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Gabriele Letta
Università Cattolica del Sacro Cuore

Luca Salmasi
Università Cattolica del Sacro Cuore

Gilberto Turati
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Dipartimento di Economia e Finanza
Università Cattolica del Sacro Cuore
Largo Gemelli 1 - 20123 Milano – Italy
tel: +39.02.7234.2976
e-mail: dip.economiaefinanza@unicatt.it

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Household Income Uncertainty and Pregnancy Decisions: Evidence from Temporary Work Suspensions*†

Gabriele Letta[a] Luca Salmasi[a] Gilberto Turati[a]

[a] Department of Economics and Finance, Università Cattolica del Sacro Cuore, Milan and Rome

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Abstract

We study how uncertainty about future household income affects the decision to continue an ongoing pregnancy. To distinguish the role of uncertainty from that of realized income losses, we exploit the Italian labour-market response to the first wave of the COVID-19 pandemic. While the temporary suspension of non-essential economic activities substantially increased uncertainty about future economic prospects, workers affected by mandatory closures continued to receive income support through the national short-time work scheme. Using nationwide administrative records on voluntary pregnancy terminations and births, we combine this institutional setting with geographic variation in exposure to temporary work suspensions across municipalities. Difference-in-differences estimates show that abortion rates declined throughout Italy following the lockdown, but the decline was significantly smaller in municipalities more exposed to work suspensions. The effect is concentrated among married women outside the labour force and is strongest in municipalities with greater industrial employment. This pattern is consistent with uncertainty affecting households whose economic prospects were more exposed to temporary work suspensions. We find no corresponding effects on births. The estimates suggest that uncertainty about future household income can substantially affect pregnancy decisions even in the absence of permanent job loss.

Keywords: Household income uncertainty; Pregnancy decisions; Abortion; Temporary work suspensions; COVID-19.

JEL Classification: J13, I12, I18, J22.

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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, data do not need to be weighted by coefficients reporting a survey to the universe.

1 Introduction

Unexpected changes in household economic prospects may alter the decision whether to carry an ongoing pregnancy to term. This decision arises after conception and may concern both planned and unintended pregnancies, making it a distinct margin of reproductive decision-making from the decision whether to conceive. Voluntary pregnancy termination (VPT) is the most direct observable manifestation of this margin and, since its legalization, has become an integral component of family planning and women's reproductive autonomy (e.g., Doepke et al., 2022, Clarke, 2023, Myers, 2017). Yet, despite its importance for both fertility dynamics and public policy, credible evidence on how uncertainty about future household income affects the decision to continue an ongoing pregnancy remains scarce.

Most existing evidence relies on realized labour-market shocks, such as unemployment, job displacement, or plant closures, to study how adverse economic conditions affect fertility decisions (Currie and Schwandt, 2014, Del Bono et al., 2012, 2015, Dettling and Kearney, 2014, Schaller, 2016; Schaller et al., 2020, Bardits et al., 2023, Cavallini, 2024). In these settings, current income losses, expectations about future household income, and changes in labour-market opportunities occur simultaneously, making it difficult to disentangle whether households respond to the immediate deterioration of their economic circumstances or to uncertainty about future household income. Identifying the independent role of uncertainty is important for understanding how households adjust reproductive decisions in response to unexpected shocks and for evaluating policies that protect current income while leaving future economic prospects uncertain.

The Italian labour-market response to the first wave of the COVID-19 pandemic provides such a setting. In March 2020, the government ordered the temporary suspension of all non-essential economic activities. Crucially, workers affected by these mandatory closures continued to receive substantial income support through the national short-time work scheme (*Cassa Integrazione Guadagni*, CIG). The policy largely protected current household income while increasing uncertainty about future household income. Because the suspension applied to industries defined administratively according to pre-pandemic ATECO classifications, exposure to work suspensions varied substantially across local labour markets. We exploit this geographic variation as plausibly exogenous variation in households' exposure to uncertainty about future household income.

We combine nationwide administrative records on voluntary pregnancy terminations with information on the municipal share of workers suspended during the national lockdown. Exploiting the

predetermined industrial composition of local labour markets, we compare changes in abortion rates before and after the lockdown across municipalities with different exposure to temporary work suspensions. We find that abortion rates declined following the lockdown throughout Italy, but the decline was significantly smaller in municipalities more exposed to work suspensions. The effect emerges immediately after the onset of the lockdown and gradually disappears over the following months, consistent with a temporary increase in household income uncertainty. Consistent with the proposed mechanism, the effect is strongest where temporary work suspensions are more likely to generate uncertainty about household economic prospects, especially among married women outside the labour force and among women living in municipalities with greater industrial employment.

Our findings prove robust to a wide range of specification and identification checks, including event-study analyses, placebo tests, randomization inference, alternative definitions of treatment intensity, and alternative estimators. We find little support for competing explanations based on changes in sexual behaviour or on supply-side and household factors such as access to abortion services and domestic violence. This evidence supports the interpretation that temporary work suspensions affected pregnancy decisions primarily through increased household income uncertainty.

This paper provides evidence on the role of household income uncertainty in pregnancy decisions. By exploiting a setting in which current household income was largely protected despite a sharp increase in uncertainty about future economic prospects, we shed new light on a mechanism that has been difficult to isolate in previous studies. The evidence also indicates that policies designed to stabilize current income may not fully offset the behavioural consequences of uncertainty about future household income.

Our main contribution is to the literature on economic shocks and fertility. We provide, to the best of our knowledge, the first causal evidence that a transitory increase in household income uncertainty, absent permanent job loss, affects the decision to continue an ongoing pregnancy.[1] Our results complement those of Huttunen and Kellokumpu, 2016, who show that realized job loss affects fertility primarily through women's career concerns rather than household income losses. We show instead that uncertainty about the future earnings of the household, even in the absence of permanent displacement, is sufficient to affect pregnancy decisions. Read alongside each other, the two studies indicate that households respond to threats to their future economic prospects as well as to realized income losses.

We also contribute to the literature on the socio-economic determinants of abortion demand (Clarke,

[1]More broadly, a large literature in economics and demography documents that fertility is procyclical and declines during economic crises; see, e.g., Currie and Schwandt, 2014, Del Bono et al., 2012, 2015, Dettling and Kearney, 2014, Schaller, 2016; Schaller et al., 2020, Modena et al., 2014, Comolli, 2017, Caltabiano et al., 2017, Fahlén and Oláh, 2018.

2023) by showing that a temporary, plausibly exogenous increase in economic uncertainty, generated by labour-market policies unrelated to reproductive health, affects abortion decisions. Previous work has mainly focused on access to abortion services or on policies affecting income support and labour-market conditions (Herbst, 2011, González, 2013, González and Trommlerová, 2023, Bardits et al., 2023, Cavallini, 2024).[2] By exploiting municipal-level variation in work suspensions within a quasi-experimental framework, we provide evidence that household income uncertainty is an important determinant of abortion behaviour.

The analysis further speaks to the literature on the demographic consequences of the COVID-19 pandemic.[3] Existing studies document a temporary decline in fertility during the pandemic and attribute it to a combination of reduced social interactions and heightened uncertainty (Aassve et al., 2021, Sobotka et al., 2023, Bailey et al., 2022, Bailey et al., 2023, Trommlerová and González, 2024, González and Trommlerová, 2024, Luppi et al., 2020, Rosina et al., 2022). We complement this literature by exploiting geographic variation in exposure to temporary work suspensions and showing that the behavioural response operated primarily through pregnancy terminations rather than conceptions.

The remainder of the paper is organized as follows. Sections 2–4 present the institutional setting and then develop the data and empirical strategy. Section 5 reports the baseline estimates. Sections 6–8 study heterogeneity before turning to robustness and alternative explanations. Section 9 studies whether the estimated effects operate through pregnancy terminations or conceptions, and Section 10 concludes.

2 Institutional Background

2.1 Abortion in Italy

The Italian National Health Service (*Servizio Sanitario Nazionale*, hereafter *NHS*) is a publicly funded, tax-financed system providing universal access to healthcare services. Voluntary pregnancy termination

[2] A related literature studies the effects of contraceptive access, abortion restrictions, political institutions, conscientious objection, and abortion denial on pregnancy terminations; see, among others, Girma and Paton, 2006, 2011, Bailey, 2010, Ananat and Hungerman, 2012, Bentancor and Clarke, 2017, Cintina, 2017, Colman et al., 2013, Fischer et al., 2018, Lindo et al., 2020, Venator and Fletcher, 2021, González et al., 2023, Pieroni et al., 2023, Bo et al., 2015, Autorino et al., 2020, Muratori, 2023, Miller et al., 2023, and Londoño-Vélez and Saravia, 2025.

[3] See, among others, Dingel and Neiman, 2020, Adams-Prassl et al., 2020, Alon et al., 2021, Crossley et al., 2021, Montenovo et al., 2022, Bluedorn et al., 2023, Alon et al., 2020, Del Boca et al., 2020, Hupkau and Petrongolo, 2020, Zamarro and Prados, 2021, Deryugina et al., 2021, Galasso et al., 2020, Etheridge and Spantig, 2022, and Barili et al., 2024.

(VPT; *Interruzione Volontaria di Gravidanza*, IVG) has been legal and provided free of charge through the NHS since the approval of Law 194/1978.

Under Law 194/1978, abortion is available on broad health, social, economic, and family grounds during the first 90 days of gestation. Beyond this threshold, termination is permitted only when the pregnancy poses a serious risk to the woman's physical or mental health. Access requires certification by a gynaecologist, who verifies the pregnancy and estimates the gestational age; except in emergency cases, a mandatory seven-day reflection period applies before the procedure can be performed. Abortions are carried out exclusively in public or accredited hospitals.

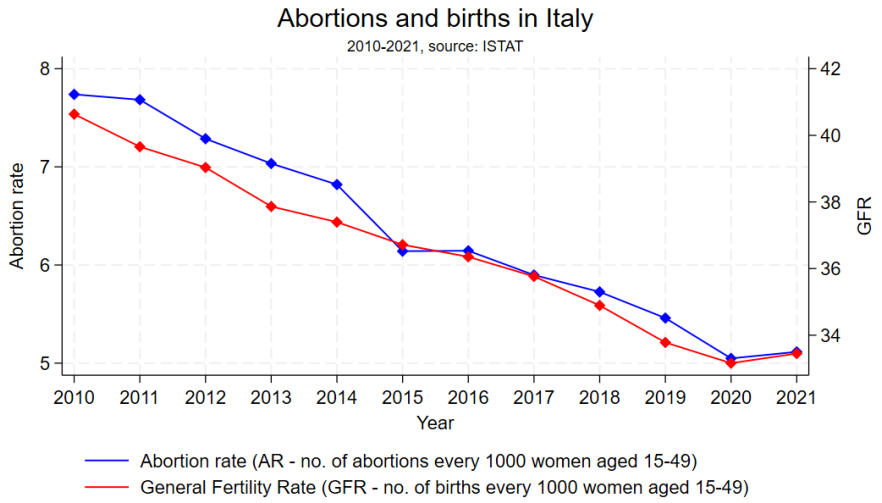
An important institutional feature of the Italian system is the widespread use of conscientious objection. Article 9 of Law 194 allows physicians and other healthcare professionals to refuse participation in abortion procedures on moral or religious grounds, except when the woman's life is in immediate danger. Conscientious objection remains common among Italian gynaecologists (de la Fuente Fonnest et al., 2000, Muratori, 2023) and has been shown to increase waiting times (Bo et al., 2015), induce cross-regional mobility for abortion services (Autorino et al., 2020), and potentially contribute to the underreporting of illegal abortions (Muratori, 2023).

Since legalization, every VPT performed through the NHS has been recorded by a nationwide surveillance system managed by the Ministry of Health and the Istituto Superiore di Sanità (ISS), with ISTAT contributing to its operation. These administrative records provide virtually complete information on legal abortions performed in Italy and constitute the basis of our empirical analysis.

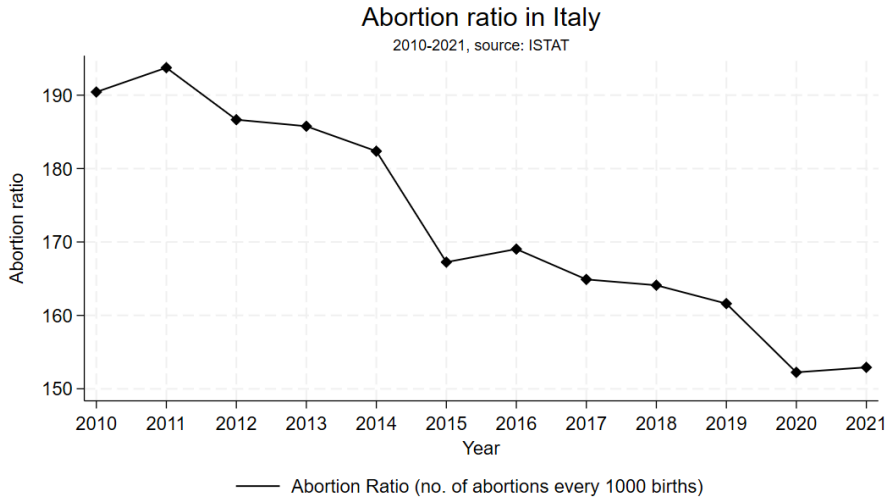
Figure 1 reports the long-run evolution of abortion and fertility in Italy.[4] Both abortion and fertility have followed a persistent downward trend over the last four decades. According to the Ministry of Health, the number of legal abortions declined to 63,653 in 2021, continuing a long-run pattern generally attributed to changes in reproductive behaviour and to better family-planning provision, including greater contraceptive use (MoH, 2022).

Despite this common national trend, abortion rates remain geographically heterogeneous (Appendix Figure A1). Part of this variation may reflect differences in the prevalence of conscientious objection. In 2021, 63.4% of Italian gynaecologists declared themselves conscientious objectors, with substantial regional variation and particularly high rates in Southern and North-Eastern Italy (Appendix Figure A2).

[4]The Abortion Rate (AR) is defined as the number of VPTs per 1,000 women aged 15–49, while the General Fertility Rate (GFR) is defined as the number of live births per 1,000 women of reproductive age.



(a)



(b)

Figure 1: Long-run decline in abortion and fertility indicators in Italy, 2010–2021. Panel (a) reports the abortion rate and the general fertility rate. Panel (b) reports the abortion ratio. Source: ISTAT.

2.2 The COVID-19 Pandemic and the Italian Economy

Italy was among the first European countries to experience a large-scale outbreak of COVID-19 and, consequently, the first to adopt nationwide containment measures. Following the first detected case in Lombardy in February 2020, the virus spread rapidly across the country, generating a sharp increase in mortality and placing unprecedented pressure on the healthcare system (Buonanno et al., 2020, Depalo, 2021, Cerqua et al., 2021). Excess mortality remained well above historical levels throughout 2020 and 2021 (Appendix Figure B1).[5]

To contain the spread of the virus, the Italian government introduced progressively stricter restrictions during March 2020. A nationwide stay-at-home order came into force on March 9, followed two days later by the closure of schools, restaurants, retail activities, and most public gatherings. A further decree (*DPCM* of March 22, 2020) suspended all economic activities classified as "non-essential" according to their five-digit ATECO industry code, while allowing essential sectors to continue operating. This distinction constitutes the main source of identifying variation exploited in our analysis. Because the sectoral composition of local economies differs substantially across municipalities, the decree generated considerable geographic heterogeneity in the share of workers temporarily suspended from work.

To cushion the economic consequences of these mandatory closures, the government substantially expanded the *Cassa Integrazione Guadagni* (CIG), Italy's short-time work scheme. The program compensated workers whose activity had been reduced or suspended and was extended to firms previously excluded from the scheme. As a result, furloughed workers continued to receive substantial income support despite being temporarily inactive. While the policy largely mitigated immediate income losses, it also generated considerable uncertainty about future household income. This distinction between realized income losses and uncertainty about future household income is central to our empirical interpretation.

Although the nationwide lockdown ended on May 4, 2020, restrictions were only gradually relaxed during the following months. Beginning in autumn 2020, containment policies became region-specific through a system of color-coded risk zones (yellow, orange, and red), with progressively stricter limitations on mobility and economic activity (Conteduca and Borin, 2022). Unlike the nationwide shutdown implemented in spring 2020, however, economic activities were never again suspended across the entire country.

[5]Excess mortality is defined as the number of deaths observed in 2020–2021 relative to the average over the period 2015–2019.

3 Data

Our analysis combines several administrative data sources. The main municipal panel links voluntary pregnancy terminations and the female population used to construct outcome rates with the share of workers suspended during the national lockdown. We then present the additional data employed in the complementary analyses and as controls. Appendix Table A1 summarizes all data sources, while Appendix Table A2 reports descriptive statistics for the abortion records.

3.1 Voluntary pregnancy terminations

Our main outcome is constructed from the administrative records of all voluntary pregnancy terminations (VPTs) performed in Italy between 2018 and 2021. The data are collected through the national epidemiological surveillance system on VPTs and were accessed through the ISTAT Laboratory for the Analysis of Microdata (ADELE). This is the only period for which the administrative records identify the municipality of residence of the woman undergoing the procedure.

After excluding approximately 8,500 records with missing information or referring to women residing abroad, the final dataset contains 268,054 VPTs. We aggregate these individual records into a quarterly panel of 126,448 municipality-quarter observations.[6]

The records describe the women and the procedures in detail, including demographic and socioeconomic characteristics as well as reproductive histories. We exploit these variables in the heterogeneity and mechanism analyses.[7]

Our main outcome is the municipal *Abortion Rate* (AR), defined according to the official ISTAT methodology as the number of VPTs among resident women per 1,000 resident women aged 15–49. Each procedure is assigned to the woman's municipality of residence rather than to the municipality in which it was performed, so that the outcome captures reproductive decisions among the local population rather than the geographic distribution of hospitals or specialized facilities. We focus on the abortion

[6]The original data are available monthly. We use quarterly observations in the baseline analysis to accommodate seasonal abortion patterns and the large number of zero outcomes generated by monthly aggregation across almost 8,000 municipalities. Quarterly timing also matches the institutional setting because the suspension of non-essential activities was introduced on March 22, 2020, near the end of the first quarter. Given the mandatory seven-day reflection period, any response could begin to emerge only from April 2020. We therefore define 2020Q2 as the first post-treatment quarter. Section 5.2 reports estimates using alternative temporal aggregations, including monthly data.

