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Do vaccination-based access restriction policies curb hesitancy?

A comparison between Italy and Spain*

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Abstract

Vaccine hesitancy is a crucial public health issue worldwide. In this paper, we evaluate the causal impact of a vaccination-based access restriction policy on vaccine uptake by exploiting the introduction of the EU Digital COVID-19 Certificate at the country level. We compare two countries, Italy and Spain, which—despite using the Certificate as a prerequisite for many daily activities—experienced different adoption patterns. In Spain, the Certificate was introduced regionally, temporarily suspended by the Courts, and later reintroduced in several regions. In Italy, it was introduced in June 2021 and progressively expanded, culminating in mandatory vaccination for individuals aged over 50. We test the impact of the Certificate on vaccination rates using a Sharp Regression Kink Design, identifying changes in the slope of vaccination rates around the announcements by the two governments relative to the introduction of the Certificate. We find that the digital COVID-19 Certificate increased vaccinations in Italy in a range spanning from 3 to 11 percent. In Spain, by contrast, only the reintroduction seems to have been able to affect vaccinations. This result for Spain is also supported by a Difference-in-Differences model, exploiting the staggered reintroduction of the Certificate at the regional level.

JEL codes: I12, I18

Keywords: vaccine hesitancy; Digital COVID-19 Certificate, Sharp Regression Kink Design

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1 Introduction

Immunization is recognized as one of the greatest achievements in public health: according to available estimates, the use of vaccines in the last 50 years allowed for the saving of 154 million lives at the global level (e.g., Baidya et al., 2025). Despite this success, vaccine-preventable conditions still claim a great number of victims across the world.

One main reason that helps explain this problem is vaccine hesitancy, defined as one of the *top-10 global health threats* by the World Health Organization in 2019 (WHO, 2019). Vaccine refusal has detrimental consequences for society, generating social costs that affect both public health and economic systems (Farrenkopf, 2022; Olivera Mesa et al., 2023; Sousa & Furtado, 2024; Yakum et al., 2022). The attitude towards vaccines and the prevalence of hesitancy is quite heterogeneous within and across countries (Yaqub et al., 2014). The available literature suggests, for instance, that hesitancy may likely be correlated with general distrust towards the healthcare sector and healthcare professionals (Rudisill et al., 2025), which can also vary according to cultural and ethnic factors (Armstrong et al., 2007; Costa-Font & Docrat, 2024). But misinformation and social norms are likely to play a role (Baidya et al., 2025; Brillì et al., 2020; Loiacono et al., 2025).

In this paper, we discuss the effectiveness of policies to combat vaccine hesitancy by exploiting the COVID-19 pandemic, which caused an enormous epidemiological and socio-economic burden to most societies in recent years (Gianino et al., 2021; Milman et al., 2021; Wang & Wang, 2022). During the first wave of the pandemic, when the virus was largely unknown, many governments adopted harsh measures, such as stay-at-home orders and the suspension of economic activities, to limit the spread of the virus and its most severe consequences. Thanks to substantial financial and human resources (Commission, 2025), researchers were able to develop multiple vaccines against COVID-19 in a remarkably short time, employing both traditional technologies and the novel mRNA approach. These vaccines were approved by regulatory authorities in different countries since the end of 2020. After problems of supply shortages caused by the huge demand for vaccines in the first quarter of 2021, which fueled discussions on vaccine prioritization (e.g., Persad et al., 2020), the biggest vaccination campaign in history started unfolding across countries to guarantee the worldwide 70% coverage target aimed by the World Health Organization by mid-2022 (WHO, 2021). In 2021, approximately 13 billion doses

had been administered across 184 countries (OWID, 2022).

However, despite efforts to make COVID-19 vaccines universally accessible, vaccination levels varied widely across and within countries, and uptake started to level off by mid-2021¹. In response to this trend, governments started to define new strategies to promote COVID-19 vaccination in order to increase uptake among hesitant individuals. These strategies varied from simple awareness campaigns to the request of COVID-19 Certificates to access many activities, up to mandatory vaccinations for certain categories of people.

To discuss the effectiveness of different strategies, we focus on Italy and Spain, two countries that share a similar public regionally decentralized healthcare system². In particular, we consider the use of the Digital COVID-19 Certificate (introduced at the EU level to allow free movement of people across Member States) to allow certain activities *within* each country, like moving to reach the workplace and entering public indoor spaces. While the Italian government adopted a centralized “incremental” approach, moving from less to more restrictive versions of the Certificate (and ending up with mandatory vaccinations for workers), the Spanish government faced the initial opposition of some regional Superior Courts of Justice (*Tribunales Superiores de Justicia*) to the first version of the Certificate, ending up in a more “decentralized” and less restrictive version of this policy.

