,664
R-squared	0.67587	0.72934	0.68392	0.73554				
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Region $\times$ quarter-year FE		✓		✓		✓		✓
Weight			✓	✓			✓	✓
Mean	7.657	7.657	7.657	7.657	7.657	7.657	7.657	7.657

Table B1: The coefficients in Columns (1) to (8) represent the municipal OLS and PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the provincial rates of call made by overall users to the public hot-line for domestic violence. Columns (1) to (4) report the OLS estimates. Columns (5) to (8) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the quarterly average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the calls at the province level and quarterly frequency. The SEs are clustered at provincial level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impacts of work suspension on reported calls by victims to the hot-line for IPV (linear/non-linear quarterly estimates)								
	(1) Calls by victims	(2) Calls by victims	(3) Calls by victims	(4) Calls by victims	(5) Calls by victims	(6) Calls by victims	(7) Calls by victims	(8) Calls by victims
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	-0.23919 [0.26084]	-0.50435 [0.36837]	-0.26151 [0.26453]	-0.55169 [0.37355]	-0.01676 [0.04446]	0.01258 [0.05714]	-0.01683 [0.04350]	0.00743 [0.05577]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.09622 [0.29742]	-0.12410 [0.35366]	0.07768 [0.29684]	-0.16942 [0.35796]	0.03860 [0.05547]	0.09995 * [0.05467]	0.03873 [0.05372]	0.09556 * [0.05316]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	-0.35888 [0.25717]	-0.58615 * [0.31585]	-0.37717 [0.26213]	-0.59867 * [0.31820]	-0.03499 [0.04572]	-0.02175 [0.05275]	-0.03696 [0.04449]	-0.02232 [0.05110]
Observations	1,712	1,664	1,712	1,664	1,712	1,664	1,712	1,664
R-squared	0.62030	0.68493	0.63010	0.69234				
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Region $\times$ quarter-year FE		✓		✓		✓		✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Weight			✓	✓			✓	✓
Mean	3.789	3.789	3.789	3.789	3.789	3.789	3.789	3.789

Table B2: The coefficients in Columns (1) to (8) represent the municipal OLS and PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the provincial rates of call made by victims only to the public hot-line for domestic violence. Columns (1) to (4) report the OLS estimates. Columns (5) to (8) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the quarterly average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the calls at the province level and quarterly frequency. The SEs are clustered at provincial level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on reported calls by users to the hot-line for IPV (linear/non-linear weekly estimates)								
	(1) Calls by users	(2) Calls by users	(3) Calls by users	(4) Calls by users	(5) Calls by users	(6) Calls by users	(7) Calls by users	(8) Calls by users
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	-0.04726 [0.03344]	-0.10055 ** [0.04807]	-0.04921 [0.03361]	-0.10427 ** [0.04780]	-0.04017 [0.04489]	-0.04010 [0.04427]	-0.11557 [0.07337]	-0.11693 [0.07180]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.00882 [0.03689]	0.03197 [0.04924]	0.00671 [0.03675]	0.02733 [0.04919]	0.04743 [0.05087]	0.04669 [0.04978]	0.09403 [0.07494]	0.09020 [0.07410]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	-0.04464 [0.03560]	-0.06970 [0.04940]	-0.04556 [0.03584]	-0.07024 [0.04916]	-0.03157 [0.04972]	-0.03157 [0.04887]	-0.06175 [0.07166]	-0.05994 [0.06927]
Observations	22,256	22,256	22,256	22,256	22,256	22,256	22,256	22,256
R-squared	0.27477	0.28633	0.28279	0.29438				
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Regional $\times$ Week_year FE		✓		✓			✓	✓
Week-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Weight			✓	✓		✓		✓
Mean	0.610	0.610	0.610	0.610	0.610	0.610	0.610	0.610

Table B3: The coefficients in Columns (1) to (8) represent the municipal OLS and PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the provincial rates of call made by overall users to the public hot-line for domestic violence. Columns (1) to (4) report the OLS estimates. Columns (5) to (8) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the quarterly average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the calls at the province level and weekly frequency. The SEs are clustered at provincial level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impacts of work suspension on reported calls by victims to the hot-line for IPV (linear/non-linear quarterly estimates)								
	(1) Calls by victims	(2) Calls by victims	(3) Calls by victims	(4) Calls by victims	(5) Calls by victims	(6) Calls by victims	(7) Calls by victims	(8) Calls by victims
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	-0.02069 [0.02020]	-0.04505 [0.03317]	-0.02218 [0.02045]	-0.04780 [0.03288]	-0.02214 [0.04561]	-0.02136 [0.04460]	-0.08869 [0.09213]	-0.08869 [0.09213]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.00262 [0.02149]	0.00564 [0.03193]	0.00120 [0.02160]	0.00286 [0.03189]	0.03688 [0.05189]	0.03739 [0.05050]	0.06318 [0.08054]	0.06318 [0.08054]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	-0.02844 [0.01972]	-0.04771 [0.03239]	-0.02959 [0.02013]	-0.04932 [0.03246]	-0.03843 [0.04532]	-0.03967 [0.04421]	-0.08288 [0.08433]	-0.08288 [0.08433]
Observations	22,256	22,256	22,256	22,256	22,256	22,256	22,256	22,256
R-squared	0.16877	0.17673	0.17489	0.18294				
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Region $\times$ Week_year FE		✓		✓			✓	✓
Week-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Weight			✓	✓		✓		✓
Mean	0.298	0.298	0.298	0.298	0.298	0.298	0.298	0.298

Table B4: The coefficients in Columns (1) to (8) represent the municipal OLS and PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the provincial rates of call made by victims only to the public hot-line for domestic violence. Columns (1) to (4) report the OLS estimates. Columns (5) to (8) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the quarterly average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the calls at the province level and weekly frequency. The SEs are clustered at provincial level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

1.12 Identification strategy

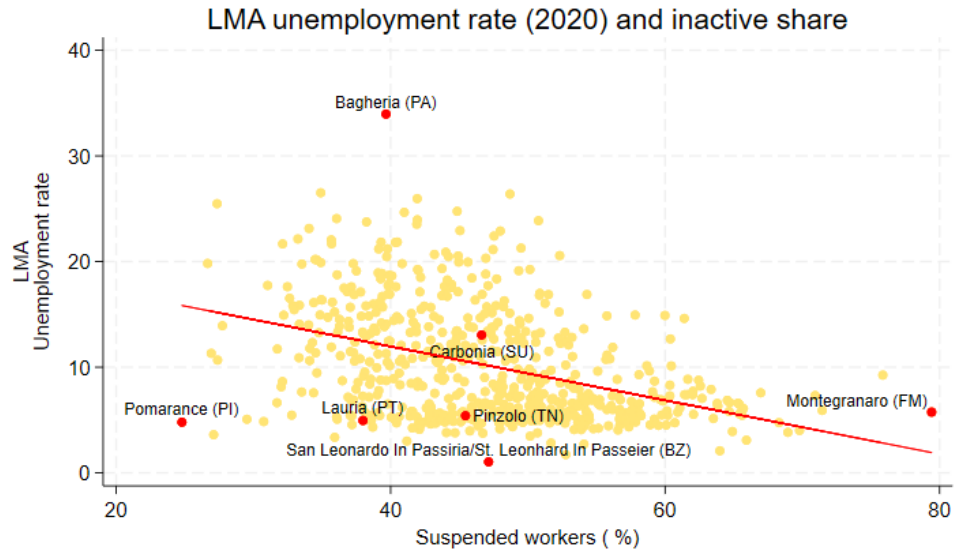


Figure C1: Predicted values between Local Market Areas' unemployment rate (2020, left panel) and suspended workers' share. The highlighted LMAs correspond to the ones having the maximum/minimum 2020's unemployment rate and inactive share. Carbonia (SU) has the median value of the inactive share.

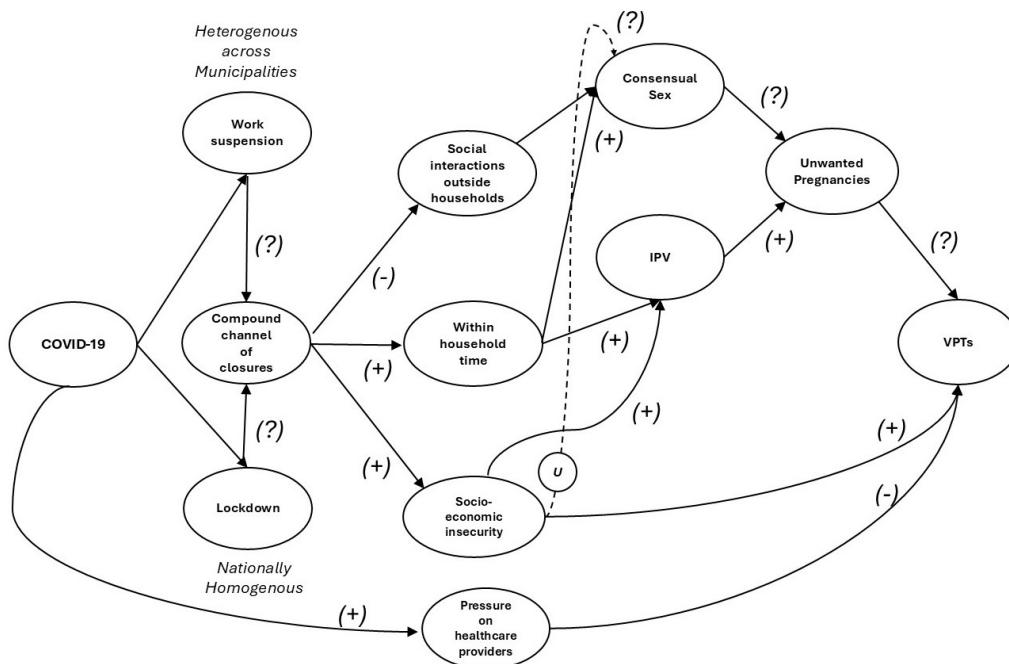


Figure C2: Supposed phenomena in action starting from the Covid pandemic: before the identification strategy.

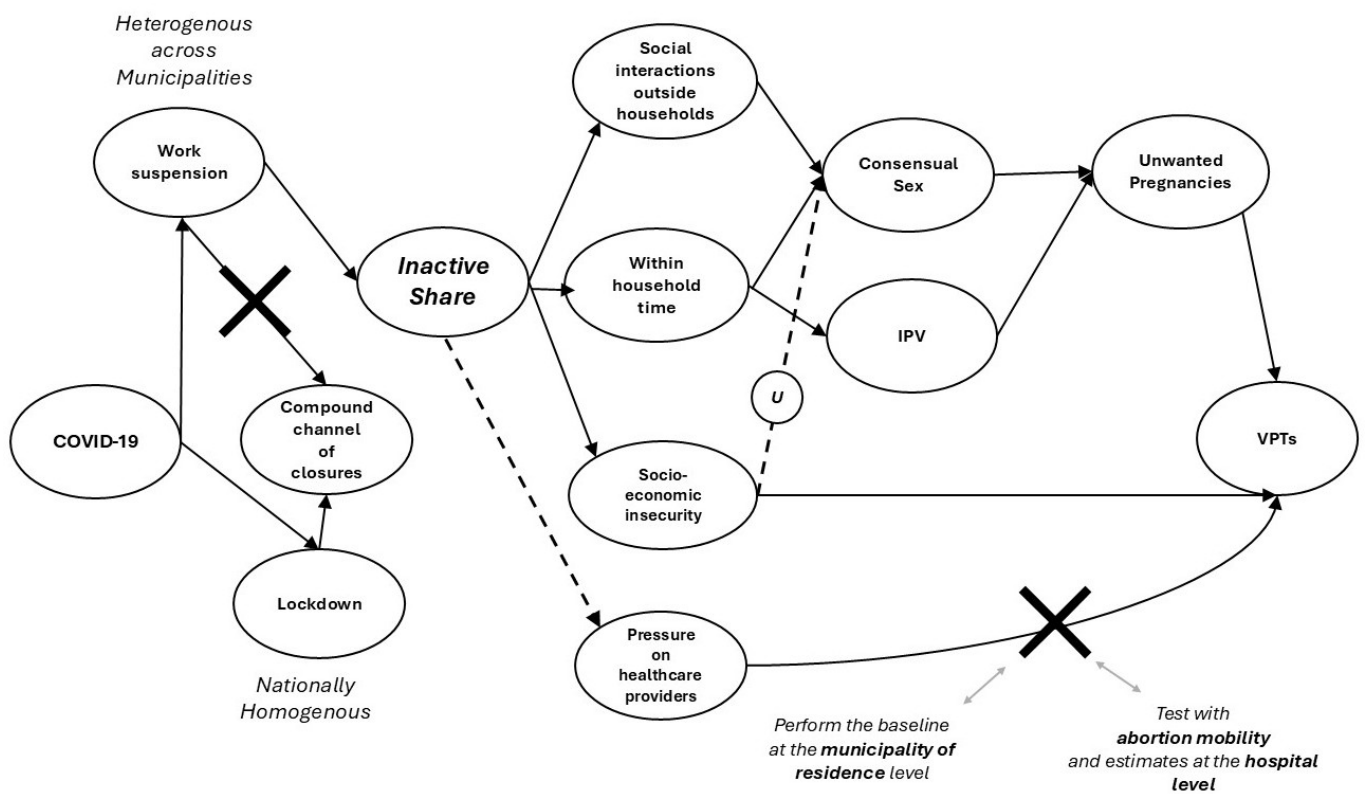


Figure C3: Identification strategy.

1.13 Covid-19 data

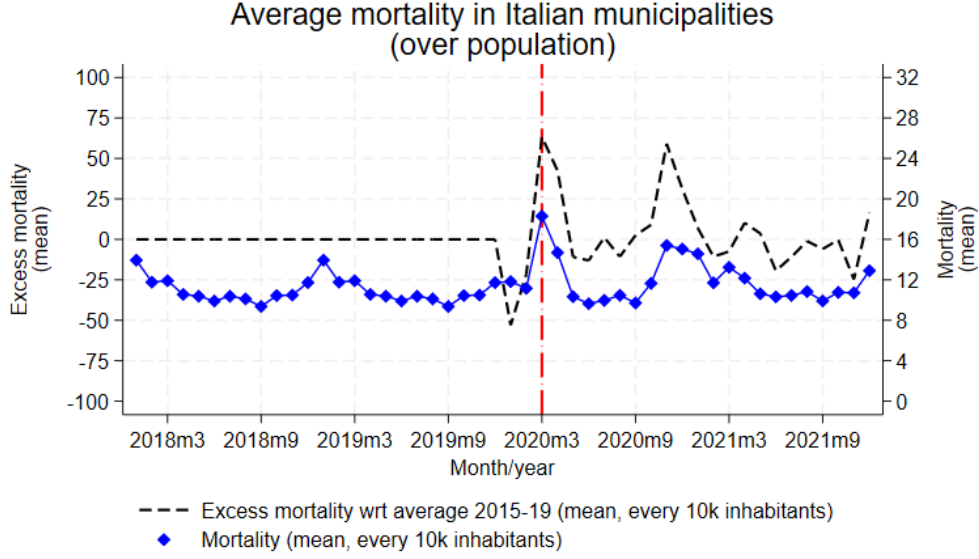


Figure D1: Path of municipal excess mortality in Italy between 2018 and 2021.

We report the method through which Conteduca and Borin, 2022 build their measures of policy restrictions during the pandemic outbreak below. For what concerns the stringency indexes, each index is constructed by looking at a set of variables, which take daily values, for each municipality, according to the severity of the applied restrictions, as explained in the table reported in Figure D2, directly drawn from the paper by Conteduca and Borin, 2022. Each variable in the tables gives birth to a sub-index I_{mti} as follows:

$$I_{mti} = 100 * \frac{v_{mti}}{V_i} \quad (1.7)$$

- v_{mti} = values associated with variable i at date t in municipality m ;
- V_i = is the maximum value of indicator i .

After the implementation of the Green Pass (6 August, 2021), the computation of I_{mti} slightly changes:

$$I_{mti} = 100 * \frac{\sigma_{mt}^g v_{mti}^g + (1 - \sigma_{mt}^g) v_{mti}^{ng}}{V_i} \quad (1.8)$$

- v_{mti}^g = variables indicating the restrictions *with* Green Pass;
- v_{mti}^{ng} = variables indicating the restrictions *without* Green Pass;
- σ_{mt}^g = share of individuals holding a GP at time t in municipality m .

The sub-indicators I_{mti} are aggregated to produce a **stringency index** $ItSI_m$ as follows:

$$ItSI_{mt} = \sum_i w_i I_{mti} \quad (1.9)$$

- $w_i = \frac{1}{9}$ for all indicators, except for **C2_1_Production**, **C2_2_Shops**, **C2_3_Bar_Restaurants** (for this subset, $w_i = \frac{1}{27}$).

Table 1 Policy indicators available in the dataset

Variable	Description	Value	Label
C1_Schools	Restrictions on in-person schooling	0	No restrictions
		0.5	Partial remote learning in upper secondary schools
		1	Full remote learning in upper secondary schools
		1.5	Full remote learning in upper secondary schools and final two years of lower secondary schools
		2	Full remote learning in upper and lower secondary schools
		2.5	In-person activities only in pre-school education
C2_1_Production	Restrictions on in-person production activities	3	No in-person activity
		0	No restrictions
		1	Remote working recommended
		2	Mandatory remote working for most activities
		3	Shutdown of all but essential production activities
C2_2_Shops	Restrictions on shops and personal services activities	0	No restrictions
		1	Limited restrictions (e.g., people allowed in stores)
		2	Closure of some shops
C2_3_BarsRestaurants	Restrictions on bars and restaurants	3	Shutdown of all but essential production activities
		0	No restrictions
		1	Dine-in allowed at some times of day
		2	Dine-in not allowed; takeaway and delivery allowed
C3_PublicEvents	Restrictions on in-person public events	3	Closure of all bars and restaurants
		0	No restrictions
		1	Cancellation of some public events
		2	Cancellation of most public events
		0	No restrictions

(a)

Table 1 continued

Variable	Description	Value	Label
C4_Gatherings	Restrictions on in-person gatherings	1	Gatherings over 1,000 people allowed
		2	Gatherings up to 1,000 people allowed
		3	Gatherings up to 100 people allowed
		4	Gatherings up to 10 people allowed
		0	No restrictions
C5_PublicTransport	Restrictions on public transportation	1	Reduced capacity
		2	Shutdown of public transport
		0	No restrictions
C6_StayAtHome	Restrictions on quarantines and isolation	1	Recommended sheltering
		2	Mandatory sheltering (excluded essential activities)
		3	Mandatory sheltering (with very few exceptions)
		0	No restrictions
		1	Limited restrictions (e.g., curfew)
C7_InternalMovement	Restrictions on domestic travel and movement	2	No movement between regions
		3	No movement between municipalities
		4	No movement within a municipality
		0	No restrictions
		1	Limited control (e.g., negative test)
C8_InternationalTravel	Restrictions on international travel	2	Mandatory quarantine
		3	Entry ban on some countries
		4	Entry ban on all countries
		0	No campaigns
H1_PublicCampaigns	Presence of public information campaigns	1	Public campaigns on some media
		2	Coordinated campaigns on all media

Source: Authors' elaboration adapting Hale et al. (2021) to the restrictions and provisions in place in Italy since January 1, 2020

(b)

Figure D2: Source: Conteduca and Borin, 2022.

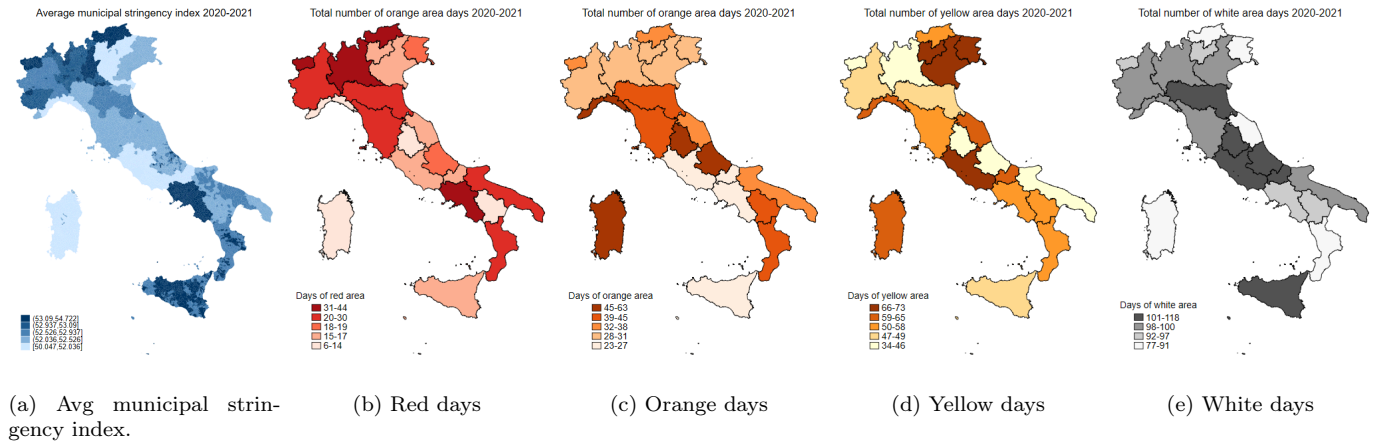


Figure D3: Elaboration on the data by Conteduca and Borin, 2022

1.14 Temporal sensitivity

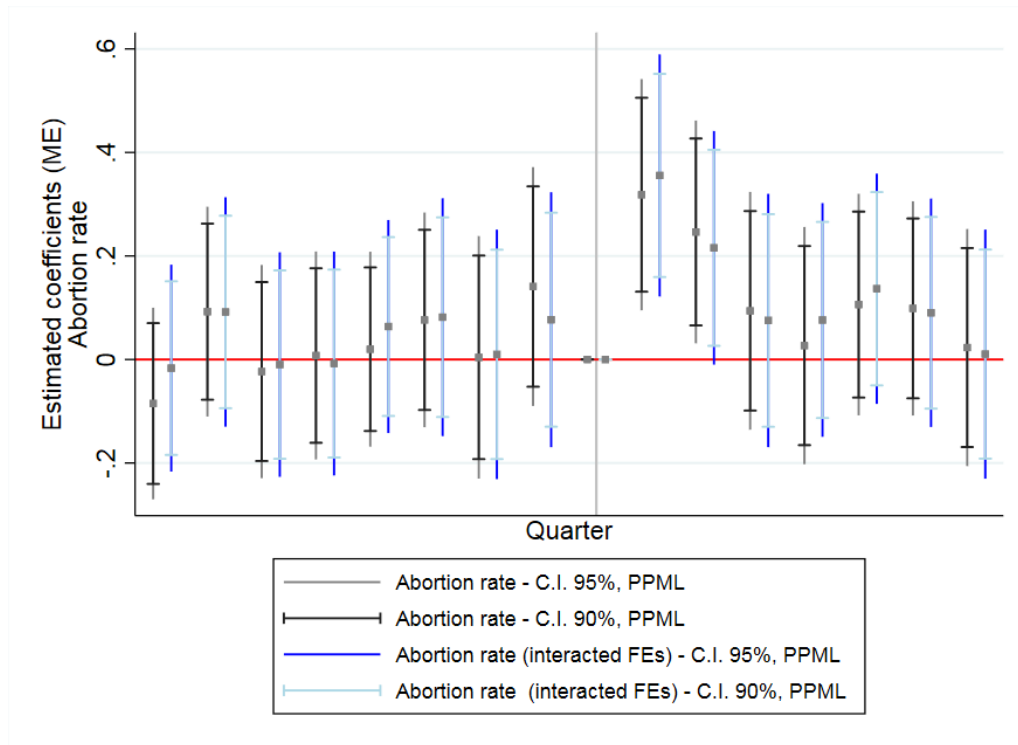


Figure E1: PPML Event-study estimates. The figure reports the marginal effects of temporal units and their confidence intervals, both for the baseline specification and the one with interaction FEs between province dummies and quarterly dummies. Confidence intervals are reported at both 90% and 95%. The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical line is set on quarter 9, which corresponds to the first quarter of 2020, the first lead before the treatment, occurring on Q2 2020.

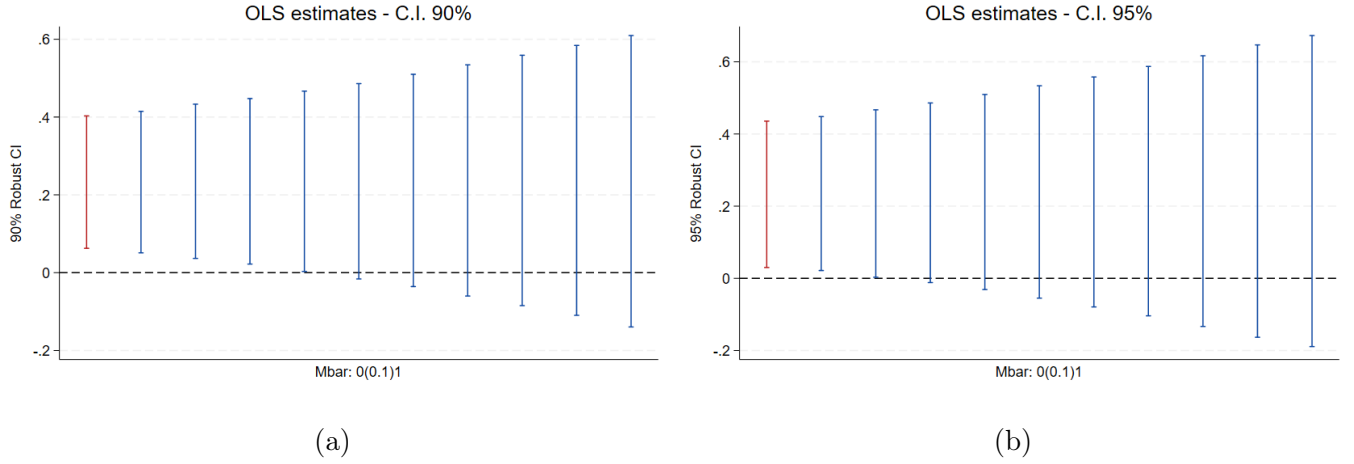


Figure E2: Parallel trends sensitivity analysis, — break points for the treatment coefficient on the second quarter of 2020, based on estimates from Equation 1.3. Confidence interval at 90% (a), 95% (b). The x-axis reports the varying magnitude of the break parameter M , allowing for deviations from the common trend, ranges from 0% to 100%, by intervals of 10%.

Impacts of work suspension on ARs (linear annual estimates)							Impacts of work suspension on ARs (non-linear annual estimates)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
$Post * \mathbb{1}(I. S_{20} \in Q_4)$	0.45936 *** [0.18155]	0.40473 *** [0.14232]	0.44251 ** [0.18278]	0.38749 *** [0.14344]	0.46248 ** [0.20957]	0.40370 ** [0.16431]	$Post * \mathbb{1}(I. S_{20} \in Q_4)$	0.48261 ** [0.19869]	0.40296 *** [0.15296]	0.46742 ** [0.20052]	0.38752 ** [0.15456]	0.54760 ** [0.22249]	0.42632 ** [0.17294]
$Post * \mathbb{1}(I. S_{20} \in Q_3)$	0.027750 [0.17144]	0.22828 * [0.13291]	0.27461 [0.17227]	0.22259 * [0.13375]	0.30372 * [0.18107]	0.24423 * [0.14047]	$Post * \mathbb{1}(I. S_{20} \in Q_3)$	0.29128 [0.18411]	0.22390 [0.13928]	0.29055 [0.18542]	0.22038 [0.14043]	0.35035 * [0.19054]	0.25932 * [0.14573]
$Post * \mathbb{1}(I. S_{20} \in Q_2)$	0.15870 [0.15953]	0.13585 [0.12357]	0.16328 [0.15987]	0.13681 [0.12403]	0.16474 [0.16327]	0.13535 [0.12677]	$Post * \mathbb{1}(I. S_{20} \in Q_2)$	0.16671 [0.17143]	0.13396 [0.12889]	0.17390 [0.17192]	0.13784 [0.12965]	0.19209 [0.17193]	0.14084 [0.13113]
Observations	31,612	31,612	31,612	31,612	31,612	31,612	Observations	29,048	29,048	29,048	29,048	29,048	29,048
R-squared	0.34647	0.35665	0.34674	0.35692	0.35777	0.36792	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	Year FE	✓	✓	✓	✓	✓	✓
Prov. x Year FE					✓	✓	Prov. x Year FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	4.374	4.374	4.374	4.374	4.374	4.374	Mean	4.374	4.374	4.374	4.374	4.374	4.374

(a) Annual Specification (OLS)

(b) Annual Specification (PPML)

Figure E3: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal abortion rates, by aggregating data at the annual frequency. Table (a) reports OLS estimates. Table (b) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the annually average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impacts of work suspension on ARs (linear monthly estimates)							Impacts of work suspension on ARs (non-linear monthly estimates)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
$Post * 1(I. S_{03/20} \in Q_4)$	0.04378 *** [0.01522]	0.03780 *** [0.01188]	0.04125 *** [0.01530]	0.03556 *** [0.01194]	0.04285 ** [0.01736]	0.03628 *** [0.01356]	$Post * 1(I. S_{03/20} \in Q_4)$	0.04659 *** [0.01702]	0.03775 *** [0.01301]	0.04413 *** [0.01708]	0.03568 *** [0.01305]	0.04807 ** [0.01865]	0.03810 *** [0.01446]
$Post * 1(I. S_{03/20} \in Q_3)$	0.01904 [0.01422]	0.01608 [0.01100]	0.01989 [0.01414]	0.01666 [0.01093]	0.02235 [0.01495]	0.01811 [0.01157]	$Post * 1(I. S_{03/20} \in Q_3)$	0.02005 [0.01563]	0.01358 [0.01176]	0.02110 [0.01556]	0.01617 [0.01170]	0.02566 [0.01606]	0.01866 [0.01224]
$Post * 1(I. S_{03/20} \in Q_2)$	0.01162 [0.01317]	0.00987 [0.01019]	0.00120 [0.01325]	0.00931 [0.01024]	0.01064 [0.01343]	0.00850 [0.01041]	$Post * 1(I. S_{03/20} \in Q_2)$	0.01236 [0.01449]	0.00965 [0.01050]	0.01198 [0.01458]	0.00919 [0.01091]	0.01279 [0.01439]	0.00865 [0.01095]
Observations	379,344	379,344	371,441	371,441	371,441	371,441	Observations	348,576	348,576	340,609	340,609	340,519	340,519
R-squared	0.03428	0.03622	0.03402	0.03595	0.04544	0.04611	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓	Month-year FE	✓	✓	✓	✓	✓	✓
Prov.-Month_year FE					✓	✓	Prov.-Month_year FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	0.368	0.368	0.368	0.368	0.368	0.368	Mean	0.368	0.368	0.368	0.368	0.368	0.368

(a) Monthly Specification (OLS)

(b) Monthly Specification (PPML)

Figure E4: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal abortion rates, by aggregating data at the monthly frequency. Table (a) reports OLS estimates. Table (b) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the monthly lagged average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impacts of work suspension on ARs (linear semestral estimates)							Impacts of work suspension on ARs (non-linear semestral estimates)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
<i>Post * 1(I. S_{03/20} ∈ Q₄)</i>	0.22968 **	0.20237 ***	0.22546 **	0.19714 ***	0.23061 **	0.20126 **	<i>Post * 1(I. S_{03/20} ∈ Q₄)</i>	0.24131 **	0.20148 ***	0.23656 **	0.19705 **	0.25776 **	0.21183 **
	[0.09077]	[0.07116]	[0.09114]	[0.07150]	[0.10459]	[0.08196]		[0.09935]	[0.07648]	[0.09988]	[0.07695]	[0.11101]	[0.08623]
<i>Post * 1(I. S_{03/20} ∈ Q₃)</i>	0.13875	0.11414 *	0.13668	0.11126 *	0.15137 *	0.12155 *	<i>Post * 1(I. S_{03/20} ∈ Q₃)</i>	0.14565	0.11195	0.14389	0.10965	0.17412 *	0.12821 *
	[0.08572]	[0.06646]	[0.08600]	[0.06675]	[0.09039]	[0.07009]		[0.09220]	[0.06964]	[0.09258]	[0.07004]	[0.09504]	[0.07263]
<i>Post * 1(I. S_{03/20} ∈ Q₂)</i>	0.07935	0.068793	0.08049	0.06764	0.08222	0.06726	<i>Post * 1(I. S_{03/20} ∈ Q₂)</i>	0.08336	0.06698	0.08571	0.06788	0.09578	0.06953
	[0.07976]	[0.06178]	[0.07984]	[0.06193]	[0.08149]	[0.06325]		[0.08572]	[0.06444]	[0.08589]	[0.06470]	[0.08574]	[0.06536]
Observations	63,224	63,224	63,224	63,224	63,224	63,224	Observations	58,096	58,096	58,096	58,096	58,096	58,096
R-squared	0.19149	0.19967	0.19158	0.19974	0.20417	0.21164	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Semester-year FE	✓	✓	✓	✓	✓	✓	Semester-year FE	✓	✓	✓	✓	✓	✓
Prov.-Sem._year FE					✓	✓	Prov.-Sem._year FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	2.187	2.187	2.187	2.187	2.187	2.187	Mean	2.187	2.187	2.187	2.187	2.187	2.187

(a) Semestral Specification (OLS)

(b) Semestral Specification (PPML)

Figure E5: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal abortion rates, by aggregating data at the semestral frequency. Table (a) reports OLS estimates. Table (b) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). The policy covariates include the semestral average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

1.15 Treatment sensitivity

Impacts of work suspension on ARs for varying treatment specifications (linear)							Impacts of work suspension on ARs for varying treatment specifications (non-linear)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
<i>Post * (I. S._{q2/20})</i>	0.00325 ** [0.00141]			0.00327 ** [0.00158]			<i>Post * (I. S._{q2/20})</i>	0.00360 ** [0.00161]			0.00388 ** [0.00174]		
<i>Post * 1 (I. S._{q2/20} ≥ P50)</i>		0.07450 *** [0.02858]			0.07678 ** [0.03177]		<i>Post * 1 (I. S._{q2/20} ≥ P50)</i>		0.07825 *** [0.03170]			0.08517 ** [0.03428]	
<i>Post * 1 (I. S._{q2/20} ∈ T3)</i>			0.10914 *** [0.03744]			0.11317 *** [0.04301]	<i>Post * 1 (I. S._{q2/20} ∈ T3)</i>			0.11610 *** [0.04193]			0.12601 *** [0.04647]
<i>Post * 1 (I. S._{q2/20} ∈ T2)</i>			0.01415 [0.03371]			0.02023 [0.03484]	<i>Post * 1 (I. S._{q2/20} ∈ T2)</i>			0.01754 [0.03694]			0.02838 [0.03734]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	Observations	116,192	116,192	116,192	116,192	116,192	116,192
R-squared	0.10048	0.10043	0.10046	0.11225	0.11221	0.11224	R-squared						
Policy covariates	✓	✓	✓	✓	✓	✓	Policy covariates	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter FE				✓	✓	✓	Prov. × Quarter FE				✓	✓	✓
Mean	1.164	1.134	1.103	1.164	1.134	1.103	Mean	1.164	1.134	1.103	1.164	1.134	1.103

(a) OLS

(b) PPML

Figure F1: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and PPML (b) estimates of the ITT effect of being part of differently defined portions of the suspended workers share distribution. Table (a) reports OLS estimates. Table (b) the Marginal Effects of the Pseudo-Poisson Maximum Likelihood ones (PPML). Columns 1 and 4 report the effect of the distribution defined continuously (from 0 to 100). Columns 2 and 5 the effect of being a municipality above the median of the distribution. Columns (3) and (6) are with respect of the terciles of the distribution. The policy covariates include the quarterly average provincial stringency index, the total number of red, orange and yellow area days (white area is the reference) and provincial excess mortality. The panel is built by aggregating the abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Placebo linear estimates: impact of work suspension on municipal ARs, at Q2 of the different years in the sample															
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR	(7) AR	(8) AR	(9) AR	(10) AR	(11) AR	(12) AR	(13) AR	(14) AR	(15) AR
<i>Post March 2018</i> * $\mathbb{I}(I \cdot S_{q/20} \in Q_4)$	0.09653	0.06472	0.08486												
	[0.07505]	[0.08175]	[0.07739]												
<i>Post March 2019</i> * $\mathbb{I}(I \cdot S_{q/20} \in Q_4)$				0.05436	0.01368	0.04662	0.05396								
				[0.05313]	[0.09276]	[0.06974]	[0.08085]								
<i>Post March 2020</i> * $\mathbb{I}(I \cdot S_{q/20} \in Q_4)$								0.20488 **	0.15689 ***	0.14117 **		0.14289			
								[0.09449]	[0.05250]	[0.05838]		[0.08823]			
<i>Post March 2021</i> * $\mathbb{I}(I \cdot S_{q/20} \in Q_4)$										0.04008			0.00559	-0.04315	0.01066
										[0.05540]			[0.10336]	[0.06281]	[0.05341]
Observations	94,836	34,612	63,224	94,836	31,612	63,224	63,224	31,612	94,836	63,224	94,836	63,224	31,612	63,224	126,448
R-squared	0.13586	0.31096	0.17926	0.13586	0.29820	0.17926	0.16994	0.28081	0.13589	0.17005	0.13589	0.16453	0.29015	0.16449	0.11216
Considered years	'18-'19, '21	'18	'18-'19	'18-'19, '21	'19	'18-'19	'19-'20	'20	'18-'20	'19-'20	'18-'19, '21	'20-'21	'21	'20-'21	'18-'21
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	1.144	1.144	1.144	1.144	1.160	1.144	1.160	1.181	1.103	1.104	1.085	1.181	1.013	1.058	1.080

Table F1: The coefficients in Columns (1) to (16) represent the municipal OLS placebo estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, obtained by interacting the suspended workers share's distribution with Q2 of various years across the dataset, and at different sample sizes. Columns 1-3 consider 2018Q2 as the treatment event. Columns 4-7 2019Q2. Columns 8-10 and 11 2020Q2. Columns 11 and 13-15 2021Q2. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Placebo non-linear estimates: impact of work suspension on municipal ARs, at Q2 of the different years in the sample															
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR	(7) AR	(8) AR	(9) AR	(10) AR	(11) AR	(12) AR	(13) AR	(14) AR	(15) AR
<i>Post March</i> 2018 * $\mathbb{1}(I. S_{\cdot q2/20} \in Q_4)$	0.07931 [0.07533]	0.06583 [0.10626]	0.07583 [0.08493]												
<i>Post March</i> 2019 * $\mathbb{1}(I. S_{\cdot q2/20} \in Q_4)$				0.04414 [0.05396]	-0.00377 [0.11132]	0.04229 [0.07427]	0.04748 [0.08611]								
<i>Post March</i> 2020 * $\mathbb{1}(I. S_{\cdot q2/20} \in Q_4)$								0.27922 ** [0.11336]	0.19013 *** [0.06092]	0.18673 *** [0.06854]		0.14851 * [0.08940]			
<i>Post March</i> 2021 * $\mathbb{1}(I. S_{\cdot q2/20} \in Q_4)$											0.03807 [0.06325]		0.01624 [0.12063]	-0.05485 [0.07733]	0.00500 [0.05947]
Observations	84,804	23,669	53,824	84,804	53,080	52,824	52,792	22,472	84,756	52,792	84,804	52,200	22,372	52,200	116,192
Policy covariates								✓	✓	✓	✓	✓	X	✓	✓
Excess mortality							✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓		✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time-span	18-19, 21	18		18-19, 21	19	18-19	19-20	20	18-21	19-20	18-19, 21	20-21	21	20-21	18-21
Mean	1.114	1.144		1.144	1.160	1.114	1.160	1.181	1.103	1.104	1.085	1.181	1.013	1.058	1.080

Table F2: The coefficients in Columns (1) to (16) represent the municipal Marginal Effects of the Pseudo-Poisson Maximum Likelihood (PPML) placebo estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, obtained by interacting the suspended workers share's distribution with Q2 of various years across the dataset, and at different sample sizes. Columns 1-3 consider 2018Q2 as the treatment event. Columns 4-7 2019Q2. Columns 8-10 and 11 2020Q2. Columns 11 and 13-15 2021Q2. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

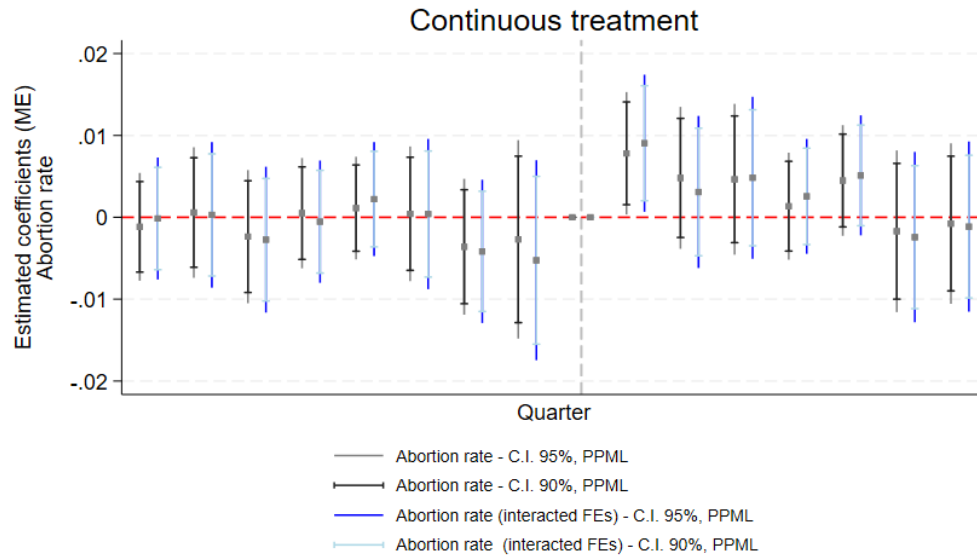


Figure F2: PPML Event-study estimates with the continuous treatment specification. The figure reports the marginal effects of temporal units and their confidence intervals, both for the baseline specification and the one with interaction FEs between province dummies and quarterly dummies. Confidence intervals are reported at both 90% and 95% The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical line is set on quarter 9, which corresponds to the first quarter of 2020, the first lead before the treatment, occurring on Q2 2020.