[7]The available information includes age, citizenship, marital status, education, employment status, occupational position and sector of activity; previous pregnancies, live births, miscarriages and VPTs; gestational age and fetal malformations; and characteristics of the procedure, including urgency, type of intervention, hospitalization regime and healthcare facility.

rate instead of the abortion ratio, which scales abortions by the number of observed pregnancies, because the pandemic may have affected both conceptions and pregnancy-termination decisions, potentially with different timing and intensity. Short-run changes in conceptions would therefore mechanically alter the denominator of the abortion ratio. For this reason, we study VPTs and conceptions resulting in live births as separate outcomes, while using the abortion ratio as a complementary measure in Section 8.

3.2 Suspended workers

Our treatment measure is the municipal share of workers whose activities were suspended during the national lockdown, released by ISTAT in May 2020.[8] ISTAT constructs this measure by combining the pre-pandemic sectoral composition of employment in each municipality with the list of activities suspended by the March 22 decree at the five-digit ATECO level.

Local employment composition is drawn from the *Frame SBS Territoriale*, a statistical register covering approximately 4.4 million active private-sector firms. For each municipality, ISTAT estimates the share of workers employed in activities classified as non-essential and therefore subject to mandatory suspension.[9]

The sectoral composition refers to local units observed in 2017, the latest available year preceding the pandemic. The treatment measure is therefore predetermined with respect to both the COVID-19 outbreak and the March 2020 policy response. Although local industrial composition may be correlated with persistent municipal characteristics, these are absorbed by municipality fixed effects; Section 4 discusses the identifying assumptions in detail.

The data cover all 7,978 Italian municipalities.[10] For robustness, we also aggregate exposure to the 610 Local Labor Market Areas (LMAs), functional areas defined by ISTAT on the basis of commuting patterns and the geographic integration of local labor markets.

[8] Source: ISTAT, May 2020.

[9] The employment estimates are based on job positions and hours worked. The register excludes agriculture, hunting and fishing, public administration, finance and insurance, household and own-production activities, and international organizations. ISTAT also provides a broad decomposition of suspended employment between industry and services, which we use in the heterogeneity analysis.

[10] A small number of municipalities merged during the sample period. We harmonize municipal boundaries and treat the resulting units consistently throughout the analysis.

3.3 Population and complementary data

Annual municipal population data by age come from ISTAT. We use the official demographic series for 2019–2021 and the inter-census population reconstruction for 2018. These data provide the number of resident women aged 15–49 used as the denominator of the abortion and fertility-related outcomes and, in selected specifications, to construct population weights.[11]

We complement the main panel with additional sources. Daily municipal birth registrations from ISTAT for 2018–2021, covering 1,664,972 live births. We shift each date of birth backward by 270 days to approximate the timing of conceptions that resulted in a live birth and aggregate these inferred conception dates by municipality and quarter. From these data, we construct a rate of pregnancies carried to term, defined as the number of inferred conceptions resulting in a live birth per 1,000 women aged 15–49.[12]

We also link the healthcare facilities reported in the VPT records to the official registry of healthcare institutions maintained by the Ministry of Health. This allows us to identify the municipality in which each procedure was performed and to distinguish changes in abortion decisions among residents from changes in the local provision of abortion services.[13]

We also assemble measures of pandemic severity and containment policies. Beginning in November 2020, Italian regions were assigned to color-coded restriction regimes according to local epidemiological conditions. Following Conteduca and Borin, 2022, we measure the number of days spent under each regime and the associated stringency of local restrictions. We also use quarterly municipal excess mortality constructed from ISTAT mortality records (Appendix D). These variables are included in extended specifications to assess whether the estimated relationship reflects differential exposure to the epidemiological evolution of the pandemic or to subsequent containment measures.[14]

[11] Quarterly population data would in principle be preferable. Over the four-year sample period, however, within-year changes in the population of women aged 15–49 are negligible for the purposes of the analysis. Newborns do not enter the relevant age group, while COVID-19 mortality was concentrated among substantially older individuals.

[12] Because birth registrations for 2022 are unavailable, we cannot observe pregnancies conceived between April and December 2021 that resulted in births in the following year. The resulting sample is therefore shorter at the end of the period. Estimates are unaffected by using more granular temporal aggregations.

[13] Because hospitals are not located in every municipality, the municipality-of-facility panel contains 5,492 municipality-quarter observations.

[14] Because these variables are realized after the initial treatment, we do not interpret them as controls for all time-varying confounding factors. Rather, we use them to assess whether the outcome covaries with the local evolution of the pandemic and its policy response, similarly to Franzoni et al., 2024.

3.4 Descriptive evidence

Figure 2 maps the municipal distribution of the suspended-workers share, showing substantial geographic variation in exposure to the suspension of non-essential activities. This variation largely reflects the pre-pandemic industrial structure of local labor markets. As shown in Appendix Figure C1 and Appendix Table C1, municipalities with greater exposure to work suspensions are concentrated in provinces characterized by stronger industrial specialization, particularly male industrial employment, whereas the prevalence of teleworkable occupations, measured by the Working From Remote (WFR) index of Barbieri et al., 2022, displays a substantially weaker association with the suspended-workers distribution.

Figure 3 plots seasonally adjusted mean abortion rates across quartiles of the suspended-workers distribution. Before March 2020, abortion rates follow broadly similar trajectories across quartiles, providing preliminary evidence against pronounced differential pre-treatment trends. Rates then decline sharply in all quartiles in 2020Q2 and subsequently recover, but the decline is visibly smaller among municipalities in the highest quartile of exposure, whose abortion rates remain above those of the remaining groups during the quarters immediately following the suspension.

The common decline is consistent with the reduction in social interactions and conceptions documented in other European countries. For Spain, Trommlerová and González, 2024 show that abortion rates fell following stay-at-home restrictions, primarily because of fewer conceptions rather than reduced access to abortion services. A similar aggregate mechanism is plausible in Italy, where mobility toward major places of social interaction collapsed during the lockdown (see Figure 5 below). Such nationwide changes are common to all municipalities and are absorbed by time fixed effects. Our empirical design instead exploits the differential evolution of abortion rates across municipalities with different exposure to work suspensions.

4 Empirical Strategy

We identify the effect of uncertainty about future household income on pregnancy termination decisions by exploiting the nationwide suspension of non-essential economic activities introduced during the first wave of the COVID-19 pandemic. Our empirical strategy compares the evolution of municipal abortion rates across local labor markets that differed in their exposure to these mandatory work suspensions within a difference-in-differences framework.

On March 22, 2020, the Italian government classified all economic activities as either “essential” or



Figure 2: Geographic distribution of the municipal share of workers suspended during the national lockdown. Municipalities are classified according to quartiles of the share of workers employed in activities suspended between March 22 and May 4, 2020. Source: ISTAT.

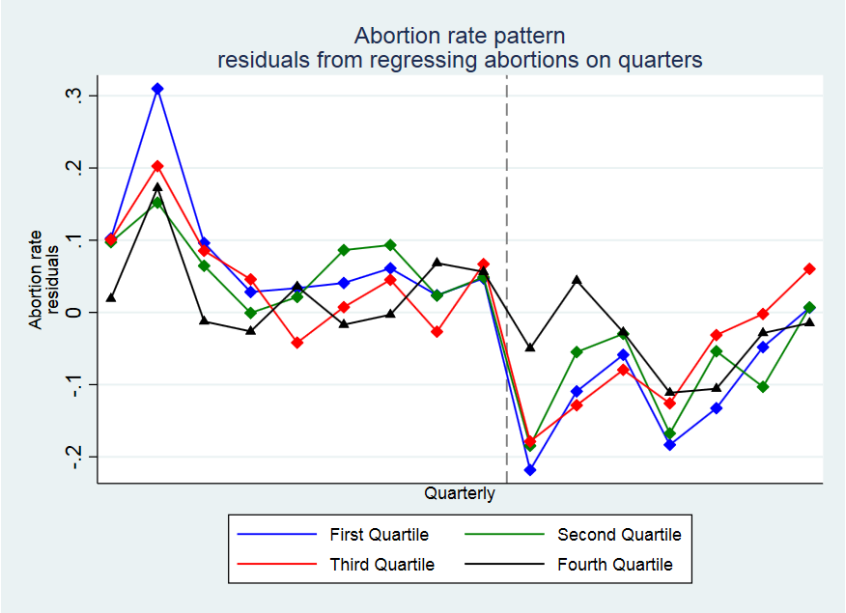


Figure 3: Seasonally adjusted abortion rates by exposure to work suspensions. Municipalities are grouped into quartiles of the suspended-workers distribution. The vertical dashed line indicates the first post-treatment quarter (2020Q2).

"non-essential" according to their five-digit ATECO code. Workers employed in non-essential activities were temporarily suspended from work until early May 2020, with some restrictions remaining in place for longer depending on the sector. Importantly, most suspended workers continued to receive income support through the *Cassa Integrazione Guadagni* (CIG), Italy's short-time work scheme. The policy therefore largely protected households from immediate income losses while simultaneously increasing uncertainty about future household income.

Our identification exploits the fact that municipalities differed substantially in their pre-pandemic industrial composition and, consequently, in the share of workers employed in activities subject to mandatory suspension. Because the classification of essential activities was imposed unexpectedly by the central government and depended exclusively on pre-existing industrial classifications, the resulting geographic variation provides plausibly exogenous variation in households' exposure to uncertainty about future household income. This strategy has been widely adopted in the Italian economic literature to study the consequences of the first-wave lockdown (Borri et al., 2021, Di Porto et al., 2022, Bordignon et al., 2023).[15]

[15]It is important to distinguish suspended employment from unemployment. While unemployment rates are typically higher in Southern Italy, the share of suspended workers during the first lockdown was considerably larger in Centre-Northern municipalities because of their industrial composition. Figure C2 in Appendix C shows that the municipal share of suspended workers is negatively correlated with unemployment rates, consistently with the evidence in Cerqua and Letta, 2022.

Our empirical design does not compare municipalities exposed and unexposed to the pandemic. All municipalities experienced the same nationwide lockdown and were subject to the same national restrictions. Instead, identification comes from differences in the intensity of work suspensions across municipalities and, therefore, in the degree of uncertainty about future household income experienced by local households.

To estimate the effect of work suspensions on abortion rates, we estimate the following two-way fixed-effects difference-in-differences specification:

$$AR_{mpt} = \beta_1 + \sum_{k=2}^{4} \beta_k Post_t \times \mathbb{1}(Inactive\ Share_{q2/2020} \in Q_k) + X'_{mt}\beta + \tau_m + \gamma_t + \delta_{p,t} + \varepsilon_{mt} \quad (1)$$

where AR_{mpt} denotes the abortion rate in municipality m, province p, and quarter t. $Post_t$ equals one from 2020Q2 onwards, while $\mathbb{1}(Inactive\ Share_{q2/2020} \in Q_k)$ identifies municipalities belonging to the k-th quartile of the suspended-workers distribution, with the first quartile serving as the reference category.

The vector X'_{mt} includes time-varying measures of pandemic severity and containment policies, namely the local stringency index and excess mortality, together with the number of days spent under each restriction regime. Municipality fixed effects (τ_m) absorb all time-invariant differences across municipalities, while quarter fixed effects (γ_t) capture nationwide shocks common to all municipalities, including the national lockdown. In our preferred specification, we additionally include province-by-quarter fixed effects ($\delta_{p,t}$), which absorb any shock common to municipalities within the same province during a given quarter.

The coefficients of interest, β_k, measure the differential change in abortion rates after the introduction of the nationwide suspension of non-essential activities for municipalities belonging to higher quartiles of the suspended-workers distribution relative to municipalities in the lowest quartile. Since exposure to work suspensions is inherently continuous, the quartile specification provides a flexible, non-parametric comparison across different levels of treatment intensity (Callaway et al., 2021). Section 5.2 shows that the results are robust to alternative definitions of treatment intensity, including a continuous specification and categorical alternatives based on terciles or a median split.

Because the abortion rate is derived from an underlying count outcome and the municipality-quarter panel contains a large number of zero observations, we also estimate the corresponding Poisson Pseudo-

Maximum Likelihood (PPML) specification as a robustness check. For ease of interpretation, we report average marginal effects.

Identification relies on the assumption that, absent the policy, municipalities with different exposure to suspended activities would have experienced parallel trends in abortion rates. To assess this assumption, we estimate the following event-study specification:

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

The omitted period is 2020Q1, the quarter immediately preceding the suspension of non-essential activities. The coefficients on the lead indicators measure pre-treatment differences between municipalities in the highest quartile of the suspended-workers distribution and those in the lowest quartile, while the lag coefficients trace the dynamic treatment effects after 2020Q2. The specification includes the same controls and fixed effects as the baseline model. The absence of statistically significant pre-treatment coefficients provides evidence consistent with the identifying assumption. As for the baseline specification, we also estimate the corresponding PPML event-study model and report average marginal effects.

5 Results

This section first presents the baseline estimates and the dynamic response to the shock. We then examine heterogeneity across the sectors in which workers were suspended and the characteristics of the women undergoing a VPT, in order to identify which households respond and assess the mechanisms underlying the main result.

5.1 Baseline estimates

Table 1 reports the baseline difference-in-differences estimates. The main finding is remarkably stable across specifications. Municipalities in the highest quartile of the suspended-workers distribution experienced a significantly smaller decline in abortion rates following the nationwide suspension of non-essential activities than municipalities in the lowest quartile, whereas the second and third quartiles display no statistically detectable differential response.

Across specifications, the estimated differential effect ranges between 0.11 and 0.13 abortions per

Effect of work suspensions on municipal abortion rates

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post \times \mathbb{1}(Suspended\ Share \in Q_4)$	0.13134 ***	0.11340 ***	0.12879 ***	0.11133 ***	0.13168 **	0.11087 ***
	[0.04565]	[0.03563]	[0.04587]	[0.03585]	[0.05185]	[0.04061]
$Post \times \mathbb{1}(Suspended\ Share \in Q_3)$	0.05711	0.04825	0.05570	0.04698	0.06342	0.05119
	[0.04266]	[0.03300]	[0.04276]	[0.03311]	[0.04501]	[0.03488]
$Post \times \mathbb{1}(Suspended\ Share \in Q_2)$	0.03486	0.02961	0.03505	0.02949	0.03353	0.02714
	[0.03950]	[0.03058]	[0.03953]	[0.03062]	[0.02949]	[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			✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province × quarter-year FE					✓	✓
Population weights		✓		✓		✓
Pre-treatment mean	1.103	1.103	1.103	1.103	1.103	1.103

Table 1: Effect of work suspensions on municipal abortion rates. The table reports estimates from linear two-way fixed-effects difference-in-differences regressions. The dependent variable is the number of VPTs per 1,000 resident women aged 15–49. Municipalities are classified into quartiles of the suspended-workers distribution, with the first quartile serving as the omitted category. Policy covariates include the municipal stringency index, the number of days spent under each restriction regime, and excess mortality. Columns (5) and (6) include province-by-quarter-year fixed effects. Weighted specifications use the logarithm of municipal population as analytical weights. *Pre-treatment mean* denotes the average outcome among municipalities in the highest-exposure quartile before 2020Q2. Standard errors clustered at the municipality level are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

1,000 women of reproductive age, corresponding to approximately 10–12% of the average pre-treatment abortion rate among municipalities in the highest-exposure quartile. The magnitude of the estimates is virtually unchanged after controlling for local pandemic severity, containment measures, and province-by-quarter fixed effects, suggesting that the results are not driven by differential exposure to the epidemiological evolution of the pandemic. Weighting municipalities by population also leaves the estimates essentially unaffected.

This pattern is consistent with the hypothesis that greater uncertainty about future household income induced some households to revise decisions concerning an ongoing pregnancy. The corresponding PPML estimates, reported in Appendix Table D1, are nearly identical in magnitude and sign, with the same levels of statistical significance.

Figure 4 reports the event-study estimates, which serve both to assess the parallel-trends assumption and to characterize the dynamic response to the shock.

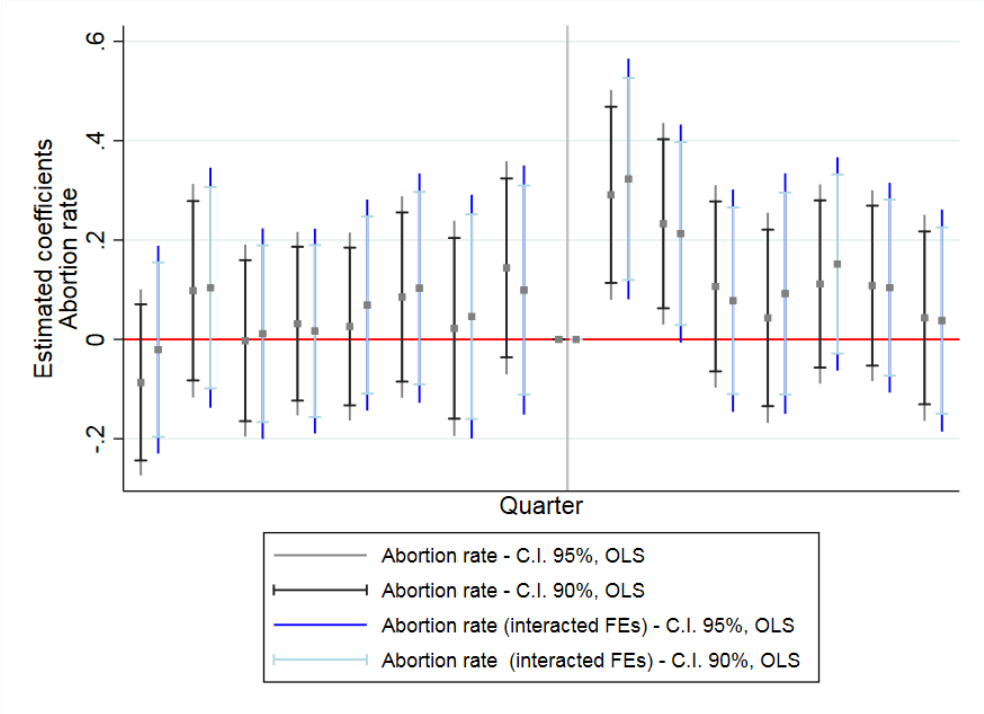


Figure 4: Dynamic effect of work suspensions on municipal abortion rates. The figure reports event-study coefficients and 95% confidence intervals for municipalities in the highest quartile of the suspended-workers distribution relative to those in the lowest quartile. Estimates are shown for the baseline specification and the specification including province-by-quarter fixed effects. The omitted period is 2020Q1, the quarter immediately preceding the suspension of non-essential activities. The vertical dashed line marks the beginning of the post-treatment period.