Our empirical analysis aims to estimate the effect of vaccination-based access restriction policies on vaccination rates using a Sharp Regression Kink Design (Nielsen et al., 2010, Card et al., 2015). We use official announcements from the Italian government regarding the introduction of the standard “Green Pass” (the Italian version of the EU Digital COVID Certificate), the “Super Green Pass”, and mandatory vaccinations as exogenous shocks to the information available to the public regarding COVID-19 Certificates and the associated vaccination requirements. For each announcement, we identify a specific date when the new information became publicly available. We also consider similar announcements from the Spanish government concerning the introduction, the suspension by regional courts, and the reintroduction of the “Certificado COVID” (the

¹Vaccine hesitancy, often tied to political determinants, is one of the reasons explaining within-country variability. For instance, in both the U.S. and Europe, evidence suggests that vaccine refusal was fueled by conspiracy theories and populist views, strictly linked to far-right parties (Backhaus et al., 2023; Hornsey et al., 2020; Lasco, 2025; Paoletti et al., 2024; Romer & Jamieson, 2020).

²Unsurprisingly, the management of COVID-19 reignited the debate on the appropriate level of governance of healthcare policies in both countries, with different approaches adopted across pandemic waves. For example, while Spain implemented a more centralized strategy during the first wave, the Italian government was unable to enforce uniform national policies in the same period, allowing for regional experimentation, which may help explain the observed differences in outcomes across regions. See, e.g., (Angelici et al., 2023).

Spanish version of the EU certificate).

We find that the announcement of the Digital COVID-19 Certificate increased vaccinations in Italy by a range from 3.12 to 11.03 percent among non-vaccinated individuals. The two announcements most effective in Italy were the first introduction of a Digital COVID-19 Certificate and the mandatory vaccinations for individuals over 50. Conversely, in Spain, only the last announcement (i.e., the reintroduction of the certificate) was effective in increasing vaccinations. This difference is partly explained by the initial abrogations of the mandatory use by the Spanish Superior Courts of Justice.

By studying the effects of COVID-19 Certificates, we contribute to the literature on the policies to combat vaccine hesitancy. Different papers analyze the effectiveness of monetary incentives. For instance, using an online randomized experiment performed on about 2400 individuals across the U.S., Iyer et al., 2022 show that monetary incentives are able to decrease vaccine reluctance. However, novel research disputes this finding, showing that, for vaccine-hesitant adults, vaccine incentives may have a counterproductive effect if tied to monetary rewards. Using an experiment involving about 500 individuals living in Los Angeles County, Huynh et al., 2025 conclude that one out of seven individuals were willing to be administered the vaccine without an explicit payment, whereas they would refuse the vaccine if conditional on a reward. This behavior was due to their perceived lack of safety and their decline in altruistic motivation. As a matter of fact, in the absence of explicit rewards, nudging policies and informational treatments can increase vaccine uptake (Dai et al., 2021; Jensen et al., 2022; Reñosa et al., 2021). To this extent, nudging strategies are particularly suitable for public policy and public health interventions, given the low amount of resources required to be sustained (Yoong et al., 2020). They could also help informational cascades to arise, whose consequent herding behavior is positive in shifting up vaccination intentions (Epstein et al., 2025). While part of the scientific literature defined the adoption of COVID-19 Certificates as a nudging technique to boost vaccinations across the population (Sotis et al., 2021; Spitale et al., 2022), the restrictive features and the mandatory nature of such intervention cannot really fall within the precise scope of nudging (Congiu & Moscati, 2022). The COVID-19 Certificate stands in the gray area between mandatory vaccination interventions and nudging. In fact, many countries decided to implement them, and while not being legally mandated, they loomed as a requirement for individuals to enter most public indoor spaces (e.g., restaurants,

gyms). Our findings suggest that policies based on vaccination certificates to allow certain activities can be effective tools to increase uptake.

The remainder of the paper is structured as follows: we first summarize essential background information and some of the previous literature in Section 2; we then present the data and the empirical strategy in Section 3, and then describe and discuss the results of our exercise in Section 4. Section 5 ends the paper.

2 Background

The development of COVID-19 vaccines has been extraordinarily rapid compared to the standard time required for developing vaccines in the past.³ While many scientists foresaw vaccines against the new virus not to be available for several years, the first COVID-19 vaccine has been made available under an Emergency Use Authorization by the Food and Drug Administration (FDA) in the USA since 11 December 2020, for all individuals over 16 years of age. The same vaccine was fully authorized by the FDA since 23 August 2021, when the vaccination campaign had already started, and governments in developed countries had to face the challenge of convincing people to get their shot.