Impacts of work suspension on ARs (linear estimates without VPTs beyond gest. limits)							Impacts of work suspension on ARs (non-linear estimates without VPTs beyond gest. limit)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
								(ME)	(ME)	(ME)	(ME)	(ME)	(ME)
<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	0.12711 *** [0.04446]	0.10910 *** [0.03469]	0.12504 *** [0.04469]	0.10737 *** [0.03492]	0.12628 ** [0.05043]	0.10531 *** [0.03949]	<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	0.13487 *** [0.05016]	0.10765 *** [0.03829]	0.13240 *** [0.05031]	0.10562 *** [0.03844]	0.13769 ** [0.05461]	0.10738 *** [0.04238]
<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.05544 [0.04168]	0.04717 [0.03228]	0.05413 [0.04177]	0.04595 [0.03238]	0.05906 [0.04397]	0.04760 [0.03411]	<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.05787 [0.04611]	0.04417 [0.03468]	0.05639 [0.04619]	0.04282 [0.03476]	0.06745 [0.04750]	0.04757 [0.03620]
<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.03729 [0.03844]	0.03291 [0.02980]	0.03742 [0.03847]	0.03276 [0.02984]	0.03403 [0.03897]	0.02866 [0.03028]	<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.03919 [0.04252]	0.03186 [0.03185]	0.03985 [0.04256]	0.03215 [0.03192]	0.04074 [0.04199]	0.02891 [0.03199]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	Observations	115,504	115,504	115,504	115,504	115,504	115,504
R-squared	0.10189	0.10664	0.10192	0.10666	0.11380	0.11759	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter FE					✓	✓	Prov. × Quarter FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103	Mean	1.103	1.103	1.103	1.103	1.103	1.103

(a) OLS

(b) PPML

Figure F3: The coefficients in Columns (1) to (6) represent the municipal OLS (a) and the Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, after removing from the pooled cross-sections the VPTs performed after the gestational limits (before constructing the panel). The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs (linear estimates w/out Metropolitan Cities)							Impacts of work suspension on ARs (non-linear estimates w/out Metropolitan Cities)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
								(ME)	(ME)	(ME)	(ME)	(ME)	(ME)
<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	0.13125 *** [0.04576]	0.11333 *** [0.03580]	0.12888 *** [0.04598]	0.11156 *** [0.03602]	0.13183 *** [0.05200]	0.11136 *** [0.04082]	<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	0.14015 *** [0.05119]	0.11406 *** [0.03925]	0.13732 *** [0.05133]	0.11191 *** [0.03938]	0.14597 *** [0.05596]	0.11630 *** [0.04358]
<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.05615 [0.04276]	0.04712 [0.03316]	0.05482 [0.04284]	0.04603 [0.03325]	0.06231 [0.04513]	0.05009 [0.03506]	<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.05962 [0.04703]	0.04587 [0.03549]	0.05798 [0.04708]	0.04450 [0.03554]	0.07222 [0.04847]	0.05199 [0.03707]
<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.03411 [0.03960]	0.02915 [0.03075]	0.03433 [0.03957]	0.02913 [0.03075]	0.03270 [0.04017]	0.02673 [0.03128]	<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.03665 [0.04362]	0.02929 [0.03283]	0.03704 [0.04359]	0.02934 [0.03282]	0.03854 [0.04312]	0.02689 [0.03299]
Observations	126,224	126,224	126,224	126,224	126,224	126,224	Observations	115,968	115,968	115,968	115,968	115,968	115,968
R-squared	0.10030	0.10501	0.10033	0.10503	0.11212	0.11594	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter FE					✓	✓	Prov. × Quarter FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103	Mean	1.103	1.103	1.103	1.103	1.103	1.103

(a) OLS

(b) PPML

Figure F4: The coefficients in Columns (1) to (6) represent the municipal OLS (a) and the Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, after removing from the panel the chieftowns of the Italian Metropolitan Cities. The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs (linear estimates w/out urgent VPTs)							Impacts of work suspension on ARs (non-linear estimates w/out urgent VPTs)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
								(ME)	(ME)	(ME)	(ME)	(ME)	(ME)
$Post * 1(I. S_{q2/20} \in Q_4)$	0.09557 ** [0.03902]	0.08961 *** [0.03069]	0.08975 ** [0.03938]	0.08505 *** [0.03097]	0.07036 [0.04490]	0.06337 * [0.03524]	$Post * 1(I. S_{q2/20} \in Q_4)$	0.10303 ** [0.04473]	0.08936 *** [0.03432]	0.09639 ** [0.04508]	0.08429 ** [0.03457]	0.07855 [0.04910]	0.06404 * [0.03829]
$Post * 1(I. S_{q2/20} \in Q_3)$	0.05835 [0.03581]	0.05018 * [0.02812]	0.05477 [0.03599]	0.04720 * [0.02827]	0.04516 [0.03789]	0.03498 [0.02973]	$Post * 1(I. S_{q2/20} \in Q_3)$	0.06003 [0.04066]	0.04651 [0.03089]	0.05595 [0.04088]	0.04327 [0.03104]	0.04834 [0.04140]	0.03193 [0.03198]
$Post * 1(I. S_{q2/20} \in Q_2)$	0.01108 [0.03380]	0.00975 [0.02639]	0.01050 [0.03385]	0.00911 [0.02644]	0.00467 [0.03444]	0.00327 [0.02693]	$Post * 1(I. S_{q2/20} \in Q_2)$	0.01091 [0.03820]	0.00663 [0.02876]	0.01076 [0.03826]	0.00645 [0.02883]	0.00775 [0.03752]	0.00177 [0.02888]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	Observations	112,576	112,576	112,576	112,576	112,576	112,576
R-squared	0.09908	0.10414	0.09913	0.10418	0.11107	0.11519	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter FE					✓	✓	Prov. × Quarter FE					✓	✓
Weight		✓		✓	✓	✓	Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103	Mean	1.103	1.103	1.103	1.103	1.103	1.103

(a) OLS

(b) PPML

Figure F5: The coefficients in Columns (1) to (6) represent the municipal OLS (a) and the Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, after removing from the pooled cross-sections the VPTs performed with urgency (before constructing the panel). The reference category is the 1st quartile of the distribution. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

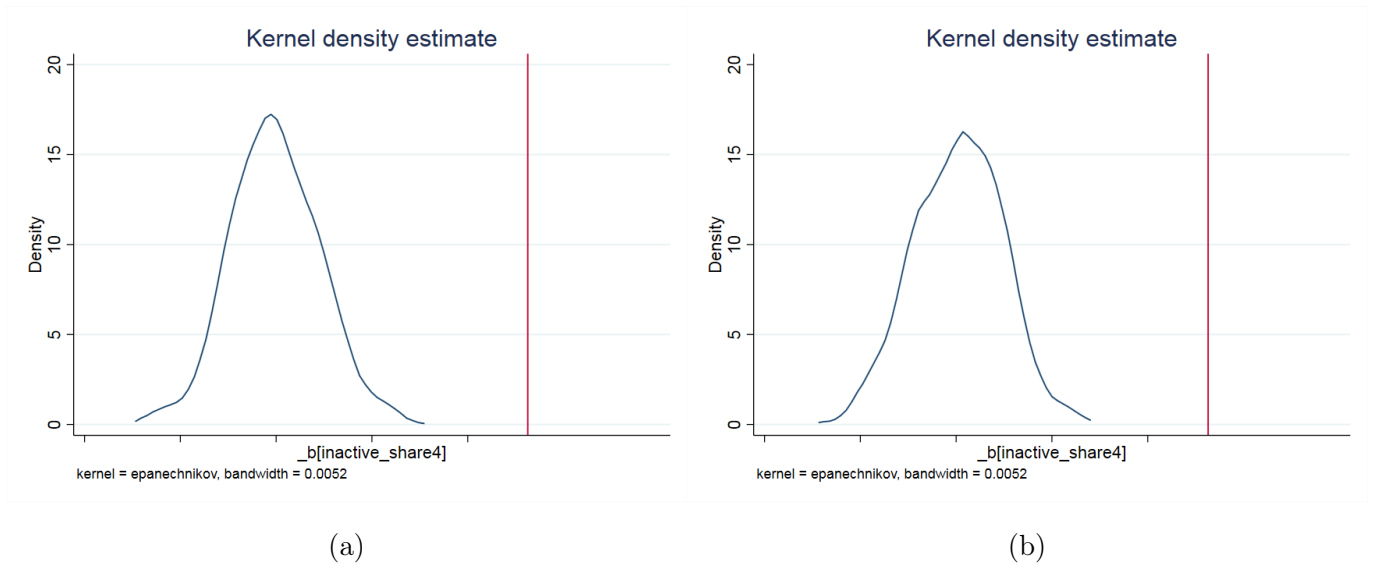


Figure F6: Randomization inference test with 1000 repetitions. The graph reports the density kernel for the coefficients estimated by randomly assigned the treatment to the observed units, modelling the empirical strategy as in Equation 1.1. The x-axis reports the estimated coefficients, the y-axis their density. The specification does not include interacted provincial FEs in (a), while it includes them in (b). The actual estimated baseline coefficient is the red line at the extreme right of the distribution.

1.16 Labor Market Areas

Impacts of Local Market Area-level work suspension on ARs (linear and non-linear estimates)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR
Panel A: Treatment at municipal level, SEs clustered at LMA level												
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_4)$	0.13134 *** [0.04862]	0.11340 *** [0.03783]	0.12879 *** [0.04878]	0.11133 *** [0.03794]	0.13168 ** [0.05419]	0.11087 *** [0.04142]	0.13978 ** [0.05449]	0.11324 *** [0.04169]	0.13676 ** [0.05461]	0.11082 *** [0.04178]	0.14560 ** [0.05762]	0.11515 *** [0.04386]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_3)$	0.05711 [0.04341]	0.04825 [0.03324]	0.05570 [0.04377]	0.04698 [0.03550]	0.06342 [0.04587]	0.05119 [0.03450]	0.06016 [0.04797]	0.04614 [0.03569]	0.05855 [0.04836]	0.04471 [0.03597]	0.07307 [0.04954]	0.05239 [0.03668]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_2)$	0.03486 [0.03874]	0.02961 [0.03022]	0.03505 [0.03879]	0.02949 [0.03028]	0.03353 [0.03960]	0.02714 [0.03066]	0.03709 [0.04280]	0.02896 [0.03220]	0.03769 [0.04284]	0.02919 [0.03226]	0.03967 [0.04247]	0.02714 [0.03216]
Panel B: Treatment at LMA level, SEs clustered at LMA level												
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_4)$	0.11716 *** [0.03876]	0.10023 *** [0.03233]	0.11368 *** [0.03864]	0.09756 *** [0.03226]	0.16362 *** [0.04492]	0.12432 *** [0.03757]	0.12117 *** [0.04501]	0.09695 *** [0.03682]	0.11632 *** [0.04467]	0.09328 ** [0.03058]	0.18155 *** [0.04974]	0.13264 *** [0.04091]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_3)$	0.03891 [0.03765]	0.03939 [0.03105]	0.03684 [0.03771]	0.03760 [0.03108]	0.03655 [0.03998]	0.02933 [0.03347]	0.03660 [0.04291]	0.03514 [0.03459]	0.03393 [0.04277]	0.03284 [0.03455]	0.04065 [0.04561]	0.03081 [0.03717]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_2)$	0.02931 [0.04500]	0.01995 [0.03683]	0.02870 [0.04477]	0.01925 [0.03668]	0.02503 [0.04229]	0.00985 [0.03434]	0.03901 [0.04911]	0.02550 [0.03911]	0.03788 [0.04868]	0.02445 [0.03882]	0.03718 [0.04573]	0.01803 [0.03692]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	116,192	116,192	116,192	116,192	116,192	116,192
R-squared	0.10041	0.10528	0.10044	0.10530	0.11224	0.11621						
Policy covariates			✓	✓	✓	✓			✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Provincial x Quarter_year FE					✓	✓					✓	✓
Weight		✓		✓		✓		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103	1.103	1.103	1.103	1.103	1.103	1.103

Table G1: The coefficients in Columns (1) to (12) of Panel A represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share LMA distribution on ARs. Those in Panel B represent the effect of belonging to different quartiles of the suspended workers' share LMA distribution on ARs. The reference category is the 1st quartile. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at LMA level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

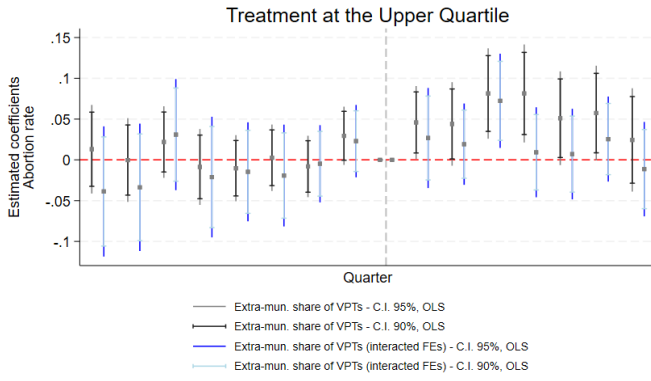
1.17 Abortion Mobility

Impacts of work suspension on ARs at the hospital level (linear)							Impacts of work suspension on ARs at the hospital level (non-linear)						
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR		(1) AR (ME)	(2) AR (ME)	(3) AR (ME)	(4) AR (ME)	(5) AR (ME)	(6) AR (ME)
<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	-1.34072 [1.13140]	-1.50525 * [0.87256]	-1.20354 [1.16186]	-1.39824 [0.88944]	-1.15342 [2.71143]	-1.67544 [2.21840]	<i>Post * 1(I. S._{q2/20} ∈ Q₄)</i>	-0.34996 [0.71949]	-0.34081 [0.58730]	-0.31330 [0.71348]	-0.31080 [0.58216]	-0.71250 [1.02622]	-0.76142 [0.84359]
<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.04345 [0.80849]	-0.23301 [0.54258]	-0.18356 [0.86028]	-0.12472 [0.57442]	0.78914 [2.75011]	0.25097 [2.22934]	<i>Post * 1(I. S._{q2/20} ∈ Q₃)</i>	0.27605 [0.52626]	0.18864 [0.45530]	0.29475 [0.52362]	0.20608 [0.45498]	-0.91513 [0.99737]	-0.79908 [0.82143]
<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.42521 [0.78781]	0.15314 [0.49816]	0.53469 [0.80517]	0.24105 [0.51111]	0.38287 [2.73009]	0.01008 [2.24476]	<i>Post * 1(I. S._{q2/20} ∈ Q₂)</i>	0.56802 [0.48842]	0.44409 [0.41542]	0.58490 [0.47835]	0.46441 [0.40827]	-0.22870 [0.89360]	-0.28396 [0.73754]
Observations	5,492	5,492	5,492	5,492	4,552	4,552	Observations	5,492	5,492	5,492	5,492	4,548	4,548
R-squared	0.87758	0.87388	0.87767	0.87394	0.88971	0.88212	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓	Hospital FE	✓	✓	✓	✓	✓	✓
Quarter_year FE	✓	✓	✓	✓	✓	✓	Quarter_year FE	✓	✓	✓	✓	✓	✓
LHA x Quarter_year FE					✓	✓	LHA x Quarter_year FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	9.640	9.640	9.640	9.640	9.640	9.640	Mean	9.640	9.640	9.640	9.640	9.640	9.640

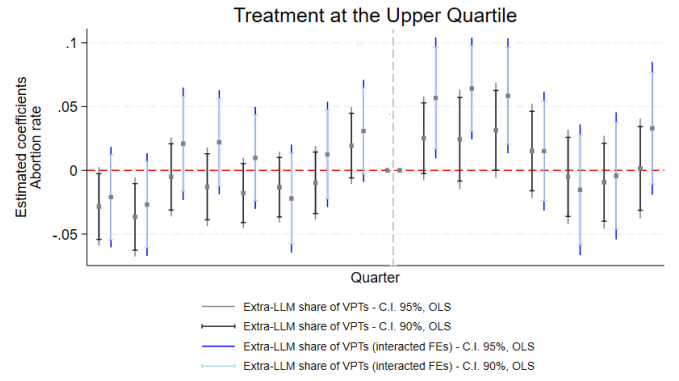
(a) OLS

(b) PPML

Figure H1: The coefficients in Columns (1) to (6) of both tables represent the hospital-level OLS (a) and the Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share municipal distribution on the ARs calculated at the hospital level. The reference category is the 1st quartile. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at hospital level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

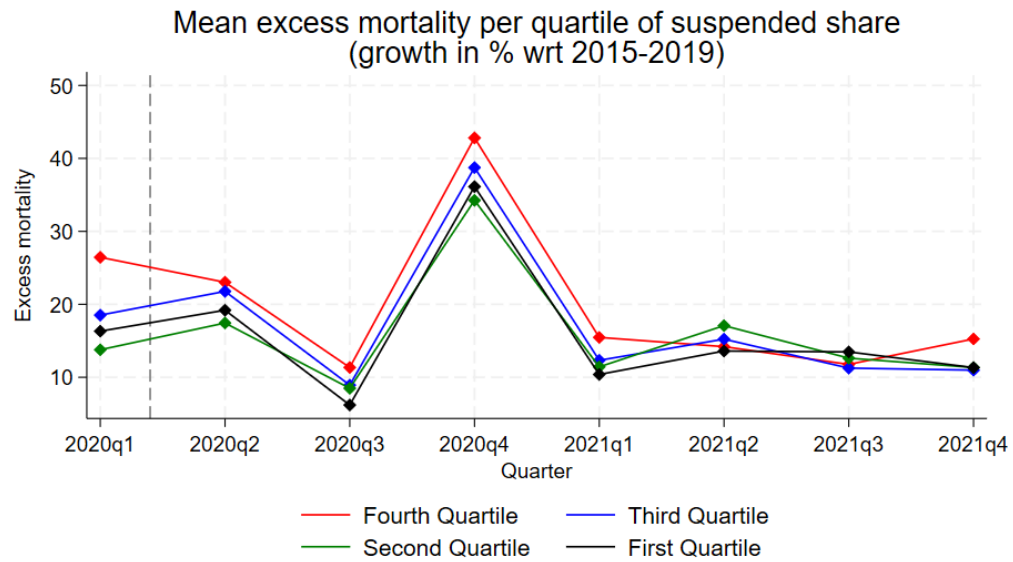


(a)

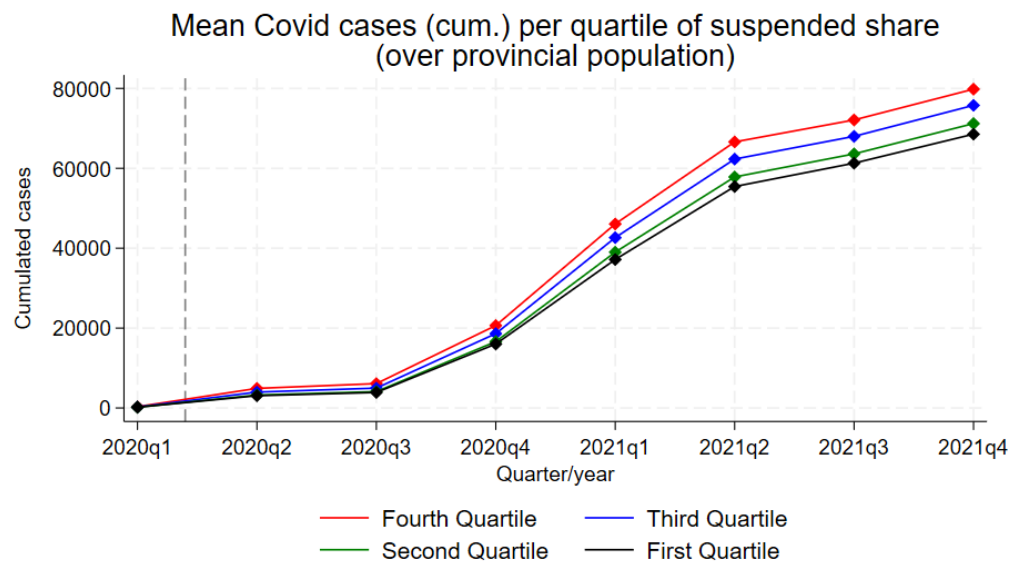


(b)

Figure H2: OLS Event-study estimates (dynamic specification of Equation 1.3) for the inter-municipal (a) and inter-LMA (b) abortion mobility analysis. The figure reports the coefficient on temporal units and their confidence intervals, both for the baseline specification and the one with interaction FEs between province dummies and quarterly dummies. Confidence intervals are reported at both 90% and 95%. The x-axis represents all quarters from 2018Q1 to 2021Q4. The vertical line is set on quarter 9, which corresponds to the first quarter of 2020, the first lead before the treatment, occurring on Q2 2020.



a)



b)

Figure H3: Quarterly municipal excess mortality (a) and Covid-19 provincial cumulated cases (b) per quartile of the inactive shares' distribution over 2020-2021.

	Descriptive statistics							
	Inter-municipal mobility				Inter-LMA mobility			
	Mean	SD	Min	Max	Mean	SD	Min	Max
<i>Abortion mobility</i>	0.233	0.423	0	1	0.249	0.432	0	1
<i>Number of previous live births</i>	1.100	1.157	0	20	1.125	1.156	0	30
<i>Number of previous stillbirths</i>	0.008	0.118	0	10	0.008	0.115	0	10
<i>Number of previous miscarriages</i>	0.191	0.534	0	11	0.192	0.534	0	11
<i>Number of previous VPTs</i>	0.393	0.809	0	20	0.360	0.762	0	20
<i>Gestational age (<90 days)</i>	0.961	0.195	0	1	0.959	0.198	0	1
<i>Gestational age (>90 days)</i>	0.039	0.195	0	1	0.041	0.198	0	1
<i>Weeks of amenorrhea</i>	8.588	2.824	3	26	8.612	2.851	3	26
<i>Urgent abortion</i>	0.244	0.429	0	1	0.242	0.428	0	1
<i>Non-urgent abortion</i>	0.756	0.429	0	1	0.758	0.428	0	1
<i>Presence of child malformations</i>	0.048	0.213	0	1	0.049	0.216	0	1
<i>Absence of child malformations or not indicated</i>	0.952	0.213	0	1	0.951	0.216	0	1
<i>Presence of complications</i>	0.029	0.167	0	1	0.028	0.166	0	1
<i>Medical abortion</i>	0.357	0.479	0	1	0.348	0.476	0	1
<i>Surgical abortion</i>	0.643	0.479	0	1	0.652	0.476	0	1
<i>Italian citizenship</i>	0.669	0.471	0	1	0.704	0.457	0	1
<i>Age</i>	30.787	7.356	10	60	30.910	7.363	10	60
Level of education								
<i>Elementary school</i>	0.051	0.221	0	1	0.044	0.206	0	1
<i>Middle school</i>	0.367	0.482	0	1	0.370	0.483	0	1
<i>High school</i>	0.430	0.495	0	1	0.448	0.497	0	1
<i>University degree or others</i>	0.151	0.358	0	1	0.138	0.345	0	1
Marital status								
<i>Single</i>	0.614	0.487	0	1	0.598	0.490	0	1
<i>Married</i>	0.344	0.475	0	1	0.358	0.480	0	1
<i>Separated</i>	0.017	0.127	0	1	0.017	0.130	0	1
<i>Widow</i>	0.026	0.158	0	1	0.027	0.162	0	1
Professional condition								
<i>Employed</i>	0.440	0.496	0	1	0.448	0.497	0	1
<i>Unemployed</i>	0.234	0.423	0	1	0.222	0.415	0	1
<i>Looking for first job</i>	0.017	0.128	0	1	0.017	0.130	0	1
<i>Housewife</i>	0.191	0.393	0	1	0.202	0.402	0	1
<i>Student</i>	0.112	0.315	0	1	0.107	0.309	0	1
<i>Other</i>	0.006	0.079	0	1	0.007	0.082	0	1
Professional branch of activity								
<i>Not in professional condition</i>	0.560	0.496	0	1	0.552	0.497	0	1
<i>Agriculture, hunting and fishing</i>	0.006	0.074	0	1	0.008	0.089	0	1
<i>Industry</i>	0.028	0.165	0	1	0.028	0.164	0	1
<i>Trade, services, hospitality (private)</i>	0.136	0.343	0	1	0.147	0.348	0	1
<i>Public administration</i>	0.042	0.200	0	1	0.041	0.198	0	1
<i>Other private services</i>	0.229	0.420	0	1	0.216	0.412	0	1
Obs.	97,550				173,791			

Table H1: Descriptive statistics of the variables used for the analysis of inter-municipal and inter-LMA mobility of abortions.

1.18 Socio-economic Information

Impact of work suspension on municipal ARs across both treatment and outcome sectors								
	Service Share				Industry Share			
	(1) AR not in prof. condition	(2) AR services	(3) AR industry	(4) AR P.A.	(5) AR t in prof. condition	(6) AR services	(7) AR industry	(8) AR P.A.
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$ (<i>services</i>)	0.00888 [0.03517]	-0.01529 [0.02728]	0.01282 [0.01148]	0.00827 [0.01118]				
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$ (<i>services</i>)	-0.01111 [0.03013]	-0.02795 [0.02217]	0.01076 [0.01027]	0.00897 [0.00729]				
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$ (<i>services</i>)	-0.03148 [0.02931]	-0.01314 [0.02264]	0.00714 [0.00957]	0.00893 [0.00718]				
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$ (<i>industry</i>)					0.07977 ** [0.03712]	0.04531 [0.02958]	-0.00052 [0.01460]	0.00780 [0.00917]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$ (<i>industry</i>)					0.03449 [0.03519]	0.00661 [0.02689]	-0.00830 [0.01157]	0.00150 [0.01012]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$ (<i>industry</i>)					0.05034 [0.03118]	-0.00635 [0.02338]	-0.01250 [0.01038]	0.00308 [0.00840]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10406	0.08887	0.07932	0.07678	0.10410	0.08890	0.07933	0.07678
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.572	0.410	0.0317	0.0507	0.512	0.361	0.0757	0.0363

Table I1: The coefficients in Columns (1) to (8) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the treatment distribution differentiated across sectors, and the outcome heterogeneized by the economic branch of activity where the aborting women are active. Columns 1-4 consider show the effect of the industrial inactive share. Columns 5-8 the service share. Columns 1 and 5 report the effect on the ARs of women out of the labor force. Columns 2 and 6 that on those active in the services. Columns 3 and 7 that on industrial workers. Columns 4 and 8 that on civil servants. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs, differentiating the outcome and the treatment across sectors (non-linear estimates)

	Industry Share				Service Share			
	(1) AR not in prof. condition	(2) AR services	(3) AR industry	(4) AR P.A.	(5) AR t in prof. condition	(6) AR services	(7) AR industry	(8) AR P.A.
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>services</i>)	0.00316 [0.04292]	-0.00961 [0.03474]	0.01335 [0.03339]	0.00681 [0.02680]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>services</i>)	-0.01117 [0.03472]	-0.02102 [0.02914]	0.04114 * [0.02401]	0.01872 [0.02135]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>services</i>)	-0.03788 [0.03450]	-0.00893 [0.03017]	0.03069 [0.02013]	0.01074 [0.02015]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>industry</i>)					0.10119 ** [0.04326]	0.05653 [0.03707]	0.03940 [0.03877]	0.01968 [0.02661]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>industry</i>)					0.03869 [0.03911]	0.00980 [0.03381]	0.01252 [0.03643]	0.02236 [0.02447]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>industry</i>)					0.05503 * [0.03343]	-0.01578 [0.02995]	-0.01671 [0.03898]	-0.00502 [0.02189]
Observations	105,104	97,719	36,465	42,136	105,104	97,719	36,465	42,136
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.572	0.410	0.0347	0.0507	0.512	0.361	0.0757	0.0363

Table I2: The coefficients in Columns (1) to (8) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the treatment distribution differentiated across sectors, and the outcome heterogeneized by the economic branch of activity where the aborting women are active. Columns 1-4 consider show the effect of the industrial inactive share. Columns 5-8 the service share. Columns 1 and 5 report the effect on the ARs of women out of the labor force. Columns 2 and 6 that on those active in the services. Columns 3 and 7 that on industrial workers. Columns 4 and 8 that on civil servants. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs - outcome across marital status; treatment across sectors (linear estimates)								
	Industry Share				Service Share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	single	married	separated	widow	single	married	separated	widow
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (services)	0.00107 [0.04030]	-0.02319 [0.02880]	0.00026 [0.00432]	0.00941 [0.00767]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (services)	-0.01561 [0.02999]	-0.02276 [0.02681]	0.00061 [0.00371]	-0.00599 [0.00553]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (services)	-0.02311 [0.03611]	-0.03385 [0.02929]	0.00303 [0.04846]	0.00048 [0.02652]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (industry)					0.08384 * [0.04353]	0.03899 [0.03038]	0.00360 [0.00470]	-0.00737 [0.00832]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (industry)					0.01386 [0.03960]	0.02836 [0.02784]	0.00009 [0.00434]	-0.01190 ** [0.00566]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (industry)					-0.00482 [0.03595]	0.01194 [0.02420]	0.00414 [0.00411]	-0.00311 [0.00470]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10013	0.09401	0.08803	0.07710	0.10019	0.09401	0.08803	0.07708
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.700	0.385	0.0295	0.0214	0.585	0.412	0.0284	0.0275

Table I3: The coefficients in Columns (1) to (8) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized across the marital status of the aborting women and the treatment across sectors. Coefficients in Columns 1-4 display the effect of service suspensions. Coefficients in Columns 5-8 those of industrial suspensions. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs - outcome across marital status; treatment across sectors (non-linear estimates)								
	Industry Share				Service Share			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	single	married	separated	widow	single	married	separated	widow
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>services</i>)	0.00666 [0.04582]	-0.05523 [0.03603]	-0.03730 [0.05273]	0.05348 ** [0.02684]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>services</i>)	-0.01534 [0.03577]	-0.03143 [0.03038]	-0.01379 [0.05832]	-0.01448 [0.02402]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>services</i>)	-0.02515 [0.03611]	-0.04631 [0.02929]	-0.05966 [0.04846]	0.00028 [0.02652]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>industry</i>)					0.09310 * [0.04874]	0.07597 ** [0.03778]	0.09492 * [0.05293]	-0.06138 ** [0.02603]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>industry</i>)					0.01029 [0.04297]	0.05643 * [0.03363]	-0.05002 [0.05082]	-0.06404 *** [0.02190]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>industry</i>)					-0.01201 [0.03904]	0.02515 [0.03039]	0.05996 [0.04146]	-0.02966 [0.01815]
Observations	108,608	98,096	17,105	29,934	108,608	98,096	17,105	29,934
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.700	0.385	0.0295	0.0214	0.585	0.412	0.0284	0.0275

Table I4: The coefficients in Columns (1) to (8) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized across the marital status of the aborting women and the treatment across sectors. Coefficients in Columns 1-4 display the effect of service suspensions. Coefficients in Columns 5-8 those of industrial suspensions. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impacts of work suspension on ARs, differentiating the outcome across age classes (linear/non-linear estimates)														
	(1) AR <14	(2) AR 15–19	(3) AR 20–24	(4) AR 25–29	(5) AR 30–34	(6) AR 35–39	(7) AR >40	(8) AR <14	(9) AR 15–19	(10) AR 20–24	(11) AR 25–29	(12) AR 30–34	(13) AR 35–39	(14) AR >40
$Post * \mathbb{1}(I, S_{q2/20} \in Q_4)$	0.00185 [0.00134]	0.01287 [0.01252]	0.02969 [0.02310]	0.03175 [0.02059]	-0.04616 [0.02697]	0.06145 [0.02411]	0.03969 [0.02105]	0.09436 [0.08622]	0.03178 [0.02470]	0.03520 [0.03358]	0.04410 [0.03035]	-0.05819 [0.03474]	0.08383 [0.03127]	0.06247 [0.02773]
$Post * \mathbb{1}(I, S_{q2/20} \in Q_3)$	0.00140 [0.00118]	-0.00001 [0.01002]	0.01308 [0.02026]	0.04123 [0.01831]	-0.03659 [0.02212]	0.01179 [0.01695]	0.03267 [0.01760]	0.05178 [0.05173]	0.00585 [0.02028]	0.01743 [0.03009]	0.06142 [0.02550]	-0.05023 [0.02783]	0.01934 [0.02406]	0.05139 [0.02284]
$Post * \mathbb{1}(I, S_{q2/20} \in Q_2)$	0.00001 [0.00113]	-0.01435 [0.00882]	0.00803 [0.01723]	0.03552 [0.01688]	-0.03641 [0.02085]	0.00652 [0.01643]	0.03422 [0.01574]	0.04099 [0.05545]	-0.02004 [0.01915]	0.01292 [0.02478]	0.04680 [0.02378]	-0.05009 [0.02560]	0.01282 [0.02272]	0.05424 [0.02089]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	2,039	55,156	80,005	85,144	89,676	87,596	74,520
R-squared	0.07374	0.07729	0.08903	0.08805	0.08292	0.08008	0.07885							
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter_year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province × Quarter_year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0861	0.0740	0.177	0.217	0.262	0.233	0.139	0861	0.0740	0.177	0.217	0.262	0.233	0.139

Table I5: The coefficients in Columns (1) to (14) represent the municipal OLS and Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized across age classes. Columns 1-7 refer to OLS estimates. Columns 8-14 to the MEs of Pseudo-Poisson Maximum Likelihood (PPML) estimates. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs, differentiating the outcome across educational attainment (linear/non-linear estimates)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	elementary	middle school	high school	university	elementary	middle school	high school	university
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	-0.00208	0.04775	0.04389	0.03345 *	-0.00816	0.06329	0.05145	0.05765 **
	[0.01005]	[0.03215]	[0.03426]	[0.01775]	[0.03454]	[0.03866]	[0.04016]	[0.02826]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.044	0.01889	0.01082	0.02707 *	0.00177	0.01877	0.01242	0.04824 **
	[0.00603]	[0.02557]	[0.03099]	[0.01449]	[0.02337]	[0.03029]	[0.03605]	[0.02341]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	-0.00331	0.01750	-0.00418	0.02277 *	-0.01383	0.02353	-0.00438	0.03541 *
	[0.00645]	[0.02297]	[0.02796]	[0.01326]	[0.02055]	[0.02741]	[0.03237]	[0.02081]
Observations	126,448	126,448	126,448	126,448	35,064	95,980	106,192	74,312
R-squared	0.10851	0.09152	0.08960	0.08042				
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter_year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province $\times$ Quarter_year FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.0467	0.376	0.520	0.135	0.0467	0.376	0.520	0.135

Table I6: The coefficients in Columns (1) to (8) represent the municipal OLS and Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized across educational attainments. Columns 1-4 refer to OLS estimates. Columns 5-8 to the MEs of Pseudo-Poisson Maximum Likelihood (PPML) estimates. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

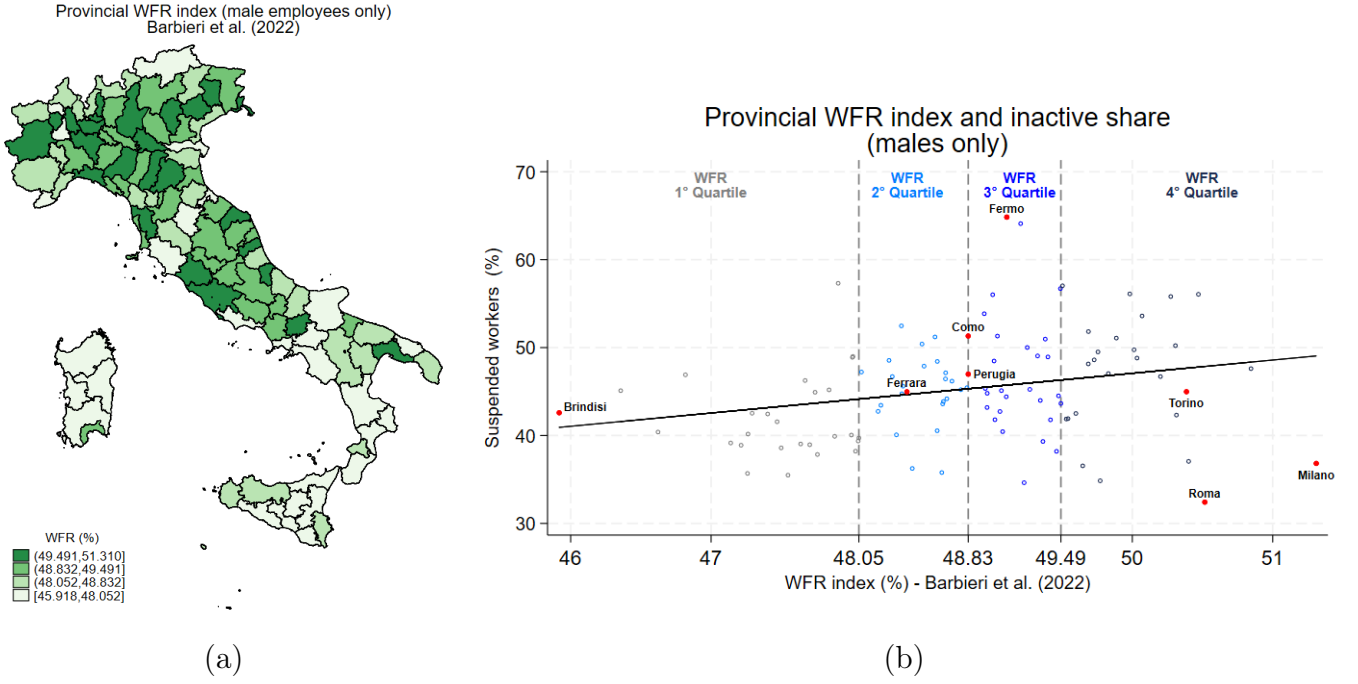


Figure I1: Map (a): provincial distribution of the weighted average of the *Working From Remote* sectoral index. Graph (b): suspended workers share over the *WFR* index for active males. The black line reports the fitted values from an unconditional regression of former share on the latter. The vertical dashed lines define the 1st, 2^o and 3rd quartile of the *WFR* distribution. Labeled provinces have the min., max. and median value of both variables. The *WFR* is built as conceived by Barbieri et al., 2022; the provincial index is then computed by weighing the sectoral values from Barbieri et al., 2022, by the provincial share of sectoral active males.