The coefficients for the pre-treatment periods are small and statistically indistinguishable from zero, providing no evidence of differential pre-treatment trends between municipalities in the highest and

lowest quartiles of exposure. Although the estimates exhibit some quarter-to-quarter variation before the shock, they display no systematic pattern.

The effect emerges immediately in 2020Q2, remains statistically significant during the following quarter, and gradually disappears thereafter, becoming indistinguishable from zero by the end of 2020. This short-lived response is consistent with households reacting to a temporary increase in uncertainty about future household income rather than to a persistent deterioration in economic conditions.

The corresponding PPML event-study estimates, reported in Appendix Figure D1, display a nearly identical pattern, confirming both the absence of differential pre-treatment trends and the temporary nature of the estimated effect.

5.2 Heterogeneous effects

The baseline estimates establish a positive differential effect on abortion rates in municipalities with the greatest exposure to work suspensions. We now examine which households drove this response and whether the resulting pattern is consistent with uncertainty about future household income as the underlying mechanism. We proceed from the source of exposure to the characteristics of the women undergoing a VPT, considering the sectoral composition of suspended employment, women's employment and marital status, reproductive history, and geography. Across these dimensions, a coherent profile emerges: the response is associated with suspensions in male-dominated industrial employment and is concentrated among married women who are outside the labor force and already have children.

We begin by distinguishing exposure to suspended employment in industry and services. This comparison is informative because female employment is disproportionately concentrated in services (Casarico and Lattanzio, 2022), whereas industrial employment is predominantly male. If the baseline effect primarily reflected women's own labor-market exposure, one would therefore expect service-sector suspensions to play an important role. Table 2 shows instead that the positive differential effect on abortion rates is associated with the industrial component of suspended employment, while the service-sector component has no detectable effect. Although we do not observe partners or household employment arrangements directly, this sectoral pattern is more consistent with exposure operating through another household member than through the woman's own employment.

Impact of work suspensions on municipal abortion rates by sectoral composition (OLS)

	Industry inactive share			Services inactive share		
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.11880 ***	0.11528 **	0.12829 **	-0.01773	-0.01388	-0.01645
	[0.04568]	[0.04623]	[0.05384]	[0.04775]	[0.04807]	[0.05077]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.02693	0.02427	0.03758	-0.04640	-0.04235	-0.03867
	[0.04599]	[0.04634]	[0.05003]	[0.04026]	[0.04041]	[0.04178]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.01450	0.01393	0.01966	-0.05198	-0.04960	-0.05771
	[0.04355]	[0.04364]	[0.04408]	[0.03983]	[0.03989]	[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 covariates		✓	✓		✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE			✓			✓
Mean	1.087	1.087	1.087	1.172	1.172	1.172

Table 2: The table reports OLS estimates where treatment intensity is defined separately using the distribution of suspended workers in industry and services. Columns (1)–(3) use the industrial inactive-share distribution, while Columns (4)–(6) use the services inactive-share distribution. The omitted category is the first quartile of the corresponding inactive-share distribution. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality and quarter-year fixed effects. Columns (3) and (6) additionally include province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

We next construct separate abortion rates according to women's employment status, distinguishing women outside the labor force from those in private employment (services or industry) and the public sector. Public-sector employees provide a useful placebo group because their employment is excluded by construction from the suspended-workers measure. Table 3 shows that the response is concentrated among women outside the labor force: for this group, the fourth-quartile estimate corresponds to approximately 15.5% of the pre-treatment mean. By contrast, we detect no significant response among women in private employment (services or industry) or in the public sector. The same pattern emerges when both the treatment and the outcome are disaggregated by sector (Appendix Table E1).

These findings are difficult to reconcile with a mechanism operating through women's own employment or career concerns. They instead point toward exposure through the employment circumstances of other household members. This distinction is particularly relevant in light of Huttunen and Kellokumpu, 2016, who show that fertility responds to women's own job displacement but not to their partners' displacement, despite the larger income losses associated with the latter. In our setting, the women who

respond have no observed employment of their own, making career-related explanations unlikely.

Impact of work suspension on abortion rates by employment sector (OLS)

	(1) AR Not in labour force	(2) AR Services	(3) AR Industry	(4) AR Public sector
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.06984 **	0.04511	0.01454	0.01346
	[0.03532]	[0.02936]	[0.01293]	[0.01030]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.05002	0.02213	0.00006	-0.00022
	[0.03094]	[0.02465]	[0.01083]	[0.00891]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.03253	0.00178	0.00934	0.00574
	[0.02852]	[0.02204]	[0.00968]	[0.00806]
Observations	126,448	126,448	126,448	126,448
R-squared	0.10409	0.08890	0.07934	0.07679
Policy covariates	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓	✓	✓
Mean	0.508	0.376	0.068	0.035

Table 3: The table reports OLS estimates of the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, separately by employment status and economic sector. Column (1) refers to women not participating in the labour force, Column (2) to women employed in services, Column (3) to women employed in industry, and Column (4) to women employed in the public sector. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

The results by marital status reinforce this household interpretation. Table 4 shows that the effect is concentrated among married women, for whom the estimated increase ranges between 0.07 and 0.085 abortions per 1,000 women, or approximately 15% of the group's pre-treatment mean. The estimate for single women is smaller and only weakly significant, while no response emerges among separated or widowed women. Because the administrative data do not identify cohabitation, the result for single women may partly reflect unmarried women living with a partner. The age profile, with the effect concentrated among older women (Appendix Table E3), is also consistent with the response occurring primarily within established households.

Impact of work suspension on abortion rates by marital status (OLS)

	(1) AR Single	(2) AR Married	(3) AR Separated	(4) AR Widowed
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.07200 *	0.06963 **	-0.00023	-0.00282
	[0.04115]	[0.02993]	[0.00511]	[0.00799]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.03583	0.03278	-0.00147	-0.00378
	[0.03845]	[0.02634]	[0.00432]	[0.00546]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.00633	0.02771	-0.00166	-0.00890
	[0.03005]	[0.02468]	[0.00367]	[0.00469]
Observations	126,448	126,448	126,448	126,448
R-squared	0.10016	0.09405	0.08802	0.07705
Policy covariates	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓	✓	✓
Mean	0.631	0.379	0.028	0.026

Table 4: The table reports OLS estimates of the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, separately by marital status. Columns (1)–(4) refer respectively to single, married, separated, and widowed women. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

Reproductive history provides the clearest evidence on which households respond. Table 5 distinguishes women according to whether they had previously been pregnant or given birth. The effect is concentrated among women with at least one previous pregnancy and, especially, among those with at least one previous live birth. For the latter group, the fourth-quartile estimate is approximately 0.097 abortions per 1,000 women, corresponding to about 15% of the pre-treatment mean; the third-quartile estimates are also statistically significant. Women with no previous pregnancies display no detectable response.

Table 6 further disaggregates the estimates by the number of previous live births and VPTs. The effect is stronger among women with one previous child and among those with two, for whom the fourth-quartile estimate amounts to approximately 21% of the pre-treatment mean. Women with no children again show no response. By previous VPT history, the increase is modest among women undergoing their first abortion and considerably larger among women with exactly one previous VPT, reaching approximately 26% of the group mean.[16]

[16]Estimates for women with three or more previous live births or VPTs are small and imprecise, as are those for women with more than one previous VPT, plausibly because these groups account for relatively few observations. See Appendix Table F2.

Impact of work suspension on abortion rates according to past reproductive experience (OLS)

	(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
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_4)$	0.10948 ***	0.02238	0.09800 **	0.04712	0.09660 **	0.00297	-0.01574	0.05414 **
	[0.04144]	[0.03154]	[0.03974]	[0.03186]	[0.03957]	[0.00321]	[0.02222]	[0.02446]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_3)$	0.06753 **	-0.00302	0.06796 **	0.01226	0.06636 **	0.00354	-0.01006	0.01331
	[0.03442]	[0.02778]	[0.03243]	[0.02585]	[0.03236]	[0.00250]	[0.01667]	[0.02181]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_2)$	0.05178 *	-0.01648	0.04370 **	0.02988	0.04171	0.00376 *	-0.01272	0.03215
	[0.03132]	[0.02474]	[0.02997]	[0.02366]	[0.02990]	[0.00218]	[0.01554]	[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 5: 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 differentiated according to whether the women undergoing abortion had previous pregnancies, and by the type of those pregnancies. 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. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean among treated units. *** p<0.01, ** p<0.05, * p<0.1

Impact of work suspension on abortion rates by previous reproductive history (OLS)

	(1) AR No prev. births	(2) AR One prev. birth	(3) AR Two prev. births	(4) AR No prev. VPTs	(5) AR One prev. VPT	(6) AR Two prev. VPTs
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.03527	0.03973 *	0.06118 **	0.07773 *	0.04618 **	0.00601
	[0.03425]	[0.02407]	[0.02716]	[0.04403]	[0.02013]	[0.01618]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	-0.00184	0.03932 *	0.03557 *	0.05121	0.00927	0.00404
	[0.02990]	[0.02111]	[0.02097]	[0.03427]	[0.01458]	[0.01296]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	-0.00640	0.03297 *	0.02566	0.00315	0.02862 *	0.00375
	[0.02644]	[0.01818]	[0.01986]	[0.03427]	[0.01458]	[0.01296]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.08540	0.09021	0.10048	0.09458	0.09009	0.07543
Policy covariates	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓	✓	✓	✓	✓
Mean	0.445	0.247	0.287	0.867	0.178	0.038

Table 6: The table reports OLS estimates of the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, separately by previous reproductive history. Columns (1)–(3) classify women according to the number of previous live births, while Columns (4)–(6) classify women according to the number of previous VPTs. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

This reproductive-history gradient is consistent with uncertainty affecting pregnancy decisions when household resources are already committed to existing children. For these households, uncertainty about future income increases the perceived financial cost of an additional child. The pandemic may also have tightened constraints on parental time: Italian mothers experienced substantial increases in childcare and housework, especially when they were outside the labor force.[17] The absence of a response among childless women, who faced the same local exposure to work suspensions, indicates that uncertainty mattered particularly where the income and time required by an additional child were already constrained.

Table 7 reports estimates by macro-area. The response is concentrated in Northern Italy, where both the third- and fourth-quartile coefficients are statistically significant, while no detectable effect emerges in the Centre or South. This geographic pattern is consistent with the sectoral evidence: exposure to suspended industrial employment was greatest in Northern municipalities because of their pre-pandemic productive structure. We therefore interpret the geographic heterogeneity primarily as reflecting differences in the source and intensity of labor-market exposure rather than as an independent mechanism.

[17]The increase in childcare and housework among Italian parents during the pandemic is documented by Del Boca et al., 2020, Biroli et al., 2021, Brini et al., 2021, and Mangiavacchi et al., 2021. Del Boca et al., 2022 show that women outside the labor force were particularly likely to absorb this additional burden.

Impact of work suspension on municipal abortion rates by Italian macro-area (OLS)

	(1) AR North	(2) AR North	(3) AR Center	(4) AR Center	(5) AR South	(6) AR South
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.19098 ***	0.16360 ***	0.00555	0.03342	0.03087	0.01954
	[0.06749]	[0.05401]	[0.17073]	[0.12425]	[0.07455]	[0.06002]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.14382 **	0.12009 ***	0.18416	0.14645	0.03211	0.02817
	[0.05869]	[0.04581]	[0.14595]	[0.10643]	[0.06254]	[0.05065]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.00007	0.00375	0.03399	0.02947	0.07342	0.06292
	[0.05744]	[0.04472]	[0.13372]	[0.09751]	[0.06590]	[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 × Quarter-year FE	✓	✓	✓	✓	✓	✓
Weight		✓		✓		✓
Mean	1.083	1.083	1.240	1.240	1.102	1.102

Table 7: The table reports OLS estimates from specifications estimated separately for Northern, Central, and Southern Italy. Columns (1)–(2) refer to the Northern sub-sample, Columns (3)–(4) to the Central sub-sample, and Columns (5)–(6) to the Southern sub-sample. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. The panel is constructed by aggregating abortions at the municipality-of-residence level. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.

The heterogeneity analysis provides the main evidence supporting our interpretation. The response is concentrated among mothers in established households who are outside the labor force, particularly in municipalities where suspensions disproportionately affected male-dominated industrial employment. We cannot directly observe partners' employment or identify the principal earner within each household. Nonetheless, the joint pattern is consistent with households in which the woman's economic circumstances depend heavily on another household member whose employment prospects became uncertain.

This evidence complements the findings of Huttunen and Kellokumpu, 2016. In their setting, fertility responds to women's own displacement and the associated career consequences, but not to men's displacement despite its larger effect on current family income. Our results consider a different margin: uncertainty about future household income in the absence of permanent displacement and with current earnings partly insured. When women have no observed career exposure of their own, the employment uncertainty faced elsewhere within the household becomes relevant for the decision to continue

an ongoing pregnancy. Together, the two studies suggest that reproductive decisions respond to anticipated changes in the household's future economic position, whether these arise from women's career prospects or from uncertainty surrounding the earnings of another household member, rather than only to contemporaneous income losses.

6 Robustness

We subject the baseline results to a broad set of robustness checks. Specifically, we examine the sensitivity of the estimates to model and sample choices, including alternative treatment definitions; conduct placebo exercises; and assess the potential role of spillovers across municipalities.

6.1 Robustness to alternative specifications

We begin by assessing whether the baseline findings depend on specific modelling choices. Appendix Tables G1–G3 report estimates at frequencies ranging from monthly to annual, with semi-annual estimates shown as an intermediate case. Across all temporal aggregations, the estimated effects remain remarkably stable. As expected, coefficient magnitudes vary mechanically with the length of the aggregation period, but their relative size and statistical significance are largely unchanged. In every specification, the response remains concentrated among municipalities in the highest quartile of the suspended-workers distribution.

The results are equally robust to alternative definitions of treatment intensity. Appendix Table H1 reports a continuous specification alongside categorical definitions based on a median split or terciles. Although statistical precision declines somewhat under the continuous specification, the qualitative conclusions remain unchanged. Likewise, the corresponding event-study estimates based on the continuous treatment measure (Appendix Figure H1) display the same dynamic pattern as the baseline specification, with no evidence of differential pre-treatment trends.

We also examine sensitivity to sample composition. We begin by excluding abortions performed after the 90-day gestational limit, which are generally motivated by medical rather than fertility-related reasons. We then exclude the chief towns of the 15 Italian metropolitan cities, whose size could disproportionately influence the control group. The most conservative exercise combines the first restriction with the exclusion of all procedures classified as urgent, yielding an extremely conservative sample in which nearly all abortions are expected to reflect voluntary fertility choices. The corresponding estimates

are reported in Appendix Tables H2–H4. The first two exercises leave the baseline findings essentially unchanged. Under the third and most restrictive specification, the estimates become smaller and less precise, as expected given the substantial reduction in sample size, but the resulting pattern remains consistent with the baseline results.

6.2 Randomization inference and placebo

We first assess whether the estimated treatment effect could arise by chance. To this end, we implement a randomization inference procedure based on 1,000 permutations, randomly reassigning treatment status across municipalities and comparing the resulting placebo distribution with the baseline estimate. As shown in Appendix Figure H2, the estimated treatment effect lies well in the right tail of the placebo distribution, both with and without province-by-quarter fixed effects. This indicates that the observed relationship is highly unlikely to result from random assignment of treatment exposure.

We next examine whether the baseline estimates could instead reflect recurring seasonal patterns. Although the event-study analysis provides no evidence of differential pre-treatment trends, abortion rates display some residual quarter-to-quarter variation and the treatment effect is largely reabsorbed after two quarters. To rule out the possibility that our estimates simply capture seasonal dynamics correlated with local sectoral composition, we conduct a series of time-placebo exercises.

Specifically, we repeatedly re-estimate the baseline specification by assigning the treatment discontinuity to the second quarter of years other than 2020.[18] The corresponding estimates are reported in Appendix Table G4.

The placebo estimates confirm that the interaction between exposure to work suspensions and the timing of the first-wave lockdown is the only specification that systematically explains abortion dynamics. Placebo treatment dates in years other than 2020 generate no comparable effects, supporting the interpretation that the baseline estimates do not reflect recurring seasonal patterns. The absence of significant placebo effects in 2021 is also consistent with the temporary nature of the response documented by the event-study estimates.

[18]The placebo exercises are based on the following alternative samples and treatment dates: 1) 2018–2021 excluding 2020, with 2018Q2 as the placebo threshold; 2) 2018 only, with 2018Q2; 3) 2018–2019, with 2018Q2; 4) 2018–2019, with 2019Q2; 5) 2018–2021 excluding 2020, with 2019Q2; 6) 2019 only, with 2019Q2; 7) 2019–2020, with 2019Q2; 8) 2018–2020, with 2020Q2; 9) 2019–2020, with 2020Q2; 10) 2020 only, with 2020Q2; 11) 2020–2021, with 2020Q2; 12) 2018–2021 excluding 2020, with 2021Q2; 13) 2021 only, with 2021Q2; 14) 2020–2021, with 2021Q2; and 15) 2018–2021, with 2021Q2.