A similar process occurred in Europe. The European Medicines Agency (EMA) recommended granting a conditional marketing authorization for the first COVID-19 vaccine on 21 December 2020. After the EU authorization, the *Agenzia Italiana del Farmaco* (AIFA, the Italian Medicines Agency) formally granted the authorization on 22 December 2020, while Spain directly authorized commercialization under the EMA authorization. The full authorization for the same vaccine has been granted for all the EU on 10 October 2022, when most pandemic-related restrictions had already been lifted.

The majority of authorized COVID-19 vaccines were designed as two-dose regimens to ensure maximal efficacy, with the second dose administered three to four weeks after the first for mRNA vaccines (Pfizer-BioNTech and Moderna), and four to twelve weeks for adenoviral vector vaccines (AstraZeneca). The primary challenge in the early phase of the vaccination campaign was rationing and prioritizing limited vaccine supplies, as demand substantially outstripped availability. Conceptually, initial demand was driven by *pro-vaxxers*, i.e., individuals for whom the benefits from COVID-19 vaccines largely exceeded

³In addition to helping counteract the diffusion of COVID-19, this experience also had positive spillover effects on the development of vaccines against other diseases, like flu (Leuchter et al., 2022).

the associated costs. However, as production increased, vaccine availability expanded, and *pro-vaxxers* obtained their vaccine, the uptake of first-dose vaccinations began to plateau, underscoring the need for targeted strategies to address vaccine hesitancy and increase coverage among reluctant populations. Those reluctant included two types of individuals: *no-vaxxers*, i.e., those for whom costs of COVID-19 vaccines largely exceed benefits; *hesitant*, i.e., those for whom benefits and costs of COVID-19 vaccines are almost equal. While *no-vaxxers* are difficult to convince, the *hesitant* can be pushed in some ways to accept vaccinations.

Here, we focus on the role of the COVID Certificate as a way to convince *hesitant* individuals. Originally, the EU Digital COVID Certificate was introduced on 1 July 2021 to ease the free movement of people across the EU Member States. The Certificate was a digital (or paper-format) proof that an individual: i) had been vaccinated against COVID-19, or ii) received a negative test result, or iii) recovered from COVID-19. The three conditions required to obtain the Certificate differed in terms of the length of certificate validity: 270 days for vaccination against COVID-19, 72 hours for a negative PCR test or 24 hours for a negative antigen test, and 180 days for COVID-19 recovery.

Of the 27 EU Member States, only 15 countries adopted the COVID-19 Certificate for domestic purposes, such as access to indoor events or venues. The intensity of the requirements varied across countries adopting COVID-19 certificates, ranging from mild to more strict measures. Our interest in this paper is on the national policies based on COVID-19 Certificates adopted in Italy and Spain.

Spain adopted a mild policy at the national level, leaving room for different decisions at the regional level. In particular, the Spanish Central government did not establish the mandatory use of the COVID-19 Certificate at the national level. Only a few Autonomous Communities (i.e., Andalusia, Balearic Islands, Canary Islands, Cantabria, and Galicia) decided, for the first months after the official announcement by the Central government at the end of May 2021, to require the *Certificado Covid* (i.e., proof of vaccination, testing, or recovery) to visit indoor public spaces. However, a few months after the official announcement, the Spanish Superior Courts of Justice of these communities (Canary Islands on 29 July 2021, Cantabria and Andalusia on 6 August 2021, Galicia on 13 August 2021, and Balearic Islands on 23 August 2021) abrogated the mandatory use for leisure activities within the jurisdiction of the Community. It is only after the worsening of the

burden of COVID-19 due to the presence of a new variant that the Spanish Superior Courts of Justice of eleven autonomous communities (i.e., Catalonia, Navarre, Aragon, Galicia, Murcia, Balearic Islands, Valencia region, Basque Country, Canary Islands, Andalusia, and Cantabria) established, at the beginning of December 2021, to make the COVID-19 Certificate a condition to entry in specific public spaces. This heterogeneous policy roll-out is shown in Figure A1.

Unlike Spain, Italy adopted a highly centralized approach, leaving no discretion to its regions or autonomous provinces. The COVID-19 Certificate (hereafter referred to as the *Green Pass*, GP) was required to access all indoor public venues (e.g., cinemas, exhibition centres, gyms, museums, theatres, stadiums, and swimming pools), public transportation, and—most notably—workplaces. The degree of restrictiveness associated with the GP, however, varied over time in accordance with the provisions established by law. These restrictions were progressively introduced with a sequence of law decrees issued throughout the pandemic period. The unfolding of the policy is illustrated in the timeline shown in Figure A2. More detailed information on the specific GP-related measures contained in each decree is provided in Table A1.