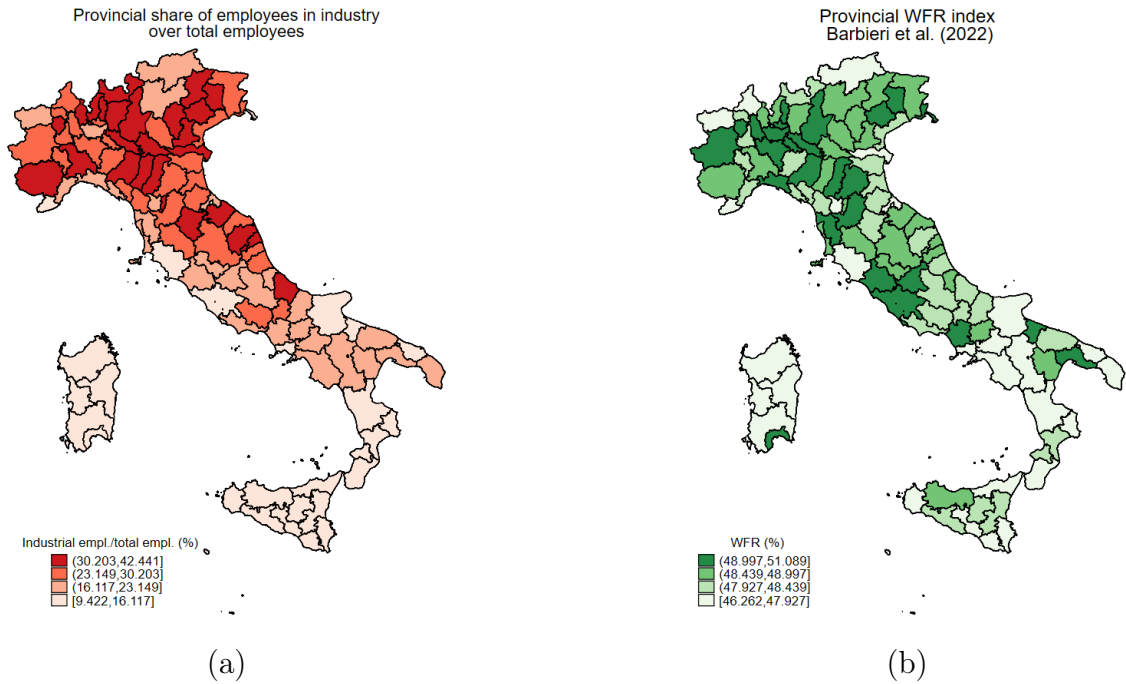


Figure I2: Industrial sector-related variables from *LFS* 2019. Map (a): provincial share of active workers in the private industrial sector. Map (b): provincial distribution of the weighted average of the *Working From Remote* sectoral index as conceived by Barbieri et al., 2022; the provincial index is computed by weighing the sectoral values from Barbieri et al., 2022, by the provincial share of sectoral active individuals.

Correlation between inactive share and labor market indicators					
	(1)	(2)	(3)	(4)	(5)
	I. S.	I. S.	I. S.	I. S.	I. S.
	(%)	(%)	(%)	(%)	(%)
(1) <i>WFR</i>	0.85864				
	[0.76011]				
(2) <i>WFR</i> (<i>males</i>)		1.50612			
		**			
		[0.59623]			
(3) <i>Ind. Sh.</i>			0.48438		

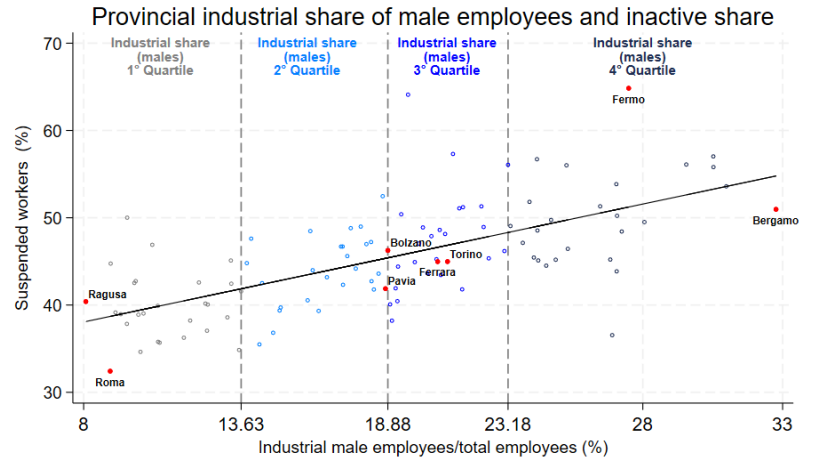
			[0.05660]		
(4) <i>Ind. Sh.</i> (<i>m/tot em.</i>)				0.67572	

				[0.07827]	
(5) <i>Ind. Sh.</i> (<i>m/m em.</i>)					0.37001

					[0.04299]
Obs.	107	107	107	107	107
R-squared	0.01481	0.06125	0.48876	0.43047	0.44668
Mean	45.27	45.27	45.27	45.27	45.27

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

(a)



(b)

Figure I3: Table (a): Table of regressions of the provincial inactive share (%) on 1) *WFR*; 2) *WFR (males)*; 3) *Ind. Sh.* (Industrial share) ; 4) *Ind. Sh. (m/tot. em.)* (Industrial share - males/total employees); 5) *Ind. Sh. (m/m em.)* (Industrial share (males/total male employees)). *Mean* reports the mean of the provincial inactive share in 2020. SEs are robustly estimated. Graph (b): suspended workers share over the provincial share of industrial active males on total active individuals. The black line reports the fitted values from an unconditional regression of the former share on the latter. The vertical dashed lines define the 1st, 2^o and 3rd quartile of the latter distribution. Labeled provinces have the min., max. and median value of both variables.

1.19 Fertility

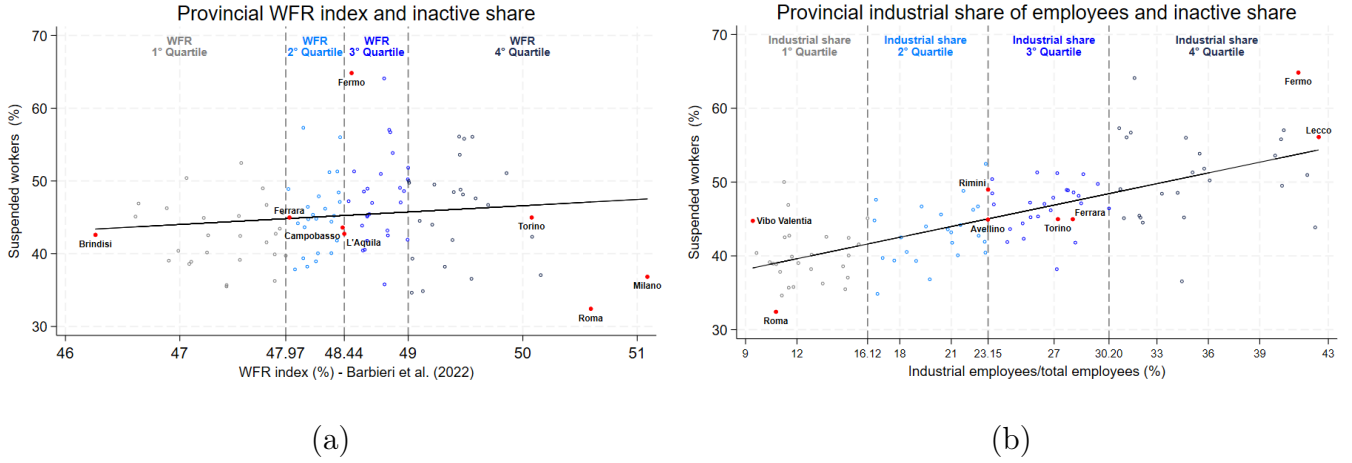


Figure I4: Graph (a): suspended workers share over the Barbieri et al., 2022's *WFR* index. Graph (b): suspended workers share over the provincial share of industrial active people on total active individuals. The black line reports the fitted values from an unconditional regression of the variable in the y-axis on that on the x-axis. The vertical dashed lines define the 1st, 2^o and 3rd quartile of the distribution of the x-axis variables. Labeled provinces have the min., max. and median value of both variables.

Impact of work suspension on ARs, differentiating the outcome according to past reprod. behaviors (non-linear)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	w/ prev. pregnancies	w/out prev. pregnancies	w/ prev. deliveries	w/ prev. abortions	w/ prev. live births	w/out prev. stillbirths	w/ prev. miscarriages	w/ prev. VPTs
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.13273 *** [0.04645]	0.2771 [0.03877]	0.12369 *** [0.04505]	0.06329 * [0.03698]	0.12274 *** [0.04481]	0.02151 [0.03845]	-0.01620 [0.03022]	0.07755 ** [0.03266]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.08275 ** [0.03889]	-0.00484 [0.03327]	0.08565 ** [0.03699]	0.01967 [0.03147]	0.08431 ** [0.03690]	-0.02675 [0.03254]	-0.00769 [0.02549]	0.01510 [0.02880]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.07003 * [0.03513]	-0.02193 [0.02974]	0.06365 ** [0.03390]	0.04790 * [0.02892]	0.06177 * [0.02282]	0.00298 [0.03003]	-0.01149 [0.02367]	0.04935 * [0.02574]
Observations	109,632	100,112	107,600	95,525	107,552	8,909	77,276	84,020
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.729	0.366	0.650	0.357	0.649	0.00672	0.166	0.227

Table J1: The coefficients in Columns (1) to (8) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to the past reproductive behavior of the aborting women. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on ARs, differentiating the outcome according to past reprod. behaviors, and treatment across sectors (linear)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	w/ prev. pregnancies	w/out prev. pregnancies	w/ prev. deliveries	w/ prev. abortions	w/ prev. pregnancies	w/out prev. pregnancies	w/ prev. deliveries	w/ prev. abortions
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4) (serv.)$	0.00760 [0.01270]	-0.00740 [0.00992]	0.00939 [0.01220]	0.00138 [0.00961]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3) (serv.)$	0.00420 [0.01078]	-0.01712 ** [0.00767]	0.00718 [0.01040]	-0.00038 [0.00799]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2) (serv.)$	-0.00253 [0.01069]	-0.01558 ** [0.00776]	0.00032 [0.01030]	-0.00221 [0.00792]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4) (ind.)$					0.02952 ** [0.01270]	0.00168 [0.00992]	0.03140 *** [0.01220]	0.01173 [0.00961]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3) (ind.)$					0.01859 [0.01240]	-0.00984 [0.00922]	0.01786 [0.01168]	0.00274 [0.00946]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2) (ind.)$					0.01047 [0.01144]	-0.00454 [0.00911]	0.01378 [0.01076]	0.00478 [0.00914]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.03397	0.02404	0.03188	0.03137	0.03399	0.02403	0.03190	0.03137
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Month–year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.245	0.138	0.216	0.125	0.242	0.119	0.217	0.118

Table J2: The coefficients in Columns (1) to (8) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to the past reproductive behavior of the aborting women, and the treatment across sectors. Columns 1-4 report the coefficients on the service suspensions. Columns 5-8 those on the industrial ones. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Impact of work suspension on ARs, differentiating the outcome according to past reprod. behaviors, and treatment across sectors (non-linear)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AR	AR	AR	AR	AR	AR	AR	AR
	w/ prev. pregnancies	w/out prev. pregnancies	w/ prev. deliveries	w/ prev. abortions	w/ prev. pregnancies	w/out prev. pregnancies	w/ prev. deliveries	w/ prev. abortions
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4) \text{ (services)}$	0.00810 [0.01512]	-0.00836 [0.01242]	0.00984 [0.01474]	0.00206 [0.01316]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3) \text{ (services)}$	0.00607 [0.01237]	-0.02173 [0.01015] **	0.00934 [0.01209]	0.00065 [0.01064]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2) \text{ (services)}$	-0.00286 [0.01243]	-0.02059 [0.01040] **	0.00027 [0.01212]	-0.00286 [0.01073]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4) \text{ (industry)}$					0.03367 ** [0.01405]	0.00090 [0.01187]	0.03705 *** [0.01349]	0.01407 [0.01252]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3) \text{ (industry)}$					0.02183 [0.01430]	-0.01306 [0.01136]	0.02187 [0.01362]	0.00285 [0.01235]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2) \text{ (industry)}$					0.01055 [0.01335]	-0.00633 [0.01119]	0.01511 [0.01270]	0.00466 [0.01205]
Observations	321,057	292,810	315,229	279,321	321,057	292,810	315,229	279,321
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Month–year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.245	0.138	0.216	0.125	0.242	0.119	0.217	0.118

Table J3: The coefficients in Columns (1) to (8) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to the past reproductive behavior of the aborting women, and the treatment across sectors. Columns 1-4 report the coefficients on the service suspensions. Columns 5-8 those on the industrial ones. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on ARs, differentiating the outcome according to the number of previous kids or abortions (linear/non-linear estimates)											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR	AR
	w/ no prev. live births	w/ 1 prev. live birth	w/ 2 prev. live births	w/ 3 prev. live births	w/ 4 prev. live births	w/ at least 5 live births	w/out prev. VPTs	w/ 1 prev. VPT	w/ 2 prev. VPTs	w/ 3 prev. VPTs	w/ at least 4 VPTs
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_4)$	0.04195 [0.04081]	0.04869 [0.03068]	0.09200 [0.03624]	0.02031 [0.02281]	-0.04016 [000]	008 [0.03769]	0.08889 [0.04875]	0.07133 [0.02850]	0.02992 [0.02747]	0.03649 [0.03551]	0.02478 [0.06687]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_3)$	0.04051 [0.03539]	0.03069 [0.02618]	0.05358 [0.02818]	0.04526 [0.01953]	0.06918 [000]	0.01153 [0.03769]	0.06112 [0.03756]	0.01321 [0.02258]	0.01946 [0.01967]	0.00722 [0.02191]	-0.02844 [0.04080]
$Post * \mathbb{1}(I. S_{-q2/20} \in Q_2)$	-0.00974 [0.03137]	0.04180 [0.02281]	0.04765 [0.02657]	-0.00539 [0.01871]	-0.02907 [000]	-0.06613 [0.03652]	0.00257 [0.03756]	0.05102 [0.02258]	0.00904 [0.01967]	-0.01681 [0.02191]	-0.02844 [0.04080]
Observations	103,632	86,862	90,721	61,719	25,289	13,717	113,968	78,815	39,447	14,225	11,664
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Mean	0.445	0.247	0.287	0.0901	0.0199	0.0140	0.867	0.178	0.0380	0.00733	0.0132
*** p<0.01, ** p<0.05, * p<0.1											

Table J4: The coefficients in Columns (1) to (11) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on ARs, with the outcome heterogeneized according to the number of previous children (Columns 1-6) or abortions (Column 7-11). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

1.20 Live births

Impact of work suspension on municipal rates of pregnancies resulting into live births (non-linear)						
	(1) Pregnancy rate	(2) Pregnancy rate	(3) Pregnancy rate	(4) Pregnancy rate	(5) Pregnancy rate	(6) Pregnancy rate
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_4)$	0.18648 [0.15421]	0.09564 [0.11545]	0.19157 [0.15473]	0.09772 [0.11593]	0.19198 [0.17103]	0.08489 [0.13174]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_3)$	0.07411 [0.12459]	0.05157 [0.09789]	0.07043 [0.12520]	0.04717 [0.09835]	0.05417 [0.13563]	0.02808 [0.10705]
$Post * \mathbb{1}(I. S_{.q2/20} \in Q_2)$	0.05540 [0.11916]	0.02771 [0.09300]	0.05419 [0.11930]	0.02651 [0.09318]	0.04106 [0.12193]	0.01579 [0.09547]
Observations	101,816	101,816	101,816	101,816	101,816	101,816
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Month–year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE					✓	✓
Weight		✓		✓		✓
Mean	7.722	7.722	7.722	7.722	7.722	7.722

Table K1: The coefficients in Columns (1) to (6) represent the municipal Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal rates of pregnancies turning into live births 9 months after, normalized by the municipal population in fertile age. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating births at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Placebo linear estimates: impact of work suspension on municipal pregnancy rates, at Q2 of the different years in the sample											
	(1) Pregn. rate	(2) Pregn. rate	(3) Pregn. rate	(4) Pregn. rate	(5) Pregnancy rate	(6) Pregn. rate	(7) Pregn. rate	(8) Pregn. rate	(9) Pregn. rate	(10) Pregn. rate	(11) Pregn. rate
$Post * \mathbb{1}(I. S_{q2/18} \in Q_4)$	0.14968 [0.23275]	0.27593 [0.24249]	0.15891 [0.23310]								
$Post * \mathbb{1}(I. S_{q2/19} \in Q_4)$				0.04109 [0.13925]	0.43332 * [0.22139]	0.06468 [0.15492]	0.48555 ** [0.20271]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$								0.33835 [0.25577]	0.23009 [0.15803]	0.29522 * [0.16219]	0.26974 [0.24524]
Observations	71,127	31,612	63,224	71,127	31,612	63,224	63,224	31,612	94,836	63,224	39,515
R-squared	0.16697	0.30120	0.17944	0.16697	0.27891	0.17944	0.16780	0.27321	0.13491	0.16778	0.23611
Policy covariates								✓	✓	✓	
Excess mortality							✓	✓	✓	✓	
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time-span	'18-'19, '21	'18	'18-'19	'18-'19, '21	'19	'18-'19	'19-'20	'20	'18-'20	'19-'20	'20-'21
Mean	8.113	8.113	8.113	7.759	7.454	7.759	7.454	7.640	7.722	7.631	7.640

Table K2: The coefficients in Columns (1) to (11) represent the OLS placebo estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the pregnancy rates, obtained by interacting the suspended workers share's distribution with Q2 of various years across the dataset, and at different sample sizes. Columns 1-3 consider 2018Q2 as the treatment event. Columns 4-7 2019Q2. Columns 8-11 2020Q2. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating births at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on municipal Abortion Ratios (linear estimates)							Impact of work suspension on municipal Abortion Ratios (non-linear estimates)						
	(1) Ab.ty ratio	(2) Ab.ty ratio	(3) Ab.ty ratio	(4) Ab.ty ratio	(5) Ab.ty ratio	(6) Ab.ty ratio		(1) Ab.ty ratio	(2) Ab.ty ratio	(3) Ab.ty ratio	(4) Ab.ty ratio	(5) Ab.ty ratio	(6) Ab.ty ratio
$Post * 1(I. S_{q2/20} \in Q_4)$	9.80620 ** [4.38194]	10.11692 ** [4.33384]	9.98114 ** [4.38197]	10.31095 ** [4.33737]	6.23252 [4.81508]	6.83577 [4.80299]	$Post * 1(I. S_{q2/20} \in Q_4)$	11.06531 * [6.03860]	10.26344 * [5.63701]	11.20935 * [6.04102]	10.42181 * [5.64457]	6.88156 [6.71017]	6.81692 [6.28474]
$Post * 1(I. S_{q2/20} \in Q_3)$	0.37257 [4.41271]	0.65708 [4.26418]	0.58223 [4.41333]	0.87678 [4.26832]	-1.49803 [4.58851]	-1.28786 [4.45510]	$Post * 1(I. S_{q2/20} \in Q_3)$	3.22136 [5.56678]	2.21137 [5.13370]	3.39440 [5.57498]	2.40031 [5.14596]	0.34136 [5.81824]	-0.27634 [5.37480]
$Post * 1(I. S_{q2/20} \in Q_2)$	-3.21523 [4.32421]	-1.94473 [4.15086]	-3.11239 [4.32240]	-1.85594 [4.15039]	-4.34428 [4.40992]	-3.23510 [4.22969]	$Post * 1(I. S_{q2/20} \in Q_2)$	0.06919 [0.11916]	0.29399 [0.09300]	0.23400 [0.11930]	0.46207 [0.09318]	-2.09225 [0.12193]	-1.56822 [0.09547]
Observations	102,739	102,739	102,739	102,739	102,739	102,739	Observations	86,216	86,216	86,216	86,216	86,216	86,216
R-squared	0.15676	0.15528	0.15679	0.15533	0.17062	0.16935	R-squared						
Policy covariates			✓	✓	✓	✓	Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓	Month-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE					✓	✓	Prov. × Quarterly FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	117.9	117.9	117.9	117.9	117.9	117.9	Mean	117.9	117.9	117.9	117.9	117.9	117.9

(a) OLS

(b) PPML

(a) OLS

(b) PPML

Figure K1: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal abortion ratios. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on municipal pregnancy rates, with the outcome differentiated across marital status (all)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pregn. rate	Pregn. rate	Pregn. rate	Pregn. rate	Pregn. rate	Pregn. rate	Pregn. rate	Pregn. rate
	(Single)	(Married or civ. union)	(Divorced or separ.)	(Widow)	(Single)	(Married or civ. union)	(Divorced or separ.)	(Widow)
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.02728 [0.07717]	-0.00948 [0.09427]	0.01379 [0.01178]	0.00449 [0.00229] *	0.03302 [0.07913]	-0.00340 [0.09309]	0.03525 [0.02414]	0.07689 [0.03169] **
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	-0.01102 [0.08329]	0.02078 [0.10440]	0.02121 [0.01235] *	0.00286 [0.00342]	-0.00469 [0.08366]	0.02426 [0.10483]	0.05365 [0.02572] **	0.03324 [0.04003]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.17972 [0.11375]	-0.00920 [0.12561]	0.00444 [0.01608]	-0.00039 [0.00261]	0.16801 [0.10398]	-0.00531 [0.13006]	0.01671 [0.03168]	-0.02350 [0.04980]
Observations	102,739	102,739	102,739	102,739	99,073	100,646	53,295	7,761
Method	OLS	OLS	OLS	OLS	PPML	PPML	PPML	PPML
R-squared	0.12967	0.14508	0.09098	0.08720				
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Month–year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	2.814	4.563	0.138	0.00815	2.814	4.563	0.138	0.00815

Table K3: The coefficients in Columns (1) to (8) represent the municipal OLS and Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal rates of pregnancies turning into live births 9 months after, normalized by the municipal population in fertile age. The outcome is differentiated across the marital status of mothers. Columns (1) to (4) refer to OLS estimates. Columns (5) to (8) to the MEs of the Pseudo-Poisson Maximum Likelihood (PPML). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating births at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impacts of work suspension on pregnancy rates, differentiating the outcome by marital status and the treatment by sector								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pregn. rate (single)	Pregn. rate (married or civ. un.)	Pregn. rate (divor. or sep.)	Pregn. rate (widow)	Pregn. rate (single)	Pregn. rate (married or civ. un.)	Pregn. rate (divor. or sep.)	Pregn. rate (widow)
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>services</i>)	0.13308 [0.11315]	0.27145 ** [0.11902]	-0.02234 [0.01722]	0.00308 [0.00340]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>services</i>)	0.06007 [0.07897]	0.15189 [0.09768]	0.00930 [0.01392]	-0.00026 [0.00314]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>services</i>)	0.01078 [0.07624]	0.24291 [0.09539]	-0.00146 [0.01406]	0.00276 [0.00278]				
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$ (<i>industry</i>)					-0.01129 [0.12338]	-0.05927 [0.12956]	0.03041 * [0.01629]	0.00001 [0.00263]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$ (<i>industry</i>)					-0.04940 [0.11442]	-0.01671 [0.11609]	0.02447 * [0.01407]	0.00216 [0.00299]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$ (<i>industry</i>)					-0.02661 [0.10511]	-0.05316 [0.10823]	0.01628 [0.01229]	0.00246 [0.00251]
Observations	102,739	102,739	102,739	102,739	102,739	102,739	102,739	102,739
R-squared	0.12964	0.14516	0.09100	0.08719	0.12962	0.14508	0.09099	0.08718
Policy covariates	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓	✓	✓
Mean	2.712	1.467	0.121	0.00868	2.177	1.806	0.138	0.00862

Table K4: The coefficients in Columns (1) to (8) represent the municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal rates of pregnancies turning into live births 9 months after, normalized by the municipal population in fertile age. Columns (1) to (4) report the coefficients on the suspended workers in the service sectors. Columns (5) to (8) refer to the industrial sector. The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on municipal pregnancy rates - differentiated according to minors in the family (all)						
	(1)	(2)	(3)	(4)	(5)	(6)
	Pregn. rate (1 minor)	Pregn. rate (no minors)	Pregn. rate (> 1 minor)	Pregn. rate (1 minor)	Pregn. rate (no minors)	Pregn. rate (> 1 minor)
$Post * \mathbb{1}(I. S_{q2/20} \in Q_4)$	0.08043 [0.07219]	-0.03657 [0.08965]	-0.02557 [0.04105]	0.08527 [0.07312]	-0.03071 [0.08872]	-0.02786 [0.04615]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_3)$	0.08673 [0.07804]	-0.10174 [0.09648]	0.03708 [0.04430]	0.08836 [0.07934]	-0.09353 [0.09501]	0.04328 [0.04962]
$Post * \mathbb{1}(I. S_{q2/20} \in Q_2)$	0.04504 [0.09393]	0.12412 [0.13213]	0.00417 [0.05715]	0.04748 [0.09768]	0.11070 [0.12317]	0.00609 [0.06291]
Observations	102,739	102,739	102,739	99,086	100,581	91,247
Method	OLS	OLS	OLS	PPML	PPML	PPML
R-squared	0.10669	0.10427	0.13050			
Policy covariates	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Month–year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE	✓	✓	✓	✓	✓	✓
Mean	2.813	3.595	1.166	2.813	3.595	1.166

Table K5: The coefficients in Columns (1) to (6) represent the municipal OLS and Marginal Effects of the PPML estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the municipal rates of pregnancies turning into live births 9 months after, normalized by the municipal population in fertile age. The outcome is differentiated across the number of children already present in the head of the family's household. Columns (1) to (3) refer to OLS estimates. Columns (4) to (6) to the MEs of the Pseudo-Poisson Maximum Likelihood (PPML). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating births at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** p<0.01, ** p<0.05, * p<0.1

1.21 Contraception

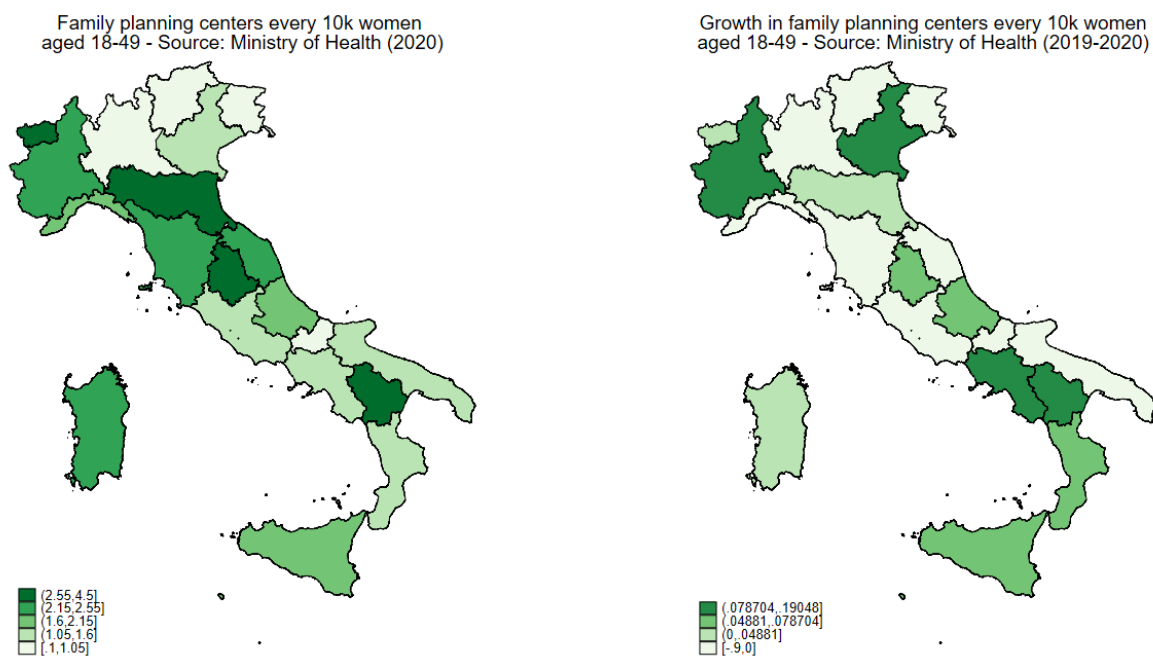


Figure L1: Family planning centres in Italy in 2020. Left panel: number of family planning centres every 10,000 women aged between 18 and 49 in 2020. Right panel: growth in family planning centres every 10,000 women aged between 18 and 49 between 2019 and 2020. Source: Ministry of Health

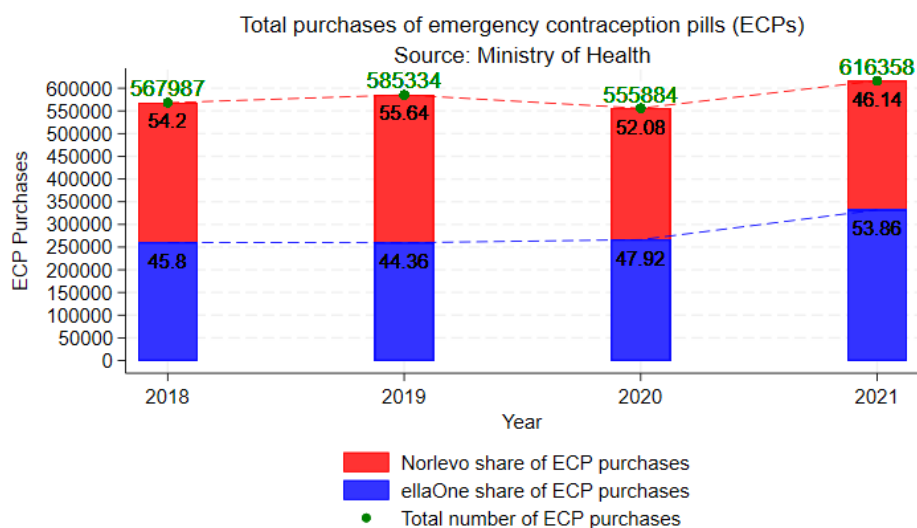


Figure L2: ECP sales in Italy between 2018 and 2021. Source: Ministry of Health.

Impact of work suspension $\times$ Post 2020Q3 on municipal ARs of minors (linear estimates)							Impact of work suspension $\times$ Post 2020Q3 on municipal ARs of minors (non-linear estimates)						
	(1)	(2)	(3)	(4)	(5)	(6)		(1)	(2)	(3)	(4)	(5)	(6)
	AR	AR	AR	AR	AR	AR		AR	AR	AR	AR	AR	AR
	(min.)	(min.)	(min.)	(min.)	(min.)	(min.)		(minors)	(minors)	(minors)	(minors)	(minors)	(minors)
$Post * 1(I. S_{q2/20} \in Q_4)$	0.00882	0.00513	0.00864	0.00498	0.01316	0.00848	$Post * 1(I. S_{q2/20} \in Q_4)$	0.03421	0.01806	0.03430	0.01787	0.05223 *	0.03286
	[0.00918]	[0.00655]	[0.00904]	[0.00648]	[0.01091]	[0.00776]		[0.03219]	[0.02180]	[0.03187]	[0.02162]	[0.02964]	[0.0000]
$Post * 1(I. S_{q2/20} \in Q_3)$	0.00636	0.00400	0.00630	0.00393	0.00933	0.00615	$Post * 1(I. S_{q2/20} \in Q_3)$	0.02138	0.01196	0.02072	0.01130	0.01326	0.00847
	[0.00692]	[0.00535]	[0.00690]	[0.00534]	[0.00755]	[0.00583]		[0.02315]	[0.01613]	[0.02329]	[0.01618]	[0.02432]	[0.0000]
$Post * 1(I. S_{q2/20} \in Q_2)$	-0.00347	-0.00357	-0.00344	-0.00360	-0.00326	-0.00370	$Post * 1(I. S_{q2/20} \in Q_2)$	-0.01500	-0.01307	-0.01480	-0.01315	-0.01683	-0.01471
	[0.00443]	[0.00380]	[0.00443]	[0.00380]	[0.00467]	[0.00393]		[0.01949]	[0.01376]	[0.01947]	[0.01376]	[0.02091]	[0.0000]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	Observations	32,208	32,208	32,208	32,208	29,024	29,024
R-squared	0.06461	0.06524	0.06462	0.06525	0.07395	0.07377	Policy covariates			✓	✓	✓	✓
Policy covariates			✓		✓	✓	R-squared						
Municipal FE	✓	✓	✓	✓	✓	✓	Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. $\times$ Quarterly FE					✓	✓	Prov. $\times$ Quarterly FE					✓	✓
Weight		✓		✓		✓	Weight		✓		✓		✓
Mean	1.098	1.098	1.098	1.098	1.098	1.098	Mean	1.098	1.098	1.098	1.098	1.098	1.098

(a) OLS

(b)

Figure L3: The coefficients in Columns (1) to (6) of both tables represent the municipal OLS (a) and Marginal Effects of the PPML (b) estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution, interacted with a dummy $Post_t = 1$ if $t \geq 2020Q3$ (when ECP was made available OTC for minors) and 0 otherwise, on the municipal ARs of underage women. Table (a) refer to OLS estimates. Table (b) to the MEs of the Pseudo-Poisson Maximum Likelihood (PPML). The policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area is the reference) and municipal excess mortality. The panel is built by aggregating abortions at the municipality of residence level. The SEs are clustered at municipal level. *Mean* reports the mean of the treated units pre-treatment. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Chapter 2

Monetary incentives versus public funding in healthcare research: what matters the most?

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Abstract. We study the impact of two policy interventions on scientific research productivity in a major private Italian hospital. The first is a performance-based monetary incentive program (a Management-By-Objectives, or MBO, bonus) introduced by the hospital management to reward non-academic physicians for publishing research. The second is the hospital’s recognition as a Scientific Institute for Recovery and Research (IRCCS), which allowed its academic medical researchers to access dedicated public research funding. Using detailed panel data on physicians’ publications from 2012 to 2022, we employ several difference-in-differences strategies to evaluate each policy’s effect. We find that the introduction of monetary incentives did not lead to any significant impact on research output of the previously less research-active (non-academic) physicians, unless they were both treated by the MBO policy and had also access to IRCCS funding (“double-treated”). The IRCCS recognition caused instead a major boost in the publication rates of academic doctors and Medical Directors affected by both policies. We also assess the impact of such policies on the research quality of the hospital, by accounting for citations. We document increased cross-collaboration between the monetary-incentivized groups, indicating the emergence of knowledge spillovers; however, such increase was quite substantial for the non-academic doctors, while being modest for structured researchers in relative terms. Our findings might be able to inform the design of policies to incentivize research in healthcare organizations, highlighting the lower significance of performance-based incentives in absence of adequate research funding means.

Keywords: Health Systems, Medical Research, Incentives, Management-By-Objective, Public Funding.

JEL Classification: I12, I18, J13, J16.