6.3 Commuting and labor-market spillovers

Our baseline specification defines treatment at the municipality level. A potential concern is that the relevant labor market extends beyond municipal boundaries because workers commute across municipalities within the same Local Labor Market Area (LMA).[19] If households respond to labor-market conditions prevailing within the broader commuting area rather than within their municipality of residence, the baseline estimates may be affected by spillovers across neighboring municipalities.

To address this concern, we re-estimate the baseline specification with standard errors clustered at the LMA level, allowing abortion outcomes to be correlated within commuting areas. A separate specification assigns each municipality the suspended-workers share of its Local Labor Market Area.[20]

Appendix Table I1 reports the results. Panel A shows that clustering standard errors at the LMA level leaves both point estimates and statistical significance virtually unchanged, indicating that within-LMA correlation does not materially affect statistical inference. Panel B reports estimates based on the LMA-level treatment measure. Although coefficient magnitudes vary slightly across specifications, they remain highly comparable to the baseline estimates and continue to imply increases in abortion rates of approximately 10–15% relative to the pre-treatment mean among the most exposed municipalities.

These exercises indicate that the baseline results are not driven by the municipal definition of treatment. Accounting for commuting patterns and broader labor-market interactions leaves the estimated relationship between work suspensions and abortion rates essentially unchanged.

7 Alternative mechanisms and competing explanations

Although our identification strategy isolates geographic variation in exposure to temporary work suspensions, several mechanisms other than household income uncertainty could account for the estimated effects. In this section, we examine the main alternative explanations and show that none is consistent with the full body of evidence.

[19] Appendix Figure I1 reports the geographic distribution of treatment intensity across Local Labor Market Areas.

[20] This specification also mitigates potential measurement error arising from imperfect reporting of the municipality of residence, since misclassification is likely to be less important at the LMA level.

7.1 Access to abortion services

The baseline estimates are interpreted as changes in women's demand for pregnancy termination. An alternative explanation is that the pandemic affected the local provision of abortion services rather than women's fertility decisions. If hospital congestion, mobility restrictions, or epidemiological pressure reduced access to VPT services more severely in municipalities with lower exposure to work suspensions, the positive differential effect observed in highly exposed municipalities could arise even in the absence of differential demand.[21]

This alternative explanation yields a clear empirical prediction. If the baseline estimates were driven by a relative deterioration in abortion services in less exposed municipalities, women residing there should become more likely to travel elsewhere to obtain a VPT. Conversely, if the estimated increase reflects higher demand in the most exposed municipalities, no such pattern should emerge; if anything, abortion mobility should increase among women living in the most exposed areas.

We begin by examining whether epidemiological pressure differed systematically across municipalities with different exposure to work suspensions. We then study service access from two perspectives: an individual-level model of abortion mobility and a version of the baseline model that aggregates abortions by municipality of procedure rather than residence.

Epidemiological pressure. As a first step, we examine whether municipalities with different exposure to work suspensions also experienced systematically different epidemiological conditions. Appendix Figure J2 reports the evolution of excess mortality, measured as the percentage change in quarterly municipal mortality relative to the corresponding quarter of 2015–2019, together with cumulative provincial COVID-19 cases normalized by population. Neither measure displays meaningful differences across the suspended-workers distribution during the quarters in which the treatment effect emerges. Differential hospital congestion correlated with treatment exposure therefore appears unlikely to account for the baseline estimates.

Abortion mobility. The most direct implication of the supply-side explanation concerns abortion mobility. If access to VPT services deteriorated disproportionately in municipalities with lower exposure

[21] Whether the pandemic disrupted access to abortion services in Italy remains debated. The Italian ISS argued that the provision of VPT services was largely maintained (INIH, 2022), whereas press reports (Muratori and Di Tommaso, 2020, Torrisi, 2021) and some healthcare studies (Ong et al., 2023) documented temporary disruptions associated with hospital overload. At the same time, several procedural restrictions were relaxed during the emergency (Bojovic et al., 2021).

to work suspensions, women residing there should become more likely to obtain the procedure elsewhere. Conversely, if the baseline estimates reflect an increase in demand in highly exposed municipalities, abortion mobility should, if anything, increase among women living in those municipalities.

To distinguish between these competing predictions, we estimate an individual-level difference-in-differences model of abortion mobility. Following Balia et al., 2020, the outcome is an indicator equal to one if a woman undergoing a VPT obtains the procedure outside either her municipality or her Local Labor Market Area of residence. The specification exploits the rich set of demographic, socioeconomic, reproductive, and health-related characteristics available in the ISTAT records while maintaining the same treatment definition adopted in the baseline analysis.[22]

Tables J3 and J4 provide little support for the supply-side explanation. Women residing in municipalities with greater exposure to work suspensions are significantly more likely to obtain a VPT outside their municipality or Local Labor Market Area of residence. The estimated increase ranges from approximately 8 to 25% of the pre-treatment mean, depending on the specification.

This pattern is difficult to reconcile with a contraction in abortion services in less exposed municipalities. Instead, it is consistent with higher demand in the most exposed areas, where women appear willing to travel in order to obtain a VPT despite the mobility restrictions in place during the first wave of the pandemic.

Location of service provision. As a complementary exercise, we aggregate abortions according to the municipality in which the procedure is performed rather than the municipality of residence. Because the relevant population served by each hospital is unobserved, this exercise cannot separately identify changes in supply and demand. We therefore interpret it as descriptive evidence rather than as a causal analysis.

The corresponding estimates are reported in Table J1. Across specifications, coefficients are uniformly small and statistically insignificant. Moreover, the estimated effect for municipalities in the highest quartile of exposure is consistently negative. Although these estimates are descriptive, they provide no indication that abortion services contracted disproportionately in municipalities with greater exposure to work suspensions.

The evidence offers little support for a supply-side interpretation of the baseline results. Epidemi-

[22]The estimation sample excludes municipalities (or LMAs) where no VPT services are available, since women residing there necessarily obtain the procedure elsewhere. The full specification and the complete list of controls are reported in the Appendix.

ological conditions evolved similarly across municipalities with different exposure to work suspensions. At the same time, abortion mobility increased among women living in the most exposed areas, whereas hospital-level analyses reveal no differential changes in service provision. This combination of results is more consistent with an increase in women's demand for pregnancy termination than with changes in the local availability of abortion services.

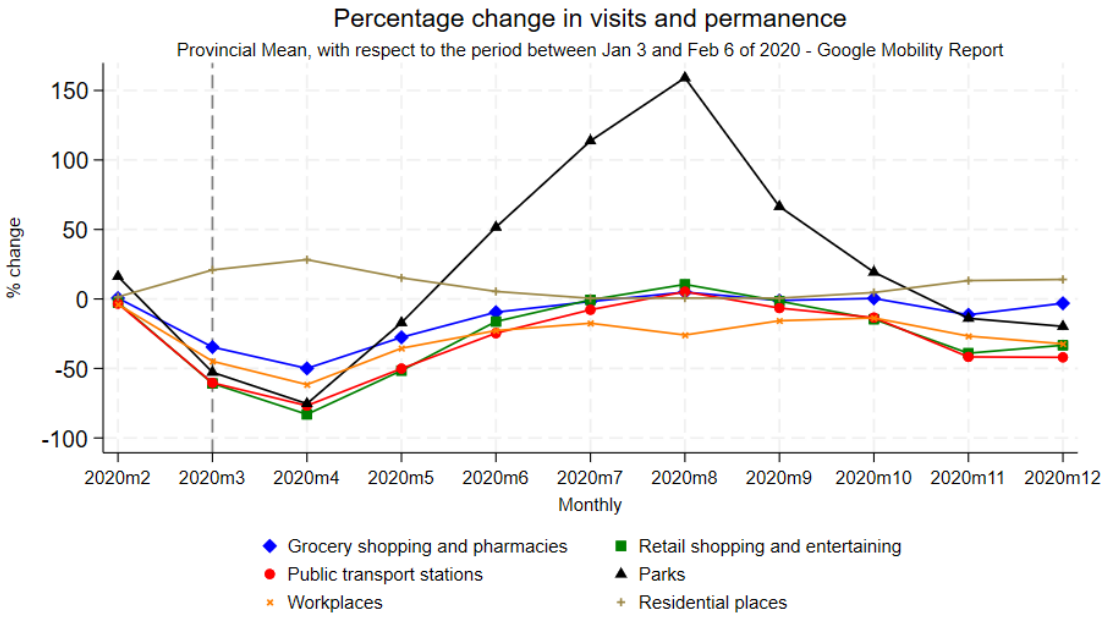


Figure 5: Mean monthly provincial percentage variation, relative to the baseline period between January 3 and February 6, 2020, in visits to and time spent in different types of places.

7.2 Domestic violence

A second alternative explanation is that the positive differential effect on abortion rates reflects changes in intimate partner violence rather than uncertainty about future household income. A large literature connects domestic violence with unintended pregnancy and abortion (Hall et al., 2014, Citernesi et al., 2015). Several mechanisms suggest that the first-wave lockdown could have increased violence within households: prolonged cohabitation and heightened economic stress (Dugan et al., 1999, Card and Dahl, 2011, Diaz and Saldarriaga, 2023), alongside changes in intra-household bargaining power (Aizer, 2010).

This explanation also yields a testable prediction. If domestic violence increased disproportionately in areas more exposed to work suspensions, indicators of reported violence should display a similar geographic pattern.

To examine this possibility, we use administrative data on calls to the Italian national domestic-violence hotline (1522), available at the provincial level. Figure K1 documents the sharp nationwide increase in hotline calls during the first months of the pandemic. However, previous work argues that part of this increase reflects greater awareness and reporting rather than changes in the underlying incidence of violence (Colagrossi et al., 2022).

We therefore estimate the same two-way fixed-effects difference-in-differences specification adopted in the baseline analysis, replacing abortion rates with the provincial rate of calls to the 1522 hotline. Exploiting variation in the provincial distribution of suspended workers allows us to test whether provinces more exposed to work suspensions experienced a differential increase in reported domestic violence.

Appendix Table K1 provides no evidence that provinces with greater exposure to work suspensions experienced a differential increase in domestic-violence reports. Estimated effects are uniformly small, statistically insignificant, and in several specifications negative, both when considering all hotline calls and when restricting the analysis to victims only.

Although hotline calls are an imperfect proxy for the true incidence of domestic violence, the available evidence offers little support for this mechanism. The geographic pattern of reported violence does not mirror the geographic pattern of abortion responses, suggesting that differential changes in domestic violence are unlikely to account for our baseline findings.

7.3 Contraception

A further alternative explanation is that municipalities more exposed to work suspensions also experienced differential changes in access to contraception. If this were the case, the positive differential effect on abortion rates could reflect changes in contraceptive availability rather than uncertainty about future household income. Because detailed local data on contraceptive use are unavailable, we rely on two complementary sources of indirect evidence.

We first consider the availability of family planning centres (*consultori familiari*), which provide reproductive-health services and contraceptive counselling. Appendix Figure L1 relates both the number of centres per 10,000 women aged 18–49 and their growth between 2019 and 2022 to the provincial share of suspended workers. We find no evidence of a systematic relationship between exposure to work suspensions and the local availability of reproductive-health services.

We next consider emergency contraception pills (ECPs), which remained widely available throughout the pandemic because pharmacies were never closed and pharmacists cannot invoke conscientious

objection. Figure L2 shows that national ECP sales remained broadly stable during the study period, providing no indication of major disruptions in access.

A potentially more informative source of variation arises from the regulatory reform introduced in October 2020, which made emergency contraception available over the counter for minors. If improved access to ECPs played an important role in explaining our baseline findings, the reform should have differentially affected abortion rates among underage women. We therefore estimate a two-way fixed-effects difference-in-differences specification in which treatment is interacted with a post-2020Q3 indicator and the outcome is the abortion rate among minors. Appendix Table L1 reports no statistically significant effects across specifications.

The contraception analyses provide little support for that explanation of the baseline results. Although we cannot directly observe contraceptive use, neither the availability of family planning services nor the expansion of access to emergency contraception appears to account for the observed increase in abortion rates.

8 Pregnancies carried to term

Our interpretation predicts that uncertainty about future household income primarily affects the decision to continue an ongoing pregnancy. Whether the same shock also affects pregnancies carried to term is less clear. On the one hand, greater uncertainty may discourage households from continuing existing pregnancies or planning new births. On the other hand, increased time spent together during the lockdown could have increased conceptions. Examining pregnancies resulting in live births therefore provides an additional test of the mechanisms underlying our baseline findings.

Existing evidence points in both directions. Restrictions on abortion access have been shown to increase subsequent births (Lindo et al., 2020), whereas adverse labor-market conditions reduce fertility while increasing abortion (Cavallini, 2024).

To investigate this margin, we use daily birth-registration data covering the period 2018–2021. Following the demographic literature, we approximate conception dates by shifting each birth backward by 270 days and aggregate the resulting series at the municipality-quarter level. We then estimate the same empirical specification used in the baseline analysis, replacing the abortion rate with the rate of pregnancies resulting in live births.

Table 8 reports the results. Across all specifications, the estimated coefficients are positive but

Effect of work suspensions on municipal rates of pregnancies resulting in live births

	(1) Pregnancy rate	(2) Pregnancy rate	(3) Pregnancy rate	(4) Pregnancy rate	(5) Pregnancy rate	(6) Pregnancy rate
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.18441	0.09482	0.18762	0.09508	0.19109	0.08248
	[0.15293]	[0.11474]	[0.15364]	[0.11531]	[0.17377]	[0.13211]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.07355	0.05142	0.06774	0.04531	0.04755	0.02530
	[0.12351]	[0.09754]	[0.12426]	[0.09808]	[0.13557]	[0.10721]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.05488	0.02759	0.05190	0.02512	0.03168	0.01163
	[0.11799]	[0.09259]	[0.11817]	[0.09278]	[0.12208]	[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			✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province × quarter-year FE					✓	✓
Population weights		✓		✓		✓
Pre-treatment mean	7.722	7.722	7.722	7.722	7.722	7.722

Table 8: Effect of work suspensions on municipal rates of pregnancies resulting in live births. The table reports estimates from linear two-way fixed-effects difference-in-differences regressions. The dependent variable is the number of pregnancies resulting in a live birth per 1,000 resident women aged 15–49. Municipalities are classified into quartiles of the suspended-workers distribution, with the first quartile serving as the omitted category. Policy covariates include the municipal stringency index, the number of days spent under each restriction regime, and excess mortality. Columns (5) and (6) additionally include province-by-quarter-year fixed effects. Weighted specifications use the logarithm of municipal population as analytical weights. *Pre-treatment mean* denotes the average outcome among municipalities in the highest-exposure quartile before 2020Q2. Standard errors clustered at the municipality level are reported in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

imprecisely estimated and never statistically different from zero. We therefore find no evidence that municipalities more exposed to work suspensions experienced differential changes in pregnancies resulting in live births. Additional placebo exercises, reported in Appendix K, assign the treatment to periods in which conceptions could not have been affected by the lockdown. Although a few coefficients are statistically significant, they display neither a consistent sign nor a systematic pattern, suggesting that they are driven by noise or residual seasonality rather than by a genuine treatment effect.

The absence of an effect on pregnancies resulting in live births is not inconsistent with our interpretation. Decisions regarding the continuation of an ongoing pregnancy and decisions leading to a live birth operate on different margins and over different time horizons. Abortions respond immediately to unexpected changes in household expectations, whereas pregnancies resulting in live births reflect the entire process from conception through pregnancy continuation to childbirth. These margins need not respond in the same direction or with the same timing.

As a complementary exercise, Appendix Table M2 reports estimates using the abortion ratio as the outcome. The results are broadly consistent with the baseline findings but become statistically insignificant once province-by-quarter fixed effects are introduced. We therefore regard the abortion ratio as a useful descriptive measure rather than our preferred outcome.

The estimates suggest that the immediate response to uncertainty about future household income operates primarily through decisions concerning ongoing pregnancies rather than through aggregate fertility. In this respect, voluntary pregnancy termination provides a particularly informative measure of households' short-run adjustment to unexpected economic shocks.

9 Conclusions

This paper studies how uncertainty about future household income affects the decision to continue an ongoing pregnancy. Exploiting the Italian labour-market response to the first wave of the COVID-19 pandemic, we examine the effects of a temporary increase in household income uncertainty in a setting where realized income losses were substantially mitigated. We find that, although abortion rates declined throughout Italy following the lockdown, the decline was significantly smaller in municipalities more exposed to temporary work suspensions. The effect is temporary and concentrated among married women outside the labour force, especially in municipalities with greater industrial employment. We detect no accompanying change in births.

The results point to household income uncertainty as the main mechanism linking temporary work suspensions to pregnancy decisions. The observed pattern is difficult to reconcile with explanations based on changes in sexual behaviour (Trommlerová and González, 2024), domestic violence, or access to abortion services (Bo et al., 2015, Autorino et al., 2020, Muratori, 2023), all of which receive little empirical support in our data. Instead, the evidence consistently suggests that households adjust pregnancy decisions in response to changes in expected future economic conditions, even when current household income is largely protected.

Our results contribute to a growing literature showing that fertility decisions respond to labour-market conditions (Currie and Schwandt, 2014, Del Bono et al., 2012, 2015, Schaller, 2016; Schaller et al., 2020, Bardits et al., 2023, Cavallini, 2024). Existing studies have mainly examined realized shocks, such as unemployment or permanent job displacement. We complement this evidence by showing that uncertainty itself can affect pregnancy decisions independently of realized income losses, extending the insights of Huttunen and Kellokumpu, 2016 to a setting in which current household income is largely protected. The results also add to the literature on the socio-economic determinants of abortion demand (Clarke, 2023, Herbst, 2011, González, 2013, González and Trommlerová, 2023) by identifying household income uncertainty as an important determinant of pregnancy termination decisions.