The introduction of the COVID-19 Certificate has been officially announced at the EU level as well as at the national level, and properly publicized using official government websites offering all information to obtain the Certificate, and which activities were allowed using the Certificate. Intuitively, the official announcements by the governments had a strong informational content, and ignited a strong debate between those in favour of vaccinations and those against. They also pushed reluctant individuals to look for information, as we show below in Section 4.1. Our analysis will shed light on whether the announcement of the introduction of the Certificate was able to convince reluctant individuals to demand a first-dose of COVID-19 vaccines.

3 Empirical analysis

3.1 Data

Data on vaccine administration for Italian regions was retrieved from the *Covid-19 Open-data Vaccini* database, which contains reliable data collected by the Italian Ministry of Health through the national vaccines registry. The dataset lists daily information about

vaccine administration by region, age, gender, and distinguishes between first- and second-doses. Comparable information, collected by the Spanish Ministry of Health are available for the Spanish autonomous communities. We focus on the period from 1 April 2021 to 31 January 2022 to cover the official announcements of the relevant policies concerning the COVID Certificate by the two investigated countries.

Using this information, we compute the *first-dose vaccination rate* for the two countries defined as:

$$FDV R_{r,d} = \frac{first_doses_{r,d}}{NonVaccinatedPop_r \times 100,000} \quad (1)$$

where $FDV R_{r,d}$ represents first doses administered in region r during day d every 100,000 non-vaccinated people.⁴ Data on the regional population was obtained from the Italian national statistical institute (ISTAT) and the Spanish Statistical Office (INE).

We consider the individual decision of obtaining the first-dose vaccine as based on individual preferences and the available information relative to COVID-19 Certificates. To characterize the information set available for individuals, we used *Google Trends* and collected weekly data on individuals' interest towards the digital COVID-19 Certificate for the investigated period. As for Italy, Google Trends was queried using “green pass”, “certificato verde”, “certificazione verde COVID-19”, “super green pass”, and “obbligo vaccinale” as main keywords to retrieve interests about the adoption of the digital COVID-19 certificate, the advanced form of the digital COVID-19 certificate and the mandatory vaccination for people aged 50 and over across the Italian regions. Instead, “certificado verde”, “certificado COVID”, and “pasaporte covid” were searched to obtain the interests on the digital COVID Certificate across the Spanish autonomous communities. This information is available at the regional level on a weekly basis.

3.2 Empirical strategy

3.2.1 Regression Kink Design

To estimate the impact of the introduction of the COVID Certificate on vaccination rates $FDV R$, we use the Sharp Regression Kink Design (SRKD), as formally investigated in

⁴This variable is meant as the number of people, over the total population, that have not yet received the first dose. It is computed by subtracting from the total population the lagged cumulated sum of individuals who have received the first dose in the previous period.

Nielsen et al. (2010) and in Card et al. (2015).

Formally, let I be the information treatment, D the assignment variable, FE time and regional fixed effects to account for unobserved heterogeneities over time and across units, respectively, and e the error term. The outcome of interest Y (i.e., the natural logarithm of $FDVR$) is $Y \equiv y(I, D, FE, e)$. We are interested in the causal effect of varying I on Y , which we denote by $y_1(I, D, E) = \delta Y(I, D, E)/\delta I$. Formally:

$$\tau = \frac{\lim_{d_0 \rightarrow 0^+} \frac{\delta E[Y|D=d]}{\delta d} \Big|_{d=d_0} - \lim_{d_0 \rightarrow 0^-} \frac{\delta E[Y|D=d]}{\delta d} \Big|_{d=d_0}}{\lim_{d_0 \rightarrow 0^+} \frac{\delta b(d)}{\delta d} \Big|_{d=d_0} - \lim_{d_0 \rightarrow 0^-} \frac{\delta b(d)}{\delta d} \Big|_{d=d_0}} \quad (2)$$

In Equation 2 we define the (local) Average Treatment Effect (ATE) as the change in the slope of the outcome variable divided by the change in the information available to individuals. We assume that the denominator change is equal to 1 minus 0, i.e., the difference between having and not having access to the information about the COVID Certificate.

The information treatment I is represented by the official announcements by the government in both Italy and Spain. In particular, as for Italy, we use the government announcements about the introduction of: (1) the standard “Green Pass”, (2) the “Super Green Pass”, and (3) the introduction of mandatory vaccinations as exogenous variations in the level of information of individuals (δI). Similarly, for Spain, we consider announcements about: (4) the introduction of the standard “Green Pass”, (5) the sentences of some Spanish Superior Courts of Justice abrogating the mandatory use of the standard “Green Pass”, and (6) the reintroduction of the standard “Green Pass”. Within the SRKD, I is a deterministic function of D (which in our case is represented by time), i.e., $I = b(D)$ with a jump that is not observed in the level of the function, but in its *slope* (*kink*) at $D = 0$.