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2.1 Introduction

How should healthcare institutions incentivize physicians to engage in research? This question recently became relevant in light of the rising costs of healthcare systems, the demographic transition, and the tightening of public research budgets. In this paper we provide empirical evidence on two different approaches to foster research in healthcare: performance-based monetary bonuses (i.e., feeding individual incentives), and government funding allocations (i.e., addressing infrastructure and resources). We exploit the setting of a large private Italian teaching hospital, where both policies were subsequently implemented. In 2017 the hospital introduced a performance-based Management-By-Objectives (MBO) scheme, financially rewarding non-academic physicians for publishing papers. After that, in early 2018, the hospital was granted the IRCCS status (Scientific Research Hospital designation), thus allowing for additional public funding, mostly benefiting academic doctors. Through such combination we aim at comparing an explicit individual monetary reward to a wide institutional infrastructural intervention within the same organization and timespan.

The study can be framed within the general debate on the transformation of research funding. In the U.S., proposed budget cuts for 2025–26 have raised strong concerns among scientists, as federal agencies like the NIH and NSF have been facing impairment in their resources (Peel et al., 2025; Garisto, 2025; Terhune, 2025; Malakoff, 2025). On the other side, it is widely reckoned how private actors, like philanthropic associations and venture capital firms, increasingly stepped in (Glenza, 2025; UNUMLux, 2025; Shekhtman et al., 2024). This context has been marking a shift in funding, further away from the public institutions. The European research environment is all but alien to such issues: after Brexit, the UK experienced a decline in scientific spending (Simpkin and Mossialos, 2017; Highman et al., 2023; Gregory, 2025), whereas France and Germany saw the beginning of a debate concerning how to balance excellence, equity and independence in the presence of fiscal constraints and philanthropic actors (de Bengy Puyvallée et al., 2025; Marchandot and Morel, 2025). Such dynamics highlight a relevant change in science management, with a remarkable fading of the boundaries between private and public incentives.

Healthcare is particularly salient for this debate. Aging populations bring about greater utilization of healthcare (Tang et al., 2022), and also trigger major repercussions on economic growth and market conditions (Acemoglu and Restrepo, 2017; Aksoy et al., 2019). Healthcare cost utilization are also led by the growing prevalence of chronic illnesses and comorbidities (D. E. Bloom et al., 2020; Hacker, 2024, Ye et al., 2023), whereas the COVID-19 shock have boosted pressure on health systems, calling for additional resilience to epidemic (S. Chen et al., 2021, D. E. Bloom et al., 2022, Wu and Wang, 2024). Innovation, research and their diffusion are major elements for healthcare sustainability, quality of care and long-term efficiency, but they require strong incentives and, often scarce, resource. While the short-term impact of new treatments on efficiency is not to be taken for granted (Grant and Buxton, 2018), as innovative health technologies may initially raise costs (Chandra and Skinner, 2012), a steady and consistent research activity is expected to enhance the quality of care, by improving the long-run health outcomes of the affected population, fostering

prevention, and triggering cost savings in a lengthened perspective.

At the hospital level, research-active institutions may not only improve clinical standards, depending on innovation diffusion and translationality (Barrenho et al., 2021, 2025), but also attract high-quality professionals (AMS, 2020; N. Bloom, Propper, et al., 2015). Such institutes could also develop comparative advantages, ultimately favoring patient outcomes (N. Bloom, Propper, et al., 2015, N. Bloom et al., 2020, Ghandour et al., 2022). In academic environments, physicians and researchers are inherently motivated to publish by career oriented motives (tenure, promotions, prestige, Checchi et al., 2021), as well as altruistic reasons or due to scientific curiosity (Rousseau et al., 2021). Yet, outside academia, such *academic incentives* are missing, physicians are more oriented towards clinical task and other activities, thus facing weaker career incentives to conduct research. While this may avoid short-term concerns about research tasks potentially crowding out quality of care, this also dampens the effectiveness of translating research innovations into clinical practices and immediate interlinks between academia and patient care, therefore raising the policy question of how to effectively address the topic.

Beyond contributing to the thick debate on hospital management and incentive schemes in imperfect markets, as the one of education (Muralidharan and Sundararaman, 2011, N. Bloom, Lemos, et al., 2015) and healthcare (N. Bloom, Propper, et al., 2015, N. Bloom et al., 2020, Goodall, 2011), our study aims at shedding some light on whether performance-based pay schemes can effectively stimulate research productivity among non-academic physicians in comparable settings, and to what extent may institutional public funding generate greater or diverse effects. We also try to explore potential spillovers between groups of physicians—whether incentives targeting one group indirectly affect the research output and collaborations of the other.

Our work contributes to three strands of literature. First, we relate to the economics of management and incentives in public services (Goodall, 2011, Dal Bó et al., 2013, Burgess et al., 2017), by evaluating a hospital-level policy that rewards research rather than clinical performance. As a matter of fact, there is a wide literature strand on fiscal incentives to physicians’ activity, although focused on care tasks only (Gruber and Owings, 1996, Shurtz, 2013, 2014, Molitor, 2018, Bertoli and Grembi, 2019, Brosig-Koch et al., 2024). Second, we contribute to the literature on research funding and scientific productivity, assessing how public funds versus private incentives shape outputs in medical research. Such literature involves mixed findings, as some scholars display how additional funding often correlates with higher research productivity, even causally (Azoulay et al., 2011, Benavente et al., 2012, Ganguli, 2017, Baruffaldi et al., 2020, Babina et al., 2023, Ghirelli et al., 2023); by contrast, others find weak or non-existent effects of grants on individual publications and productivity (Jacob and Lefgren, 2011, Banal-Estañol et al., 2023). Third, we engage with the literature on knowledge spillovers and peer effects in publications (Azoulay et al., 2010, Colussi, 2018, Bosquet et al., 2022), exploring whether incentives applied to one group of physicians trigger collaborative responses across groups.

Previewing our results, we find that the MBO scheme did not significantly increase research productivity among non-academic physicians, whereas the IRCCS recognition and associated public funds boosted

the publication output of academic doctors by about $\approx 60\%$. The combination of the two policies appears cumulative, as non-academic physicians included in IRCCS funding are the only responding positively (and sizeably) to the MBO scheme. We also document that 1) such effects are feasibly driven mostly by funding allocated to operational and ordinary research activities; 2) that there is null increase in collaborative publications across groups (highlighting the relevance of within-group collaboration instead); 3) that there is an ambiguous effect on papers' citation, with academics increasing the bulks of their citations due to higher productivity, but with the MBO-eligible individuals showing a positive effect of citations per paper (the opposite displayed for the academics); this suggests that, although not focusing on raising productivity, non-academic physicians may value quality over quantity, as we cannot document the presence of gaming behaviors.

The remainder of the paper is structured as follows. Section 2 describes the institutional framework and the two policies. Section 3 presents data sources and sample construction. Section 4 presents dynamic estimates of both schemes, while Section 5 focuses on the impact of the IRCCS recognition. Section 6 reports robustness checks. Potential mechanisms are discussed in Section 7, while Section 8 treats the impact on networks, spillovers and research quality. Section 8 concludes.

2.2 Institutional Framework

The study is set in a leading private hospital in Rome, Italy, affiliated with a prestigious private Italian university. The hospital is a major healthcare hub which offers both clinical services and medical education. It is also renowned for research: in recent years it has ranked among the top hospitals in Italy for research output. As a matter of fact, it has been consistently listed among the top 50 hospitals worldwide over the recent years, and more than 75 of its affiliated researchers are included in the top 2% ranking of scientists globally (according to Standord's standardized citation metrics). These characteristics make it an ideal environment for the studying of policies aimed at boosting research activity.

In 2017–2018, the hospital underwent two significant research-related policy changes. First, in 2017 (in anticipation of a forthcoming evaluation for obtaining research status), the hospital management implemented a performance-based MBO policy to encourage publication by hospital physicians who did not have university affiliations ("non-academic" physicians). This MBO program (which has been active every year since 2017) has offered monetary bonuses for publications: each physician would receive a payout for each peer-reviewed journal article published in a given year, with the amount of the payment being proportional to the journal's Impact Factor. Specifically, for every Impact Factor point of a published article (as indexed by the Web of Science website), the physician earned €500, up to a maximum bonus of €10,000 per year (equivalent to 20 points). Thus, publishing in higher-impact journals yielded larger rewards. The total annual budget allocated to the MBO bonus fund was €1 million. The bonus is also weighted according to whom participates to the publication: first-authors of papers get the whole computed bonus if there are not

internal collaborators in the publication (100%), and 60% of it if they co-author with internal affiliates. Non-first author researchers who co-author with external colleagues only get 50% of the calculated reward, which goes down to 40% if the publication involves at least another internal affiliated member. A comprehensive scheme of the rewarding policy is presented in Table A1, in the Appendix. Only physicians employed as medical doctors (clinicians) without a university faculty affiliation were eligible for this scheme. Academic faculty physicians were excluded from MBO by definition. The goal of this program was to foster research activity among practitioners who traditionally focused on patient care and had lower research output. Second, in February 2018, the hospital achieved the status of IRCCS (*Istituto di Ricovero e Cura a Carattere Scientifico*, or Scientific Institute for Research and Healthcare). IRCCS is a special designation conferred by the Italian Ministry of Health to institutes that excel in biomedical and healthcare service-related research while also delivering high-quality healthcare. The designation process is rigorous and explicit by the law: after preparing the process over the previous years, the hospital had to submit documentation in early 2017 to the regional government to receive approval; once documentation was approved in August 2017, it underwent expert evaluations and on-site visits by the Ministry late in 2017. Upon recognition in 2018, the hospital was officially accredited as an IRCCS in two specialty research areas (Personalized Medicine and Innovative Biotechnologies). Even though IRCCS are an Italian particularity, there are several international comparable institutions that may be associated in features to IRCCS, as they too are nationally accredited to perform research. Institutes like IHU (*Instituts hospitalo-universitaires*, France), *Universitätskliniken* (Germany), AMC (*Academic Medical Centers*, U.S.), IIS (*Institutos de Investigación Sanitaria*, Spain) are alike to IRCCSs, albeit regulated to a lesser extent.

As an IRCCS, the hospital under matter gains access to dedicated public research funds – notably an annual “Current Research” fund available exclusively to IRCCS institutions, and preferential direct channels to compete for “Targeted Research” grants (which, otherwise, are granted to other hospitals only by means of being appointed by their Regions of reference, upon being awarded the funds themselves in public tenders). The IRCCS status also carries obligations: the hospital must maintain high research standards, and it is subject to occasional re-evaluation by experts. The quality standards foreseen by the government in order to obtain the IRCCS recognition are presented in Table A2.

Concurrent with IRCCS recognition, the hospital management compiled an official list of researchers (the IRCCS research staff “perimeter”) who would be eligible to use the newly available public research funds (updated every year)¹. This IRCCS staff list consisted mainly of academically affiliated physicians (university-employed doctors working at the hospital), but it was also made by some non-academic hospital physicians who were active in research. Importantly, all academic physicians, whether included in the

¹As discussed later in the paper, the IRCCS list is updated every year. However, the updating is a formal recognition of individuals already involved in the IRCCS’ activities due to their productivity and research activity, even without having official access to the funds until they are acknowledged by the Ministry as formally taking part to the research group. In reality, it would be more accurate to consider such group of individuals as featured by a time-varying size pattern. We take this into account when we discuss the validity of our approach.

IRCCS list or not, were not part of the MBO bonus program (as noted above), and were appointed by the university. Although such appointment was of course based on preemptive characteristics, it was not a matter of direct self-selection. Meanwhile, non-academic physicians on the IRCCS list were still eligible for the MBO bonus (because they were hospital-employed clinicians). In other words, there was a subset of “double-treated” individuals: a few non-academic doctors who qualified for the IRCCS research staff list (thus benefiting from the IRCCS public funding) while also being eligible for the MBO monetary incentive. We summarize the groups and timing of these interventions below. In 2017 (the introduction of MBO), the relevant physician groups can be categorized as follows: 0 = those with no MBO and who will not be included in the IRCCS perimeter (pure control group), 1 = those who eventually only benefit from IRCCS funding (academic physicians not eligible for MBO), 2 = those who receive the MBO incentive only (non-academic physicians not included in IRCCS list), and 3 = those who receive both MBO and later IRCCS (non-academic physicians who were included in the IRCCS research staff list). In 2018 (the IRCCS recognition), we can instead categorize individuals as: 0 = neither IRCCS nor MBO (pure control), 1 = MBO-only, 2 = IRCCS-only, and 3 = both IRCCS and MBO. In the empirical analysis we leverage such categorizations to define treatment and control groups for each policy.

Code	MBO	IRCCS	Physician type	Policy exposure
0	No	No	Academic physicians <i>not in IRCCS list</i>	Full control: * no monetary incentives * no public funding
1	Yes	No	Medical Directors [No IRCCS] - Non-academic physicians <i>not in IRCCS list</i>	* Eligible for monetary incentive * No access to IRCCS public funding
2	No	Yes	Academic physicians <i>in IRCCS list</i>	* Access to IRCCS public funding * Not eligible for monetary incentives
3	Yes	Yes	Medical Directors [No IRCCS] - Non-academic physicians <i>in IRCCS list</i>	Exposure to both (<i>double-treated</i>): * Eligible for monetary incentives * Access to IRCCS public funding

Notes: The Management-By-Objectives (MBO) scheme was introduced in 2017 and provides performance-based monetary bonuses for publications to non-academic physicians only. IRCCS recognition in 2018 granted access to dedicated public research funding to physicians included in the official IRCCS research staff list, updated annually. Academic physicians are never eligible for MBO incentives by institutional design. Treatment codes are used to define difference-in-differences comparisons relative to the introduction of each policy.

Table 2.1: Classification of physicians by type and exposure to monetary incentives (MBO) and public research funding (IRCCS).

2.3 Data

We construct a panel dataset of the professionals working in the hospitals, joint with their research output spanning 2012 to 2022. The core personnel data come from the hospital’s administrative records, which include all individuals structurally employed in a professional capacity (physicians, researchers, and other healthcare staff management) from 2017 onward. From that, we identify our population of interest: physicians and medical researchers continuously employed at the hospital between 2017 and 2022. For each person, the HR data provide demographic information (age, gender, place of birth), employment details (job title or role, department and unit, contract type, and hiring date), and indicate whether the individual has an academic affiliation (university-employed physician) or is a hospital-only employed clinician. There were not available data on medical interns, residents, PhD students, and external collaborators, who are therefore excluded by definition. Because the administrative records begin in 2017, we supplemented them by benchmarking the administrative information on the starting dates with hire dates and career details from online CVs, institutional websites, LinkedIn pages and the official website of Italian physicians (FNOMCeO), enabling us to infer each physician’s presence at the hospital in earlier years. Using the hire date information, we retrospectively extend the panel back to 2012 for all individuals who were active within the hospital between 2012 and 2017. We resort to this choice as we are not being able to identify workers who were active before 2017 and left the hospital before that year, hence not allowing us to have any information about their career. Thus, in order not to build an unbalanced panel with attrition focused only on the post-treatment part of the sample, in our main analyses we keep the units who are balanced for the whole longitudinal dataset only. However, for the sake of robustness, we also constructed an unbalanced panel by collecting online information². The resulting balanced panel covers 584 physicians and researchers, each observed annually from 2012 through 2022, yielding 6,424 person-year observations. This balanced structure ensures that we can track research output for a fixed cohort over time, including several years before and after two policy changes. We create indicators for each person’s group status (medical directors vs. faculty members, MBO vs. non-MBO, IRCCS list member vs. not, etc.) which remain fixed for the analysis in the main empirical

²Retrieving online information was necessary to construct the unbalanced panel. As a matter of fact, the attrition in the panel after 2017 was easily obtainable by observing which professionals were yearly absent or present in the official records provided by the institution. But to trace their affiliation before 2017, while for most of them the hire date was an available information, for those no longer or not yet employed in either 2020 or 2023, such variable was missing. We hence collected online CVs, LinkedIn self-reported information, or data from institutional websites to reconstruct the affiliation. In some cases, especially for the oldest doctors, no CV or online info was available. We thence looked at their profile on the official online portal of Italian physicians (*FNOMCeO*), which reports date and place of birth, and the dates and places of their medical degree, their habilitation as physicians, their registration in the local medical books, and (if they did it), of their medical residency. We assume that MDs completing their last residency in the university affiliated to the hospital under study, or getting their habilitation immediately after completing their degree in said university if they did not specialize themselves, could be considered as enrolled in the institutional ranks that same year, as for most of our attrition subjects these events were reported happening decades before the starting year of our panel. In the few cases of inconsistency (no official CV information online, no LinkedIn profiles, a degree/residency completed in an other university or hospital), we sought for additional online information via scraping from Italian websites for reviewing physicians (*TopDoctors*, *IlMioDottore*, etc.) or online articles. The source material of this operation has been stored and may be made available upon request.

strategy, reflecting their initial category. Some individuals (3.4%) switch position in the time series under consideration (e.g., a physician became an academic or vice-versa during the period); we handle such cases in robustness checks by excluding switchers. In addition, some individuals are aggregated to the IRCCS perimeter with one or few years' lag compared to the initial timing (2018). While this would make the setting more adequate to a staggered adoption analysis, in the main estimates we considered such *ever-treated* individuals as treated from the start, due to their preemptive involvement with research activity and with the official IRCCS-research group, thus considering them as informally treated individuals expecting formal recognition at some point. We later corroborate the validity of such design by keeping in the sample only those who receive the IRCCS-status from the beginning, and by performing some Staggered DiD estimates as well, confirming the robustness of our main estimates.

To measure research output, we gathered data on scientific publications from Clarivate's Web of Science database. We retrieved all publications (articles, reviews, etc.) from 2012 to 2022 for the authors in our panel (and also on those in the unbalanced dataset). We accurately crafted such process by ensuring the recovered authors were the ones affiliated to the hospital, by matching names and surnames, fields of research, and by double checking we were not dealing with homonyms. This removes the portion of sampling errors that other studies, like Jacob and Lefgren, 2011, have to account for due to their matching based on researchers' surnames only. Each publication record provides the publication year, journal, number of co-authors, citations (as of the time of data collection, which is Fall 2024), and some other bibliometric details. We then matched authorship on these papers to the individuals in our hospital staff panel by name. From this, we computed the number of publications per person per year, which is our main outcome variable. In addition, we recorded the total citations each person's publications received (to evaluate research impact).

Table 2.2 reports the summary statistics for the selected sample. About 52% of the 584 researchers in the panel are academic physicians (as in, they hold a university faculty appointment while also performing clinical activity), while the remainder are non-academic hospital physicians or other research/healthcare staff. In terms of treatment groups, it is clear how the units differentiated by status (IRCCS-only, MBO-only, double-treated) do not sum up to the total number of individuals in the sample: this is due to the switching units, which are taken care of in the robustness checks. On average, academic physicians in the IRCCS perimeter have higher scientific output than non-academics, reflecting different incentive structure and attitudes towards research. The average number of publications per person per year is around 5 versus less than 0.46 publication for non-IRCCS, non-academics. Their joint subset is instead in the middle way with respect to prolificacy, reporting almost 2.3 average yearly publications. The pure control group, which includes academic physicians and other professionals non included in either policy, presents a slightly higher mean annual publication number than doctors eligible for MBO only (0.85). If we look at the times the works published in a given year were reportedly cited in Fall 2024, the period when data were collected, the statistics follow, intuitively, the same pattern. The variability of the reported variables is quite substantial in all groups. With respect to the size of the groups, the individuals ever being officially included into the

IRCCS research staff list starting from 2018, and not belonging to the non-academic MD category, are 198 individuals (approximately 34% of the sample). Within the IRCCS group, 69 individuals are non-academic physicians (thus eligible for MBO as well). The non-academic non-IRCCS physicians are instead 178 (30.5% of the sample), while the pure control group is made by 162 individuals (27.7%).

	Mean	SD	Min	Max	N
<i>Sample composition</i>					
Non-IRCCS listed Medical Directors (MBO-treated)					178
IRCCS-listed academics (IRCCS-only)					198
IRCCS-listed Medical Directors (double-treated)					69
Non-IRCCS listed academics (pure control)					162
Total units of the balanced panel					584
<i>Workers' statistics</i>					
Age	51.127	7.857	31	70	
Publications	2.186	4.564	0	102	
Times cited (WoS)	57.833	204.140	0	7037	
Times cited (All outlets)	61.463	217.740	0	7509	
Female	0.420	0.494	0	1	
Male	0.580	0.494	0	1	
Medical Director (hospital only)	0.409	0.492	0	1	
Healthcare Professions' Manager	0.018	0.134	0	1	
Sanitary Director	0.050	0.218	0	1	
Faculty Member with Clinical Functions	0.523	0.500	0	1	
<i>Publications' statistics</i>					
Publications of non-IRCCS listed Medical Directors (MBO-treated)	0.461	1.054	0	12	
Publications of IRCCS-listed academics (IRCCS-only)	4.929	6.880	0	102	
Publications of IRCCS-listed Medical Directors (double-treated)	2.258	2.768	0	20	
Publications of non-IRCCS listed academics (pure control)	0.848	1.683	0	23	
Citations of non-IRCCS listed Medical Directors (MBO-treated)	8.537	33.919	0	1009	
Citations of IRCCS-listed academics (IRCCS-only)	148.799	356.584	0	7509	
Citations of IRCCS-listed Medical Directors (double-treated)	66.691	144.069	0	2102	
Citations of non-IRCCS listed academics (pure control)	15.540	37.684	0	485	
Observations	6424				

Table 2.2: Descriptives

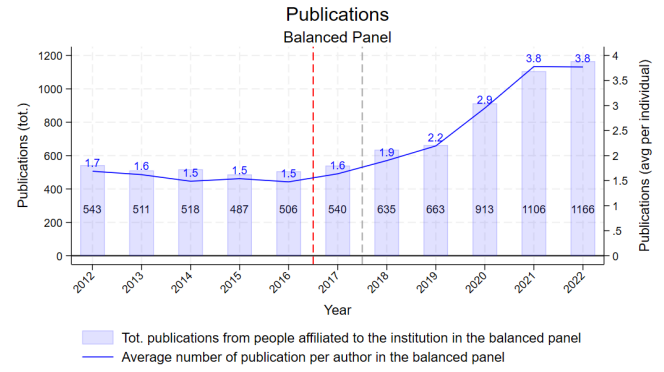
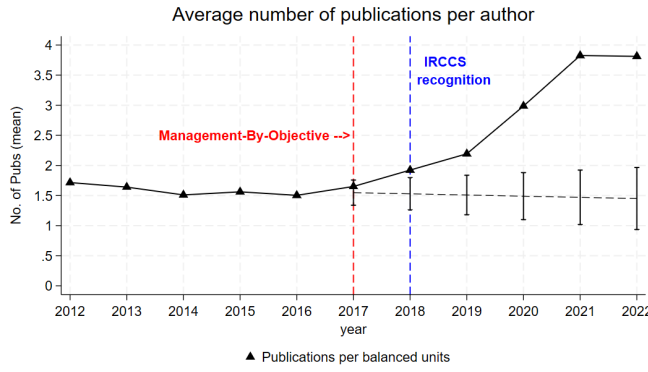
Graphs (a), (b), and (c) in Figure 2.1 plot the average number of publications per physician counted at the end of each year, from 2012 to 2022. In Graphs (a) and (b) we track the individuals in the balanced panel only, while in Graph (c) we compare the average trends of the balanced panel with several types of datasets with attrition, by removing from the panel all the individuals who are not counted for an increasing number of years. We observe a relatively flat trend in the years up until 2017, followed by a noticeable increase starting around 2018. This pattern is basically invariant in the comparing of the balanced panel with the various possibility of attrition-flawed datasets the we can build with the available information (Graph (c)). The timing also aligns with the IRCCS recognition, suggesting, at a first glance, a possible aggregate effect

of these policies on the overall research productivity of the hospital, mediating by the supposed lagged effect that would involve the execution of a scientific work before achieving publication.

It must be noted that the time-lapse between submission to acceptance/publication are usually quite short in the medical field compared to other fields, like the economic one. Whereas to publish in the best ranked economic journals the lag has been longer than 2 years for decades, even reaching more than a 40 months-lag from submission to publications for papers ranked in the upper quantiles of the distribution (Yohe, 1980, Hadavand et al., 2024), such time-lapse is on average 8 months or less in the medical field, lowering to even a couple-of-months time length for systematic reviews or literature reports (T.-A. Chen et al., 2024). In addition, a top-journal in the medical field like *JAMA Network Open*, states in its official address to authors that the median time from submission to publication is 94 days, i.e. 3 months (JAMA, 2025).

On the other side, two 2017-published papers with two citations each as in October 2024 (if using the WoS citation metrics), are “Reverse Time-Dependent Effect Of Alphafetoprotein And Disease Control On Survival Of Patients With Barcelona Clinic Liver Cancer Stage C Hepatocellular Carcinoma” (Ponziani et al., 2017, *World Journal of Hepatology*, first received in August 2017 and published in December 2017) and “The chromosome analysis of the miscarriage tissue. Miscarried embryo/fetal crown rump length (CRL) measurement: A practical use” (D’Ippolito et al., 2017, *PLoS One*, first received in March 2017 and published in June 2017). They display similar if not even shorter publication lags. In addition to that, one can even argue that the effect observed in a timely fashion with respect to the two policy implementations may reflect the fact that some *pipeline projects*, already on-going before 2017, may have been strategically accelerated or finalized in order to obtain the bonus or enabled by the IRCCS additional funds.

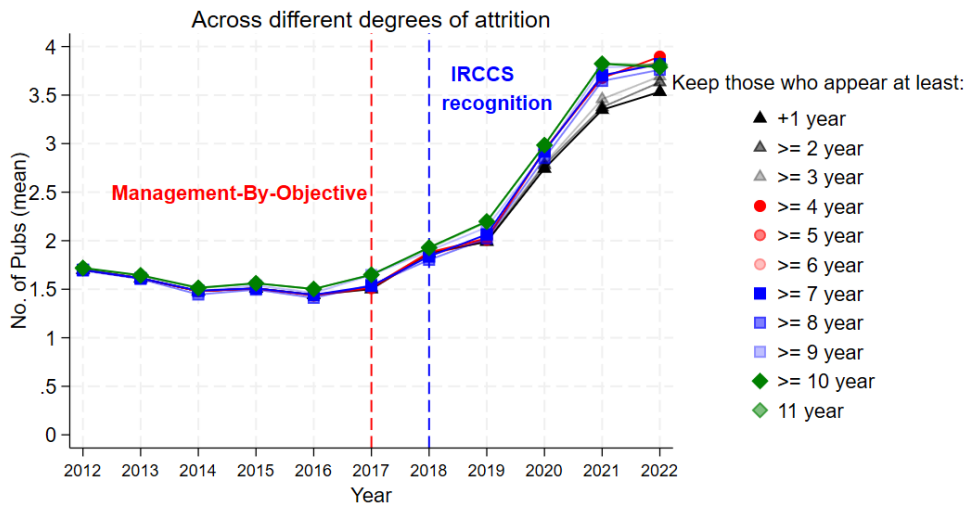
While other secular trends could also contribute to the increase (e.g., growing institutional emphasis on research), the positive, diverging pattern beginning in 2018 is quite suggestive. In the analysis below, we exploit the individual-level variation in exposure to the policies to identify their causal impact.



a) Average publications per researcher per year (2012–2022)
- Balanced Panel.

b) Total number of publications attributed to the employees of the institution for the balanced sample (2012–2022).

Average number of publications per author (unbalanced)



c) Average publications per researcher per year (2012–2022) - Unbalanced Panel. Each line represents the mean trend observed by analyzing the dataset after having removed from the panel all the units who are not reported in the data for an increasing number of years (from 0, as in, the full dataset, to 11, which is the strongly balanced panel).

Figure 2.1: Research productivity: average trend and total output (2012–2022).

In defining treatment and control groups for the analysis, we use the categorizations described in Section 2. For the MBO policy, “the treated” group consists of non-academic physicians (those subject to the MBO bonus) and the control group consists of those not eligible (academic physicians and other staff). For the IRCCS policy, the treated group consists of those included in the IRCCS researcher list (primarily academic physicians) and the control group is made by those not on the list. Additionally, we will examine subgroups such as the double-treated individuals. The next sections outline our empirical strategies in detail.

A sketch of the average evolution of publication patterns across groups is presented in Figure 2.2. A first visual inspection seems to suggest us that the bulk in the jump of yearly publications is led by researchers affected by the IRCCS recognition, with apparently no impact brought about by the performance-based policy, as the only MBO-subject individuals who evidently display a trigger in productivity overall, are the ones who are double-treated.

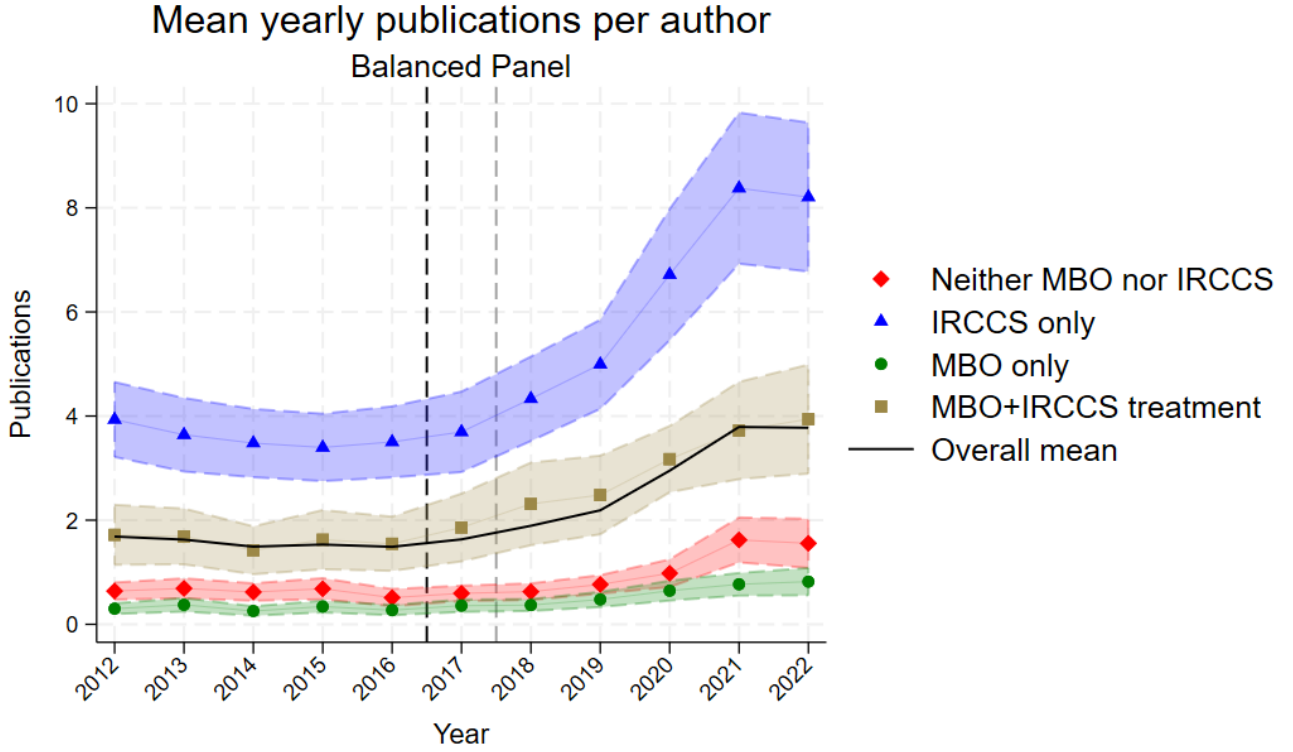


Figure 2.2: Average yearly publications across different groups of healthcare professionals, defined according their treatment status after 2017 and 2018.

2.4 Dynamic Estimates

2.4.1 Empirical Strategy

Although the policies are two different interventions, they are chronologically sub-sequent and share the same scope, so we assess them together. To observe which policy results to be more effective, we perform dynamic event-studies estimates stemming from a standard TWFE DiD framework. The first policy is the performance-based monetary incentive (MBO) implemented in 2017. The IRCCS recognition, which triggered an allocation of greater funds for research, began in 2018 and is the other policy of interest. Such event mainly affected the hospital's academic-affiliated researchers, allowing them to receive dedicated public resources and, in addition, by enhancing the prestige of the institution under the scientific point of view.

We draw from Table ?? to identifying the comparison groups, by blending into a unique differentiation scheme. We therefore have:

1. **Group 0:** *Full control*, as in, academic physicians, sanitary directors, sanitary professions' directors and other research staff officially affiliated to the hospital and/or the university, but who are not part of the IRCCS perimeter group. None of them is eligible to the MBO scheme either, as all Medical Directors (non-academic physicians) are excluded by this number;

2. **Group 1:** *MBO-only treated.* These are all the non-academic physicians who are not embedded in the IRCCS research staff, thus they benefit from the incentive but have no direct access to the additional public funds;
3. **Group 2:** *IRCCS-only treated.* As in, all academic individuals (faculty members with clinical functions, not Medical Directors) who are included in the IRCCS perimeter. Thus, they have direct access to dedicated funding, but are not rewarded explicitly due to their performance (no MBO);
4. **Group 3:** *Double-treated.* These are the non-academic physicians, hence benefitting from the MBO payments, who are also selected into the IRCCS staff by the hospital management, either because of their research merits or other reasons. Hence, they are rewarded for their productivity in addition to have straight access to IRCCS activities and resources.

We employ a TWFE difference-in-differences (DiD) dynamic framework, by comparing the change in publications across this different groups of individuals within the same model specification, as individuated by an indicator measuring the intensity of the treatment. Our category of reference is the full control group. Relative to time, we are interested into looking at the impact on publications before and after the introduction of the two policies, which occur in 2017 and 2018. Since the main analysis is dynamic, we do not have a time-invariant post-treatment framework where we compare the outcome before and after a fixed time threshold, like in the standard diff-in-diff, as we have two policy events. By contrast, we keep the year immediately preceding both policies (2016) and observe the evolution of the estimates in the years following the implementation of the policy changes.

We estimate the following equation:

$$Y_{it} = \alpha + \sum_{j \in \{1,2,3\}} \left\{ \sum_{h=2}^H \beta_{j,h} \mathbb{1}(G_i = j) \mathbb{1}(t = t_0 - h) + \sum_{g=1}^K \gamma_{j,g} \mathbb{1}(G_i = j) \mathbb{1}(t = t_0 + g) \right\} + \delta_i + \tau_t + \varepsilon_{it}. \quad (2.1)$$

where Y_{it} is the research output of individual i in year t . $G_i \in \{0, 1, 2, 3\}$ indicates the comparison group to which the unit i belongs, with $j = 0$ being the reference category (full control). $j = 1$ is the MBO-only group, $j = 2$ the IRCCS-only group, and $j = 3$ the double-treated one. The binary indicator $\mathbb{1}(G_i = j)$ takes then unitary value if i belongs to a given group of treatment, and 0 otherwise. By contrast, the temporal pre-period dummy $\mathbb{1}(t = t_0 - h)$ indicates whether the t is a year preceding the policy, thus it equals 1 if $t < t_0$, and 0 otherwise. The interaction between the two dichotomous variables yields a further binary indicator which relates the treatment status to the pre-treatment periods, i.e., the *Lags*. The same, but on the opposite direction, holds for $\mathbb{1}(t = t_0 + g)$, which are dummy indicators for the post-treatment time periods, associated as well with the variables for the treatment status (the *Leads*). To avoid multicollinearity issues, we normalized the estimates by omitting $t = 2016$, as in the first pre-policy period, which is accounted as the reference temporal category. Stemming from the latter, $h \in \{2, \dots, H\}$, while $g \in \{1, \dots, K\}$. Note

that $t_0 = 2017$, even though the IRCCS-intervention begins in 2018: this does not matter in the context of the analysis, as the estimates are normalized with respect to the same pre-treatment period, which is 2016. δ_i are individual fixed effects, τ_t are year fixed effects. By including δ_i , we control for the time-invariant differences between physicians, while τ_t for common shocks in each period. Standard errors are clustered at the individual level. These element yield coefficients for the differential trends across the different comparison groups in each year ($\beta_{j,h}$ and $\gamma_{j,g}$), which we plot to visually inspect pre-trends and dynamic effects. We present these event-study results as figures.

The key identification assumption is that, absent the two policies, the research output of the various treated groups and the control units would have followed parallel trends. The event-study evidence enables to assess whether the different groups exhibited similar trends prior to 2017 (and 2017 itself, for the IRCCS-treated individuals).

2.4.2 Results

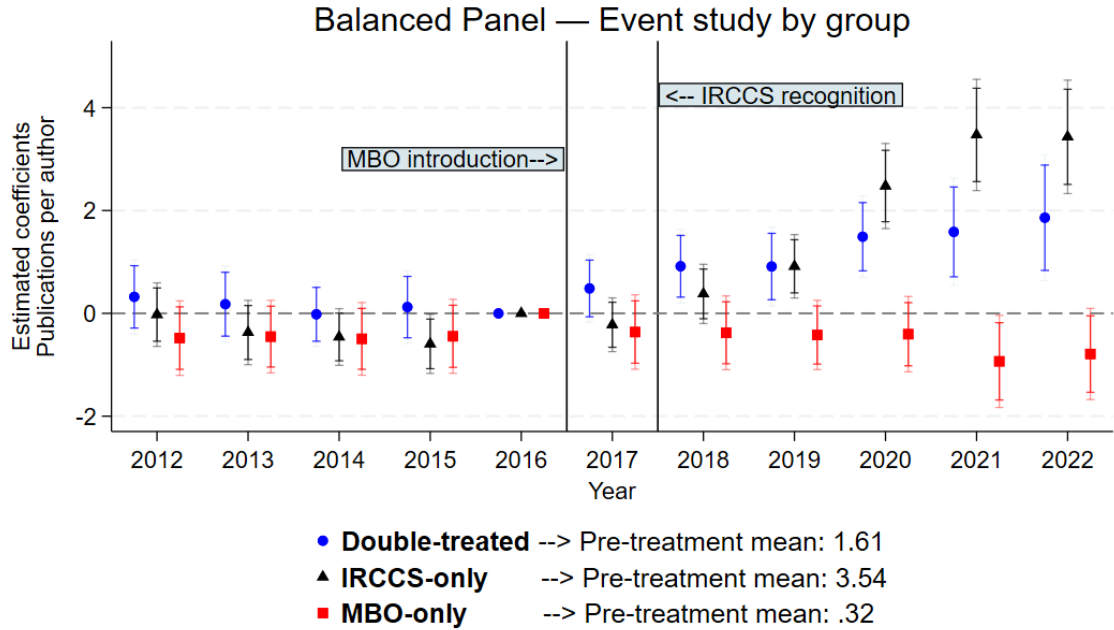


Figure 2.3: Event-study of the MBO joint with IRCCS recognition policy effect on publications across the different groups, and with respect to the full control group - Balanced panel.