The results also have broader implications for the design of income stabilization policies. Temporary income-support schemes can successfully protect current household resources while leaving uncertainty about future employment and earnings unresolved. Our estimates suggest that this residual uncertainty may itself influence important household decisions. Policies designed to cushion the economic consequences of large shocks may therefore be more effective when they reduce current income losses while also limiting uncertainty about future household economic prospects.

The results also have implications for reproductive-health policy. Previous work has emphasized the importance of ensuring effective access to abortion services, particularly where institutional barriers such as conscientious objection remain relevant (Bo et al., 2015, Autorino et al., 2020, Muratori, 2023). Our results suggest that this issue may become even more important during periods of heightened economic uncertainty, when women facing tighter household constraints may be both more likely to seek a voluntary pregnancy termination and more vulnerable to barriers in accessing reproductive healthcare. The evidence indicates that expectations about future household economic prospects can shape reproductive decisions independently of realized income losses, underscoring the importance of incorporating uncertainty into the analysis of fertility behaviour and the design of family policies.

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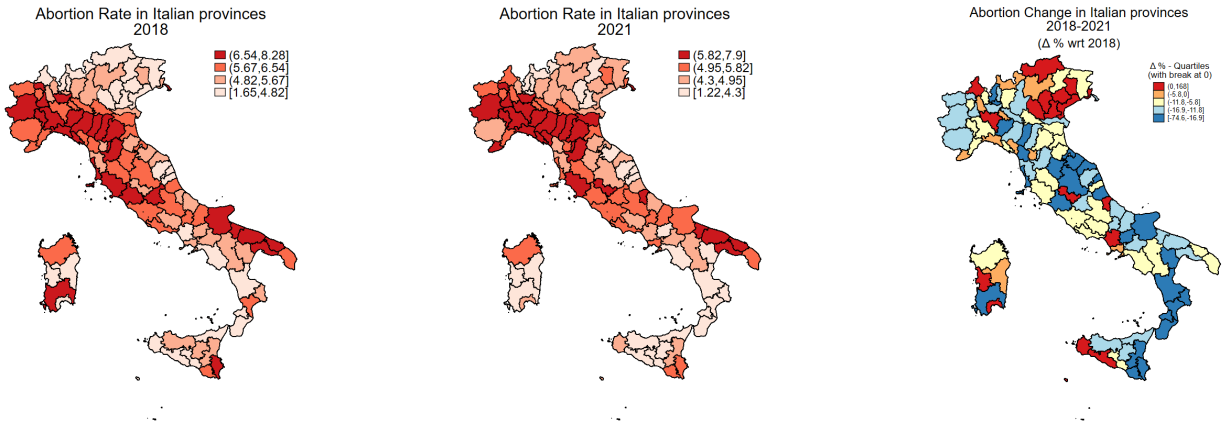
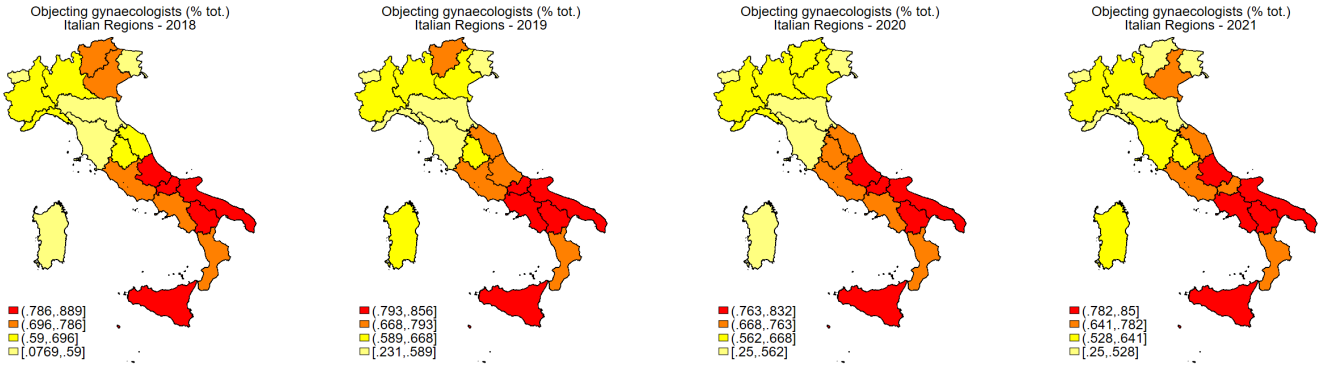
Online Appendix

A Aggregate data on Abortions



(a) (b) (c)

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



(a) (b) (c) (d)

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

Dataset	Content	Main processing for this paper
ISTAT–ADELE (Abortion microdata)	Universe of Italian voluntary pregnancy terminations 2018–2021	∗ Municipal × quarter aggregation; ∗ Outcome: Abortion Rate (AR) $AR = \frac{VPTs}{women\ 15-49}$
ISTAT–ADELE (Birth registrations)	Universe of birth records (daily) 2018–2021	∗ Births shifted 9 months back ∗ No 2021 pregnancies from Apr2021 ∗ Outcome: Qterly Preg. Rate (PR) $PR = \frac{Pregs.\ turned\ births}{women\ 15-49}$
demo.istat.it (Demographic data)	Municipal population by age and gender 2018–2021	∗ Denominators for AR and PR
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 2018–2021	∗ Provider-level panel building Quarter × municipality ∗ For Robustness checks
Conteduca et al. (2022) (*IEJ*)	Covid restriction indicators 2020–2021	∗ Controls for heterog. restrictions ∗ Quarterly days of Red Area ∗ Quarterly days of Orange Area ∗ Quarterly days of Yellow Area ∗ Quarterly stringency index
ISTAT mortality data	Municipal excess mortality 2020–2021	∗ Quarterly control for severity. ∗ Covariate: Excess Mortality (EM) $EM = \frac{2020\ deaths - avg\ deaths\ (2015/19)}{pop.}$

Table A1: Data sources and processing.

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

Table A2: Descriptive statistics.

B Covid-19 data

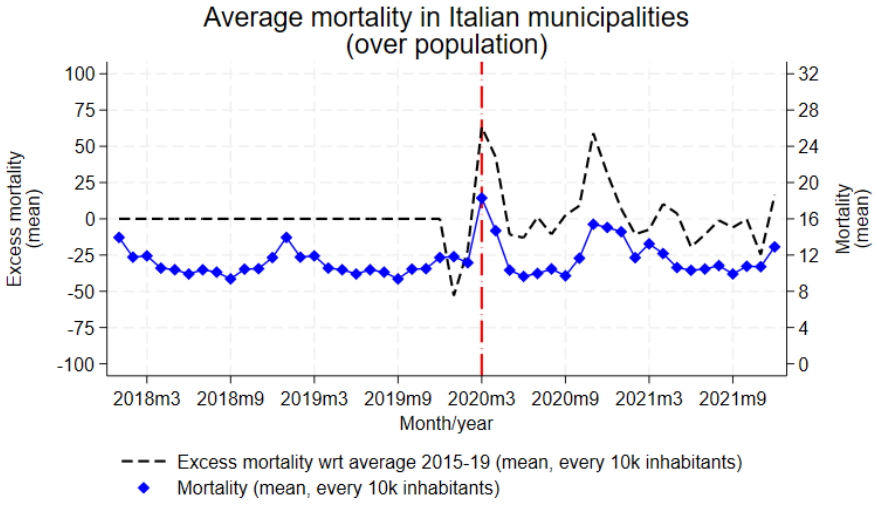


Figure B1: Path of municipal excess mortality in Italy between 2018 and 2021.

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 **??**, 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:

$$I_{mti} = 100 * \frac{v_{mti}}{V_i} \tag{3}$$

- 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:

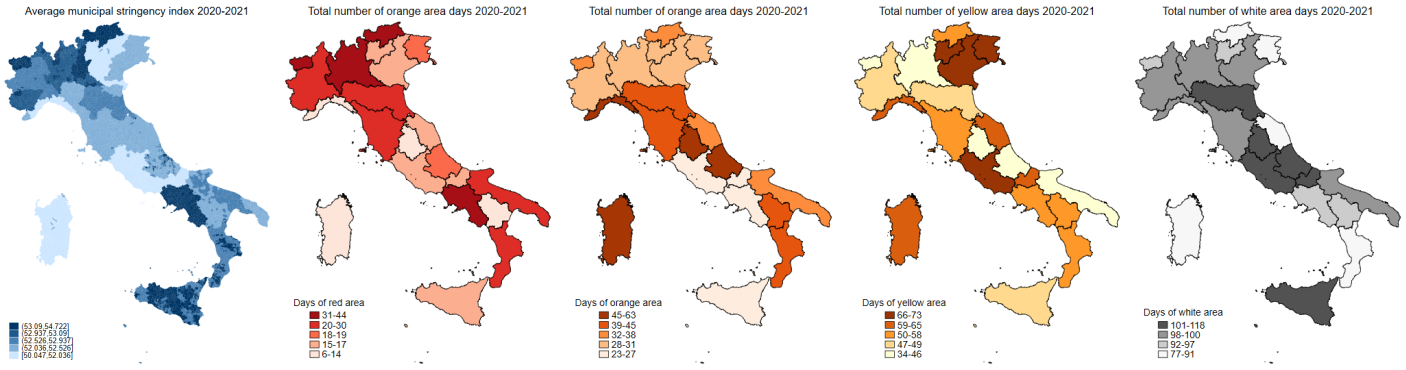
$$I_{mti} = 100 * \frac{\sigma^g_{mt} v^g_{mti} + (1 - \sigma^g_{mt}) v^{ng}_{mti}}{V_i} \tag{4}$$

- v^g_{mti} = variables indicating the restrictions *with* Green Pass;
- v^{ng}_{mti} = variables indicating the restrictions *without* Green Pass;
- σ^g_{mti} = 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:

$$ItSI_{mt} = \sum_i w_i I_{mti} \tag{5}$$

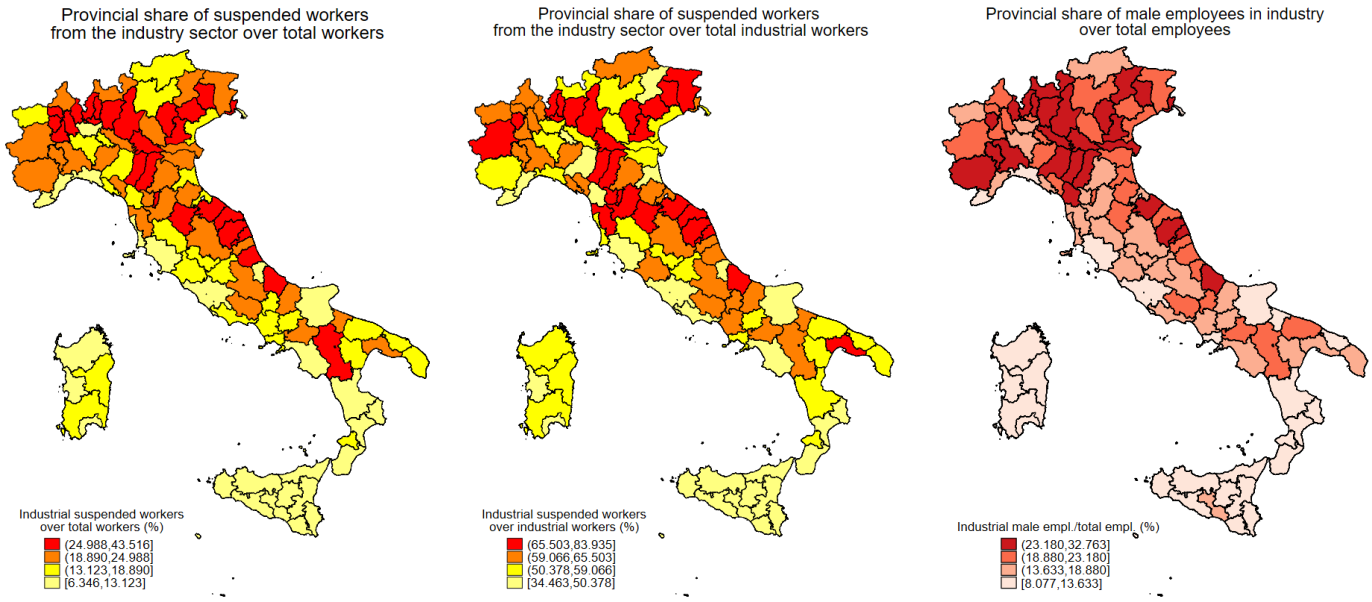
- $w_i = \frac{1}{9}$ for all indicators, except for **C2_1_Production**, **C2_2_Shops**, **C2_3_Bar_Restaurants** (for this subset, $w_i = \frac{1}{27}$).



(a) Avg municipal stringency index. (b) Red days (c) Orange days (d) Yellow days (e) White days

Figure B2: Elaboration on the data by Conteduca and Borin, 2022

C Industrial Structure



(a) (b) (c)

Figure C1: Maps (a) and (b) represent the provincial share of suspended workers in the industrial sector. Map (a) reports the share relative to the total number of private-sector workers; map (b) reports the share relative to the number of private-sector workers employed in industry only. Map (c) shows the provincial share of active male workers in the private industrial sector, using *LFS* 2019 data.

Correlation between inactive share and labor market indicators

	(1) I.S. (%)	(2) I.S. (%)	(3) I.S. (%)	(4) I.S. (%)	(5) I.S. (%)
(1) WFR	0.85864				
	[0.76011]				
(2) WFR (males)		1.50612 **			
		[0.59623]			
(3) Ind. Sh.			0.48438 ***		
			[0.05660]		
(4) Ind. Sh. (m/tot. em.)				0.67572 ***	
				[0.07827]	
(5) Ind. Sh. (m/m em.)					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

Table C1: The table reports province-level regressions of the suspended workers' share in 2020 (%) on a set of pre-pandemic labor-market indicators. Column (1) uses the *Working From Remote* (WFR) index proposed by Barbieri et al., 2022. Column (2) uses the corresponding index computed using only male employment shares. Columns (3)–(5) use alternative measures of industrial specialization: the overall industrial employment share, the share of male industrial workers over total employment, and the share of male industrial workers over total male employment. Robust standard errors are reported in brackets. *Mean* reports the average suspended-workers share across provinces.

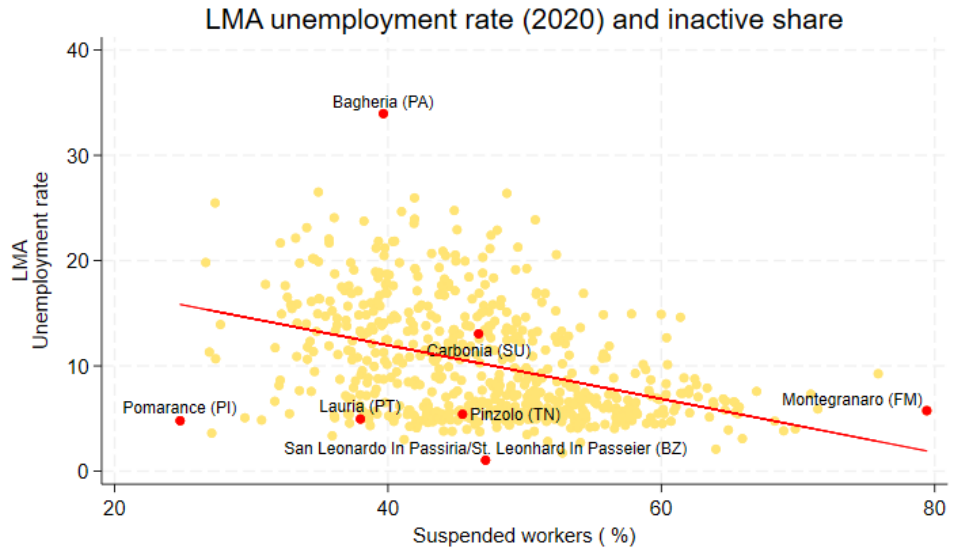


Figure C2: Predicted values between Local Market Areas' unemployment rate (2020, left panel) and suspended workers' share. The highlighted LMAs correspond to the ones having the maximum/minimum 2020's unemployment rate and inactive share. Carbonia (SU) has the median value of the inactive share.

D PPML Specification

Impact of work suspensions on municipal abortion rates (PPML estimates).

	(1) AR (ME)	(2) AR (ME)	(3) AR (ME)	(4) AR (ME)	(5) AR (ME)	(6) AR (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	0.04614	0.05855	0.04471	0.07307	0.05239
	[0.04669]	[0.03527]	[0.04697]	[0.03537]	[0.04834]	[0.03685]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$	0.03709	0.02896	0.03769	0.02919	0.03967	0.02714
	[0.04346]	[0.03258]	[0.04350]	[0.03205]	[0.04300]	[0.03279]
Observations	116,192	116,192	116,192	116,192	116,192	116,192
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter−year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarterly FE					✓	✓
Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103

Table D1: The table reports average marginal effects from Poisson Pseudo-Maximum Likelihood (PPML) regressions. Coefficients represent the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on municipal abortion rates. The omitted category is the first quartile. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality and quarter-year fixed effects. Columns (5) and (6) additionally include province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *** p<0.01, ** p<0.05, * p<0.10.