For each of the above-mentioned announcements, we need to identify a date after which new information provided by the government is available to individuals, i.e., the threshold at which treatment is assigned ($D = 0$). In our context, however, there are several dates that could potentially identify the availability of new information. For instance, one can use the date of official press conferences, but sometimes news about the content of such conferences becomes available even before because of information leakages diffused by newspapers. To decide when to set each threshold, we proceed empirically by estimating

the following equation:

$$I_{r,w}^k = \delta + \iota_r + \gamma_w + \epsilon_{r,w} \quad (3)$$

where $I_{r,w}^k$ represents the number of Google searches performed by individuals living in region r during week w , including as a term one of the $k = 1, \dots, 6$ announcements made by the Italian or Spanish governments described above. ι_r and γ_w are region- and week-specific Fixed Effects. $\epsilon_{r,w}$ is the error term.

We use Equation (3) to test when the coefficients γ_w assume a value significantly higher than the average number of searches over the entire period. We assume that the first week in which interest in a specific announcement increases suddenly represents the beginning of our post-treatment period. Nevertheless, since we consider multiple announcements that are consecutive over time, we need to assume that the announcement could be effective in shaping vaccination rates over a certain number of weeks. Here, we consider a window of three weeks before and after the announcement. Following the same assumption, we compute the percent variation in interest towards the government announcements, in both countries, from one week to another, to justify the identification and selection of the dates around which the announcements happened. However, since data on FDVR are collected daily, whereas Google trend data are collected on a weekly basis, we need to define a criterion to identify the day after which information changes within the week identified using equation 3. In this case, we decided to identify this date by graphical inspection, searching for the day immediately before the most evident variation in vaccination rates with respect to the previous trend.

When implementing the SRKD estimator in practice, one must make choices for the kernel K , bandwidth h , and polynomial order p . We use the standard triangular kernel for K , but we also evaluate our baseline by regressing the main equation using a uniform kernel. For the lag-order of the polynomial, we estimate a second-order degree polynomial ($p=2$), which is based on the trend of vaccination daily rates. With respect to the bandwidth, we perform a set of different regressions across bandwidth by adopting different strategies that will be illustrated in the next section.

3.2.2 Difference in Differences

To provide further evidence on the impact of the COVID-19 Certificate on vaccinations, we take advantage of the staggered reintroduction of the digital certificate across Spanish autonomous communities to also estimate a standard Difference-in-Differences model. The analysis is limited to Spain. We define as treated regions r those that, after day d , reintroduced the mandatory use of the digital COVID-19 certificate, whereas controls are represented by the same regions in the pre-reintroduction period and by other Spanish autonomous communities that decided not to reintroduce the mandatory use of the digital COVID-19 certificate.

We use the following TWFE baseline specification, estimated via OLS:

$$FDV R_{r,d} = \alpha + \sum_{j=2}^J \eta_j (Lag_j)_{rd} + \sum_{k=1}^K \mu_k (Lead_k)_{rd} + \lambda_r + \psi_d + \xi_{rd} \quad (4)$$

From equation 4, we can estimate an event study where η_j and μ_k are parameters associated with leads and lags, defined as in Clarke and Tapia-Schyte, 2021, which can be interpreted as anticipatory and post-announcement effects, respectively. Leads allow for discussing the common trend assumption, while lags allow for discussing whether the effect grows or fades over time. λ_r and ψ_t represent region and day fixed effects.

However, the most recent literature on the DiD approach with multiple treatment timing suggests that the comparison of the staggered effects across different groups performed via a standard TWFE strategy may lead to biased results (Borusyak et al., 2024; Callaway & Sant’Anna, 2021; Goodman-Bacon, 2021). To address this issue, we benchmark the TWFE estimates against an alternative estimator implemented in a dynamic event-study framework. Specifically, we adopt the *imputation estimator* of Borusyak et al., 2024, which recovers the counterfactual evolution of treated units by first estimating an OLS TWFE model on the subsample of not-yet-treated observations to identify the common trend component. The estimated coefficients are then used to impute untreated potential outcomes for treated units in post-treatment periods, thereby delivering treatment effect estimates that are robust to contamination arising from staggered adoption. The

imputation estimator for each period relative to treatment is defined as follows:

$$\hat{\tau}(l) = \frac{1}{N} \sum_{r,d;d-G_r=l} \hat{\tau}_{rd} \quad (5)$$

where the RHS is the average aggregation by relative time period (expressed in days d , thus $l = d - G_r$) of the regional, time-specific effects $\hat{\tau}_{r,d} = FDVR_{r,d} - \widehat{FDVR}_{r,d}(0)$. The fitted values $\widehat{FDVR}_{r,d}(0)$ in the former expression are obtained by OLS-regressing the outcome on non-treated and not-yet-treated data only ($FDVR_{r,d} = \alpha_r + \lambda_d + \varepsilon_{r,d}$ for untreated units)⁵.