Graph 2.3 presents the dynamic estimates of the effect of the two policies on publications. The graph reports the coefficients estimated through the Equation 2.1, distinguishing across the different comparison groups with respect to the reference category (e.g., full control). Overall, we observe a positive and statistically significant effect of the MBO policy only when the recipients are also part of the IRCCS perimeter (double-treated, i.e. DT). In addition to that, the effect seems to be kicking up only after 2018, which is the year of the IRCCS recognition. The most prominent impact is observed with respect to academic doctors who are also part of the IRCCS perimeter, for whom the effect takes off in 2019, one year after

the IRCCS recognition, when they increase their outcome by about 1 publication (+28% circa compared to the pre-treatment mean), on par with the increased absolute outcome of the double-treated individuals, although the relative magnitude of the increase in 2019, for the latter, is greater (+62%). Starting from 2020, in correspondence of the pandemic outbreak, the productivity of academic doctors jump up substantially, overtaking that of the double-treated unit and peaking in 2021, where they register almost a 100% increase. By contrast, the MBO-scheme alone seems to be associated with a slightly negative effect when not paired with the additional funding, although such an effect is not statistically significant.

Although the pre-treatment means are substantially different across all groups, the graph shows that the common trend assumption is validated for all the groups of comparison, and therefore the evidence that the MBO policy affects individuals only joint with IRCCS funding is corroborated even more, with the negative and significant gap relative to the control group between double-treated individuals and MBO-treated only ones widening significantly over time, starting from 2018. To show that the results are not driven by attrition, we report the event-study for the full unbalanced panel in the Appendix: Figure A1 proves that the results in case of attrition are consistent with our main findings, with just some slight difference in magnitude.

2.5 IRCCS Funding Analysis

Given the almost null effect of the MBO policy, and the substantial changed triggered by the additional IRCCS funding, we now assess the IRCCS recognition in a deeper fashion, to provide with more detailed quantitative measures of its impact on the individuals' research productivity. The recognition caused an allocation of greater funds for research, and began in 2018. Such event mainly affected the hospital's academic-affiliated researchers, allowing them to receive dedicated public resources and, in addition, by enhancing the prestige of the institution under the scientific point of view. We use a difference-in-differences approach to estimate the effect on academic physicians' research output-

2.5.1 Empirical Strategy

To identify the IRCCS recognition's effect, We estimate a baseline specification for a stadard two-way fixed effects model, via OLS estimators:

$$Y_{it} = \alpha + \theta Post2018_t \times IRCCS_i + \delta_i + \tau_t + \varepsilon_{it}, \quad (2.2)$$

where $Post2018_t$ equals 1 if $t \geq 2018$ and 0 othrwise, $IRCCS_i$ indicates if individual i is in the IRCCS perimeter (i.e., directly eligible for IRCCS funding); in the baseline, we consider double-treated individuals as control units, in order to have a lower bound of the estimated effect of the policy. Beyond the double-treated, the baseline control group also includes the other MBO-treated individuals and the pure control group. In such regards, we renovate the aforementioned clarification: while the hospital updates the IRCCS

perimeter at the end of every year, we still perform a standard 2×2 TWFE DiD estimation, by considering as treated those who are *ever*, sooner or later, included in the IRCCS perimeter. This is done in order to allow for a more clear-cut identification of the effect, although by doing this our causal quantity of interest consists in an *Intention-to-Treat Effect* (ITT) rather than an *Average Treatment Effect on the Treated* (ATT), which is what is usually estimated in the best-specified Diff-in-Diffs analyses. As a matter of fact, even though solely IRCCS-only individuals should be formally granted access to IRCCS funding, a structured research group, within the personnel working at the institution, takes shape immediately after the recognition of the scientific excellence standards of the hospital under study. Such crowd can be accounted as the set of physicians who actively take part to most IRCCS research activities, and collaborate in order to obtain the privileged funding which is granted to the hospital due to its IRCCS status. In any case, the perimeter is not re-updated yearly from scratch, but there exists a consistent core of researchers which stays constant over time, which is integrated yearly by those fellows who are “*de facto*” IRCCS collaborators to the same funded projects and thus have access to the same resources, even though their status is officially recognized only after few periods; they basically select into the treatment after the IRCCS recognition. This notwithstanding, the inclusion into the IRCCS perimeter is always established by the institution (top-down). In addition to that, reflecting the same settings of the MBO policy, the treatment is assumed as an absorbing one. However, and differently from the MBO-policy which is absorbing by construction, the inclusion in the IRCCS perimeter is not constant until the end of the time-span, as some individuals apparently “switch off” the treatment since are excluded from the list by the management at some point. There is no reason not to maintain the absorbing treatment assumption nonetheless, because while papers are written in a relatively short time frame, the exploitation of granted funds can be actually prolonged in time, so researchers who started projects (or were involved in some) would feasibly keep working to such projects by using the same means even after their IRCCS status has expired. To check for these issues, albeit holding the absorbing assumption as always true, besides the standard test for parallel trends, we also perform additional estimates accounting for the staggered inclusion of some researchers to the core of the IRCCS perimeter, and controlling for the “switchers” across groups.

The coefficient θ captures the average intention-to-treat effect on the IRCCS recognition treatment of those who only receive funding after 2018, with respect to *all* MBO-eligible individuals and the full control group. In such set-up we compare IRCCS-only listed researchers with non-listed individuals and listed-but-non academic individuals eligible for MBO, before *and* after 2018. As before, the main identification assumption is that, without the IRCCS acknowledgment, the scientific productivity across groups would have followed parallel trends in the post- period. While TWFE account for overall temporal trends and time-invariant heterogeneities, we test the assumption through event-study estimations, as we did in the previous section. Since in such framework the issue of self-selection is of paramount relevance (as the institution decides whom to include in the perimeter possibly depending on productivity), we also provide the main event-study estimates with additional validity checks which exploit the recent methodology of Rambachan and

Roth, 2023. Their so-called *honestDiD* methodology allow for estimates with potential mild violations of the parallel trend assumption (which are plausible in case of selection), by computing confidence sets granting the robustness and significance of estimated dynamic coefficients in the period after the treatment *even with deviation from the common trends*, up to a certain point which is the threshold-level computed by the methodological algorithm itself; more on this later in the paper.

In addition to the main DiD strategy, we implement further sets of estimates. In 2018, the four identified categories (0, 1, 2, 3) are defined as in Equation 2.1 (0 = control, 1 = MBO-only, 2 = IRCCS-only, 3 = both). After reporting the baseline (findings from Equation 2.2), we incorporate the estimates with a methodology to assess the differential effects across different groups. This second strategy entails the following comparisons: (1) double-treated vs. IRCCS-only; (2) double-treated vs. MBO-only; (3) double-treated vs. pure control; (4) IRCCS-only vs. MBO-only; (5) IRCCS-only vs. pure control. Such comparisons are obtained by estimating a set of equations constructed as follows, by changing the comparison groups for every regression defined in Equation 2.2.

After this, we compute a set of dynamic coefficients for the IRCCS intervention by means of event-studies for the latter set of models, interacting $IRCCS_i$ with year dummies around 2018, according to the compared groups involved. Note that the baseline comparison in (the one which follows) follows an approach which is quite “conservative”, as the binary variable takes value 1 only if i is included in the IRCCS perimeter *only*, without being subject to the MBO-policy as well (as in, she is an IRCCS-listed academic doctor); the control group consists in the set of groups 3, 1, and 0 combined. Such equation enables the visualization of potential diverging pre-2018 patterns. Here is the estimated equation (via OLS):

$$Y_{it} = \alpha + \sum_{h=2 \wedge h \neq 1}^H \beta_{j,h} \mathbb{1}(IRCCS_i) \mathbb{1}(t = 2018 - h) + \sum_{g=1}^K \gamma_{j,g} \mathbb{1}(IRCCS_i) \mathbb{1}(t = 2017 + g) + \delta_i + \tau_t + \varepsilon_{it}. \quad (2.3)$$

Although it is common practice to exclude the time period immediately before the policy kicks in for the sake of collinearity, we recover the same approach of the previous event-study, therefore excluding 2016 as our reference category. This is done to enable better comparability between our different sets of estimates.

2.5.2 Results

We report the DiD estimates from Equation 2.2 in Table 2.3. The estimates display a positive and significant impact, confirming the pattern which we already observed previously, showing the importance of IRCCS recognition compared to the performance-based policy. Col. (1) report the coefficient on the interaction $Post2018_t \times IRCCS_i$ described in Equation 2.2. The other columns display instead the coefficients for the single interaction terms for different groups’ comparisons as indicated in the columns themselves (DT vs. MBO, DT vs. IRCCS etc.). The estimated coefficient in Col. (1) implies that, on average, being listed as an IRCCS researcher-only (IRCCS) is linked with a growth of 2.32 additional publications per year (significant

at 1%) compared to the other groups. Such increase is quite outstanding, as it accounts for more than 65% of the average mean of individuals control units and IRCCS-listed non-academic physicians. When breaking down the effect by category, results are consistent. The units treated compounded publish almost 1.24 more works when compared to the MBO-treated only (+75%), 1.10 fewer works when compared to the IRCCS-only researchers (−66%), and 1.14 more works when compared to the pure control (+69%). Such findings are consistent with the stylized fact that IRCCS contributed substantially to the output of both academics and non-academic MBO-treated MDs who had the opportunity to be subject to both policies, although the researchers involved in scientific activities primarily are the ones who responded the most after the recognition, as they display higher publication rates even when compared to those who are selected into the IRCCS list and allowed to get the performance bonus. We observe this in the final columns of Table 2.3: in Col. (5), the effect of IRCCS recognition on IRCCS vs. MBO is almost +2.65 publications (+74%); in Col. (6), IRCCS publish 2.53 works more than the pure control (+69%).

	(1) IRCCS vs. others	(2) DT vs MBO	(3) DT vs IRCCS	(4) DT vs PC	(5) IRCCS vs MBO	(6) IRCCS vs PC
Publications	2.3200***	1.2398***	-1.0989***	1.1365***	2.6469***	2.5329***
(SE)	(0.2917)	(0.2285)	(0.3337)	(0.2348)	(0.3091)	(0.3148)
N	6424	2623	2739	2430	3987	3794
R ²	0.774	0.596	0.764	0.582	0.793	0.784
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Mean	3.56	1.65	1.65	1.65	3.56	3.56
Panel (Balanced)	Full	DT and MBO	DT and IRCCS	DT and control	IRCCS and MBO	IRCCS and control
Time Range	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022

Table 2.3: Impact of IRCCS recognition on Annual Publications (Difference-in-Differences).

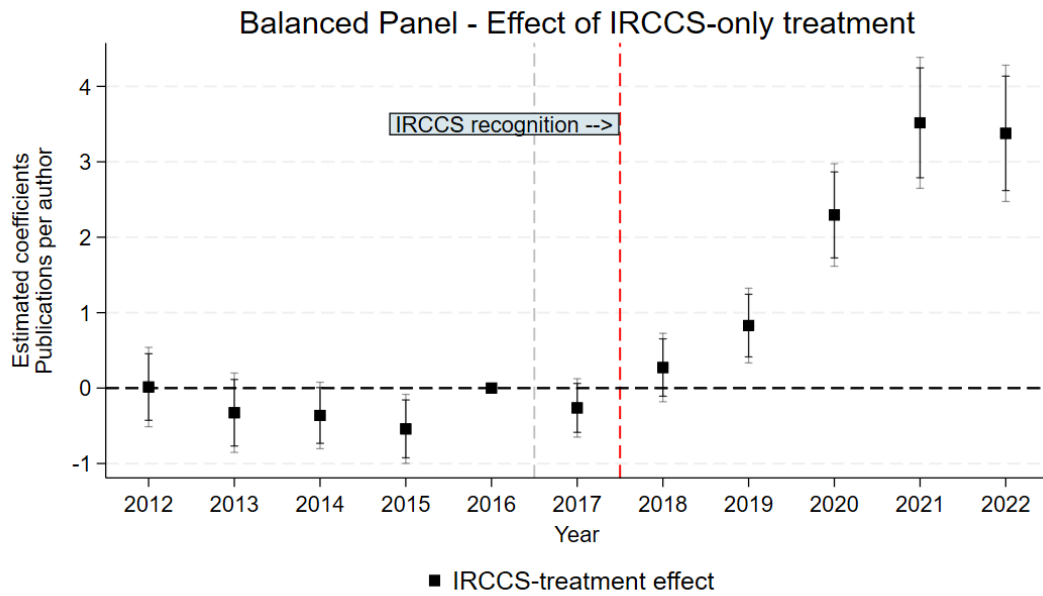
The baseline dynamic specification for the IRCCS effect is represented in the event-study a) in Figure 2.4. As above mentioned, the definition of treated unit here is quite conservative, as we are only comparing academic doctors who receive IRCCS-funding only with the rest of the personnel, included those who receive the performance payment and have access to IRCCS-funding too. Prior to 2018, the raw number of publications for the IRCCS-only group were relatively flat and non-diverging with respect to the rest of the units. While their scientific output was way higher on average (since they are professional researchers, see Table 2.2), we notice that, in terms of time-varying pattern, there is no significant difference from the slope of the publication output of non-treated ones until 2018, thus highlighting the relevant impact of the access to the IRCCS funding and the absence of evident pre-trends. In the graph, we observe a small and slightly

significant jump in 2019, reasonably due to the, existing albeit short, lag in the publishing process. The coefficient keeps increasing in 2019 and displays a greater-than-100% jump in 2020, which could be credited to both the passing of time which reduces the impact of the afore-mentioned lag, and to the boost in research activity triggered by the COVID outbreak. The effect seems to stabilize in 2022, after a further substantial growth in 2021. It appears that the IRCCS recognition's impact had already materialized few lags after its implementation, and researchers had possibly already optimized the resources in order to produce output in a timely fashion: so not only new projects have been ushered, but some of them have been completed.

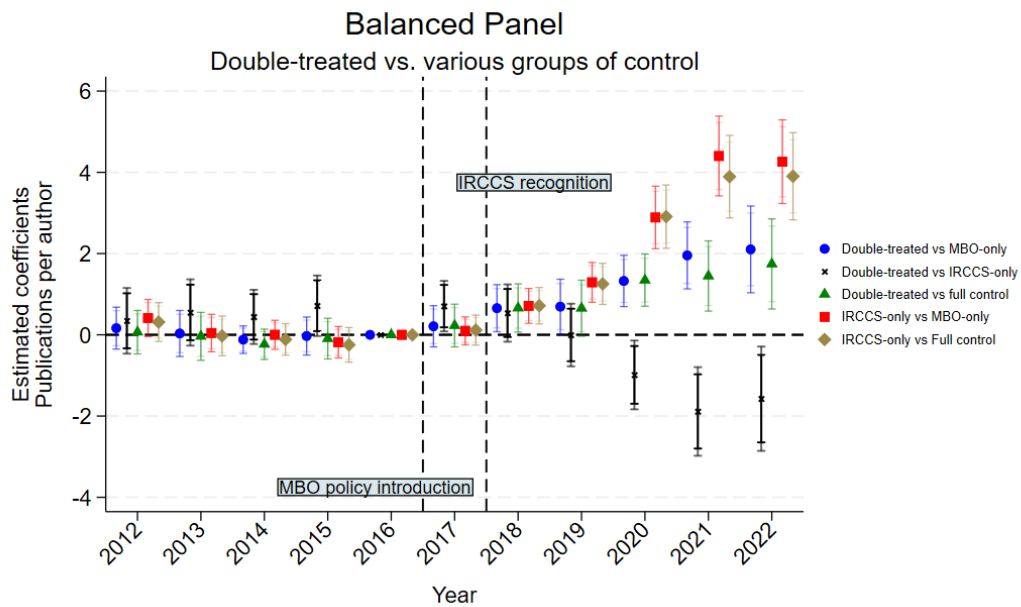
We reckon to have estimated dynamic coefficients based on an identification which was indeed conservative, but not clear-cut: as a matter of fact, some IRCCS' researchers included in the control group were non-academic, hence they were enjoying the performance-based bonus. On top of that, although we already showed that the MBO-policy effect was quite neutral, it surely contributed to bring about some bias in the baseline estimates, especially for the double-treated individuals. Therefore, we performed a number of diverse dynamic estimations again, in doing this showing the graphic representations of the estimates foreseen by Equations 2.3, in Graph b) of Figure 2.4. The latter reports the comparisons between double-treated and IRCCS-only treated units and each of the other groups. Both graphs a) and b) keep validating the hypothesis that the MBO policy does not really bias the estimates as its impact is almost non-existent; again, it slightly affects the research outputs only by shifting up the pattern of publications for the double-treated units; however, such pattern is more than offset by the IRCCS funding granted to academic researchers, whose trends follow along both in comparison with the pure controls and the MBO-treated only individuals. As we reported already four out main event studies, we show in the Appendix that the estimates for the balanced panel follow similar patterns (Figure A2).

It is however evident how the compound effect of the performance-based policy and the IRCCS access affects the dynamic comparison of IRCCS-listed academic doctors and the double treated. We observe indeed a significant shift downwards in the publication output of IRCCS-only treated in comparison to the other groups in 2015, and a non-significant one in 2017, hence immediately after the MBO implementation (Graph b). Anyway, such difference starts reverting back already the year after, with the IRCCS recognition occurrence, after which the IRCCS-only physicians start regaining their comparative advantage with respect to the other individuals, with their outcome boosted by the access to new resources. Another relevant pattern is that the double treated, when compared to academic physicians, display a slightly significant positive trend even before 2017. This is credibly due to the selection issue of non-academic doctors enrolled in the IRCCS staff due to their above-average productivity: we address this concern later on.

Overall, the IRCCS funding intervention appears to have had a substantial, fast and persistent effect on research output, fed up by the pandemic and reinforced, only for some individuals, by the performance-based scheme.



a) Event-study of IRCCS recognition effect on publications for IRCCS-only treated individuals compared with all the other units.



b) Event-study of IRCCS policy effect on publications for comparisons across different groups.

Figure 2.4: Event-studies of IRCCS policy effect.

The estimates for the IRCCS-only group suggest that the heterogeneity in incentive structures across the different groups can only modestly be offset by explicitly rewarding productivity in the short term, if such reward is not paired with additional resources. It must also be noted that additional resources already impact positively those who are selected into the perimeter, that are chosen by the management due to their supposed better performance in research-related activities; hence, such individuals possibly already embed a slightly different incentive structure, if compared to the other non-academic doctors. In any case, the presence of academics as well in the pure control groups confirms that the heterogeneity in pre-existing incentive behavioral structure may not only exist across groups, but within them as well.

To sum up, it looks like that academic researchers, already incentivized by intrinsic reasons and career motivations, react substantially when provided with more resources, whereas individuals not attracted by the same motifs or similar career progression incentives, show no incremental response to monetary rewards, or do so only in presence of extra funding. Given the relevance of such results, we next provide with a range of robustness checks to ensure that these findings are not driven by other confounding factors. The first one is included in this very same section, and addresses the concern of potential bias driven by the violation of the parallel trend assumption.

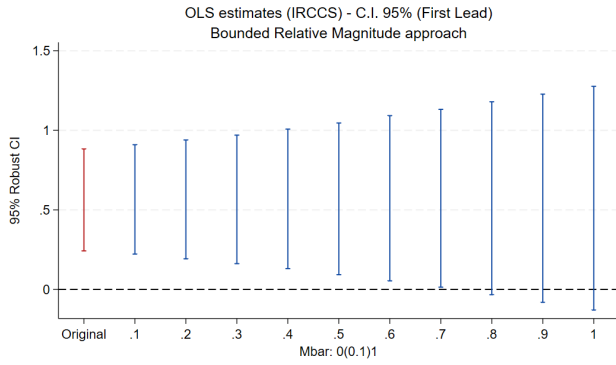
Parallel Trends

While the main estimates seem to display the absence of deviation from the parallel trends in all the provided specifications, we place additional focus on the pre-trends' issue, given the significance and magnitude of our results. This check is also required due to the fact that the treatment is endogenous and its assignment certainly depends on existing characteristics, which are heterogeneous across groups and units. First of all, notwithstanding the overall absence of statistical significance for the pre-treatment lags interacted with the treatment dummies, we still observe that, especially in our baseline graph a) in Figure 2.4, there is a slightly decreasing (albeit non-significant) trend for the IRCCS staff in the periods prior to 2016, which seems to become also statistically significant at 95% in 2015. Although a visual inspection would not assess such deviation as able to undermine the validity of the identification, and even if the trend is falling down (which, if anything, would lead our estimated post-treatment effects to be downward biased), we follow the *honest* methodology by Rambachan and Roth, 2023 to account for plausible deviations from the common trend. In accordance to their approach, we can undertake sensitivity checks even by hypothesizing some significant divergence from parallel trends. We do so by computing *bounds on relative magnitude (BM)*: as in, we replicate their methodology by estimating the entity of the divergence from the parallel patterns after the treatment, up to the size that could be able to invalidate the significance of the post-treatment outcomes, with respect to the assumption of the absence of pre-trends. Such potential *post-* deviation is assumed to have the same size as the one in the period reporting the greatest difference in trends among all of our pre-treatment coefficients (in our baseline case, 2015). We also estimate a second bunch of sensitivity checks, where the assumption regarding the entity of the post-treatment deviation is not made according

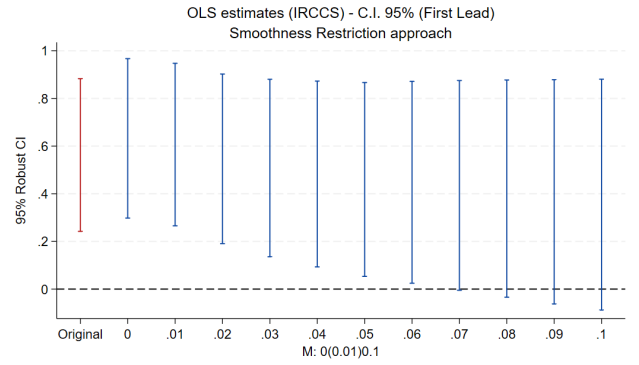
the maximum size of one of the lag-related differences, but due to a linear extrapolation of the hypothesized pre-trends which would lead the estimated results to be non-consistent (*smoothness restriction - SR*). In order to facilitate the differentiation between pre- and post- periods, we shift back to the common practice in terms of collinearity issues and account for 2016 (as in, $t = -1$) as the reference category.

We provide evidence for the *honest* confidence bounds for the baseline estimates (Figure 2.5), using both approaches (BM in graphs a) and c); SR in graphs b) and d)), and by allowing variation in the common trends able to invalidate the significance of the coefficient on the first lead estimation, which results positive and significant when accounting for 2016 as the omitted year, (graphs a) and b)) or the overall average across all post-treatment coefficients (graphs c) and d)). Results are reported, as said before, in Figure 2.5. In each graph, the y-axis represent the robust 95% confidence interval for the target estimated coefficient. The x-axis, on the other side, represents the varying parameter of interest, $Mbar$ in the bounded relative magnitude method and M in the Smoothness one. For the BM approach, it represents the percentage of the size of the post-treatment violation (based on the maximum pre-treatment difference) for which the estimate is allowed to be biased before becoming invalid. For the SR approach, the x-axis represent the percentage of the deviation in the slope from the linear extrapolation of the pre-trend divergence (based on the post-treatment evolution) that would make the estimate invalid. In all graphs, the baseline confidence interval (as in, the one with no deviation from common trend assumed) is in correspondence of $Mbar = 0$ (or $M = 0$) and it is indicated by a vertical red C.I. line.

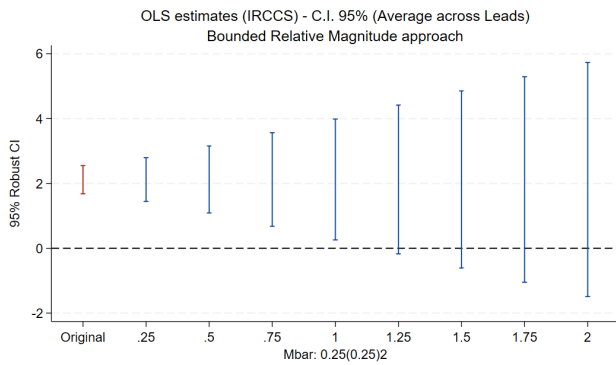
Graphs a) and b) display the robustness confidence set of the estimated coefficient in the first lead for the baseline event-study. According to the BM approach (graph a, the x-axis ranging from 0 to 100%)), the effect maintains its significance up to assuming an actual 80% deviation from the maximum divergence measured in the pre-treatment period. The smoothness restriction approach instead tells that the first lead would be invalid only if the slope divergence in the post-treatment period from the linear extrapolation obtained from the lags' pattern overcame 7%. Considering that we are referring to the first lead (when the effect is lower compared to the rest of the post-treatment period, even non-significant in our plotted estimate, and it has not fully kicked in already), we can consider such results as robust. If we look at the average effect across all leads (as in, the ITT), we observe that the estimated coefficients are even more robust: indeed, they maintain their validity up to almost a 125% bounded relative magnitude violation, and up to a 12.5% difference in the linearly extrapolated slopes. This confirms the validity of the parallel trend assumption in our framework. We also report the *honestdid* estimations for the various comparison across groups in the Appendix, in Figures from A3 to A8.



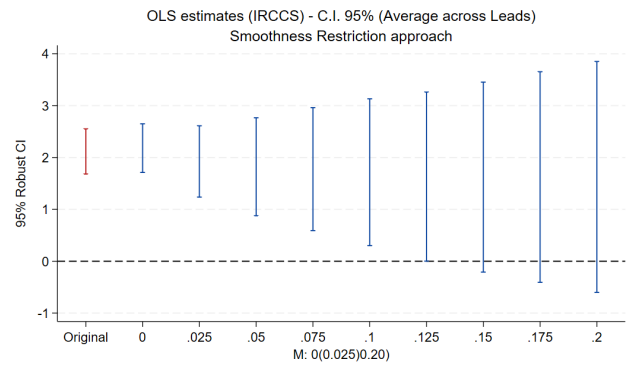
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.



c) BM approach for the significance of the average across all leads.



d) SR approach for the significance of the average across all leads.

Figure 2.5: Honest DiD robust confidence sets for overall IRCCS effect estimated with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).

Validity of the TWFE 2x2 DiD identification

The IRCCS recognition happened at once at the institutional level, and thus there exists a bulk of core researchers who stay in the IRCCS list for the whole post-treatment time-span; however the perimeter is re-updated annually by the hospital management. Therefore, the inclusion of some individuals in the IRCCS research staff could be considered the proper setting for a *partially staggered adoption roll-out*. In such context, we have a *core group* of approximately 140–150 researchers in the triennium 2018-2020 (more specifically, 152, 140, 141, as shown in Figure A9), and a growing number of physicians formally added in subsequent years (so that they become 181 in 2021, and over 200 in 2022), especially after the COVID outbreak. While the late additions often consist of previously MBO-treated physicians included for their research engagement, also some faculty members are newly added up to the IRCCS staff *officially*.

In the main analysis, treatment is defined based on the “*ever-treated*” criterion, meaning that we deem all individuals who were ever listed in the IRCCS registry as treated from 2018 onward. This is coherent with our interest towards the causal estimand studied throughout the paper, which is the *Intention-to-Treat (ITT)*:

$$\begin{aligned} \text{ITT} = & \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid \exists k \in \mathcal{T}, k \geq 2018 \wedge D_{ik} = 1] \\ & \text{where } \mathcal{T} = \{2012, 2013, \dots, 2022\} \end{aligned} \tag{2.4}$$

where $D_{ik} = \{0, 1\}$ indicates the treatment status assignment, and it implies that an individual can be deemed as treated as long as she actually receives the access to the IRCCS perimeter *at a certain point in time starting from 2018*. This is different from the *Average Treatment Effect on the Treated (ATT)*, which would rely on actual entry year:

$$\text{ATT} = \mathbb{E}[Y_{it}(1) - Y_{it}(0) \mid D_{it} = 1] \tag{2.5}$$

where $D_{it} = 1$ indicates the assignment of the treatment in the year in which an individual i formally enters the IRCCS perimeter. This approach can be considered valid under *partly* staggered entry. This holds consistently within our institutional settings, as individuals are enrolled in the IRCCS perimeter by the hospital management according to their research commitment, already ascribable within the boundaries of an “unofficial” IRCCS core. In terms of empirical appraisal, we support our claim with several strategies. First, we observe that, the approximation notwithstanding, all the basic parallel trend tests show no anticipatory behavior nor divergence in the pre-trends before the IRCCS recognition, which implies that even if selection may have been in place for the late-enrolled, it surely occurred after the IRCCS recognition, and never before. Second, such tests are all corroborated by using the *honest* methodology of Rambachan and Roth, 2023 to check for the validity of the effects in presence of hypothetical deviations from the common trends. Then, to confer more validity to such simplification, we remove from the sample all the “late-adopters”, as in those

who are not part of the initial core of IRCCS researchers, to assess whether the effect is driven by them or if it is biased upwards by selection issues. Table A3 in the Appendix proves us that is not the case, as the effects are still present (and, indeed, magnified) by excluding those who are selected into the research perimeter with some lag after the IRCCS’ recognition, showing that, if anything, what we estimate in our *ITT* analysis is a lower bound for the true *ATT*, which is biased downwards by the inclusion of the later-treated. The dynamic estimates derived from such sensitivity checks are also reported in Figure A10 and Figure A11 in the Appendix; they both corroborate the validity of our simplification procedure by showing the absence of substantial pre-trend. The reduced significance of the effect for the double-treated individuals is reasonably due to the lowered statistical power of estimates caused by the sub-setting of the dataset. This statement can be better understood by looking at Table 2.4, which displays the composition of the group of the IRCCS-enrolled individuals.

	N	Percent		N	Percent
IRCCS First - MBO	20	13.16	IRCCS Late - MBO	33	35.48
IRCCS First - No MBO	132	86.84	IRCCS Late - No MBO	60	64.52
Total	152	100	Total	93	100

Table 2.4: Composition of the IRCCS group, and distinction between “first-” and “late-” adopters,

Such robustness checks are made necessary by the fact that, if we look at the characteristics and the composition of the sub-groups of the IRCCS perimeter (2018 vs late adopters), significant heterogeneities are displayed (Table A4 in the Appendix), in doing this showing the existence of structural differences between the individuals selecting early or late into the IRCCS treatment perimeter.

Furthermore, reliance on our ITT 2x2 methodology can also be justified by following the approach by Goodman-Bacon, 2021. We hence estimate a DiD equation with variation in treatment timing (depending on when treated researchers are included in the IRCCS perimeter). Starting from the assumption that a staggered TWFE OLS DiD coefficient is a weighted average of 2x2 DiD coefficients obtained from the comparisons of all different cohorts stemming from the heterogeneous roll-out of the treatment, we decompose the DiD coefficient into sub-coefficients comparing the various cohorts, to which a weight is assigned depending on its relative importance credited to the finding in explaining the results. Results are reported in Figure 2.6, both in the graph and the table below. The decomposition shows that 84% of the estimates stem from the comparison between treated and never-treated, rather than those across heterogeneous treatment-timing cohorts. In addition, the bulk of the overall effect (about 65%) is to be credited to the first sub-coefficient, as in the one stemming from comparing the individuals treated in 2018 to the never-treated. All other coefficients deliver estimates below the overall effect, although the weights assigned to them are extremely low in magnitude, which proves how the existing selection mechanism is actually downward biasing the results. There are not significant coefficient estimates lower than 0 either. This supports the reason of opting to the estimation of an *ITT* via a basic 2x2 identification strategy rather than focusing on *ATT* as our estimand of

interest, as the staggered design would be flawed by serious endogeneity issues and it would deliver severely biased results.

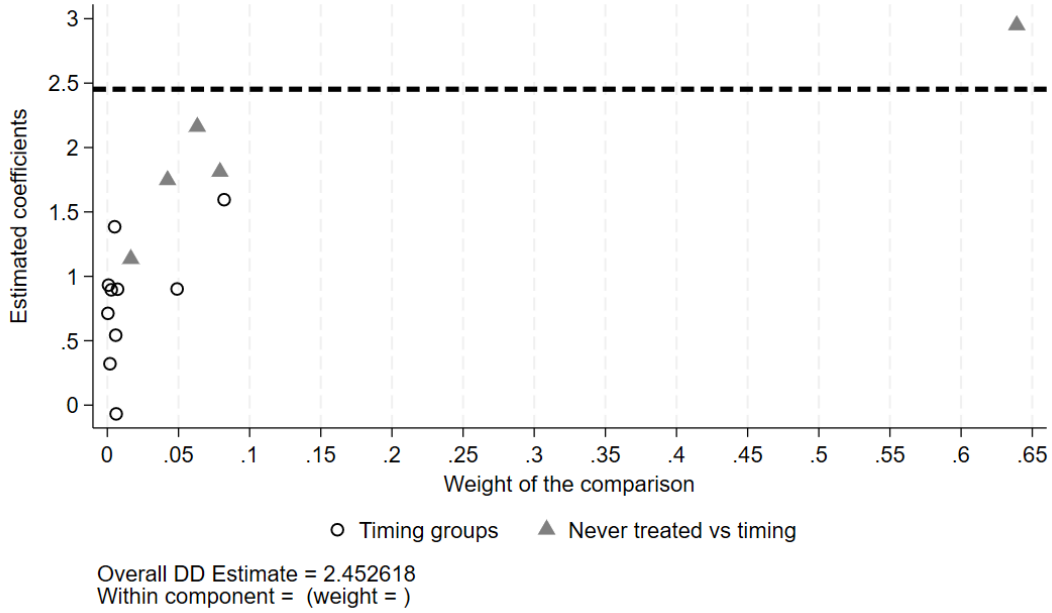


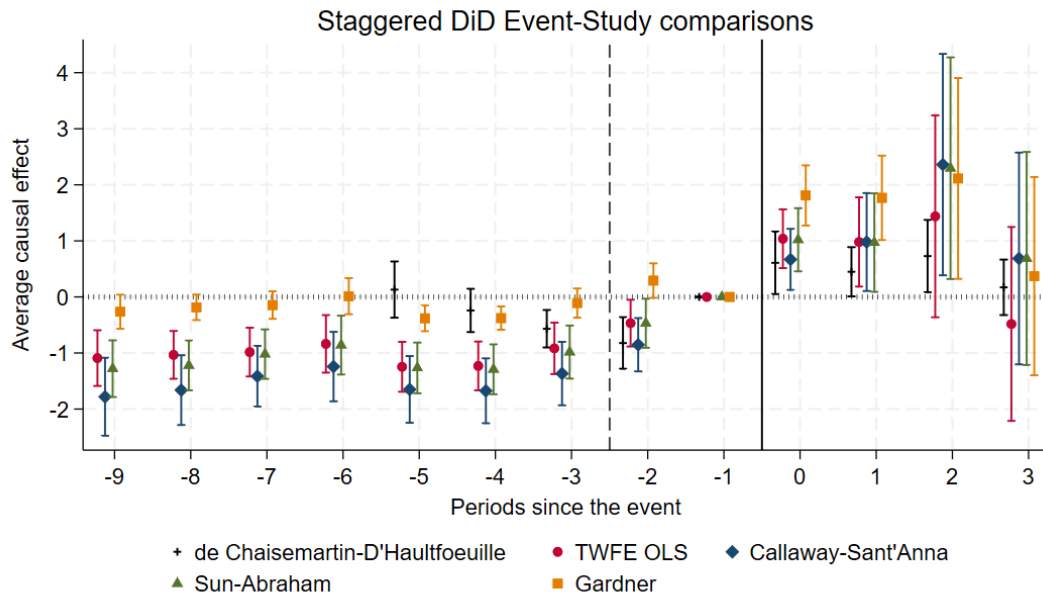
Figure 2.6: Goodman-Bacon decomposition: weights and estimates.

Eventually, we report the extent of the selection issue by estimating two sets of event-study equations; in the first set, we aim at estimating the (downward biased) staggered ATT effect on individuals enrolled in the IRCCS perimeter after 2018 excluded, hence without including all the “core” researchers treated in bulk concomitantly with the IRCCS recognition. In the second set, we perform a staggered estimate on all units, late-enrolled or otherwise. Recent econometric literature on Staggered Difference-in-Difference methodologies displayed already how TWFE OLS estimators may deliver biased estimates (de Chaisemartin and D’Haultfœuille, 2020, Callaway and Sant’Anna, 2021, Sun and Abraham, 2021, Butts and Gardner, 2022), especially when dealing with heterogenous timing, dynamically varying effects and endogenous treatment; we hinted such issues in the decomposition from Goodman-Bacon, 2021. Thus, as first step, we estimate a dynamic TWFE OLS equation for staggered treatment adoption, with the following specification:

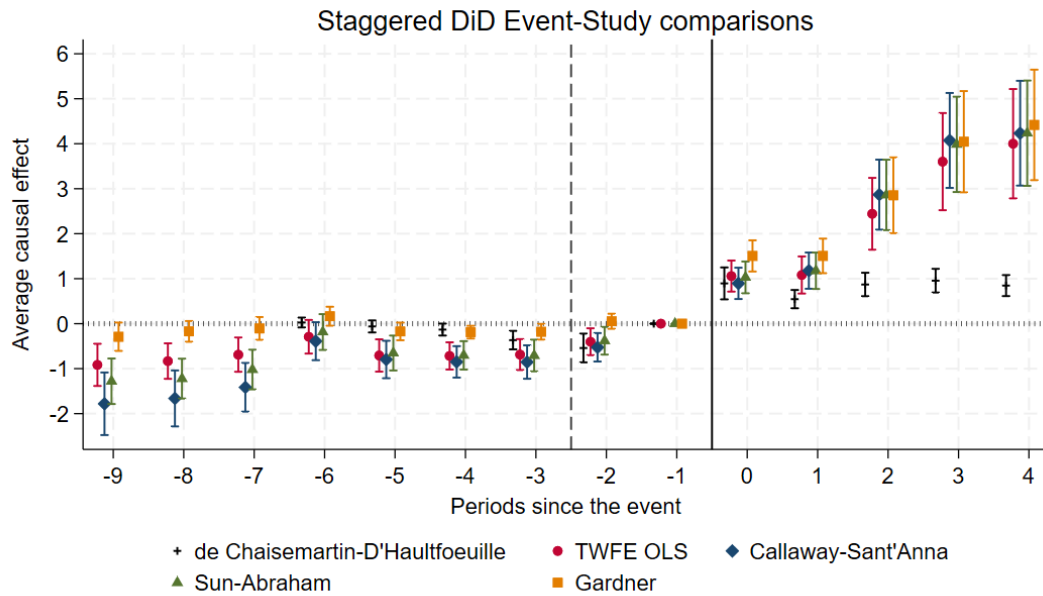
$$Y_{it} = \delta_i + \tau_t + \sum_{k \neq -1} \beta_k \cdot D_{it}^k + \varepsilon_{it}, \quad (2.6)$$

where Y_{it} is the outcome of interest for unit i in year t , δ_i and τ_t are unit and time fixed effects. D_{it}^k is a binary variable taking unitary value if i is k years away from treatment in t , 0 otherwise. We normalize the coefficient for the period immediately before treatment ($k = -1$) to zero, so that all β_k coefficients are interpreted

relative to the year before the IRCCS inclusion. After that, we integrated the estimate by better refined approaches: 1) de Chaisemartin and D’Haultfœuille, 2020 (correction to the TWFE estimator by excluding “forbidden comparisons” - e.g., already treated units used as controls. The estimator relies on 2x2 comparisons of yearly “joiners” with non-treated units); 2) Callaway and Sant’Anna, 2021 (estimate non-parametric group-time ATTs using only never-treated units as controls, allowing for flexible heterogeneity across groups and over time); 3) Sun and Abraham, 2021 (introduce event-study estimators based on interaction terms between time and cohort); 4) Butts and Gardner, 2022 (based on Gardner, 2022: implement a two-stage estimator that residualizes the outcome variable to remove fixed effects, and then estimates dynamic treatment effects). Figure 2.7 (Graphs a) and b)) shows the dynamic effect of IRCCS using TWFE and the various corrected estimators. The methods relying on linear estimates are normalized at $k = -1$ to avoid collinearity.



a) Event-study of the staggered inclusion in the IRCCS perimeter of late-enrolled only, excluding the "core" researchers treated in 2018.



b) Event-study of the staggered inclusion in the IRCCS perimeter of all treated units, including the "core" researchers treated in 2018.