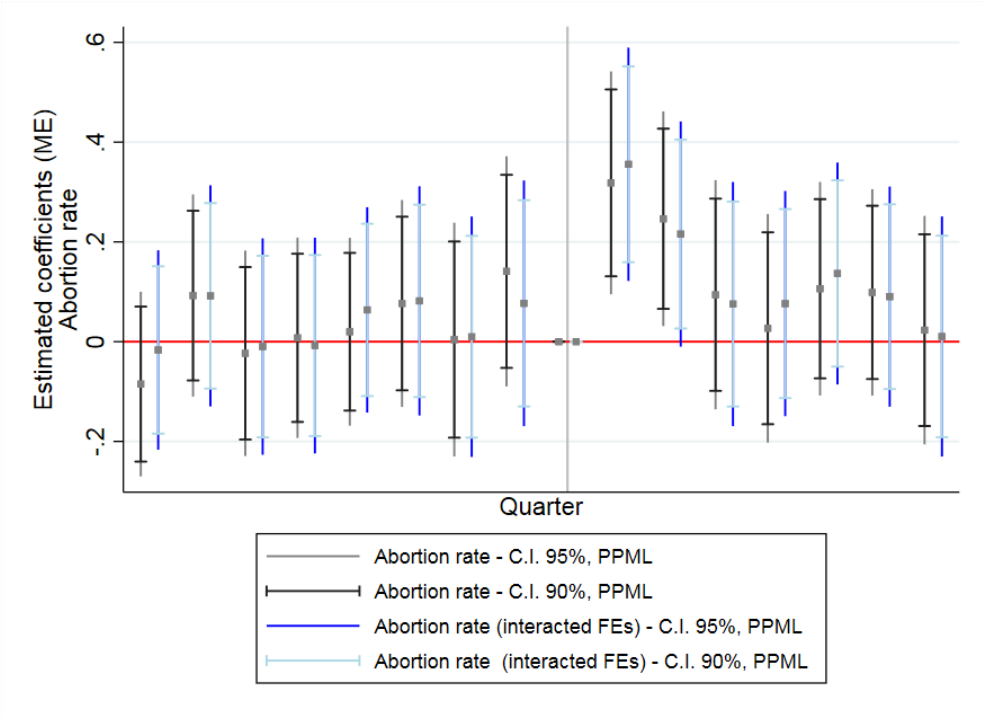


Figure D1: PPML event-study estimates. The figure reports average marginal effects and 95% confidence intervals from the baseline specification and from the model including province-by-time fixed effects. The omitted category is the quarter immediately preceding treatment ($2020Q1$). The vertical line marks the beginning of the post-treatment period.

E 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 not in prof. condition	(6) AR services	(7) AR industry	(8) AR P.A
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$ (*services*)	0.00888	-0.01529	0.01282	0.00827				
	[0.03517]	[0.02728]	[0.01148]	[0.01118]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_3)$ (*services*)	-0.01111	-0.02795	0.01076	0.00897				
	[0.03013]	[0.02217]	[0.01027]	[0.00729]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$ (*services*)	-0.03148	-0.01314	0.00714	0.00893				
	[0.02931]	[0.02264]	[0.00957]	[0.00718]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$ (*industry*)					0.07977 **	0.04531	-0.00052	0.00780
					[0.03712]	[0.02958]	[0.01460]	[0.00917]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_3)$ (*industry*)					0.03449	0.00661	-0.00830	0.00150
					[0.03519]	[0.02689]	[0.01157]	[0.01012]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$ (*industry*)					0.05034	-0.00635	-0.01250	0.00308
					[0.03118]	[0.02338]	[0.01038]	[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 E1: 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 - outcome across marital status; treatment across sectors (OLS)

	Industry Share				Service Share			
	(1) AR single	(2) AR married	(3) AR separated	(4) AR widow	(5) AR single	(6) AR married	(7) AR separated	(8) AR widow
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_4)$ (*services*)	0.00107	-0.02319	0.00026	0.00941				
	[0.04030]	[0.02880]	[0.00432]	[0.00767]				
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_3)$ (*services*)	-0.01561	-0.02276	0.00061	-0.00599				
	[0.02999]	[0.02681]	[0.00371]	[0.00553]				
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_2)$ (*services*)	-0.02311	-0.03385	0.00303	0.00048				
	[0.03611]	[0.02929]	[0.04846]	[0.02652]				
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_4)$ (*industry*)					0.08384 *	0.03899	0.00360	-0.00737
					[0.04353]	[0.03038]	[0.00470]	[0.00832]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_3)$ (*industry*)					0.01386	0.02836	0.00009	-0.01190 **
					[0.03960]	[0.02784]	[0.00434]	[0.00566]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_2)$ (*industry*)					-0.00482	0.01194	0.00414	-0.00311
					[0.03595]	[0.02420]	[0.00411]	[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 E2: 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

Impact of work suspension on abortion rates by age class (OLS)

	(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
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.00185	0.01287	0.02969	0.03175	-0.04616 *	0.06145 **	0.03969 *
	[0.00134]	[0.01252]	[0.02310]	[0.02059]	[0.02697]	[0.02411]	[0.02105]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.00140	-0.00001	0.01308	0.04123 **	-0.03659 *	0.01179	0.03267 *
	[0.00118]	[0.01002]	[0.02026]	[0.01831]	[0.02212]	[0.01695]	[0.01760]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.00001	-0.01435	0.00803	0.03552 **	-0.03641 *	0.00652	0.03422 **
	[0.00113]	[0.00882]	[0.01723]	[0.01688]	[0.02085]	[0.01643]	[0.01574]
Observations	126,448	126,448	126,448	126,448	126,448	126,448	126,448
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	0.086	0.074	0.177	0.217	0.262	0.233	0.139

Table E3: The table reports OLS estimates of the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, separately by age class. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

F Fertility

Impact of work suspension on ARs, differentiating the outcome according to past reprod. behaviors, and treatment across sectors (OLS)

	(1) AR w/ prev. pregnancies	(2) AR w/out prev. pregnancies	(3) AR w/ prev. deliveries	(4) AR w/ prev. abortions	(5) AR w/ prev. pregnancies	(6) AR w/out prev. pregnancies	(7) AR w/ prev. deliveries	(8) AR w/ prev. abortions
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$ $(serv.)$	0.00760	-0.00740	0.00939	0.00138				
	[0.01270]	[0.00992]	[0.01220]	[0.00961]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_3)$ $(serv.)$	0.00420	-0.01712 **	0.00718	-0.00038				
	[0.01078]	[0.00767]	[0.01040]	[0.00799]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$ $(serv.)$	-0.00253	-0.01558 **	0.00032	-0.00221				
	[0.01069]	[0.00776]	[0.01030]	[0.00792]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$ $(ind.)$					0.02952 **	0.00168	0.03140 ***	0.01173
					[0.01270]	[0.00992]	[0.01220]	[0.00961]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_3)$ $(ind.)$					0.01859	-0.00984	0.01786	0.00274
					[0.01240]	[0.00922]	[0.01168]	[0.00946]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$ $(ind.)$					0.01047	-0.00454	0.01378	0.00478
					[0.01144]	[0.00911]	[0.01076]	[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 F1: 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 abortion rates: higher-order reproductive histories (OLS)

	(1) AR Three prev. births	(2) AR Four prev. births	(3) AR ≥5 prev. births	(4) AR Three prev. VPTs	(5) AR ≥4 prev. VPTs
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	-0.00059	-0.00570	0.00179	0.00162	0.00014
	[0.01439]	[0.00536]	[0.00430]	[0.00405]	[0.00415]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	-0.00574	-0.00244	-0.00146	0.00001	-0.00110
	[0.01158]	[0.00462]	[0.00417]	[0.00361]	[0.00329]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	-0.00948	-0.00648	-0.00274	-0.00173	-0.00027
	[0.01122]	[0.00484]	[0.00349]	[0.00361]	[0.00329]
Observations	126,448	126,448	126,448	126,448	126,448
R-squared	0.08365	0.07765	0.12733	0.07370	0.13794
Policy covariates	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓	✓	✓	✓
Mean	0.090	0.020	0.014	0.007	0.013

Table F2: The table reports OLS estimates of the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates for women with higher-order reproductive histories. Columns (1)–(3) classify women according to the number of previous live births, while Columns (4)–(5) classify women according to the number of previous VPTs. Policy covariates include the municipal stringency index, the number of days under different restriction levels, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *** p<0.01, ** p<0.05, * p<0.10.

G Temporal sensitivity

Impact of work suspension on abortion rates: annual specification (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S_{\cdot 20} \in Q_4)$	0.45936 ***	0.40473 ***	0.44251 **	0.38749 ***	0.46248 **	0.40370 **
	[0.18155]	[0.14232]	[0.18278]	[0.14344]	[0.20957]	[0.16431]
$Post * \mathbb{1}(I.S_{\cdot 20} \in Q_3)$	0.02775	0.22828 *	0.27461	0.22259 *	0.30372 *	0.24423 *
	[0.17144]	[0.13291]	[0.17227]	[0.13375]	[0.18107]	[0.14047]
$Post * \mathbb{1}(I.S_{\cdot 20} \in Q_2)$	0.15870	0.13585	0.16328	0.13681	0.16474	0.13535
	[0.15953]	[0.12357]	[0.15987]	[0.12403]	[0.16327]	[0.12677]
Observations	31,612	31,612	31,612	31,612	31,612	31,612
R-squared	0.34647	0.35665	0.34674	0.35692	0.35777	0.36792
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Province × Year FE					✓	✓
Weight		✓		✓		✓
Mean	4.374	4.374	4.374	4.374	4.374	4.374

Table G1: The table reports coefficient estimates from linear Difference-in-Differences specifications estimated at the annual frequency. Coefficients represent the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on municipal abortion rates. The omitted category is the first quartile. Policy covariates include the annual average provincial stringency index, the total number of red, orange and yellow area days, and provincial excess mortality. All specifications include municipality and year fixed effects. Columns (5) and (6) additionally include province-by-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities.

Impact of work suspension on abortion rates: monthly specification (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{03/20} \in Q_4)$	0.04378 ***	0.03780 ***	0.04125 ***	0.03556 ***	0.04285 **	0.03628 ***
	[0.01522]	[0.01188]	[0.01530]	[0.01194]	[0.01736]	[0.01356]
$Post * \mathbb{1}(I.S._{03/20} \in Q_3)$	0.01904	0.01608	0.01989	0.01666	0.02235	0.01811
	[0.01422]	[0.01100]	[0.01414]	[0.01093]	[0.01495]	[0.01157]
$Post * \mathbb{1}(I.S._{03/20} \in Q_2)$	0.01162	0.00987	0.01120	0.00931	0.01064	0.00850
	[0.01317]	[0.01019]	[0.01325]	[0.01024]	[0.01343]	[0.01041]
Observations	379,344	379,344	371,441	371,441	371,441	371,441
R-squared	0.03428	0.03622	0.03402	0.03593	0.04544	0.04611
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓
Province × Month-year FE					✓	✓
Weight		✓		✓		✓
Mean	0.368	0.368	0.368	0.368	0.368	0.368

Table G2: The table reports coefficient estimates from linear Difference-in-Differences specifications estimated at the monthly frequency. Coefficients represent the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on municipal abortion rates. The omitted category is the first quartile. Policy covariates include the lagged monthly provincial stringency index, the number of days under different restriction levels, and provincial excess mortality. All specifications include municipality and month-year fixed effects. Columns (5) and (6) additionally include province-by-month-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities.

Impact of work suspension on abortion rates: semestral specification (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{03/20} \in Q_4)$	0.22968	0.20237	0.22456	0.19714	0.23061	0.20126
	**	***	**	***	**	**
	[0.09077]	[0.07116]	[0.09114]	[0.07150]	[0.10459]	[0.08196]
$Post * \mathbb{1}(I.S._{03/20} \in Q_3)$	0.13875	0.11414	0.13668	0.11126	0.15137	0.12155
		*		*	*	*
	[0.08572]	[0.06646]	[0.08600]	[0.06675]	[0.09039]	[0.07009]
$Post * \mathbb{1}(I.S._{03/20} \in Q_2)$	0.07935	0.06793	0.08049	0.06764	0.08222	0.06726
	[0.07976]	[0.06178]	[0.07984]	[0.06193]	[0.08149]	[0.06325]
Observations	63,224	63,224	63,224	63,224	63,224	63,224
R-squared	0.19149	0.19967	0.19158	0.19974	0.20417	0.21164
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Semester-year FE	✓	✓	✓	✓	✓	✓
Province × Semester-year FE					✓	✓
Weight		✓		✓		✓
Mean	2.187	2.187	2.187	2.187	2.187	2.187

Table G3: The table reports coefficient estimates from linear Difference-in-Differences specifications estimated at the semestral frequency. Coefficients represent the intention-to-treat effect of belonging to different quartiles of the suspended workers' share distribution on municipal abortion rates. The omitted category is the first quartile. Policy covariates include the average provincial stringency index, the number of days under different restriction levels, and provincial excess mortality over the semester. All specifications include municipality and semester-year fixed effects. Columns (5) and (6) additionally include province-by-semester-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities.

Placebo estimates: impact of work suspension on municipal abortion rates (OLS)

	(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
Post 2018$Q2 * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.09653	0.06472	0.08486												
	[0.07505]	[0.08175]	[0.07739]												
Post 2019$Q2 * \mathbb{1}(I.S._{q2/20} \in Q_4)$				0.05436	0.01368	0.04662	0.05396								
				[0.05313]	[0.09276]	[0.06974]	[0.08085]								
Post 2020$Q2 * \mathbb{1}(I.S._{q2/20} \in Q_4)$								0.20488 **	0.15689 ***	0.14117 **		0.14289 *			
								[0.09449]	[0.05259]	[0.05838]		[0.08823]			
Post 2021$Q2 * \mathbb{1}(I.S._{q2/20} \in Q_4)$											0.04008		0.00559	-0.04315	0.01066
											[0.05540]		[0.10336]	[0.06281]	[0.05341]
Observations	94,836	31,612	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	0.13358	0.17005	0.13589	0.16453	0.29015	0.16449	0.11216
Policy covariates ✓									✓	✓		✓	✓	✓	✓
Excess mortality ✓								✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time-span	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
Mean	1.144	1.144	1.144	1.114	1.160	1.114	1.160	1.181	1.103	1.104	1.085	1.181	1.013	1.058	1.103

Table G4: The coefficients in Columns (1) to (15) represent municipal-level OLS placebo estimates of the effect of belonging to the fourth quartile of the suspended workers' share distribution on abortion rates. Placebo treatment dates are assigned to the second quarter of 2018, 2019, 2020, and 2021 across different sample windows. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and excess mortality. All specifications include municipality, quarter-year, and province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

H Treatment sensitivity

Impact of work suspension on abortion rates: alternative treatment definitions (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * (I.S._{q2/20})$	0.00325			0.00327		
	**			**		
	[0.00141]			[0.00158]		
$Post * \mathbb{1}(I.S._{q2/20} \geq P50)$		0.07450			0.07678	
		***			**	
		[0.02858]			[0.03177]	
$Post * \mathbb{1}(I.S._{q2/20} \in T3)$			0.10918			0.11324
			***			***
			[0.03744]			[0.04301]
$Post * \mathbb{1}(I.S._{q2/20} \in T2)$			0.01395			0.02001
			[0.03371]			[0.03483]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10048	0.10043	0.10046	0.11225	0.11221	0.11224
Policy covariates	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE				✓	✓	✓
Mean	1.164	1.134	1.103	1.164	1.134	1.103

Table H1: The coefficients in Columns (1) to (6) represent municipal-level OLS estimates of the ITT effect of alternative definitions of treatment intensity. Columns (1) and (4) use the inactive-share measure as a continuous treatment. Columns (2) and (5) define treatment as being above the median of the inactive-share distribution. Columns (3) and (6) use terciles, with the first tercile as the reference category. Policy covariates include the quarterly average provincial stringency index, the number of red, orange and yellow area days, and provincial excess mortality. All specifications include municipality and quarter-year fixed effects. Columns (4)–(6) additionally include province-by-quarter-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

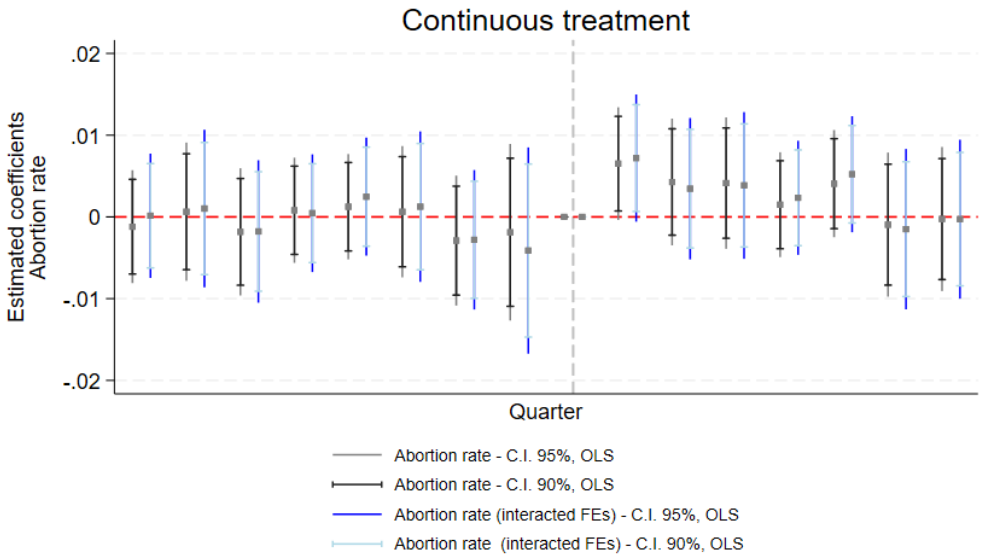


Figure H1: OLS event-study estimates based on the dynamic specification with continuous treatment intensity. The figure reports coefficient estimates and 90% and 95% confidence intervals from the baseline model and from the specification including province-by-quarter-year fixed effects. The omitted category is 2020Q1, the last pre-treatment quarter.

Impact of work suspension on abortion rates: excluding abortions beyond gestational limits (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.12711 ***	0.10910 ***	0.12504 ***	0.10737 ***	0.12628 **	0.10531 ***
	[0.04446]	[0.03469]	[0.04469]	[0.03492]	[0.05043]	[0.03949]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.05544	0.04717	0.05413	0.04595	0.05906	0.04760
	[0.04168]	[0.03228]	[0.04177]	[0.03238]	[0.04397]	[0.03411]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.03729	0.03291	0.03742	0.03276	0.03403	0.02866
	[0.03844]	[0.02980]	[0.03847]	[0.02984]	[0.03897]	[0.03028]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.10189	0.10664	0.10192	0.10666	0.11380	0.11759
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	1.056	1.056	1.056	1.056	1.056	1.056

Table H2: The coefficients in Columns (1) to (6) represent municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, after excluding abortions performed beyond the legal gestational limits prior to panel construction. The reference category is the first quartile. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.