To provide additional robustness checks to the results, we also complement the staggered DiD analysis with two additional (dynamic) methodologies: first, we make use of the more restrictive methodology by Sun and Abraham, 2021, who propose cohort-specific average treatment effects on the treated estimators (*CATT*), which can adjust the effect of time-varying treatment assignment spillovers stemming from other time periods relative to the ones when units actually receive the treatment. Second, we exploit the recent two-stage DiD estimator developed by Gardner et al., 2025 and applied in Butts and Gardner, 2022; this method addresses issues arising from the bias linked to the interaction of staggered treatment timing and heterogeneous treatment effects. Both approaches require the parallel trend assumption to hold.

Notice that one may suspect that vaccination rates of control regions, i.e., those who never reintroduced the Digital COVID-19 Certificate, are affected by the reintroduction of the mandatory use of the Certificate in the eleven treated regions. This may happen for several reasons, like the fact that individuals may live in one place, where the digital COVID-19 certificate is not mandatory, but move to another region, where the certificate is needed; or, in addition, individuals in control regions could expect that their region also will be subject to the mandatory use of the certificate. In this case, the case event model will lead us to underestimate the effect of the reintroduction of the digital COVID-19 certificate on vaccination rates because vaccination rates will also increase in control regions.

⁵Like in the standard TWFE specification, according to this approach the parallel trend assumption needs to hold, as in the absence of the treatment the $E[FDVR_{r,d}(0)] = \alpha_r + \lambda_d$, meaning that the evolution of $FDVR_{r,d}$ would only follow time-invariant regional unobservables and general time effects, which yields to the fitted values defined as follows: $\widehat{FDVR}_{r,d}(0) = \hat{\alpha}_r + \hat{\lambda}_d$

4 Results

4.1 Descriptive evidence

We start our analysis by describing how the interest in government announcements evolved. Figure A3 reports the week-to-week percentage variation in interest in government announcements for Italy (Panel A) and Spain (Panel B). We use the analysis to identify the assignment date for the information treatment. Based on the largest observed percentage changes, we identify the following dates for assignment: 12 July 2021; 15 November 2021; and 23 December 2021 for Italy; 3 June 2021; 2 August 2021; and 26 November 2021 for Spain.

Figure 1 combines information on Google Trend searches relative to the digital COVID-19 Certificate with information on vaccination rates in Italy (Panel A) and Spain (Panel B) during the period from April 2021 to January 2022. The dashed vertical lines denote the weeks including the identified assignment dates, while solid lines correspond to pre- and post-treatment periods, considering a six-week window around the announcement date. Starting with Italy, focusing on the panel A of Figure 1, note that vaccination rates initially peaked up at almost 1135 per 100,000 unvaccinated individuals around the beginning of June, due to the opening of the vaccination campaign to high school students in their final year and also to all the individuals regardless of their age, and then started to sharply decline until mid July.

Vertical bars in the upper panel of Figure 1 identify the interest in the standard “Green Pass” (light gray bars), the “Super Green Pass” (mid-gray bars), and mandatory vaccinations (dark gray bars) measured by adopting Google searches; these numbers are averaged at the regional level. Until mid-July, the popularity of these terms was very low, implying a low interest in digital COVID-19 Certificates in Italy. However, after the second week of July, when the government announces its use, the interest in the term “Green Pass” increases suddenly. This variation in interest is located in proximity to the first official press conference organized by the Italian government on the introduction of the standard version of the digital COVID-19 Certificate. In the same week, the vaccination rate stopped decreasing and started increasing again from the following week. This increasing trend continued for the following three weeks, and then the first-dose vaccination rate declined again. The declining trend goes on until November, when one can notice an

anomalous variation in Google Trend searches for the “Super Green Pass”. Also in this case, the first-dose rate mildly increases, inverting the decreasing trend observed during the previous weeks. The increase in vaccinations seems to last for the next three weeks and then remains constant for the next couple of weeks, to finally increase again, at a faster pace, when searches for mandatory vaccinations increase during the last week of December.