Figure 2.7: Event-study of IRCCS policy effect on publications for comparisons across different groups.

All the patterns are very similar, although the different methodologies show strongly heterogeneous findings in terms of magnitude and significance. The TWFE, Callaway-Sant’Anna and Sun-Abraham estimates significantly shift downwards the pre-trend dynamics, showing an evident converging pattern in the pre-treatment period. The same is displayed by the estimates performed via de Chaisemartin-D’Haultfoeulle and Gardner, although their magnitude and significant are lowered.

Such differences in the estimates are reasonably to be credited to the heterogenous nature of the methodologies, as the latter does not rely on linear time normalization, and therefore what is observed as a converging pattern driven by an initially negative difference in trends for coefficients estimated through the other methods, due to the estimates being with respect to an omitted time unit, for the non-parametric estimates translate into a positive anticipatory behavior emerging in the lags prior to the enrollment. We also observe how the visual inspection of diverging trends displays less evident heterogeneities in the full sample (b) with respect to the one excluding early adopters (a), who are the bulk of treated individuals and make up for most of the estimated coefficients. In graph (b) indeed, the post-treatment effects are downward biased to a lesser extent and appear to be diverging rather than re-absorbing. It must also be noted how, in graph (b), the estimates yielded by the methodology of de Chaisemartin and D’Haultfoeulle, 2020 are sensibly downward shifted compared to the other approaches, which display comparable coefficient magnitudes. This is reasonable, as the weights constructed through their corrected estimation methodology are based on the comparison between “joiners” (as in, late adopters) with the never-treated units. As the majority of the treated units is made up by core researchers, the results delivered by comparing late joiners to the control group are necessarily lower than those observed by estimating the effect via other procedures. This is corroborated by the fact that, in the event-study excluding early adopters, the approach by de Chaisemartin and D’Haultfoeulle, 2020 yields less understated dynamic coefficients. This suggests that selective inclusion into IRCCS (which indeed exists and is apparently due on past productivity) is possibly downward-biasing our *ITT* findings, which are consistently acknowledged as the baseline results or the present study.

2.6 Robustness Checks

In this section, we present further robustness tests to verify whether the estimated effects are driven by the policies or by other possible confounders. First, we try to assess the effects found already in our baseline model using different scales of the outcome. We then address potential bias from the COVID-19 shock; control for spillover effects in publications across groups; and account for individuals who changed status, and for the presence of potential “research superstars” who might be driving down (up) the effect of the MBO (IRCCS).

Different scales

So far we expressed the outcome in raw numbers, reporting it as the number of yearly publications per authors. However, we re-scale the outcome due to the relevant difference in numbers across heterogeneous types of researchers, with many of them never publishing, whereas others do that even dozens of times in a year. We re-perform the estimates specified in Equation 2.2, changing the outcome from the raw number of articles to: 1) their log-number (re-scaling by adding 1 as constant); 2) their inverse hyperbolic sign. Coefficients on log- and asinh- publications are reported in Table A5, in the Appendix. While the direction and significance of all coefficients is consistent with those of the baseline model, their percentage size is definitively lower compared to the one computed on raw numbers relative to the pre-treatment mean, never overcoming the threshold of 40%. However, the high amount of zeros in the outcome and the methodology employed (Diff-in-Diffs) makes the estimates sensitive to scale variations (J. Chen and Roth, 2023), which is proven by the difference in the estimates depending on the kind of outcome transformation performed. This is why we hold such findings as robustness check while keeping to account on Equation 2.2 as our baseline.

Excluding COVID-19 Years

It seems clear that the COVID-19 pandemic outbreak in 2020 substantially triggered upwards the pattern of publications. Was such event to affect research productivity, independently on the granting of IRCCS funding to the hospital, then it would be a validity concern. The main confounder could be the re-allocation of time due to the pandemic: as a matter of fact, time and efforts could have been devoted to medical assistance at the expense of research, especially for non-faculty doctors (Mantellini et al., 2020, Ahrendt et al., 2022, Franzoni et al., 2025). In such case, the diverging pattern should not be credited to the take-off of the funding policy, but to reduced research activity by physicians. On the other hand, the postponement of elective activities might give doctors more time for writing, whereas with the joint national effort put in place in the struggle against the pandemic, COVID-related research at the time could be relatively easy to perform, with lower publishing lags. While the latter two hypotheses are not necessarily a concern, since they would explain a feasible amplification of an existing IRCCS effect, the first threat to validity would be a major confounder, able to substantially bias the reported estimates. To ensure that our results are not driven by anomalies during the pandemic, we re-estimate the main models using data only up to 2019³. We thus truncate the panel at 2019, and re-run the difference-in-differences analyses. Table A6 in the appendix shows the estimates for the pre-COVID sample. Results are similar in direction to the full-sample findings, although the size of the coefficients is, as expected, reduced by the shortening of the time span.

³Note that already by looking at the main event-studies we can retrieve that the effect kicks in before the pandemic started, which is already a good validation of our main results. We therefore avoid reporting the event-studies with the reduced sample, which may be made available upon request.

Spillover Effects

A further concern for our identification strategy is the potential violation of SUTVA (*Stable Unit Treatment Value Assumption*), as in, the policies might have indirect effects on those who are not targeted directly, mostly due to collaboration. For instance, IRCCS physicians who do not receive MBO-bonuses might however “spill” their gains towards non-academic MDs or even the double-treated ones, by increasing their output and at the same time co-authoring with the performance-based remunerated doctors. Because we observe that the IRCCS-funded researchers are the ones responding more significantly to the shock (whereas the MBO does not have any impact), if there was such a spillover effect, it would be reducing the differential across groups. However, we test for spillovers by conducting the following two exercises. First, we remove from the sample the double-treated units, who bring about cross-group spillover by definition as they constitute an overlap of the two groups; then, we identify all publications involving a collaboration between an MBO-eligible physician and an IRCCS-ever listed researcher. We call publications of *Type A* those which, in the panel, can be flagged as authored by a MBO-treated individual in collaboration with at least one member of the IRCCS perimeter. *Type B* publications are instead those identifiable as authored by IRCCS staff and co-authored with at least a non-academic physician. It is worth noting that such flags are identified on the whole unbalanced panel by checking annually who is in force to the hospital, in order not to exclude those who do not appear continuously over the full longitude of the dataset. Such construction calls for the comparison between findings in the balanced and in the unbalanced panel. Then, we perform two Diff-in-diff estimations for each exercise; one comparing the groups pre- and post- the MBO introduction (2017), and the other pre- and post- the IRCCS recognition (2018). In the first set of estimates we compare IRCCS-only and MBO-only treated units to the full control group (as in, a static version of Equation 2.1, but without the compound treatment), by first excluding authors of *Type A* papers, hence the non-academic MDs who *ever* collaborates with an *ever* non-MBO IRCCS member. Then, we do the same a second time, though excluding *Type B*, as in IRCCS researchers who *ever* co-author with MBO-treated units. This quite stringent exclusion of *all* authors of cross-written papers ensures that the treatment effects only stem from either within-group or individual publications. Such approach eliminates entire collaboration links between the two groups. We avoid removing both groups from the sample at the same time in order not to lose too much statistical power for the estimates. Findings are shown in Table A7 in the Appendix for the balanced panel.

The results of these two exercises offer interest insights. In absence of *Type A researchers*, who are the MBO-treated collaborating with IRCCS, results are consistent with the main analysis in both the unbalanced and balanced samples, showing a high response for IRCCS-researchers and a negative one for the MBO ones (which is however significant in the balanced panel only). On the other side, removing *Type B researchers* (IRCCS researchers coauthoring with MBO-only units) decreases the differential effect across the two groups in both samples, reducing the impact of the policy on faculty members. In the balanced panel, the negative role of the MBO on the productivity of MDs turns non-significant; it is curious to observe how such coefficient

turns positive and different from zero in the unbalanced panel (0.25 and 0.29, which is sizable in magnitude relative to the treated units' pre-event mean), which so far is the only relevant difference we retrieved between our balanced findings and the unbalanced ones. Such outcomes show how the role of collaborating MDs is negligible in the broader context of the hospital research activity; on the other side, co-authoring IRCCS researchers significantly bring about spillover effect within their own group. As a matter of fact, their removal improves the performance of MBO-eligible group individuals relative to the control group, lowering that of their own peers. This implies that research cross-group research networks are feasibly extended to within-group collaborations as well, whereas faculty members, as a group, benefit more from the networks of more prolific and open-to-collaboration individuals than their clinical non-IRCCS counterparts, at least relative to the full control group. Therefore, there seems not to be cross-group spillover effects biasing the results; by contrast, such spillover effects appear to be relevant in a within-group context, where a bunch of possibly "superstar" researchers drive up the performance of the whole IRCCS perimeter. Which sets the fundamentals for the next robustness check. The related dynamic estimates of the spillover checks are reported in the Appendix as well, in Figure A12, Graphs a) (balanced) and b) (unbalanced) respectively.

Controlling for outliers and switchers

Our main estimates have been performed, so far, by considering group status as fixed and absorbing, based on the initial policy definitions at the moment of the implementation of the given policies. In reality, there could be some variations over time due to changes across different roles. For instance, some physicians shift from non-academic positions to academic role at a certain point in time post-treatment, exiting from the group of those who are eligible in the MBO. On the other side, non-prolific academics may lose their tenure and become MDs without faculty appointment. In either case, the effect of productive non-academic MDs considered as "fixed" MBOs at the implementation of the MBO switching into the academic group, as the opposite pattern indeed, should again bias down our estimates, as the publications of these more (less) productive individuals shall be accounted for in the outcome of the control (treatment) group in the baseline analysis⁴. To guarantee that these "switchers" are not affecting our results to a great extent, we perform a robustness dynamic check starting from our main event study (2.1), where we exclude any unit whose status changed during the observation treatment window. To be precise, we allow for three scenarios: 1) drop any non-academic physician in 2017 who becomes, at a given point, a faculty member after 2017 (shifting from MBO-eligibility to non-eligibility); 2) any faculty member who left academia after 2017, to sort into non-academic medical practice; 3) any individual who undertook either of such changes. Cross-position switchers

⁴One might argue, for the baseline comparison, that switching out of the MBO-treatment group should be accounted as a violation of the absorbing treatment assumption, as MDs who become academics lose the eligibility to the performance-based payment. However, it is reasonable to keep such individuals in the MBO-treatment group, as the purpose of the performance-based payment is exactly that of triggering an incentive mechanism for which individuals start publishing after receiving the money in the first place. If after that they keep publishing even while losing the eligibility to the MBO, and if they adjust their incentive structure to such a point that they even switch to an academic position, the policy could be considered as effective. More on this in the next section.

account for barely 3.9% of the dataset. The findings (Appendix, Figure A13) are extremely similar in all scenarios, suggesting that dynamic transitions are not relevant enough to create distortions in the effects' estimates.

In addition to that, we check the extent to which the two groups have different incentive structures driving them. We identify those individuals who may shift upwards the overall publication numbers of their respective group, especially with regards to IRCCS-affiliated members. Hence, we re-perform the analysis by removing: 1) outliers (as in, the last percentile of the annual publication distribution); 2) the top 10%; 3) the upper quartile of the annual publication distribution. We then re-estimate both the baseline dynamic specifications: they are reported in the Appendix, in Figures A14 (balanced) and A15 (unbalanced). As expected, the differential impact of the policies decreases in size when an increasingly greater units in the upper parts of the yearly publications distribution are removed from the sample. However, the directions of the effects stay the same even when eliminating the 15% of the sample; it must be noted however, that when the top 15% is excluded, the shift in the double-treated units' publications results slightly greater than the one for the IRCCS-only individuals. While this is a slight reversion of our main result, it is worth reckoning that the double-treated change takes off with a lag after the IRCCS-recognition, which leads to suggest that as the cause for the variation, rather than the performance payment implementation. Nevertheless, such reversion does not take shape in the event study for the balanced panel (Figure A15). Therefore, it is evident that the huge effect we observe is strongly driven, but not entirely caused, by a sub-group of scientific superstars among IRCCS researchers.

2.7 Mechanisms

Whereas the IRCCS recognition seemingly boosted research productivity, we ought to test whether this is actually due to improved access to funds and, in such case, how. To do so, we recover additional information on the institutional research performed by the hospital under study, drawing open data from the Research Workflow of the Italian Ministry of Health, which tracks all the funded research lines of Italian IRCCSs. As specified earlier, access to additional funds can be granted either through Current Research (*Ricerca Corrente*) or Targeted Research (*Ricerca Finalizzata*). The first one is the ordinary research activity to which the Ministry annually allocates funding, through a thorough process of assessment of the research criteria required to provide said funds. The second one is tied to the awarding of targeted grants tied to specific projects, whose financing depends on project-related criteria and upon the will of researchers to apply to such grants.

Access to IRCCS funds - Current Research

Current Research fund are granted annually to the hospital, in accordance to the research lines defined by the institution itself upon the recognition as IRCCS. In 2018, the hospital was awarded the recognition for

the two broad macro-areas of Personalized Medicine and Innovative Biotechnologies. Such broad categories are further sub-divided into smaller ones, called Research Lines (*Linee di Ricerca*). According to the research lines in which the institutions are specialized, the Ministry allocates funds after elaborating a Three-year Plan, previous assessment of the standards required by the law. This framework relates to *ordinary research*, as funds are granted on a yearly basis upon withstanding excellence criteria in the areas where institutions perform research continuously; therefore, they are not competitive and are only assigned to IRCCS institutes -and not to other Italian healthcare institutions. Such funding has ranged annually between €150m and €175m overall from 2018 to 2023, and for a single IRCCS the allocation can be of few hundreds thousands of € to about €10m, according to their past performance and excellence standards (MdS, 2025). After 2020, for instance, the institution under question received, annually, €7m circa for Current Research activities.

The official website of the Workflow of the Ministry provides with micro-data of all funded annual research lines, together with the publications recognized as being part of such lines, for each institution. Such information can be exploited to assess whether the increased research productivity can be linked to the Current Research areas, to establish a more robust causal connection between the IRCCS recognition and the improvement in productivity. To do this, we recover our main analysis's framework (Equation 2.1), integrating it with information about research lines and publications. The data availability, however, constrains our estimates. As a matter of fact, research lines were not a thing before 2018, hence we are not able to obtain an adequate counterfactual in the years prior to 2018, dampening our faculty to put in place a proper triple DiD analysis. In order to make up for this, we devised two separate strategies. First, we identify all publications ascribable to the IRCCS Current Research lines after 2018, by merging the publications in our Web of Science-obtained dataset with the ones recovered from the Ministry Workflow⁵, and flagging the managed matchings. Therefore, we first identified all authors who published merged publications in the research lines, assuming their continuous appurtenance to said ordinary research activities. By doing this, we obtain a further individual identification of the treatment status, on top of the MBO-eligibility and mere belonging to the IRCCS staff. In a second methodology, we elaborate a balanced panel where the longitudinal units are the journals where all the articles in our data are published, after removing duplicates⁶. The outlets where at least one article belonging to the research lines are published are flagged as part of the research line. Then, we compare those with said flag with those which do not publish Research Line featured articles. Both strategies employ 2x2 TWFE Diff-in-Diffs methodologies: the first one on the unbalanced panel of all researchers, the second one on a balanced panel of journals. The outcome is the yearly number

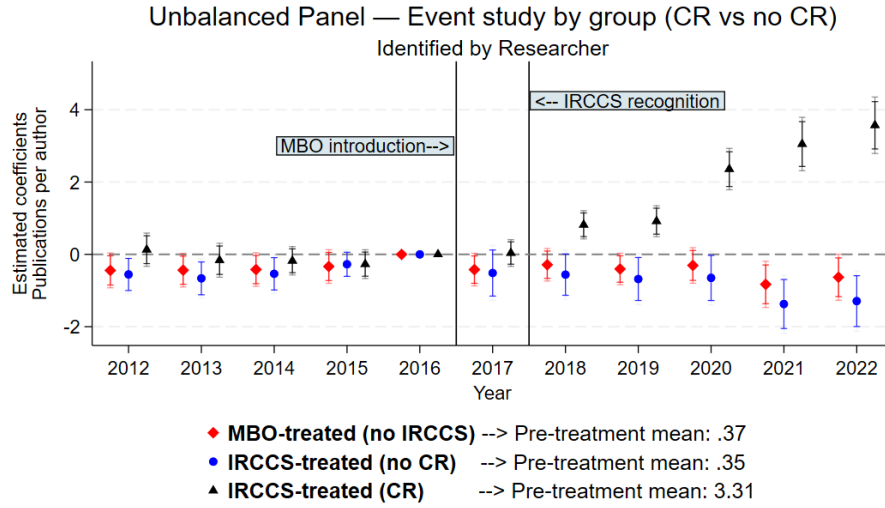
⁵We perform a two-step merging process: an exact merge based on the publication title, and then a fuzzy merge based on correspondence between the scientific outlets, reported in both sources of data, and on similarity of the publication titles (through the Blasnik, 2007 probabilistic matching procedure, by fixing a minimum score of 0.995). Note that the procedure is not able, in any case, to match all the publications provided by the Workflow, as many publications are authored by individuals who are not in our panel (external affiliates, PhD students, research fellows, collaborator and non-employees who still the affiliation anyway). For this reason, we keep our matching score extremely high to avoid false positives, as our outcome can at most be a lower bound nonetheless.

⁶As a matter of fact, the individual panel is based on counts per researcher, so co-authored articles are numbered more than once.

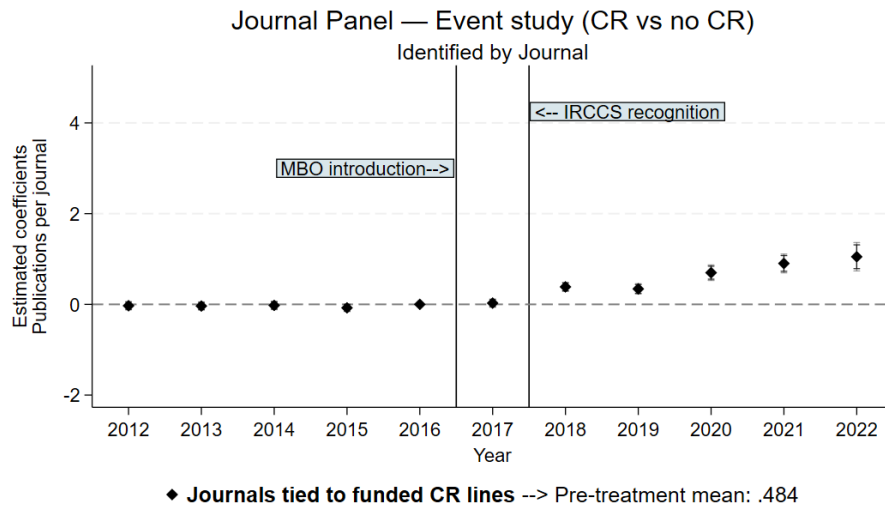
of publications per researcher in the former, and the annual number of publications present in our dataset per journal in the latter. Results are reported in Graphs a) and b) of Figure 2.8.

Graph a) mirrors 2.3: the control group is the full control one, which is compared to the group of MBO-eligible researchers and two groups of IRCCS-list members. However, in this case the further subdivision is not across double-treated and IRCCS-only researchers, but across IRCCS-researchers who published at least once within a flagged Current Research line, and those who did not. Although the number of the latter is way smaller (they are less than 50), we can observe how being part of the IRCCS perimeter, but not operating in any research areas, does not have any improving effect on research productivity, if not even a negative one. The effect on MBO-eligible non-IRCCS MD is the same as the one shown in 2.3. On the other side, we observe how the relevant boost in the activity of IRCCS researchers is basically entirely driven by individuals ascribable to the Ministry-identified research lines. Therefore, the improvement can be tracked to IRCCS ordinary funding. In the journal panel (b), whose coefficients are extremely precisely estimated, the direction of the effect is the same, although reduced in magnitude due to the different construction of the dataset. However, the hospital publications in journals flagged under the research lines increased continuously by almost 100% the pre-treatment mean from 2020. Both graphs display an evident absence of pre-trend.

While this shows the impact of research lines on productivity, we cannot say much on how funds are actually exploited. CR resources are provided based on ordinary activities, hence it is impossible to link them to specific projects or grants. However, according to the Ministry Three-year Plans of 2017-19 and 2020-22, such funds can be used for research activity defined overall, involving clinical trials, personnel costs and technological transfer, like patents (MdS, 2017, 2020). However, the autonomy of the institutions in utilizing ordinary funding may also imply the utilization of such resources for micro-expenses which, however, significantly boost incremental research activity like publication fees, biobanks, software purchases, conference and workshop funding etc.



a) Event Study of the impact of IRCCS recognition + inclusion in Current Research lines on publications, across different comparison groups - by researchers.



b) Event Study of the impact of IRCCS recognition + inclusion in Current Research lines on publications - by journals.

Figure 2.8: Event Study of the impact of IRCCS recognition + inclusion in Current Research lines on publications, identifying treatment groups by researchers (a) and journals (b).

Access to IRCCS funds - Targeted Research

The second set of IRCCS resources researchers have access to are the grants for Targeted Research. These funds are annually issued by the Ministry in the shape of public competitions. Such competitions are addressed to 4 types of entities: the Italian Regions (administrative entities, NUTS-2), the National Institute of Health (*Istituto Superiore di Sanità*, ISS), the Experimental Zooprophyllactic Institutes (*Istituti Zooprofilattici Sperimentali*, IZS), and the IRCCS. All non-IRCCS Italian healthcare institutions can access targeted funds, conditional on being allocated resources at a second step after another entity (usually the Regions) has been awarded the financing. Due to their institutional nature, IRCCSs (and IZSs) can take part to the competitions without the intermediation of the Regions or the ISS.

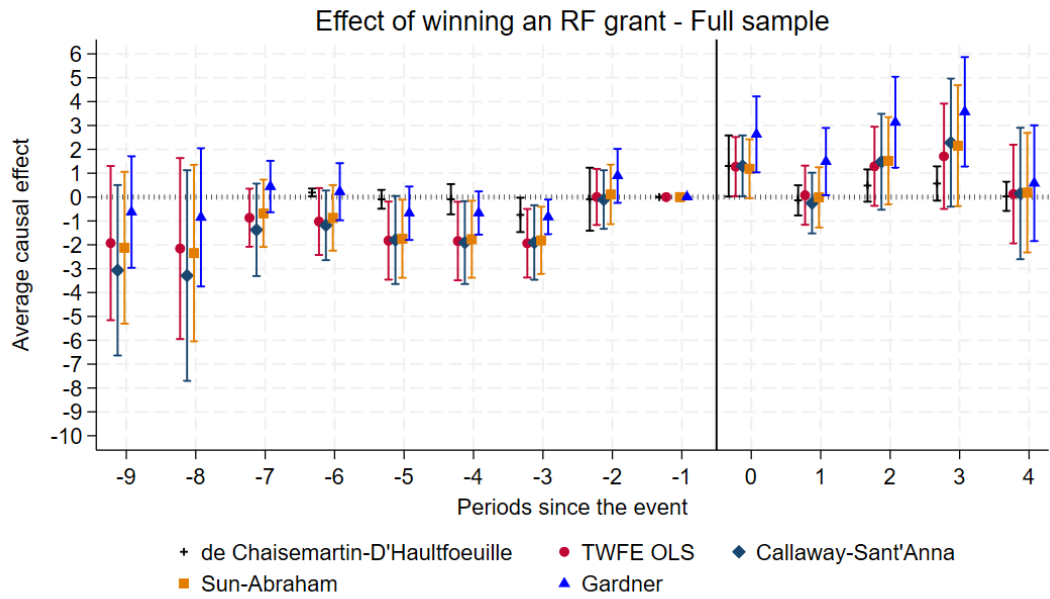
To obtain TR funds, the Ministry must first issue a public call, inviting institutions to apply for a three-year term financing for projects aimed at achieving scientific output useful to steer the National Health Service policies. The projects are submitted at the hospital/institutional level, but they are coordinated and managed by a Principal Investigator (PI) who, at times, is supported by one or more Co-PIs. The PI is a researcher of the institution applying for funds. Contrarily to the Current Research funds, TR funds are conditioned on stricter requirements of management provided in the official documentation of the Three-year Ministerial Plans. The Workflow of the Ministry reports year, awarded sum, title and additional information on all Targeted Research funds allocated yearly to the various entities. We managed to recover all funds awarded to the institution under study from 2018 to 2022 (included a project awarded in 2016, upon allocation to another institution). They are 58 projects, awarded a sum (not always entirely paid to the institution) usually ranging between €300k and €500k; funds awarded in 2022, within the scope of the post-Covid Recovery and Resilience Plan, amount all to €1m or little less. Unfortunately, the Workflow does not disclose the PIs' and Co-PIs' names. To recover them, we searched for publications acknowledging the funds as the source of financing; for the majority of cases, the financing disclosure also reported the name of the person to whom the fund was awarded; however, we double-check by looking at the researchers' CVs or at news outlet on-line⁷. Out of 58 projects, we identified PIs and Co-PIs (when present) for 56 projects. Among them, 12 were awarded to PIs/Co-PIs absent in our dataset (possibly due to not being -yet- stably employed at the institution). We eventually flagged the individuals in our unbalanced panel in according to being or having been a PI or Co-PI in one of the 42 recovered projects, and the year of their awarding. Then, we performed two different staggered dynamic DiD estimates exploiting different methodologies on the impact of IRCCS recognition, in the same fashion as in Equation 2.6. We devised two different group-comparisons: in the first, we estimated the impact of receiving TR funds by comparing those who win them against all the rest of individuals who never do so; in the second one, we compared the same individuals with a truncated

⁷For year 2019, the Workflow reports the CUP, Project Unique Code (*Codice Unico di Progetto*), the identifier required by the law for all public investments; for Targeted Funds on such projects, we just typed the identifier on the OpenCuP database of the Italian Presidency of the Council of Ministries, and obtained the PIs. For other years, such procedure was not possible. In any case, the data gathering sources have been stored and may be provided upon request.

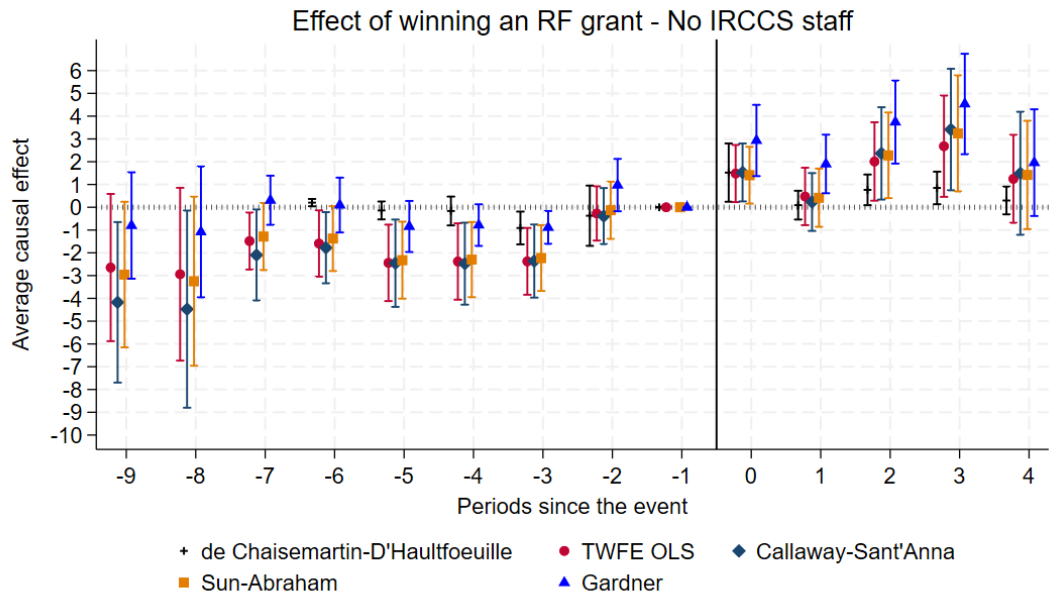
sample obtained by excluding individuals in the IRCCS-perimeter who never receive a TR fund directly. Results are in Graphs a) and b) of Figure 2.9.

When comparing treated units with the rest of the sample (a), we can observe a slightly positive effect, similar in magnitude to the one observed in the staggered Event-Studies of Figure 2.7, although in most specifications is only significant at the treatment period (0) and after 3 lags. When excluding the IRCCS individuals who are not winning awards (b), the estimates recover precision at lag 2 as well. In both cases, the effects revert back to 0 at the 4th period, possibly consistent with the 3 years duration of the finance scheme. The inconsistent and low precision of the estimates possibly signal a not necessary overlapping between PIs and more productive researchers; in addition, the significant effect observed in the main analyses may be more robustly driven by Current Research activities, for which the PI/co-PI figure is non-existent, and by within-group coauthorship, able to boost research output of expert researchers irrespective of their direct awarding of funds as Principal Investigators.

In terms of pre-trend, the pattern mirrors the pre-treatment behavior of estimates displayed in the main staggered event-study (2.7), with a slow convergence towards a common trend starting from a downward biased pre-treatment path: as already argued, this is likely traceable to selection. By contrast, the presence of a higher number of non-significant coefficient possibly mirrors the lower power of the estimates for the treated individuals in the present framework. Such findings suggest that, while the awarding of a grant is evidently relevant in shaping research productivity, in the IRCCS context the publications' boost is feasibly triggered by Current Research ordinary activities mostly. This is even meaningful by considering that those who do not obtain Targeted Research funds may always apply for other competitive research grants, whereas the stability, safety and lack of constraints of the annual Current Research allocations cannot be compensated via alternative solutions.



a) Event-study of the staggered awarding of Targeted Research funds to P.I.s of the IRCCS perimeters (full unbalanced sample).



b) Event-study of the staggered awarding of Targeted Research funds to P.I.s of the IRCCS perimeters (excluding IRCCS researchers without access to TR funds).

Figure 2.9: Event-studies of the staggered awarding of Targeted Research funds to P.I.s of the IRCCS perimeters.

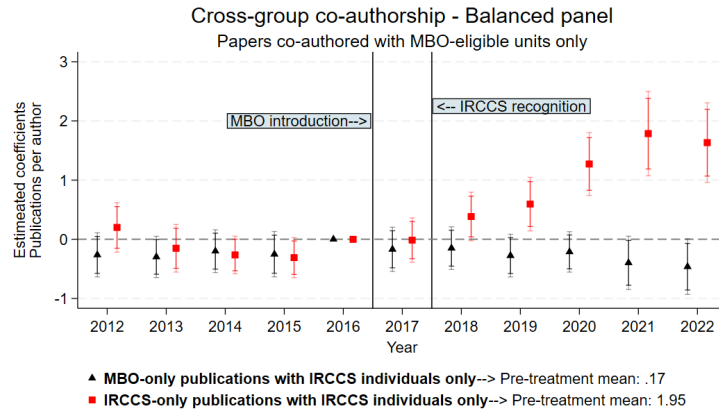
2.8 Networks and Research Quality

It is valuable to assess whether the policies had a significant impact on the nature of the research collaborations stemmed after their implementation, and the impact/quality of the output. We therefore perform two sets of additional estimates in order to tackle two questions mostly: (1) whether and to what extent the policies affected research collaborations across and within the two groups, and (2) how our results relate to research quality of the members of the institution under study, proxied by citations.

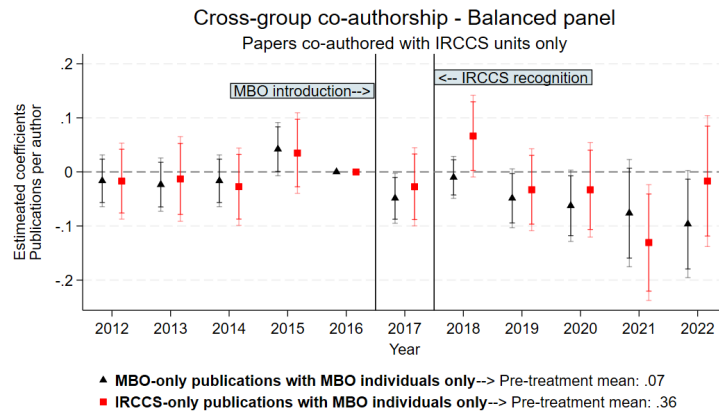
Cross- and within-groups Co-authorship

We begin this section by defining all papers listing, among the co-authors, both a non-academic doctor (subject to MBO) and an IRCCS researcher of the same institution as “networked” publications (elaborated on the whole unbalanced panel sample); this is a decent representation of network effects between the two categories of hospital employees. We make use of the same categorization we employed when controlling for the validity of *SUTVA*, by subdividing networked publications according to the groups’ perspective. *Type A* are those for which at least an IRCCS-eligible individual is a co-author, and there are no MBO-eligible co-authors. The opposite holds for *Type B* papers: at least an IRCCS physician is a co-author, and there are no IRCCS co-authors. Finally, *Type AB* are publications where there are at least both a MBO-eligible and an IRCCS researcher. Therefore, after excluding the overlapping category of the double-treated, we dynamically compare across the remaining group (as in 2.1) the impact of the policies on papers published 1) joint with MBO-eligible MDs only; 2) joint with IRCCS-researchers only; 3) joint with at least one MBO-eligible MD and one IRCCS researcher. The reference group is always the full control one.

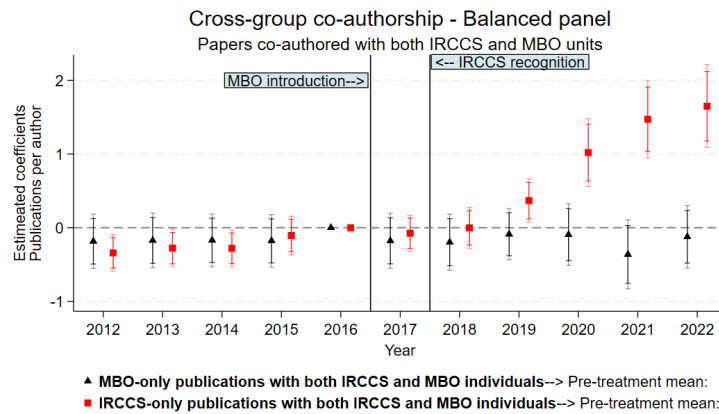
Graphs a), b), and c) in Figure 2.10 reports visually the dynamic estimates for networked publications. Panel (a) shows the event-study for *Type A* (IRCCS co-authorship), Panel (b) for *Type B ones* (MBO co-authorship), and c) for *Type AB* (mixed co-authorship). All three graphs indicate little to no difference in cross-group collaboration rates between the two groups prior to the policies. What is shown is that cross-group co-authorship is limited, as IRCCS-treated groups improve their co-authorship rates within their own group (graphs a) and b)), while MBO-eligible individuals do not substantially increase their co-authored output, neither between- nor within-groups. As already foreseen in Section 2.6, the most relevant spillover effect is brought about by IRCCS researchers within-group, with null or even negative effect (compared to the control group) led by collaboration with MDs. Results are consistent when accounting for the full panel with attrition (A16 in the Appendix).



a) Outcome: publications co-authored with IRCCS units only.



b) Outcome: publications co-authored with MBO-eligible units only.



c) Outcome: publications co-authored with both MBO-eligible units and IRCCS researchers.

Figure 2.10: Dynamic impact of the policy implementation on annual cross-group publications in the balanced panel.

Citation Impact

A further relevant question is whether a greater number of publications also triggered a positive shift in the influence and quality of works, or it came at the expense of it. We analyze the policy effect on citations, employed as proxy of publications' impact. We do that through the re-modeling the IRCCS dynamic equation (Equation 2.3) in order to adapt it to a set of different outcomes and weights. First, as discussed already in other studies such as the ones by Azoulay et al., 2011 or Waltman and van Eck, 2013, we cannot estimate the effect on citations without accounting for the publications' age. Thus, we build a yearly discounting term to control for the period when the citations were gathered, which is Fall 2024 ($[2024 - t]^{-1}$). After doing that, we normalize each paper by time, via multiplying its count of citations by the discount term, and build a yearly aggregate individual impact weight by summing up all yearly normalized citations for every researcher. This is the first normalized variable we obtain, which we use first as a Inverse Probability Weight in a regression of the treatment on the count of publications. The second outcome is obtained from the sum of all non-discounted yearly citations received by papers published by a given author in a given year; after aggregating them, we normalize such total by time, making use of the discount term above. Eventually, we build an averaged measure of publications' quality, by dividing the number of total (discounted) citations of papers published in a given year by an author by the number of total papers published by the very same author. In these regressions, as in the latest ones, the IRCCS-treatment is assigned only to IRCCS researchers non-eligible to the MBO, while the MBO-treatment also considers the double-treated.