Impact of work suspension on abortion rates: excluding metropolitan cities (OLS)

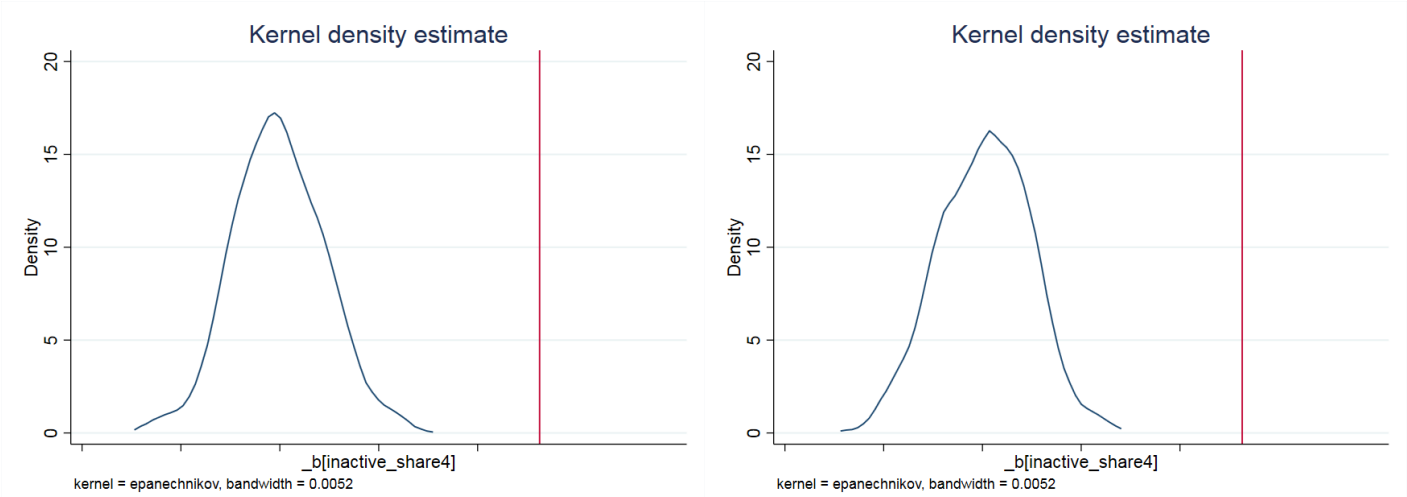
	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.13125 ***	0.11333 ***	0.12888 ***	0.11156 ***	0.13183 **	0.11136 ***
	[0.04576]	[0.03580]	[0.04598]	[0.03602]	[0.05200]	[0.04082]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.05615	0.04712	0.05482	0.04603	0.06231	0.05009
	[0.04276]	[0.03316]	[0.04284]	[0.03325]	[0.04513]	[0.03506]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.03411	0.02915	0.03433	0.02913	0.03270	0.02673
	[0.03960]	[0.03075]	[0.03957]	[0.03075]	[0.04017]	[0.03128]
Observations	126,224	126,224	126,224	126,224	126,224	126,224
R-squared	0.10030	0.10501	0.10033	0.10503	0.11212	0.11594
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103

Table H3: The coefficients in Columns (1) to (6) represent municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, after excluding the chief towns of Italian Metropolitan Cities. The reference category is the first quartile. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.

Impact of work suspension on abortion rates: excluding urgent abortions (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.09557 **	0.08961 ***	0.08975 **	0.08505 ***	0.07036	0.06337 *
	[0.03902]	[0.03069]	[0.03938]	[0.03097]	[0.04490]	[0.03524]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.05835	0.05018 *	0.05477	0.04720 *	0.04516	0.03498
	[0.03581]	[0.02812]	[0.03599]	[0.02827]	[0.03789]	[0.02973]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.01108	0.00975	0.01050	0.00911	0.00467	0.00327
	[0.03380]	[0.02639]	[0.03385]	[0.02644]	[0.03444]	[0.02693]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.09908	0.10414	0.09913	0.10418	0.11107	0.11519
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	0.875	0.875	0.875	0.875	0.875	0.875

Table H4: The coefficients in Columns (1) to (6) represent municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on abortion rates, after excluding urgent abortions prior to panel construction. The reference category is the first quartile. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.



(a) (b)

Figure H2: Randomization inference test with 1000 repetitions. The figure reports the kernel density of coefficients linearly estimated after randomly assigning treatment to observed units, following the empirical strategy in Equation 1. The x-axis reports the estimated coefficients, while the y-axis reports their density. The specification excludes province-by-time fixed effects in Panel (a) and includes them in Panel (b). The vertical line reports the actual estimated baseline coefficient.

I Labor Market Areas

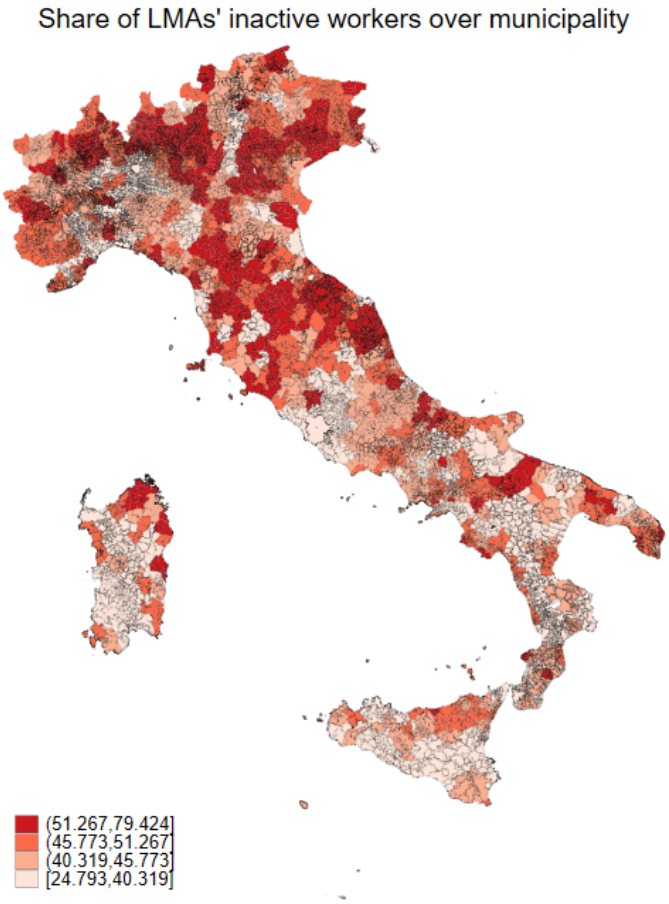


Figure I1: The map shows the LMA share of inactive workers, albeit highlighting Italian municipality borders. The map is shaded according to the position of the unit of reference in given quartiles of the inactive share's distributions.

Impact of Local Labor Market Area-level work suspension on abortion rates (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
Panel A: Treatment at municipal level, SEs clustered at LMA level						
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.13134 ***	0.11340 ***	0.12879 ***	0.11133 ***	0.13168 **	0.11087 ***
	[0.04862]	[0.03783]	[0.04878]	[0.03794]	[0.05419]	[0.04142]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.05711	0.04825	0.05570	0.04698	0.06342	0.05119
	[0.04341]	[0.03324]	[0.04377]	[0.03350]	[0.04587]	[0.03450]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.03486	0.02961	0.03505	0.02949	0.03353	0.02714
	[0.03874]	[0.03022]	[0.03879]	[0.03028]	[0.03960]	[0.03066]
Panel B: Treatment at LMA level, SEs clustered at LMA level						
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.11716 ***	0.10023 ***	0.11368 ***	0.09756 ***	0.16362 ***	0.12432 ***
	[0.03876]	[0.03233]	[0.03864]	[0.03226]	[0.04492]	[0.03757]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.03891	0.03939	0.03684	0.03760	0.03655	0.02933
	[0.03765]	[0.03105]	[0.03771]	[0.03108]	[0.03998]	[0.03317]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.02931	0.01995	0.02870	0.01925	0.02503	0.00985
	[0.04500]	[0.03683]	[0.04477]	[0.03668]	[0.04229]	[0.03434]
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	✓	✓	✓	✓	✓	✓
Province × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	1.103	1.103	1.103	1.103	1.103	1.103

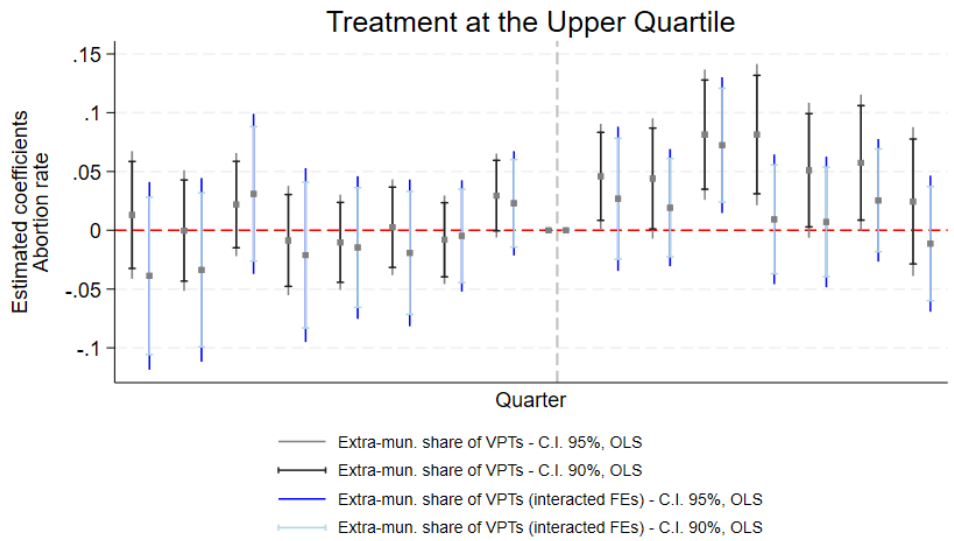
Table I1: The table reports OLS estimates. Panel A replicates the baseline analysis while clustering standard errors at the Local Labor Market Area (LMA) level. Panel B redefines treatment using the distribution of suspended workers at the LMA level and clusters standard errors at the LMA level. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. The panel is constructed by aggregating abortions at the municipality-of-residence level. Standard errors clustered at the LMA level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.

J Abortion Mobility

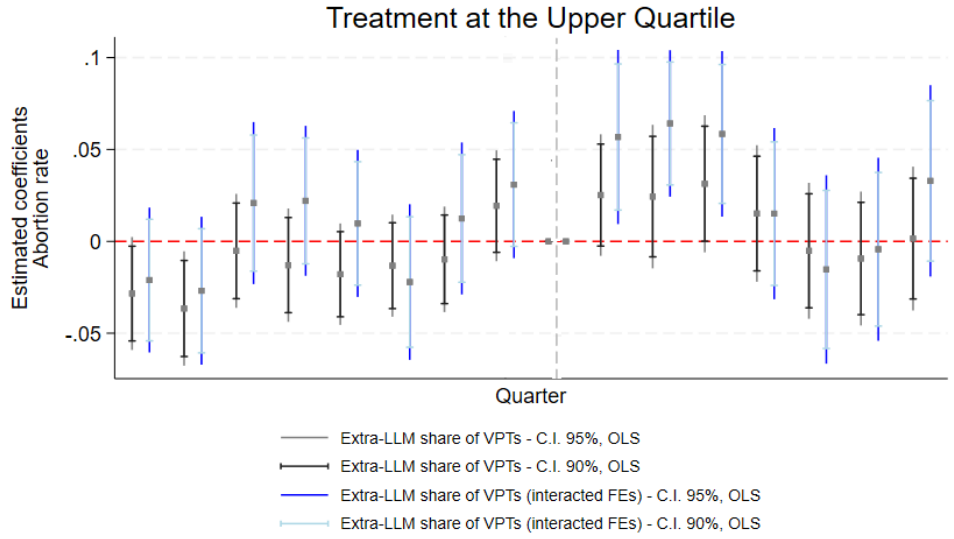
Impact of work suspension on abortion rates at the hospital level (OLS)

	(1) AR	(2) AR	(3) AR	(4) AR	(5) AR	(6) AR
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	-1.34072	-1.50525 *	-1.20354	-1.39824	-1.15342	-1.67544
	[1.13140]	[0.87256]	[1.16186]	[0.88944]	[2.71143]	[2.21840]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.04345	-0.23301	-0.18356	-0.12472	0.78914	0.25097
	[0.80849]	[0.54258]	[0.86028]	[0.57442]	[2.75011]	[2.22934]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	0.42521	0.15314	0.53469	0.24105	0.38287	0.01008
	[0.78781]	[0.49816]	[0.80517]	[0.51111]	[2.73009]	[2.24476]
Observations	5,492	5,492	5,492	5,492	4,552	4,552
R-squared	0.87758	0.87388	0.87767	0.87394	0.88971	0.88212
Policy covariates			✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
LHA × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	9.640	9.640	9.640	9.640	9.640	9.640

Table J1: The coefficients in Columns (1) to (6) represent hospital-level OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share municipal distribution on abortion rates calculated at the hospital level. The reference category is the first quartile of the inactive-share distribution. Policy covariates include the quarterly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. All specifications include hospital and quarter-year fixed effects. Columns (5) and (6) additionally include Local Health Authority (LHA) by quarter-year fixed effects. Standard errors clustered at the hospital level are reported in brackets. *Mean* reports the pre-treatment mean among treated hospitals. *** p<0.01, ** p<0.05, * p<0.10.

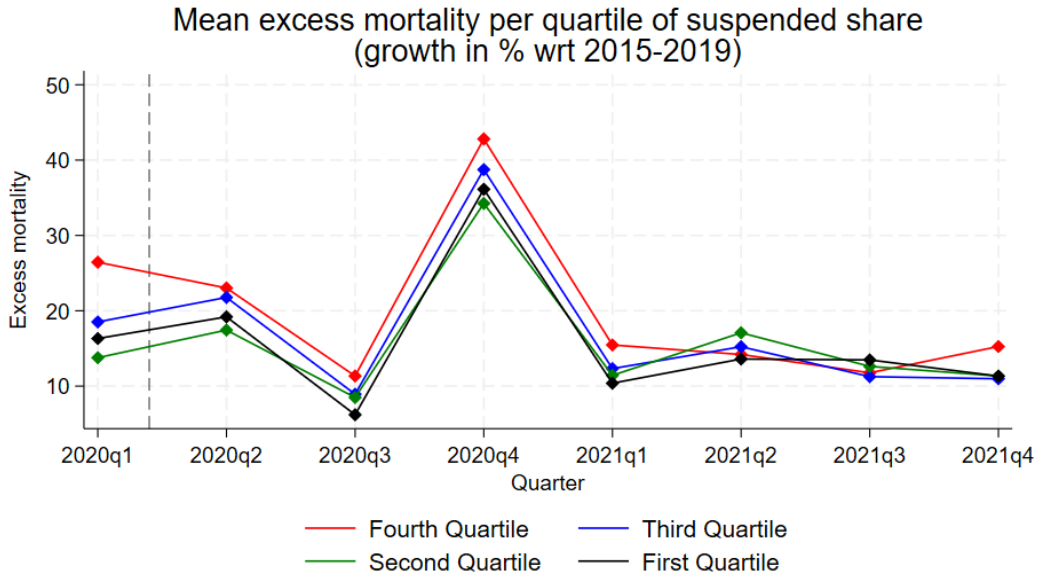


(a)

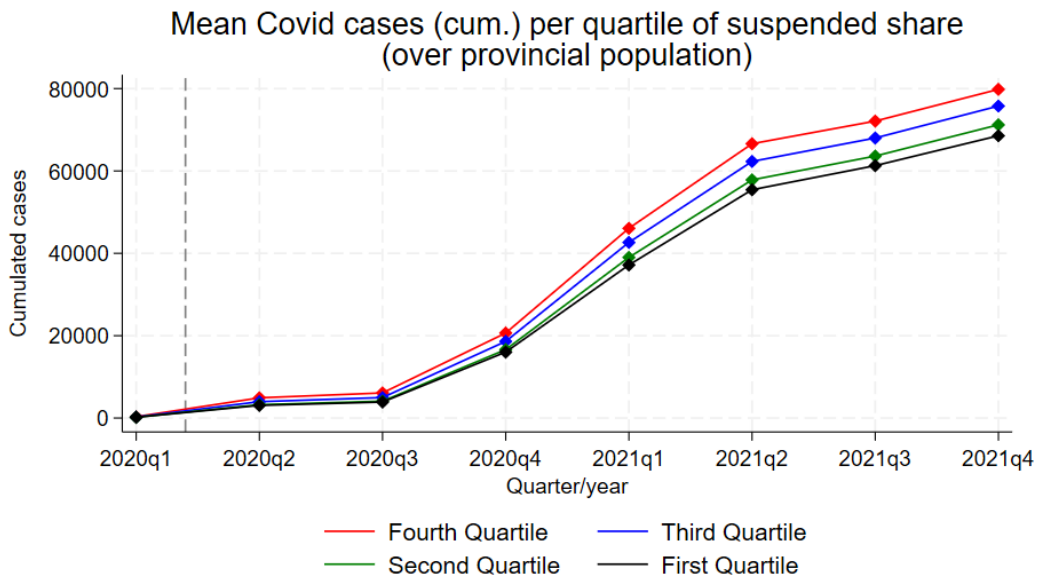


(b)