As for Spain, looking at panel B of Figure 1, note that in the initial period from 1 April 2021 to mid-May, the rate of first doses per 100,000 unvaccinated individuals showed an overall upward trend with fluctuations. Before the first official announcement and the initial rise in average community-averaged searches for “Certificado COVID” observed during the first week of June, the rate already exhibited a moderate increase. It keeps increasing steadily until the last week of July, i.e., when some Spanish Superior Courts of Justice start abrogating the mandatory use of the COVID-19 Certificate in the country. Soon after the announcement, the vaccination rate peaked at 1055, to start a sharp decline lasting until the last announcement, in mid-November, when some Spanish regions decided to reintroduce the mandatory use of the COVID-19 Certificate, until the first week of December.

4.2 Regression Kink Design

4.2.1 Baseline results

Figure 2 highlights estimates of the slope change in the rate of first doses per 100,000 unvaccinated individuals for Italy, zooming on the three windows around the announcements and using the SRKD. In each panel, the points represent the daily values of the vaccination rate (again, calculated over the unvaccinated population), and the vertical dashed line corresponds to the assignment date. In the upper-left panel, the threshold is set at 12 July 2021, representing the first day of the week (i.e., Monday) when we identified an anomalous variation in Google searches for the term “Green Pass”. Right before the threshold, we can observe a decreasing trend in the rate of first doses per 100,000 individuals, whereas right after it, we first observe a constant pattern that, a few days later, turns into an increasing dynamic in first doses. Focusing on the upper-right panel, we can observe what happens around the second threshold, i.e., the first day of

the week when we identified an anomalous variation in terms of Google searches for the term “Super Green Pass”. Also in this case, we identify a variation in the trend of first doses per 100,000 unvaccinated individuals during the post-announcement period, which passes from 100 to 200 first doses per 100,000 individuals during the three weeks after the threshold. Lastly, we observe again, in the lower panel of Figure 2, a very similar pattern, with a stable first-dose rate before the threshold, which in this case identifies the introduction of mandatory vaccinations, and a boosting effect right after. In this case, the first doses per 100,000 unvaccinated individuals pass from about 150 to 500 after the treatment.

Figure 3 shows the same estimates for Spain. The upper-left panel identifies what happens to first doses per 100,000 unvaccinated individuals around 3 June 2021, when the Spanish Government introduced the mandatory use of the COVID-19 Certificate. The rate of first doses increases sharply in level in the post-announcement period, first jumping from 650 to more than 900 per 100,000 individuals and, a few days later, settling on an average value of 850 per 100,000 individuals. Looking at the upper-right panel, first doses per 100,000 individuals started to decline after the second relevant date, i.e., 2 August 2021, and continued on the same trend over the following three weeks, exactly when some Spanish Superior Courts of Justice decided to abrogate the mandatory use of the COVID-19 Certificate. Lastly, in the lower panel, we observe an increase in first doses per 100,000 individuals after the reintroduction of the COVID-19 Certificate in some autonomous communities. First doses rise sharply around mid-December, suggesting a possible lagged effect. This may be explained by individuals choosing to delay their first dose as long as possible, anticipating a potential intervention by the Spanish Superior Courts of Justice—which, in this case, did not overturn the mandatory use of the COVID-19 Certificate.

Figure 4 provides a graphical overview of the parameters estimated using the SRKD for different bandwidths (BW). We consider three simple BW fixed at 7, 14, and 21 days, and three optimal bandwidths, estimated following Calonico et al., 2020; Calonico et al., 2014, using the MSE-optimal procedure, the minimum-threshold CER-optimal procedure and the median-threshold CER-optimal procedure. The upper panel of Figure 4 presents results for Italy: we estimate a positive effect on first doses per 100,000 unvaccinated individuals considering the first announcement, which increases in size as we use larger

BW. Starting from the smallest bandwidth, 7 days, the incremental effect amounts to 6.4 additional doses per 100,000 individuals only, and it is imprecisely estimated. Increasing the BW, the estimated effect also increases and becomes statistically significant. The estimated change in slope is 25.90 (SE = 8.800) and 46.01 (SE = 11.765) when the BW is 14 and 21 days, respectively. Considering the optimal bandwidths (i.e., 32.9 days for the MSE-optimal, 20.3 for the min CER, and 25.4 for the median CER), the estimates increase in size even more. We find that first doses grow by more than 53.04, 44.40, and 55.94 doses per 100,000 unvaccinated individuals, respectively ($SE_{bw} = \{9.858, 10.615, 10.755\}$) per day after the threshold. Such estimates are all statistically significant. Looking at the second announcement, all the estimates are almost negligible, hardly different from zero, and with opposite signs. This suggests that the second announcement about the “Super Green Pass” was not effective in boosting first doses. On the contrary, the last announcement about mandatory vaccinations for specific age groups is always positive and statistically different from zero, but the size is smaller for larger BW. The estimated effect is substantially large, 72.41 (SE = 8.000) for BW=7. For the other bandwidths (the optimal ones in this case are 33.1, 26.7, and 26.7), the effect ranges between 20.96 and 30.85.