These results are displayed in dynamic terms in the event-studies in Figure 2.11, displaying no significant anticipatory behavior or serious PT violations. Apparently, by (inversely) weighting publications by the number discounted citations (a), the effect of the IRCCS' recognition is amplified for more structured researcher, although the effect of the policy is lagged, and takes off in 2020 only, although they range between 5 and 7 citations. By looking at the sum of (discounted) citations as outcome (c), the IRCCS recognition seems to have boosted the number of quotes for IRCCS physicians, by numbers ranging around 20 citations in 2020-2022. However, the IRCCS group's post-2018 average number of (discounted) citations per publications shows a major and significant decrease from 2019 (e).

If we look at the MBO-scheme, the picture is even more insightful. We observe indeed a negative, slightly significant effect on the number of MBO-eligible individuals' publications in the IPW-weighted regression, although this occurs only after the IRCCS recognition (clearly due to the bias induced by the comparison with IRCCS individuals), as in the first year of the MBO implementation the impact seems to be positive and significant (b). Quite surprisingly, and specularly to what emerged for the IRCCS-affected individuals, the impact on the average number of discounted citations over yearly publications is quite substantial, and increasing over time (f).

Such findings leads to the conclusion that, while the two concomitant policies have an established effect on physicians' research productivity, which is positive for the structured selected academics and basically null

for the incentivized MDs, the quality impact is basically the opposite. The aggregate increase (decrease) in the IRCCS- (MBO-) treated individuals' normalized citations is clearly driven by the shift in the raw number of publications, while the average quality of the works seems to be neglected by the researchers who can directly access funding, and maximized by the MBO-incentivized MDs. While prior beliefs would suggest that, due to the embedded incentive structures, core researchers would focus on quality over quantity upon accessing funds, it looks like that, once achieved the possibility to increase their resources, they fuel their scientific output numbers at the expense of the influence brought about by their research. On the other side, MBO-eligible individuals seem not to carry over projects indifferently in bulks, but even without significantly raising their publication rates, they tend to focus on the quality of their research. This could be linked to the fact that the performance-based rewards are not only based on quantity alone, but also on the impact factor of the journal where publications are issued.

Strategic behavior of MDs

In the analyses reported insofar we came to the conclusion that the MBO policy does not directly impact the scientific productivity of the affected physicians; on the contrary, the compound effect of the IRCCS recognition on the institution seems actually to trigger a relative negative effect of the incentive scheme when combined with the increase of public funding. However, two main evidence we gathered may actually suggest that the MBO was not fully neutral for the MDs' behavior: first, we see that many MDs were subsequently enrolled into the IRCCS perimeter, and the anticipatory behavior in the event-studies performed for the staggered DiD actually highlights the presence of an evident productivity-based selection process. In such regard, the implementation of the MBO might have incentivized doctors with more research-oriented embedded incentive structure to pursue scientific activity, re-directing them towards the IRCCS perimeter and, up to a second moment, triggering a productive pattern boosted by the additional resources. Second, while IRCCSs' researchers clearly display an obvious raise in overall (discounted) citations, due to their more numerous scientific outputs, the average numbers of citations per paper is increased for MBO-affected individuals, while the effect seems negative for IRCCS-researchers after the IRCCS recognition. This may either mean that MDs value quality over quantity, or that they put in place a strategic publication-oriented research behavior with the purpose of achieving the right amount of output, weighted by the proper Impact Factor and accounting for the reduction due to internal co-authorship, in order to obtain a pre-fixed monetary sum in accordance to their preferences.

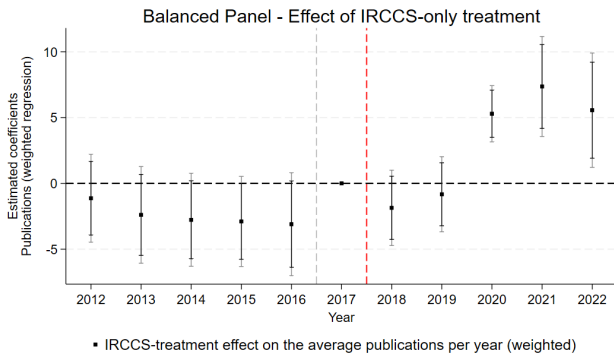
Our last check concerns the possibility, for MDs, to put in place strategic behaviors in order to achieve the exact amount of MBO bonus without really improving their overall research productivity in terms of publications. As a matter of fact, while we observed a lack of effect of the MBO implementation on the general output of the treated ones, we still found significant differences in the average quality of papers published by MDs after the MBO compared to the ones published by the IRCCS staff, with the latter being decreasing

after the additional funding and the former being positively affected by the policy. The current check is made necessary in order to assess whether the finding is lead by a trade-off between quantity and quality or by a gaming strategy performed by doctors. The bonus is indeed directly proportional to the Impact Factor of the journals where articles are published. On top of that, even though the bonus is decreased for publications co-authored with internal authors, the positive spillover effects shown in the previous section may be a proof of the fact that individuals could actual free-ride publications in order to achieve a reduced bonus, which may be not reached at all in absence of collaborations, in doing this increasing their payout. In our dataset, we have information about the amount of the yearly payout received by the individuals subject to the policy, the number of the eligible publications, and the total amount of the Impact Factors of the publishing journal discounted by the co-authorship weights. A peculiarity of our dataset is that, even though the bonus is only paid to Medical Directors, the computation of the above metrics is made for all researchers active in the institution, Faculty Members included, even though they do not perceive any related direct benefit. While the ideal setting would be that of a Difference-in-Differences strategy like the ones employed so far by performing estimates on different outcomes, the absence of yearly Impact Factor data for all the journals where paper were published before 2017, constrains our analysis. That means, we do not possess a hypothetical computation metrics for the assignment of the bonus prior to the implementation of the MBO policy itself. On the other hand, we can rely on a TWFE estimation where we can compare the metrics computed for the MDs and those computed for the faculty members, in the time-span for which we have this information available (2017-2022). The underlying assumption is that, absent the bonus, the research output of faculty members should not depend, in terms of bibliometric structure, on the weights foreseen by the incentive scheme, nor the payout. We hence estimate a TWFE equation similar to Equation 2.3, with varying comparison groups and all the rest, but instead of the interaction between *Post2018* and *IRCCS_i*, we just keep the time-invariant treatment dummy flagging the belonging to the MBO scheme. We regress 4 different outcomes: the Payout in €, the total sum of discounted points of Impact Factor, the number of papers deemed as eligible for publications (which differ from the whole number retrieved from Web of Science), and the difference between the actual number of publications and the eligible ones. The discrepancy between the latter two numbers is due to different motivations: first, the number of eligible paper is collected by the company who compute the metrics and assign the bonus. However, such number is double-checked by the individuals themselves, whom are send list of papers to be corrected and verified, in order to add forgotten or more recent papers or remove those erroneously linked to the wrong author. Such self-reporting practice leads to numerous opportunistic behaviors which cannot be entirely controlled by the metrics-assessing firm, which only performs random checks to verify the alignment between what is reported in the payout document and the actual research productivity of individuals. There are, thence, numerous mis-specifications due to either researchers not updating thei publications since they are potentially uninterested in obtaining the bonus (i.e., faculty members) and physicians pumping research output to increase their payout. Some researchers claim indeed that papers written by homonyms are authored by them, as the

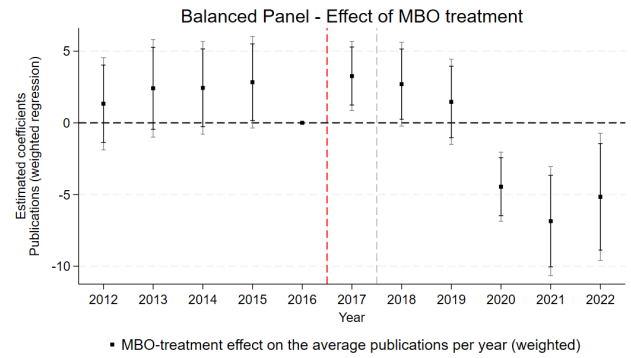
firm assign the eligible documents on a surname-based criterion. Such behavior is enabled by the controls made randomly, which allows risk-taking behavior. In order to avoid potential estimates to be driven too much by the downward-reviewing attitude of self-reporting negligence of faculty members, we drop from the sample all individuals with zero eligible documents over the whole analyzed sample. Equally, supposing that a gaming strategy still requires a minimum degree of research-oriented attitude, we remove from the sample all individuals with zero actual publications in the time-span. Results are reported in Table 2.5. We can observe how, for all the payout-related variables, how the MBO-treated individuals perform worse than their unpaid counterpart, as the coefficients all follow the same patterns observed for actual publications in the baseline analysis for the MBO-policy incentive. MBO-treated perform way worse in terms of Payout, Impact factor and eligible papers. Only double-treated, and only when compared to other MDs or to the pure control group, display positive and significant coefficients. The only peculiar pattern is observed for the case of the Delta between actual publications and self-reported eligible documents. There we observe a null effect of being MBO irrespective of the selected comparison, while both MBO and DT units display higher deltas with respect to IRCCS-only and pure control. On the other side, the double-treated ones seem to have lower deltas with respect to both MBO-only and control units. In such regard, it would seem like that the self-reporting bias notwithstanding, MDs still do not put in place a gaming strategy with the aim of perceiving the bonus, and may actually value quality over quantity in the scope of an incentive structure which is less oriented towards research activities.

	(1) MBO vs. non-MBO	(2) DT vs IRCCS	(3) DT vs MBO	(4) DT vs control	(5) MBO vs IRCCS	(6) MBO vs control
Pay Out (€)	-1037.37939***	-1466.69556***	1780.15076***	1628.63623***	-3556.25537***	-269.15292**
(SE)	(389.74774)	(223.09830)	(188.85347)	(200.98660)	(200.08897)	(133.17281)
R ²	0.610	0.668	0.639	0.635	0.702	0.483
Mean	1196.52	2216.09	2216.09	2216.09	520.66	520.66
Sum of weighted IF	-0.05247	-11.46735***	3.66103***	3.92988***	-17.96440***	0.14813
(SE)	(2.09297)	(3.33887)	(0.93505)	(0.66406)	(4.41293)	(0.76541)
R ²	0.336	0.348	0.372	0.613	0.339	0.191
Mean	3.16	5.68	5.68	5.68	1.48	1.48
Eligible Papers	-1.25746	-4.68687***	4.03192***	3.58589***	-9.94389***	-0.69990***
(SE)	(0.98802)	(0.83610)	(0.34259)	(0.38195)	(0.94491)	(0.25696)
R ²	0.581	0.615	0.687	0.669	0.637	0.571
Mean	2.82	4.88	4.88	4.88	1.46	1.46
Delta (Pubs-Eligible)	0.17500	3.65820***	-2.78376***	-2.37526***	7.48813***	0.54775***
(SE)	(0.69514)	(0.66451)	(0.27954)	(0.30812)	(0.73891)	(0.18284)
R ²	0.406	0.477	0.586	0.553	0.471	0.520
Mean	-1.03	-1.88	-1.88	-1.88	-0.47	-0.47
N	8651	4186	4224	3612	5017	4465
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Panel	Full	DT and IRCCS	DT and MBO	DT and control	MBO and IRCCS	MBO and control
	(no 0s)	(no 0s)	(no 0s)	(no 0s)	(no 0s)	(no 0s)
Time Range	2017–2022	2017–2022	2017–2022	2017–2022	2017–2022	2017–2022

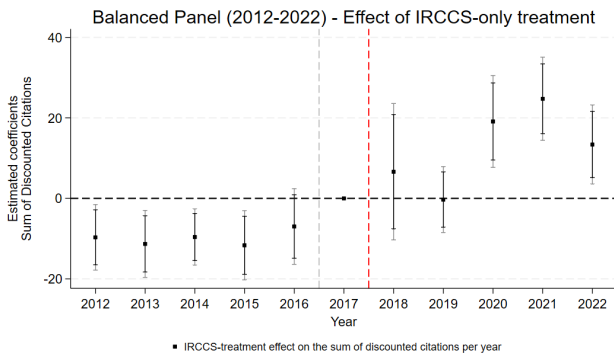
Table 2.5: Regressions between enrollment into IRCCS perimeter and several factors.



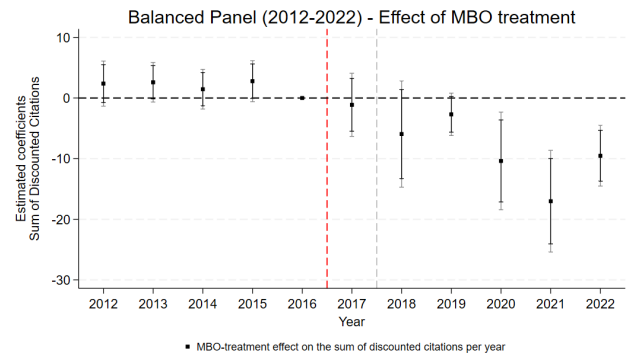
a) IRCCS-effect on publications weighted by discounted citations (as of Fall 2024).



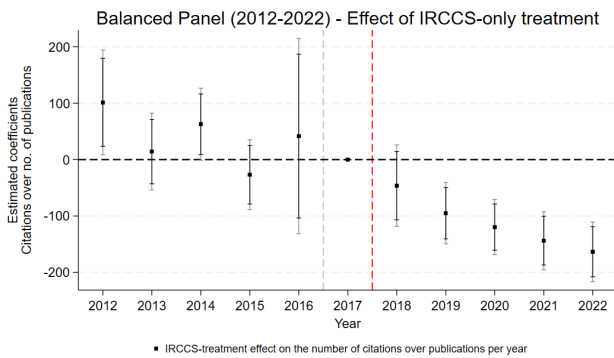
b) MBO-effect on publications weighted by discounted citations (as of Fall 2024).



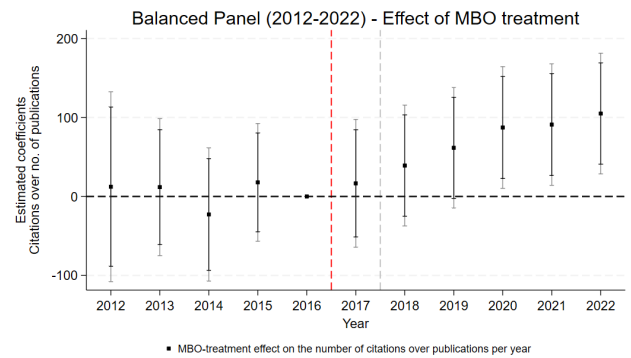
c) IRCCS-effect on the total yearly sum of discounted citations (as of Fall 2024).



d) MBO-effect on the total yearly sum of discounted citations (as of Fall 2024).



e) IRCCS-effect on the average number of discounted yearly citations (as of Fall 2024) per publications.



f) MBO-effect on the average number of discounted yearly citations (as of Fall 2024) per publications.

Figure 2.11: Event-Studies representing the dynamic estimates of the IRCCS recognition and MBO-policy impacts on the impact of published papers, proxied with yearly citations measured as of Fall 2024.

2.9 Conclusions

This paper examined two concurrent strategies to trigger research activity in a healthcare institution: direct monetary incentives based on performance for individual non-academic physicians versus increased public funding linked to institutional support. Using individual-level data from a major private Italian hospital, we found that the performance-based monetary reward (MBO) did not lead to significant improvements in the research output of clinicians, who previously had limited publication activity, and were the main target of said intervention. The MBO seemed to spur a positive impact only on those MDs who were also involved in the research activity financed through the new accessible funding. As a matter of fact, the increase of public research funds through IRCCS recognition produced a huge boost in the productivity of the academic physicians included in the research perimeter. Our findings highlight the relative importance of incentives in driving research productivity, which can be apparently improved through individual rewards only in presence of adequate, material resources. As non-academic doctors did not responded as strongly as expected, when incentivized to do so by monetary gains, our research shows instead how the different incentive structures embedded in the main groups who make up the hospital personnel under question play a major role in shaping research output patterns; indeed, the (substantially) increased number of works published by individuals already affiliated to the academic institution, seem to prove so. The findings demonstrate that lack of proper funding was the main obstacle to academic research engagement, and that mere monetary rewards are not able to narrow the gap between the objectives of non-structured researchers and those who account on that to progress in their professional careers. We also document that the combination of resources and incentives may nonetheless yield interesting outcomes with respect of cross-group collaboration: as better funding allows scientific opportunities, individual incentives should ensure such opportunities to be exploited; in our case, however, to be triggered is mostly within-group collaboration, rather than the one between groups. The absence of gain from such collaborations ought not to be underestimated in the long-run, as they could lack the scope of the synergies required to foster the translational impact of medical research (i.e., clinical intuitions shaping academic inquiry and the opposite).

Importantly, we find ambiguous effects on research quality. The increase in research output went along by a major boost in overall citation impacts for IRCCS researchers, and a substantial decrease for MBO-treated physicians, even in relevant terms. However, the direction this effect was evidently driven by the shift in the raw numbers of scientific works: as a matter of fact, we observe a persistent decrease in the number of discounted citations per paper for IRCCS-researchers after the policy, and a positive, a bit lagged but significant trend in the average (discounted) impact of the publications of MBO-incentivized physicians, underlining their greater value for quality over quantity, which may be caused by either improvement of intrinsic attitudes towards valuable research, or by gaming strategies elaborated in concomitance with the bonus scheme designed by the management, which takes into account the impact factor of publishers. Moreover, the policies appeared to foster greater collaboration between clinicians and academics, which could

further enhance the translational value of the research . This integration of efforts is particularly valuable in healthcare, where bridging the gap between frontline practice and research can accelerate innovation and implementation of findings.

This paper contributes to policy discussions on how to stimulate innovation in public institutions and healthcare organizations. It also offers evidence that targeted performance incentives can only be an effective tool to unlock research potential, but only in presence of *actual* resources being provided to those who require them. For hospital administrators and policymakers, the results may imply that investing in a culture of research via both feeding long-term professional incentives relying on career progression and scientific curiosity (properly rewarded) may be more effective than short-run performance-based remuneration. It is indeed true that incentives ought to be aligned with multifaceted targets to avoid strategic behaviors which may possibly bring about detrimental societal repercussions. The main limitation of this study is its focus on a single (albeit large and relevant) institution, over a horizon which is long enough to observe individual behaviors, but not wide enough to capture the full unfolding of long-term research outcomes. Longer-term consequences on research quality, career trajectories, and patient outcomes, although beyond the current scope of this article, should be the main target for future related study.

It is worth remarking that the presented evidence stems from a single large private hospital, featured by a strong baseline research orientation and a relatively complex organizational structure (it is one of the major Italian hospitals, and excellent in both research and clinical outcomes globally). For this reason, the estimates should be interpreted as internally valid mostly for healthcare settings that share comparable structural and governance dimensions; they ought not to be accounted as universally applicable effects. The estimates hint that research is a highly input-intensive activity, whose responsiveness to incentives and funding mechanisms is substantially dependent on existing human capital and skills, research governance, and material resources. On top of that, the heterogeneity patterns documented in the analysis provide potential research guidance on the external validity of our study. The differential responses between academic and non-academic physicians, as well as between single-treated and double-treated individuals, indicate that explicit rewards are more likely to work when embedded in broader research-oriented settings, whereas stable public funding plays a paramount role in bringing forward research output among research-oriented professionals. Our findings point towards the direction that similar policy mixes may be effective in other large hospitals or research-intensive institutions, but only in presence of adequate existing research infrastructure, and thus they are less likely to generate comparable effects in settings with weaker baseline research resources or weaker organizational incentives in terms of scientific output. Broader studies, able to better frame the external validity of the estimates to different conditions, such as public healthcare facilities or geographical heterogeneities, should be pursued. In any case, our results offers a compelling case that, within an imperfect market environment like the healthcare one, managerial triggers on incentives can only play an important role in fostering innovation if properly traded off with the necessities required by the need for proper funding.

2.A Appendix

Target	MDs without faculty structured appointments
Object	Full papers published between Jan 1 and Dec 31 (each year)
Payment per IF point	€500 per IF point
Max reimbursable IF	20 IF points per year
Max annual gross	€10,000
Author role (institution affiliation)	Bonus share
First or only author from the institution	100%
First author with other institutional co-authors	60%
Co-author on a paper first-authored by an institutional member	40%
Co-author on a paper first-authored externally	50%

- **IF** = Impact Factor of the Journal.

Table A1: Description of the MBO-rewarding scheme.

Requirement	Description	Evaluator
Scientific excellence	Proven research productivity in a specific biomedical area	Scientific Committee (Ministry of Health)
Healthcare quality	High-level and specialized clinical care	Ministry of Health / AGENAS
Integration	Strong integration between research and healthcare activities	Ministry of Health / AGENAS
Qualified personnel	Presence of qualified scientific and healthcare staff	Ministry of Health
Infrastructure	Adequate facilities and technological resources	AGENAS
External evaluation	Periodic assessment of all requirements	Ministry of Health
Recognition validity	Five-year renewable accreditation	Ministry of Health

Table A2: Description of the IRCCS' requirement to achieve recognition (Source: DL 288/2003).

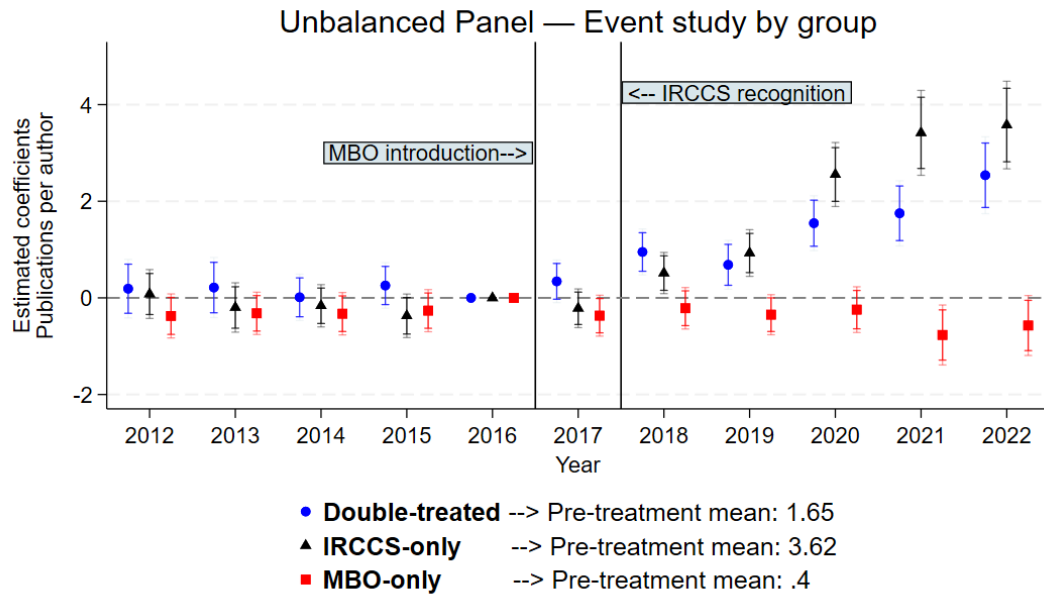
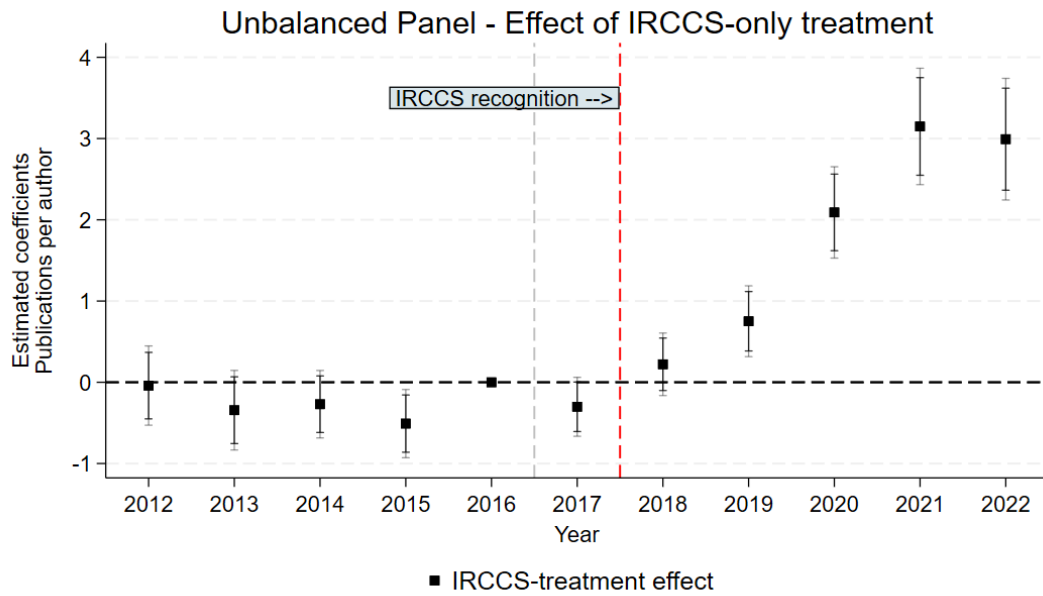
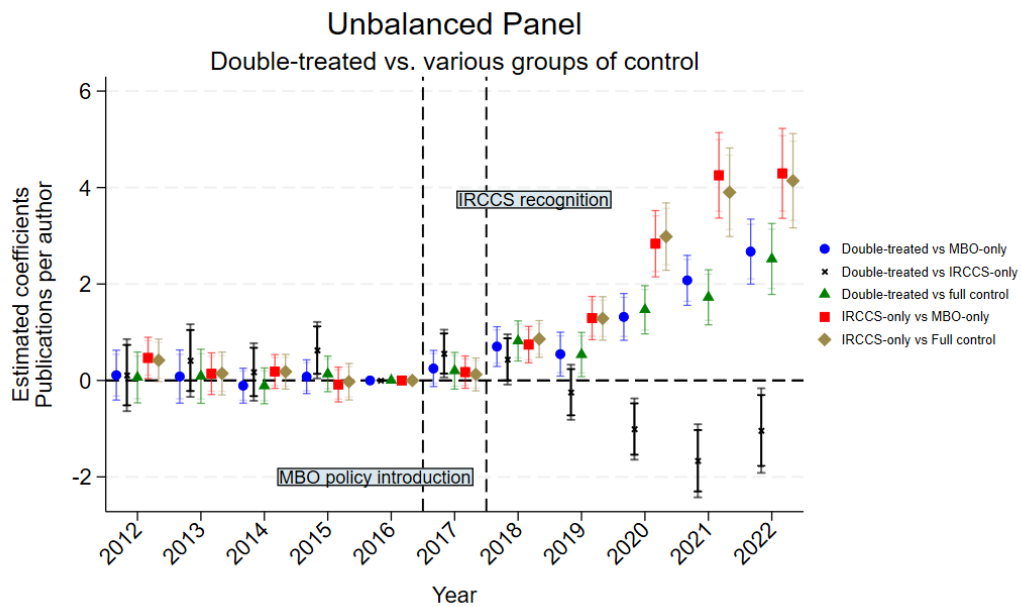


Figure A1: Event-study of the MBO joint with IRCCS recognition policy effect on publications across the different groups, and with respect to the full control group. - Full unbalanced panel.

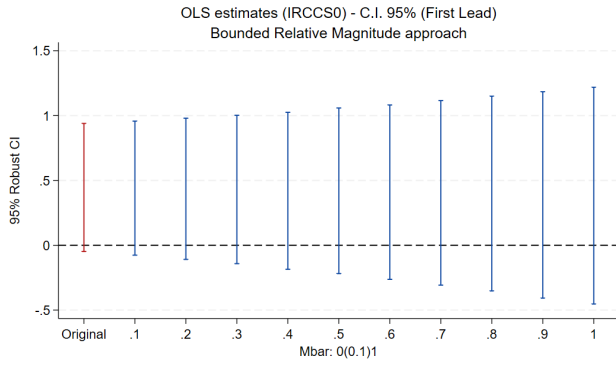


a) Event-study of IRCCS recognition effect on publications for IRCCS-only treated individuals compared with all the other units.

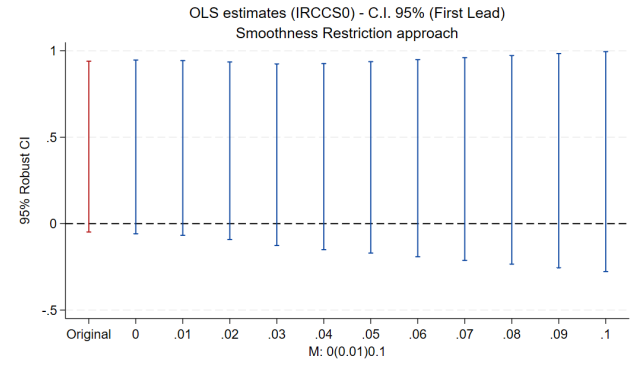


b) Event-study of IRCCS policy effect on publications for comparisons across different groups.

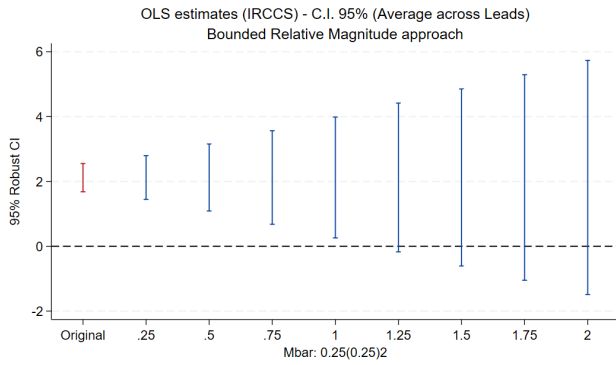
Figure A2: Event-studies of IRCCS policy effect for the unbalanced panel.



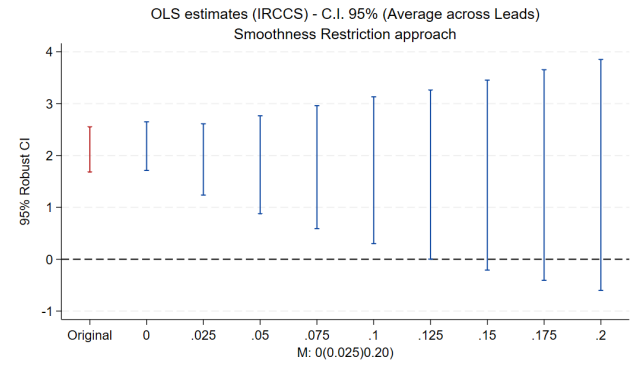
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.

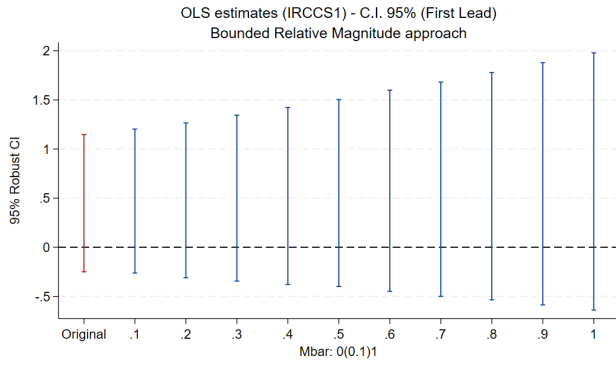


c) BM approach for the significance of the average across all leads.

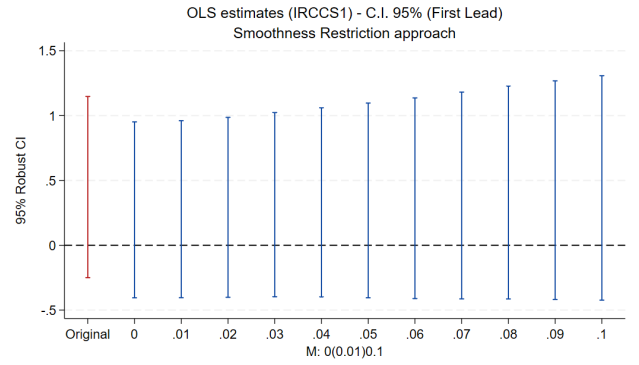


d) SR approach for the significance of the average across all leads.

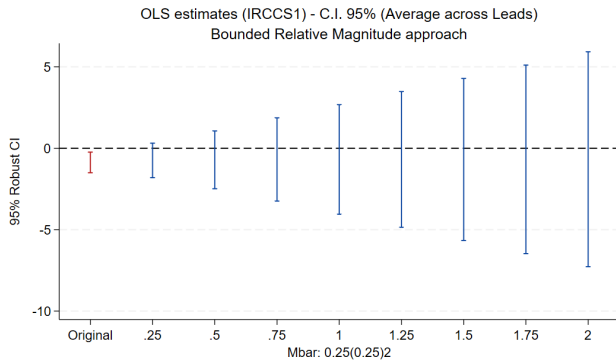
Figure A3: Honest DiD robust confidence sets in the comparison between double-treated and MBO-only units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).



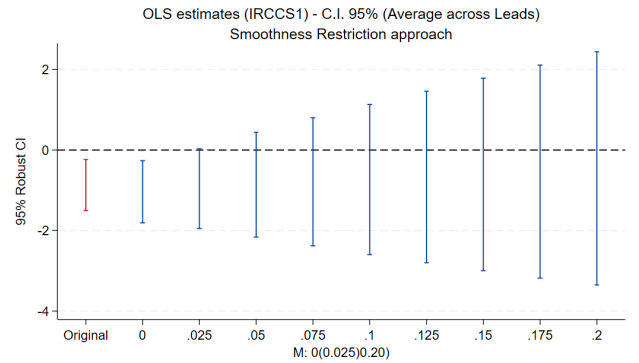
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.

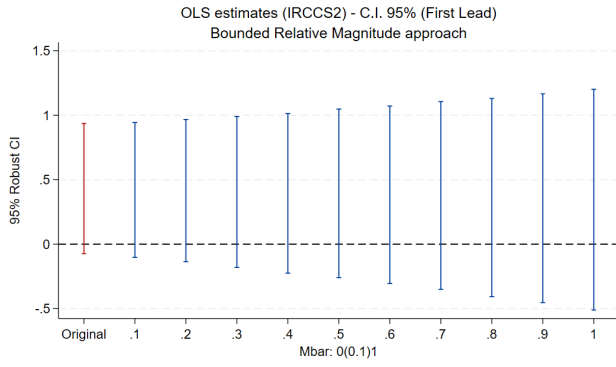


c) BM approach for the significance of the average across all leads.

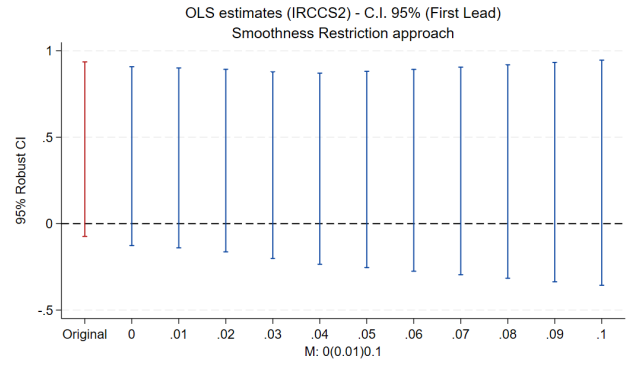


d) SR approach for the significance of the average across all leads.

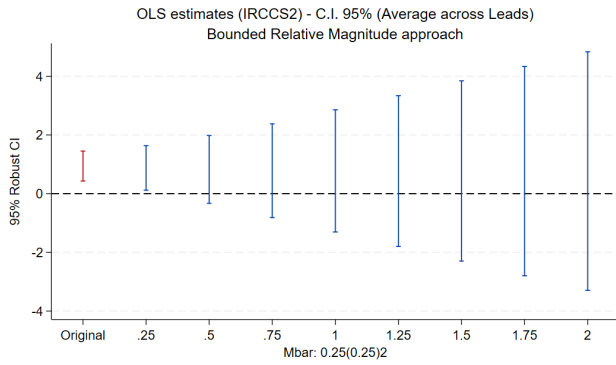
Figure A4: Honest DiD robust confidence sets in the comparison between double-treated and IRCCS-only units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).



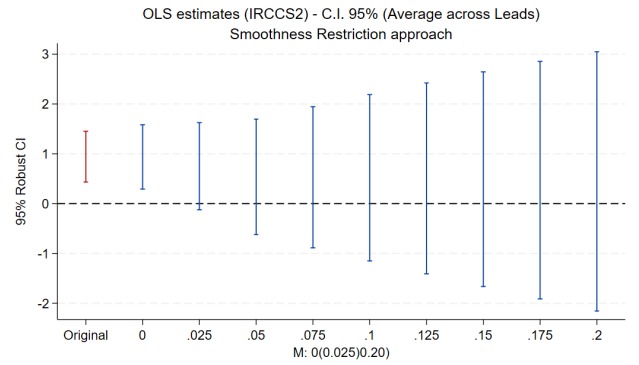
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.

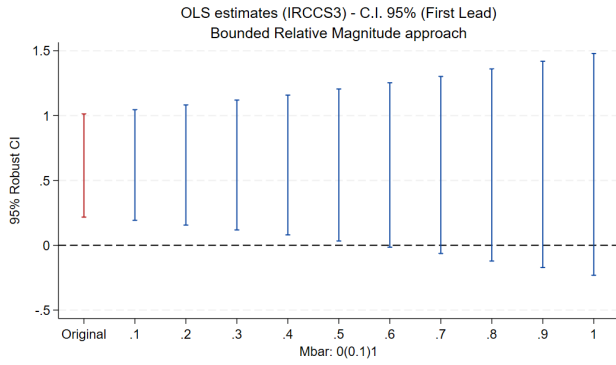


c) BM approach for the significance of the average across all leads.

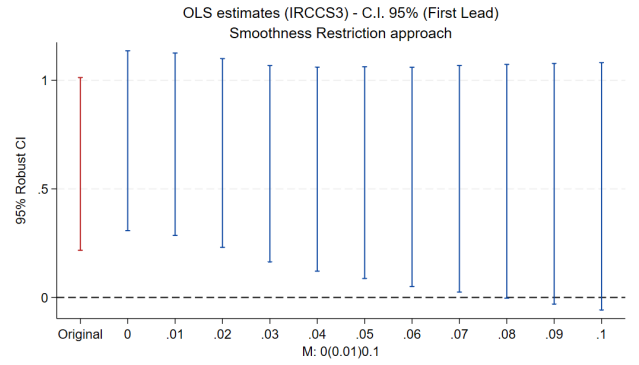


d) SR approach for the significance of the average across all leads.