Figure J1: OLS Event-study estimates (dynamic specification of Equation 2) 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 J2: 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
Abortion mobility	0.233	0.423	0	1	0.249	0.432	0	1
Number of previous live births	1.100	1.157	0	20	1.125	1.156	0	30
Number of previous stillbirths	0.008	0.118	0	10	0.008	0.115	0	10
Number of previous miscarriages	0.191	0.534	0	11	0.192	0.534	0	11
Number of previous VPTs	0.393	0.809	0	20	0.360	0.762	0	20
Gestational age (<90 days)	0.961	0.195	0	1	0.959	0.198	0	1
Gestational age (>90 days)	0.039	0.195	0	1	0.041	0.198	0	1
Weeks of amenorrhea	8.588	2.824	3	26	8.612	2.851	3	26
Urgent abortion	0.244	0.429	0	1	0.242	0.428	0	1
Non-urgent abortion	0.756	0.429	0	1	0.758	0.428	0	1
Presence of child malformations	0.048	0.213	0	1	0.049	0.216	0	1
Absence of child malformations or not indicated	0.952	0.213	0	1	0.951	0.216	0	1
Presence of complications	0.029	0.167	0	1	0.028	0.166	0 1	
Medical abortion	0.357	0.479	0	1	0.348	0.476	0	1
Surgical abortion	0.643	0.479	0	1	0.652	0.476	0	1
Italian citizenship	0.669	0.471	0	1	0.704	0.457	0	1
Age	30.787	7.356	10	60	30.910	7.363	10	60
Level of education								
Elementary school	0.051	0.221	0	1	0.044	0.206	0	1
Middle school	0.367	0.482	0	1	0.370	0.483	0	1
High school	0.430	0.495	0	1	0.448	0.497	0	1
University degree or others	0.151	0.358	0	1	0.138	0.345	0	1
Marital status								
Single	0.614	0.487	0	1	0.598	0.490	0	1
Married	0.344	0.475	0	1	0.358	0.480	0	1
Separated	0.017	0.127	0	1	0.017	0.130	0	1
Widow	0.026	0.158	0	1	0.027	0.162	0	1
Professional condition								
Employed	0.440	0.496	0	1	0.448	0.497	0	1
Unemployed	0.234	0.423	0	1	0.222	0.415	0	1
Looking for first job	0.017	0.128	0	1	0.017	0.130	0	1
Housewife	0.191	0.393	0	1	0.202	0.402	0	1
Student	0.112	0.315	0	1	0.107	0.309	0	1
Other	0.006	0.079	0	1	0.007	0.082	0	1
Professional branch of activity								
Not in professional condition	0.560	0.496	0	1	0.552	0.497	0	1
Agriculture, hunting and fishing	0.006	0.074	0	1	0.008	0.089	0	1
Industry	0.028	0.165	0	1	0.028	0.164	0	1
Trade, services, hospitality (private)	0.136	0.343	0	1	0.147	0.348	0	1
Public administration	0.042	0.200	0	1	0.041	0.198	0	1
Other private services	0.229	0.420	0	1	0.216	0.412	0	1
Obs.				97,550				173,791

Table J2: Descriptive statistics of the variables used for the analysis of inter-municipal and inter-LMA mobility of abortions.

Impact of work suspension on the likelihood of undertaking a VPT outside of one's municipality of residence

	(1) Share of extra-m. VPTs	(2) Share of extra-m. VPTs	(3) Share of extra-m. VPTs	(4) Share of extra-m. VPTs	(5) Share of extra-m. VPTs	(6) Share of extra-m. VPTs	(7) Share of extra-m. VPTs	(8) Share of extra-m. VPTs
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$	0.05254	0.05083	0.05595	0.04880	0.04717	0.05222	0.09667	0.09477
	**	**	***	**	**	**	**	**
	[0.02120]	[0.02130]	[0.02158]	[0.02120]	[0.02127]	[0.02154]	[0.04253]	[0.04406]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_3)$	0.05587	0.05518	0.06007	0.05434	0.05373	0.05854	0.08353	0.08735
	***	***	***	***	***	***	***	**
	[0.01509]	[0.01509]	[0.01592]	[0.01528]	[0.01525]	[0.01607]	[0.03190]	[0.03371]
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_2)$	0.00225	0.001144	0.00554	0.00223	0.00117	0.00550	0.03064	0.003457
	[0.00652]	[0.00667]	[0.00730]	[0.00589]	[0.00605]	[0.00668]	[0.03169]	[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 J3: The coefficients in Columns (1) to (8) report individual OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the probability of aborting outside one's municipality of residence. The reference category is the 1st quartile of the distribution. Individual controls include a large set of characteristics of the aborting woman, covering demographic, socio-economic, health, and fertility-related features, as well as clinical characteristics of the current VPT procedure. Policy covariates include the quarterly average municipal stringency index, the total number of red, orange, and yellow-zone days, and municipal excess mortality. Other municipal covariates include the population of females aged 15–49. The sample consists of repeated cross-sections, with the inactive-share distribution computed at the municipal level. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean of treated units. *** 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) Share of extra-m. VPTs	(2) Share of extra-m. VPTs	(3) Share of extra-m. VPTs	(4) Share of extra-m. VPTs	(5) Share of extra-m. VPTs	(6) Share of extra-m. VPTs	(7) Share of extra-m. VPTs	(8) Share of extra-m. VPTs
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_4)$	0.03187 ***	0.03224 ***	0.03476 ***	0.03099 ***	0.03120 ***	0.03429 ***	0.04420 *	0.04236 *
	[0.01026]	[0.01037]	[0.01074]	[0.01015]	[0.01027]	[0.01056]	[0.02464]	[0.02468]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_3)$	0.00914	0.00944	0.01180	0.00769	0.00796	0.01081	0.03764	0.03939
	[0.00920]	[0.00937]	[0.00950]	[0.00855]	[0.00876]	[0.00879]	[0.02397]	[0.02397]
$Post * \mathbb{1}(I.\ S_{\cdot q2/20} \in Q_2)$	-0.00056	-0.00032	0.00222	-0.00111	-0.00096	0.00211	-0.02996	-0.03278
	[0.00851]	[0.00878]	[0.00926]	[0.00777]	[0.00806]	[0.00851]	[0.03203]	[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 J4: The coefficients in Columns (1) to (8) report individual OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on the probability of aborting in a municipality located outside the LMA that includes one's own municipality of residence. The reference category is the 1st quartile of the distribution. Individual controls include a large set of characteristics of the aborting woman, covering demographic, socio-economic, health, and fertility-related features, as well as clinical characteristics of the current VPT procedure. Policy covariates include the quarterly average municipal stringency index, the total number of red, orange, and yellow-zone days, and municipal excess mortality. Other municipal covariates include the population of females aged 15–49. The sample consists of repeated cross-sections, with the inactive-share distribution computed at the LMA level. Standard errors are clustered at the LMA level. *Mean* reports the pre-treatment mean of treated units. *** p<0.01, ** p<0.05, * p<0.1.

K Domestic Violence

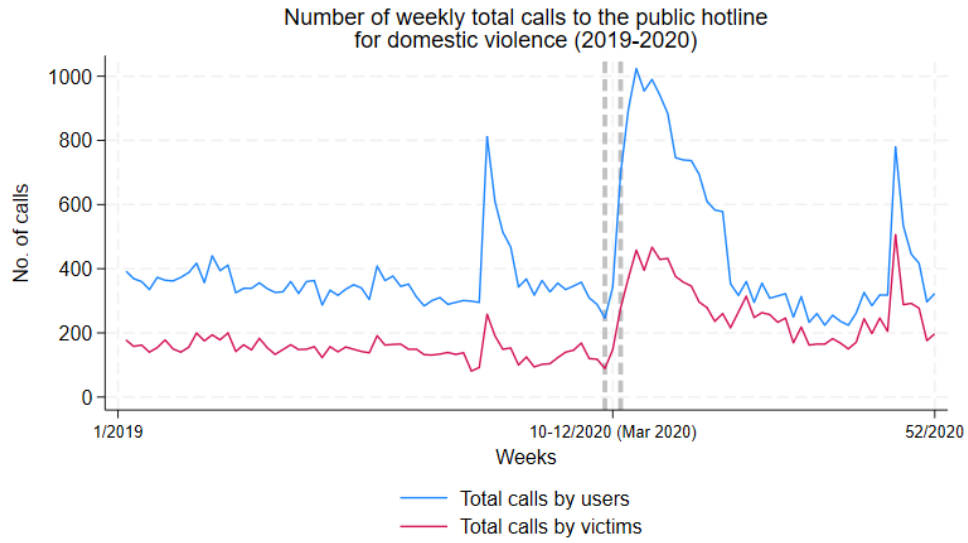


Figure K1: Weekly calls to the Italian public hotline for domestic violence (1522) in 2019 and 2020.

Impact of work suspension on calls to the national IPV hotline (OLS)

	Calls by users				Calls by victims			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	-0.54188	-0.64470	-0.56786	-0.70936	-0.23919	-0.50435	-0.26151	-0.55169
	[0.41952]	[0.56157]	[0.42165]	[0.56397]	[0.26084]	[0.36837]	[0.26453]	[0.37355]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.17320	0.22739	0.14448	0.16240	0.09622	-0.12410	0.07768	-0.16942
	[0.49035]	[0.59952]	[0.48681]	[0.60158]	[0.29742]	[0.35366]	[0.29684]	[0.35796]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	-0.57632	-0.82312	-0.59057	-0.83515	-0.35888 *	-0.58615 *	-0.37717	-0.59867
	[0.44929]	[0.54712]	[0.45280]	[0.54662]	[0.25717]	[0.31585]	[0.26213]	[0.31820]
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	0.62030	0.68493	0.63010	0.69234
Province FE	✓	✓	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓	✓	✓
Region × Quarter-year FE		✓		✓		✓		✓
Weight			✓	✓			✓	✓
Mean	7.657	7.657	7.657	7.657	3.789	3.789	3.789	3.789

Table K1: The coefficients report provincial OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on calls to the national IPV hotline. Columns (1)–(4) use the rate of calls made by all users, while Columns (5)–(8) use the rate of calls made directly by victims. The omitted category is the first quartile of the inactive-share distribution. All specifications include province and quarter-year fixed effects. Even in the most exposed provinces, estimated effects are statistically indistinguishable from zero and generally small relative to the pre-treatment mean. Policy covariates include the quarterly average provincial stringency index, the number of days spent under different restriction levels, and provincial excess mortality. Standard errors clustered at the provincial level are reported in brackets. *Mean* reports the pre-treatment mean among treated provinces. *** p<0.01, ** p<0.05, * p<0.1.

L Contraception

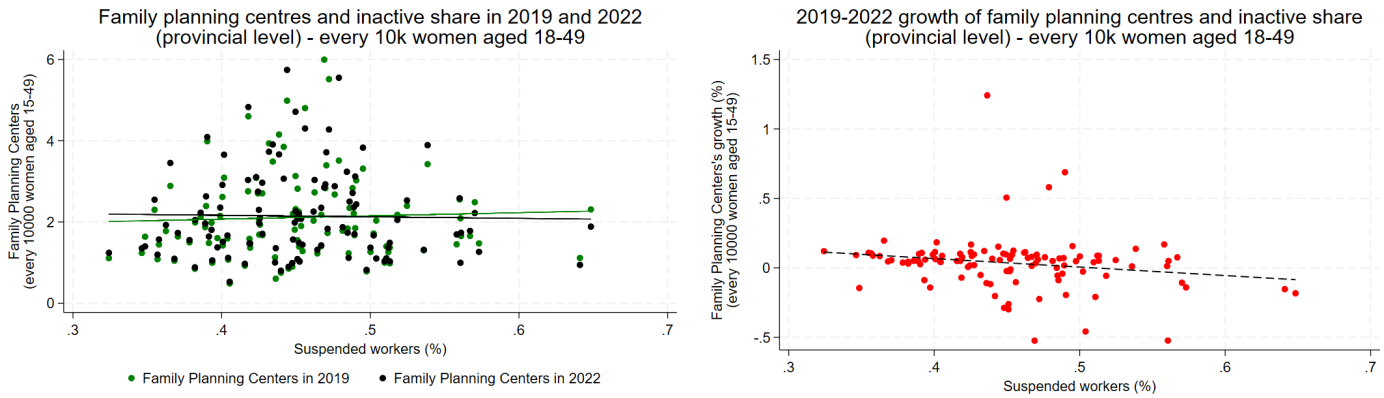


Figure L1: Annual growth of family planning centres in Italy and provincial inactive share. Left panel: number of family planning centres per 10,000 women aged 18–49 in 2019 and 2022. Right panel: 2018–2022 growth in the number of family planning centres per 10,000 women aged 18–49, shown against provincial inactive share in 2020. Source: Ministry of Health.

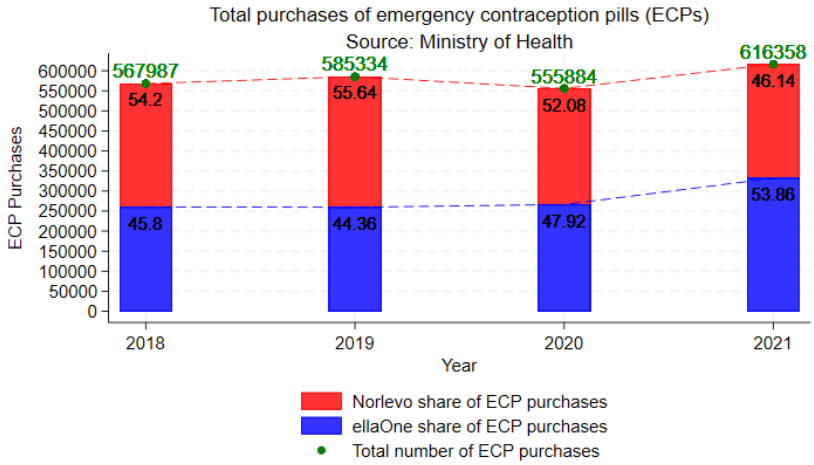


Figure L2: ECP sales in Italy between 2018 and 2021. Source: Ministry of Health.

Impact of work suspension × Post 2020Q3 on municipal ARs of minors (OLS)

	(1) AR (minors)	(2) AR (minors)	(3) AR (minors)	(4) AR (minors)	(5) AR (minors)	(6) AR (minors)
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	0.00882	0.00513	0.00864	0.00498	0.01316	0.00848
	[0.00918]	[0.00655]	[0.00904]	[0.00648]	[0.01091]	[0.00776]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.00636	0.00400	0.00630	0.00393	0.00933	0.00615
	[0.00692]	[0.00535]	[0.00690]	[0.00534]	[0.00755]	[0.00583]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	-0.00347	-0.00357	-0.00344	-0.00360	-0.00326	-0.00370
	[0.00443]	[0.00380]	[0.00443]	[0.00380]	[0.00467]	[0.00393]
Observations	126,448	126,448	126,448	126,448	126,448	126,448
R-squared	0.06461	0.06524	0.06462	0.06525	0.07395	0.07377
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Quarter-year FE	✓	✓	✓	✓	✓	✓
Prov. × Quarter-year FE					✓	✓
Weight		✓		✓		✓
Mean	1.098	1.098	1.098	1.098	1.098	1.098

Table L1: The coefficients in Columns (1) to (6) represent municipal OLS 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 emergency contraception pills became available over the counter for minors) and 0 otherwise, on the municipal abortion rates of underage women. Policy covariates include the quarterly average municipal stringency index, the total number of red, orange and yellow area days (white area omitted), and municipal excess mortality. The panel is built by aggregating abortions at the municipality-of-residence level. Standard errors are clustered at the municipal level. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.1.

M Pregnancies carried to term

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	0.27593	0.15891								
	[0.23275]	[0.24249]	[0.23310]								
$Post * \mathbb{1}(I.\ S._{q2/19} \in Q_4)$				0.04109	0.43332 *	0.06468	0.48555 **				
				[0.13925]	[0.22139]	[0.15492]	[0.20271]				
$Post * \mathbb{1}(I.\ S._{q2/20} \in Q_4)$								0.33835	0.23009	0.29522 *	0.26974
								[0.25577]	[0.15803]	[0.16219]	[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 M1: 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 (OLS)

	(1) Ab. ratio	(2) Ab. ratio	(3) Ab. ratio	(4) Ab. ratio	(5) Ab. ratio	(6) Ab. ratio
$Post * \mathbb{1}(I.S._{q2/20} \in Q_4)$	9.80620 **	10.11692 **	9.98114 **	10.31095 **	6.23252	6.83577
	[4.38194]	[4.33384]	[4.38197]	[4.33737]	[4.81508]	[4.80299]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_3)$	0.37257	0.65708	0.58223	0.87678	-1.49803	-1.28786
	[4.41271]	[4.26418]	[4.41333]	[4.26832]	[4.58851]	[4.45510]
$Post * \mathbb{1}(I.S._{q2/20} \in Q_2)$	-3.21523	-1.94473	-3.11239	-1.85594	-4.34428	-3.23510
	[4.32421]	[4.15086]	[4.32240]	[4.15039]	[4.40992]	[4.22969]
Observations	102,739	102,739	102,739	102,739	102,739	102,739
R-squared	0.15676	0.15528	0.15679	0.15533	0.17062	0.16935
Policy covariates			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Month-year FE	✓	✓	✓	✓	✓	✓
Province × Month-year FE					✓	✓
Weight		✓		✓		✓
Mean	117.9	117.9	117.9	117.9	117.9	117.9

Table M2: The coefficients in Columns (1) to (6) represent municipal OLS estimates of the ITT effect of belonging to different quartiles of the suspended workers' share distribution on municipal abortion ratios. The omitted category is the first quartile of the inactive-share distribution. Policy covariates include the monthly average municipal stringency index, the number of red, orange and yellow area days, and municipal excess mortality. All specifications include municipality and month-year fixed effects. Columns (5) and (6) additionally include province-by-month-year fixed effects. Standard errors clustered at the municipal level are reported in brackets. *Mean* reports the pre-treatment mean among treated municipalities. *** p<0.01, ** p<0.05, * p<0.10.

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