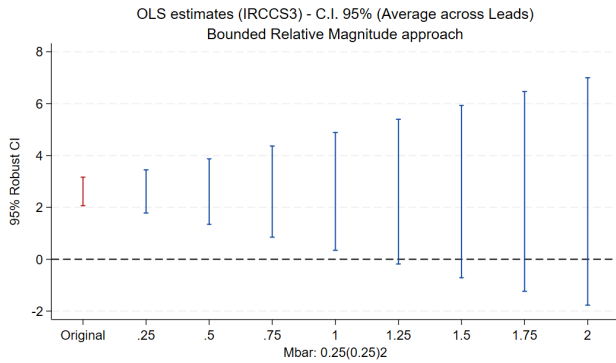
Figure A5: Honest DiD robust confidence sets in the comparison between double-treated and pure control units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).



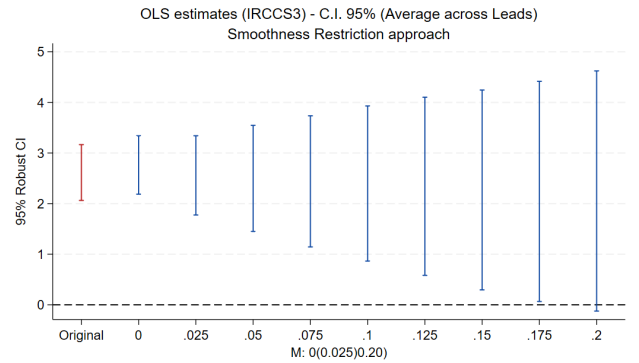
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.

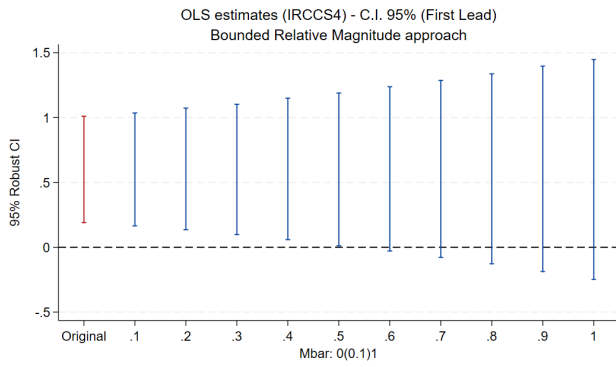


c) BM approach for the significance of the average across all leads.

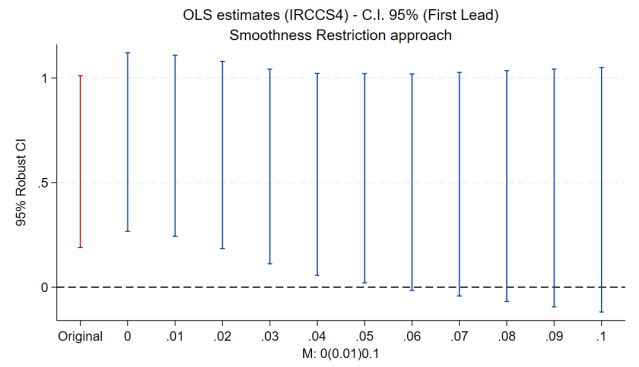


d) SR approach for the significance of the average across all leads.

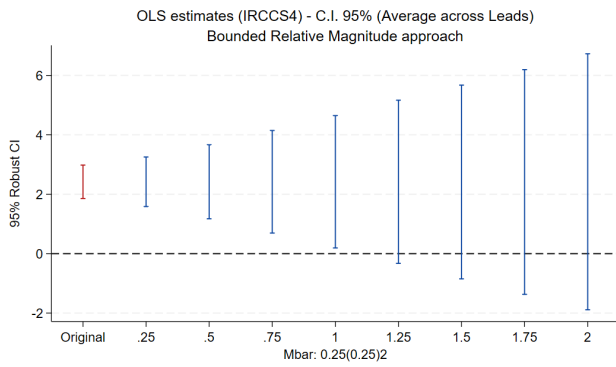
Figure A6: Honest DiD robust confidence sets in the comparison between IRCCS-only and MBO-only units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).



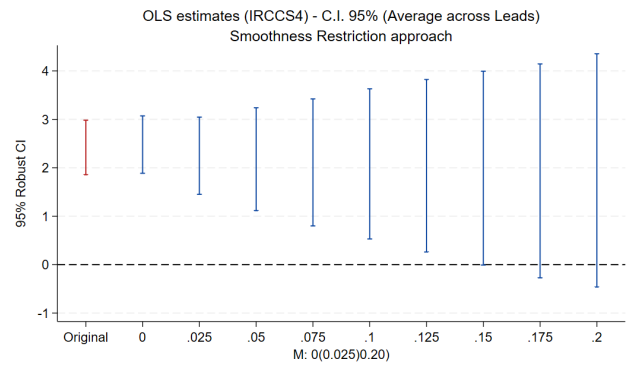
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.

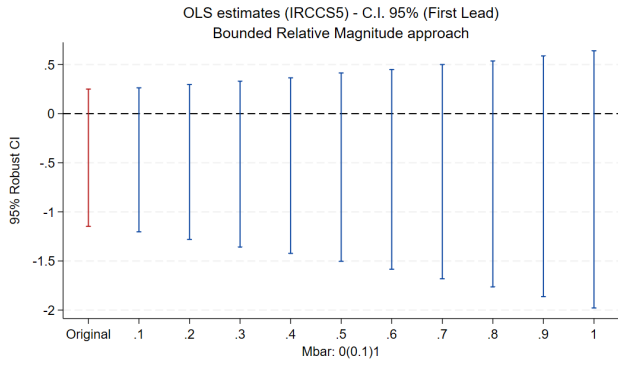


c) BM approach for the significance of the average across all leads.

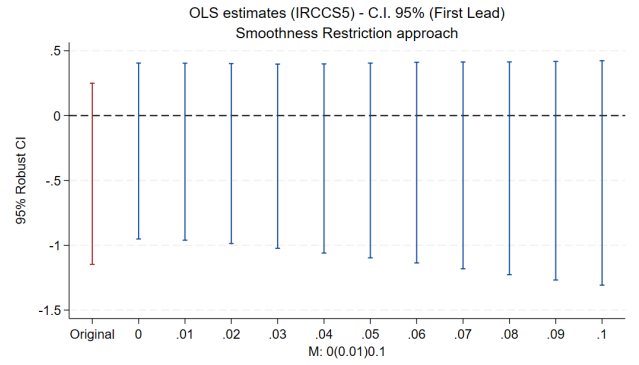


d) SR approach for the significance of the average across all leads.

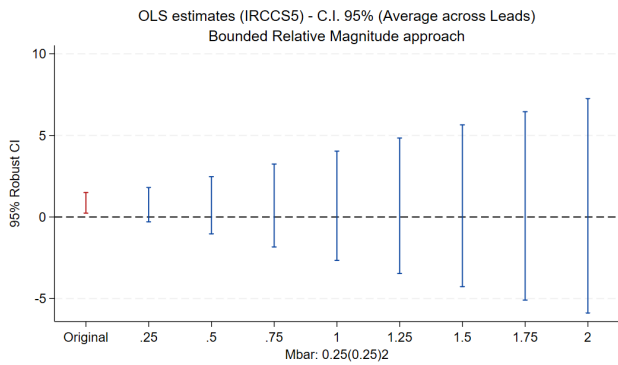
Figure A7: Honest DiD robust confidence sets in the comparison between IRCCS-only and pure control units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).



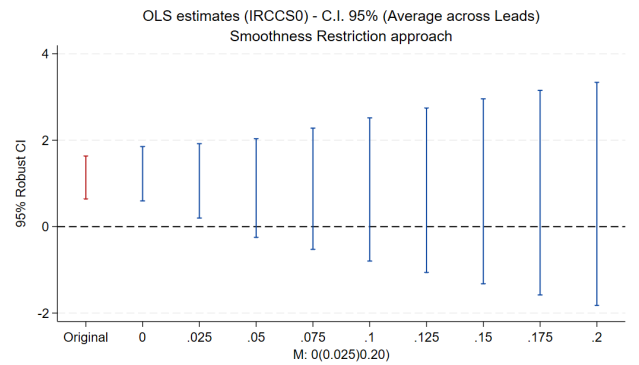
a) BM approach for the significance of the first lead.



b) SR approach for the significance of the first lead.



c) BM approach for the significance of the average across all leads.



d) SR approach for the significance of the average across all leads.

Figure A8: Honest DiD robust confidence sets in the comparison between IRCCS-only and double-treated units, estimated the or overall IRCCS effect by adopting with different methodologies (Bounded Relative Magnitude - BM: a) and c). Smoothness Restriction - SR: b) and d)) and for different quantities of interest (coefficient on the first lead of the event study, a) and b). Average across all post-treatment leads, c) and d)).

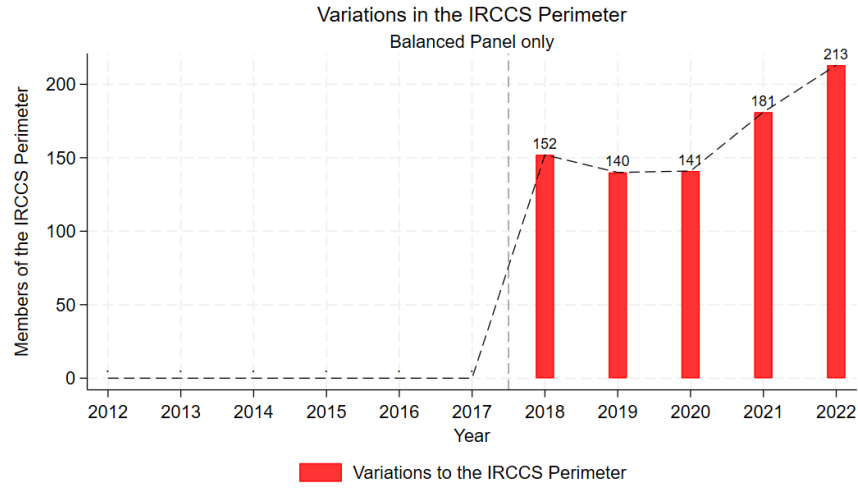


Figure A9: Evolution of the units “lately selected” into the IRCCS perimeter.

	(1) IRCCS vs. others	(2) DT vs MBO	(3) DT vs IRCCS	(4) DT vs PC	(5) IRCCS vs MBO	(6) IRCCS vs PC
Publications	3.0454***	1.4331***	-1.1422**	1.3508***	3.3460***	3.2307***
(SE)	(0.3986)	(0.4110)	(0.4977)	(0.4145)	(0.4292)	(0.4338)
N	5390	2252	1705	2059	3325	3132
R ²	0.789	0.620	0.768	0.600	0.808	0.798
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Mean	4.30	2.51	2.51	2.51	4.30	4.30
Panel (Balanced)	No late inclusion	No late inclusion	No late inclusion	No late inclusion	No late inclusion	No late inclusion
Time Range	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022

Table A3: Impact of IRCCS recognition on Annual Publications in the sample obtained after excluding units later included in the IRCCS perimeter (Difference-in-Differences).

	Core IRCCS	Late IRCCS	p-value
Age	52.8 (7.6)	50.6 (7.0)	0.025**
Publications	4.2 (5.6)	1.5 (2.1)	0.000***
Citations	182.8 (518.5)	48.5 (135.8)	0.015**
<i>Categorical variables (column %)</i>			
<i>Gender</i>			
Female	29.7%	47.9%	
Male	70.3%	52.1%	
<i>Status</i>			
Medical Director	0.0%	0.0%	
Healthcare Professions Manager	21.3%	36.2%	
Sanitary Director	0.0%	0.0%	
Faculty Clinician	2.6%	3.2%	
<i>Department</i>			
Diagnostic Imaging	20.0%	7.4%	
Emergency & Anesthesiology	7.1%	7.4%	
Cardiovascular Sciences	10.3%	10.6%	
Women, Children & Public Health	12.9%	16.0%	
Lab Sci & Infectiology	7.7%	10.6%	
Gastro/Nephro/Endo	21.9%	11.7%	
Aging & Neurosciences	14.2%	35.1%	
Directorate General	0.0%	0.0%	
Clinical Governance	1.9%	0.0%	
Health Governance	3.9%	1.1%	

Table A4: Difference in characteristics (mean, SD in parentheses, and p-value of the t-test of the diff. in means) and composition (%) of the group of researchers included in the IRCCS perimeter in 2018 (early) and those enrolled later.

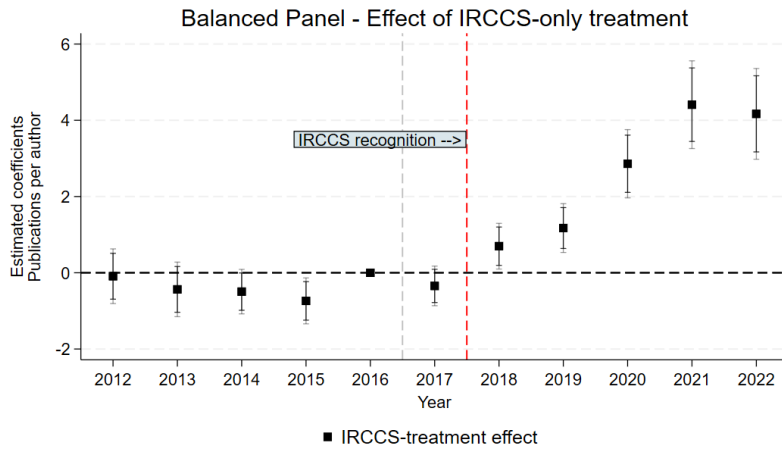


Figure A10: Event-study of IRCCS recognition effect on publications. Units included in the IRCCS perimeter in any year after 2018 are excluded from the estimates.

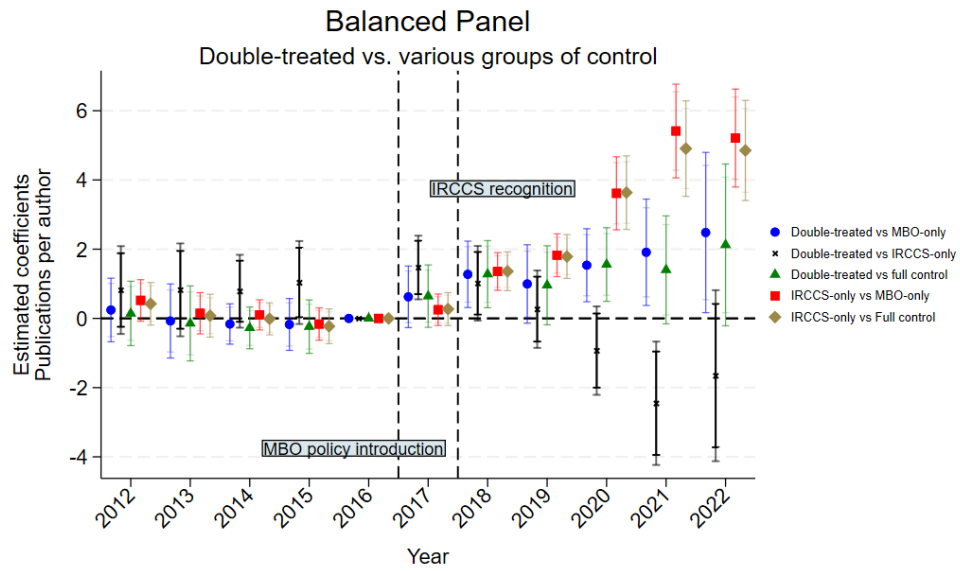


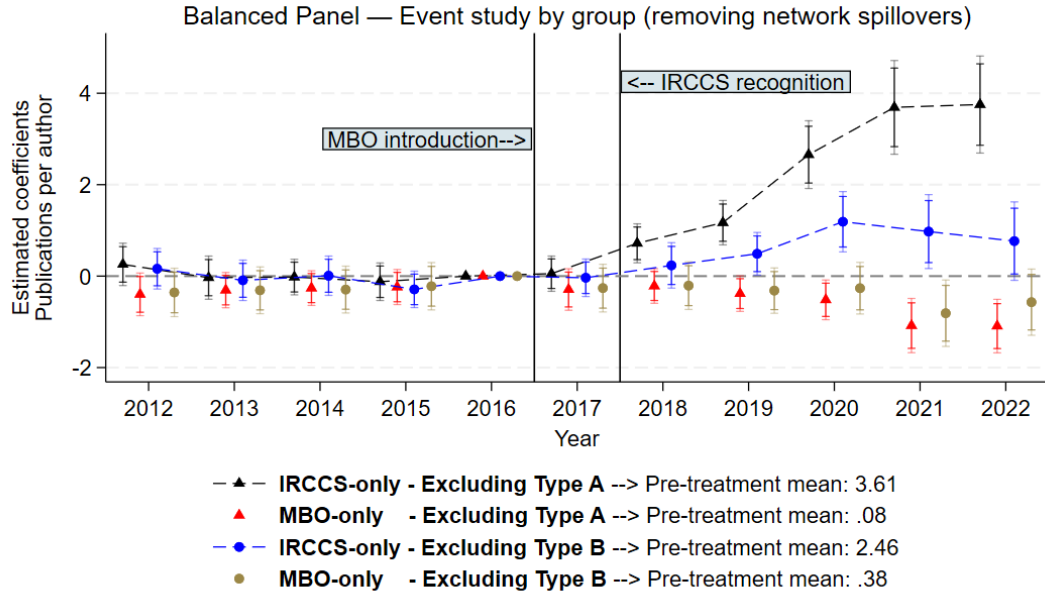
Figure A11: Event-study of IRCCS policy effect on publications for comparisons across different groups. Units included in the IRCCS perimeter in any year after 2018 are excluded from the estimates.

	(1) IRCCS vs. others	(2) DT vs MBO	(3) DT vs IRCCS	(4) DT vs PC	(5) IRCCS vs MBO	(6) IRCCS vs PC
Publications (log)	0.2700***	0.3027***	-0.0006	0.2902***	0.3333***	0.3183***
(SE)	(0.0337)	(0.0587)	(0.0577)	(0.0594)	(0.0388)	(0.0400)
Publications (asinh)	0.3213***	0.3841***	0.0190	0.3691***	0.4011***	0.3828***
(SE)	(0.0425)	(0.0754)	(0.0735)	(0.0763)	(0.0492)	(0.0507)
N	6424	2623	2739	2430	3987	3794
R ²	0.749	0.605	0.721	0.588	0.798	0.770
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Panel	Full (Balanced)	DT and MBO	DT and IRCCS	DT and control	IRCCS and MBO	IRCCS and control
Time Range	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022	2012–2022

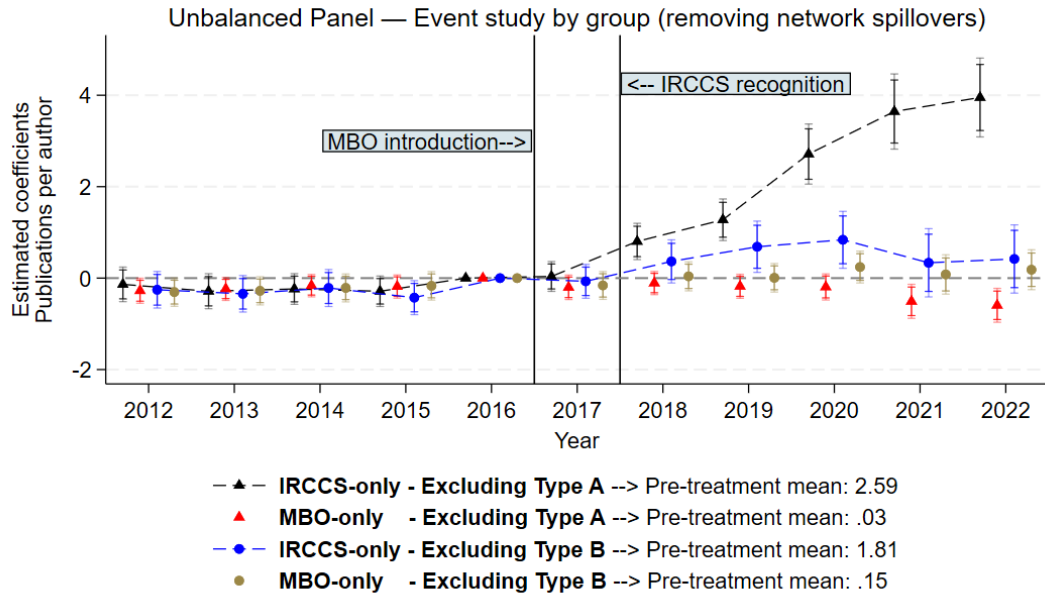
Table A5: Impact of IRCCS recognition on Annual Publications, re-scaled via log. and asinh.

	(1) IRCCS vs. others	(2) DT vs MBO	(3) DT vs IRCCS	(4) DT vs PC	(5) IRCCS vs MBO	(6) IRCCS vs PC
Publications	0.8531***	0.6286***	-0.2834	0.6658***	0.9494***	0.9867***
(SE)	(0.1666)	(0.2215)	(0.2688)	(0.2244)	(0.1659)	(0.1696)
N	4672	1940	1992	1788	2884	2732
R ²	0.842	0.661	0.833	0.625	0.864	0.854
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Mean	3.56	1.65	1.65	1.65	3.56	3.56
Panel (Balanced)	Full	DT and MBO	DT and IRCCS	DT and control	IRCCS and MBO	IRCCS and control
Time Range	2012–2019	2012–2019	2012–2019	2012–2019	2012–2019	2012–2019

Table A6: Impact of IRCCS recognition on Annual Publications in the sample truncated to exclude Covid years.



a) Balanced panel - Event-study of the two policies' effect on publications of the IRCCS-treated and MBO-treated only groups once excluded double-treated units and *Type A* researchers (MBO-treated units who *ever* co-authored with IRCCS researchers) in the first case, and double-treated units and *Type B* researchers (IRCCS-treated units who *ever* co-authored with non-academic physicians) in the second case.



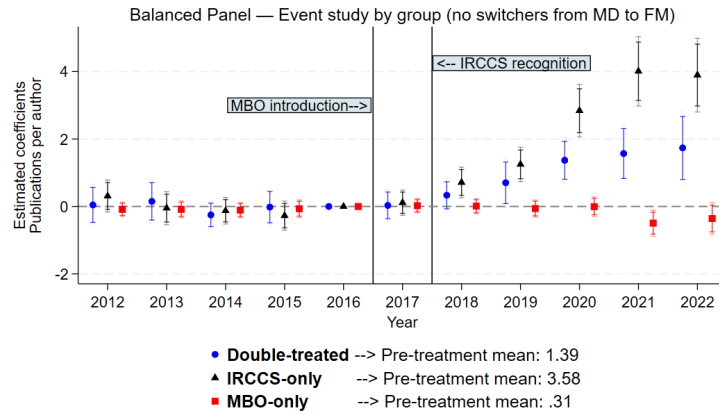
a) Unbalanced panel - Event-study of the two policies' effect on publications of the IRCCS-treated and MBO-treated only groups once excluded double-treated units and *Type A* researchers (MBO-treated units who *ever* co-authored with IRCCS researchers) in the first case, and double-treated units and *Type B* researchers (IRCCS-treated units who *ever* co-authored with non-academic physicians) in the second case.

Figure A12: Event-study of for the dynamic effects of the MBO policy and the IRCCS recognition effect on publications in different samples subset based on collaboration dynamics. Double-treated units are always excluded. a) is the Balanced panel, b) the unbalanced one.

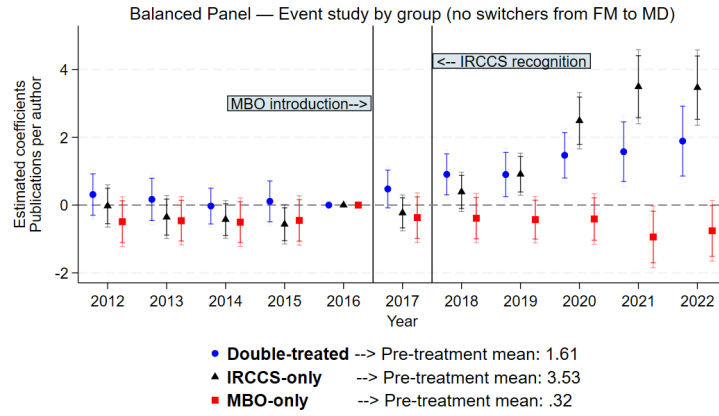
	2012-2022; SEs clustered at individual level							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pubs	Pubs	Pubs	Pubs	Pubs	Pubs	Pubs	Pubs
Post 2017*MBO-only	-0.33454 *** [0.08826]	-0.16009 [0.10775]			-0.06752 [0.06888]	0.25386 *** [0.07945]		
Post 2017*IRCCS-only	1.81748 *** [0.25013]	0.50939 *** [0.14837]			2.26424 *** [0.23770]	0.65571 *** [0.16236]		
Post 2018*MBO-only			-0.37688 ***	-0.18098			-0.09643	0.29371 ***
Post 2018*IRCCS-only			[0.08684] 2.24115 ***	[0.11162] 0.68130 ***			[0.07356] 2.63295 ***	[0.08541] 0.75051 ***
			[0.27949]	[0.17094]			[0.25929]	[0.18266]
Observations	6,985	6,717	6,985	6,717	12,195	12,794	12,195	12,794
R-squared	0.79416	0.68426	0.79865	0.68591	0.71270	0.51740	0.71924	0.51824
Individual FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Time Range	2012-2022	2012-2022	2012-2022	2012-2022	2012-2022	2012-2022	2012-2022	2012-2022
Panel I	No Type A	No Type B	No Type A	No Type B	No Type A	No Type B	No Type A	No Type B
Panel II	Balanced	Balanced	Balanced	Balanced	Unbalanced	Unbalanced	Unbalanced	Unbalanced
Mean across treated	2.035	1.269	2.035	1.260	1.011	0.583	1.033	0.597

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

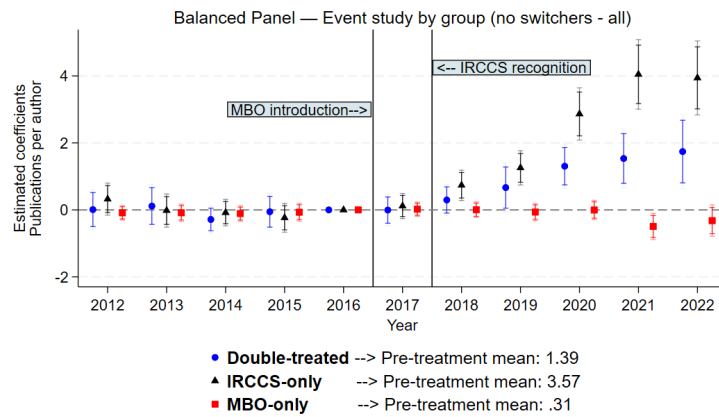
Table A7: Impact of MBO-implementation and IRCCS recognition on Annual Publications in the sub-samples obtained by excluding collaborating units.



a) Removing switchers from Medical Direction to Faculty Membership.

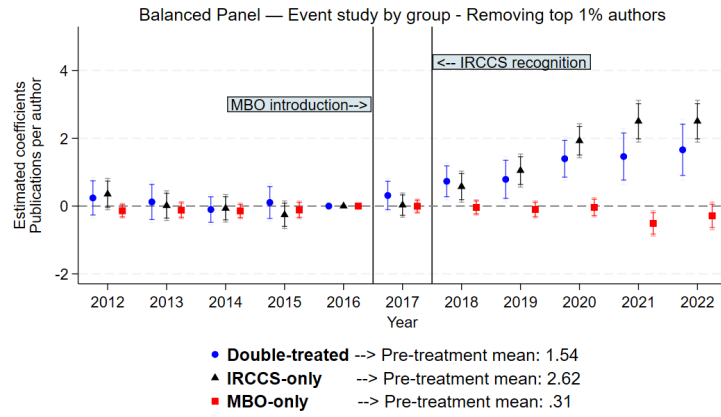


b) Removing switchers from Faculty Membership to Medical Direction.

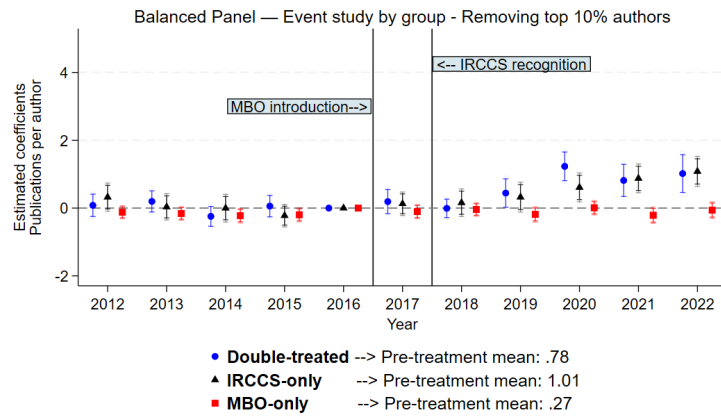


c) Removing all switcher.

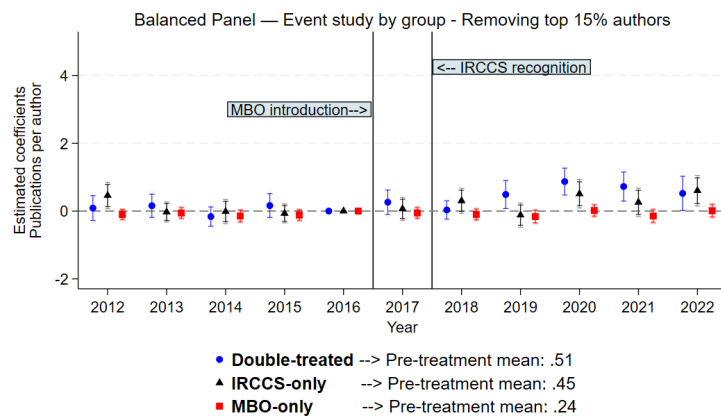
Figure A13: Dynamic impact of MBO-implementation and IRCCS recognition on Annual Publications in the sub-samples obtained by excluding those who switch across groups.



a) Removing top 1% researchers.

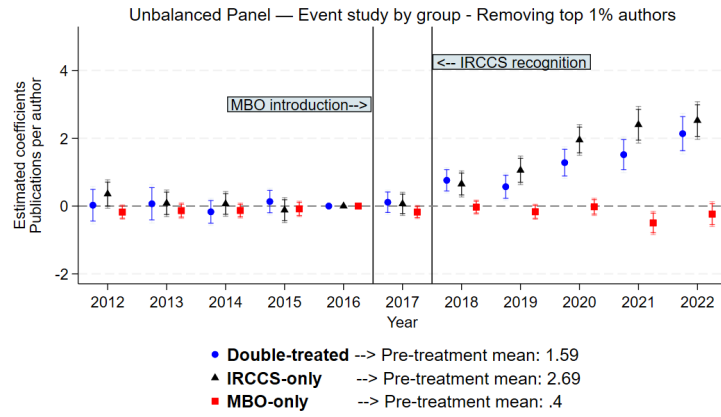


b) Removing top 10% researchers.

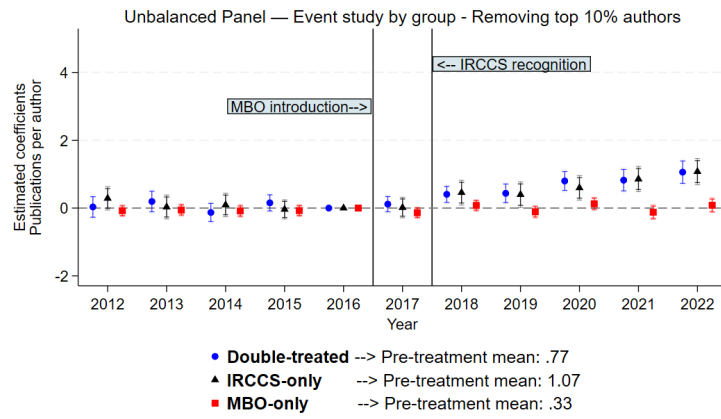


c) Removing top 15% researchers.

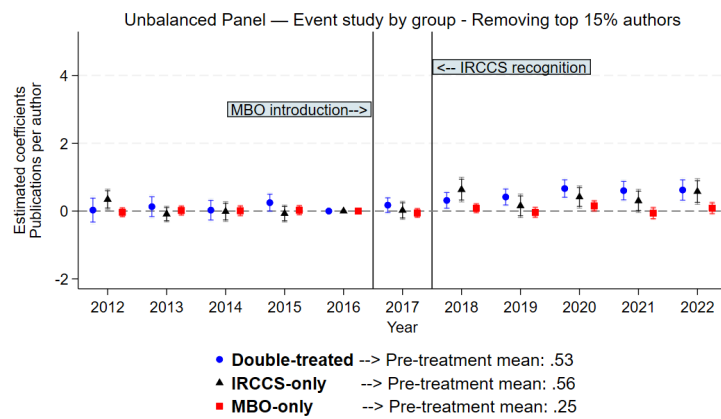
Figure A14: Balanced panel - Dynamic impact of MBO-implementation and IRCCS recognition on annual publications in the sub-samples obtained by excluding most prolific authors.



a) Removing top 1% researchers.

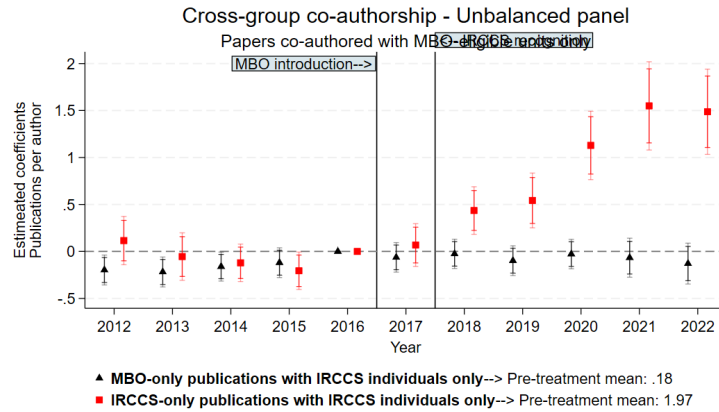


b) Removing top 10% researchers.

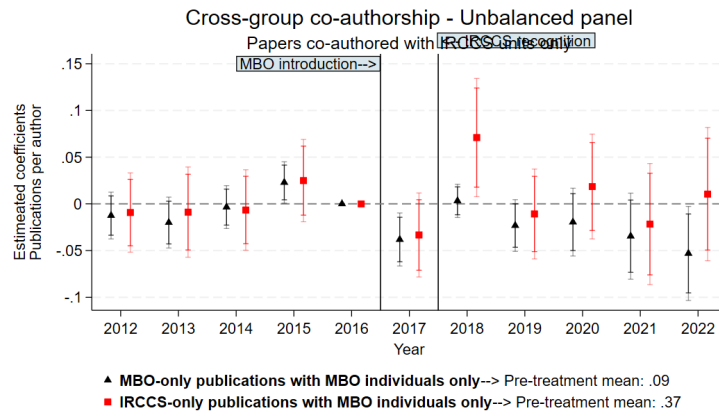


c) Removing top 15% researchers.

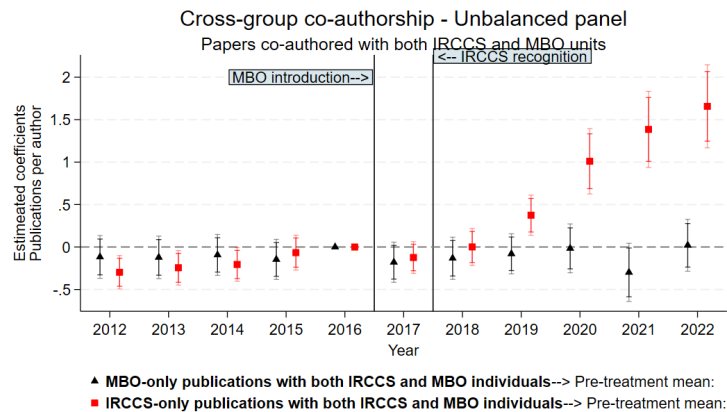
Figure A15: Unbalanced panel - Dynamic impact of MBO-implementation and IRCCS recognition on annual publications in the sub-samples obtained by excluding most prolific authors.



a) Outcome: publications co-authored with IRCCS units only.



b) Outcome: publications co-authored with MBO-eligible units only.



c) Outcome: publications co-authored with both MBO-eligible units and IRCCS researchers.

Figure A16: Dynamic impact of the policy implementation on annual cross-group publications in the balanced panel.

	(1) MBO vs. non-MBO	(2) DT vs IRCCS	(3) DT vs MBO	(4) DT vs control	(5) MBO vs IRCCS	(6) MBO vs control
Pay Out (€)	-2224.22900***	-1919.22046***	2051.05811***	1726.63208***	-4006.25513***	-383.18332***
(SE)	(655.32159)	(309.17627)	(262.29660)	(272.93503)	(214.52034)	(129.73976)
R ²	0.539	0.659	0.482	0.492	0.690	0.376
Mean	496.20	1296.96	1296.96	1296.96	215.85	215.85
Sum of weighted IF	-2.56335	-13.43816***	4.57402***	4.50302***	-19.76748***	-0.19042
(SE)	(3.50900)	(4.16684)	(1.22695)	(1.04295)	(5.03401)	(0.78501)
R ²	0.306	0.323	0.169	0.466	0.320	0.141
Mean	1.44	3.21	3.21	3.21	0.82	0.82
Eligible Papers	-3.98309**	-5.97766***	4.63950***	3.71497***	-11.06847***	-1.09332***
(SE)	(1.90916)	(1.07426)	(0.46722)	(0.49784)	(1.07856)	(0.26383)
R ²	0.532	0.595	0.569	0.569	0.616	0.429
Mean	1.26	3.12	3.12	3.12	0.61	0.61
Delta (Pubs-Eligible)	2.36909*	4.79941***	-3.38431***	-2.56286***	8.40499***	0.87501***
(SE)	(1.21334)	(0.86698)	(0.40610)	(0.42853)	(0.84863)	(0.18826)
R ²	0.336	0.453	0.414	0.448	0.440	0.354
Mean	-0.34	-0.86	-0.86	-0.86	-0.16	-0.16
N	6369	2695	2600	2418	3944	3674
Time FE	YES	YES	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES	YES	YES
Panel	Full	DT and IRCCS	DT and MBO	DT and control	MBO and IRCCS	MBO and control
Time Range	2017–2022	2017–2022	2017–2022	2017–2022	2017–2022	2017–2022

Table A8: Regressions between enrollment into IRCCS perimeter and several factors.

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