The lower panel of Figure 4 presents estimates for Spain. Most of the effects are statistically non-significant. The only announcement that seems to be effective in terms of first doses per 100,000 unvaccinated individuals is the third one, on 26 November 2021, when some autonomous communities decided to reintroduce the COVID-19 certificate. The estimated effect ranges from 12.9 (SE = 3.576) to 26.28 (SE = 3.968) for the MSE optimal BW (14.4 days) and for the min CER-optimal BW (11.7 days), respectively. When the BW is equal to 21 days, the (negative) effect is imprecisely estimated.

To ease the interpretation of the effects, we use information about the total population, population already vaccinated, and population infected on the day of each announcement to compute the share of additional individuals who got vaccinated following the announcements. These results are shown in columns (4) and (5) of Table 1.⁶ We provide a range

⁶More in detail, we calculate the ratio between the total number of additional first doses after each announcement and individuals not yet vaccinated. The numerator is obtained as the change in slope estimated after the announcement multiplied by 21 (i.e., the number of days over which the announcement is effective in increasing vaccination rates) and further multiplied by the total population in Italy or Spain divided by 100,000. The denominator is obtained as the population eligible for vaccination minus the population already vaccinated (column 4), and also further subtracting individuals who, on the day of the announcement, are infected and not vaccinated (column 5). Positive and unvaccinated individuals are calculated by multiplying currently infected individuals by the percentage of positive unvaccinated individuals. The percentage of positive unvaccinated individuals was retrieved by the Italian National Institute of Health (Italian National

of magnitudes, by considering the minimum and the maximum values for the estimated effects reported in column 3.⁷

Our computations show that the digital COVID-19 Certificate was able to increase vaccinations in Italy, among unvaccinated individuals, in a range that spans from 3.12 to 11.03 percent. The two announcements that mostly increased vaccinations are the introduction of the standard COVID-19 Certificate and the mandatory vaccinations for individuals over 50. The differential effect among announcements could be explained by considering that the salience of the first announcement was probably able to affect individuals who are moderately hesitant, and being the first, it reached a larger number of individuals. Once moderately hesitant individuals are no longer present among the unvaccinated, only a stronger intervention (i.e., mandatory vaccinations) was capable of affecting vaccination rates.

Results for Spain are different, since only the last announcement seems to have been effective in increasing vaccinations. This result can be explained by the misalignment between the Spanish government and the Spanish Superior Courts of Justice during the first announcement, with the latter abrogating the mandatory use of the COVID-19 certificate. When the pandemic started to hit harder in terms of new cases and deaths, the government reintroduced the digital COVID Certificate and was able to increase vaccinations effectively, without any interference from the Superior Courts of Justice. The effect is clearly smaller than the one estimated for Italy, with increases between 2.15 and 9.60 percent, entirely due to the third announcement.

4.2.2 Robustness checks

To assess the sensitivity of our results, we perform a battery of additional estimates. First, we estimate the Kink RD coefficient for all the announcement days and for both countries, considering the following: 1) we switch from the standard triangular kernel to a uniform kernel; 2) we perform a donut RKD by removing from the sample the observations from the first day before and after the announcement; 3) we use two-way

Institute of Health (ISS), 2022) and by the Spanish Ministry of Health (Spanish Ministry of Health, 2022), for Italy and Spain, respectively.

⁷For the first announcement in Italy, we include in the calculation the 7-day BW coefficient, as although it is not statistically significant, it is consistent in sign. For the second announcement in Italy, although the small effects are mostly slightly statistically significant, we consider the overall estimate as equal to zero, since the coefficients change in sign according to the specification, and out of six effects, three are positive, and three are negative. Eventually, for the third announcement in Spain, we exclude from the specification the negative effect estimated with the 21-day BW as it is not consistent in sign with the other effects.

clustered standard errors (by region and week) rather than clustering at the regional level only.⁸ The results are reported in Tables A3 (Italy) and A4 (Spain). We fix for comparison our baseline specification at the 21-day bandwidth, as it delivers conservative estimates when compared with the optimal bandwidths. All the estimates are consistent with the baseline for both countries, with the donut coefficient reasonably magnifying the size of the results in the same direction.⁹

Second, we consider the role of a potential confounder, able to affect the causality of our results. For instance, suppose that—around the assignment date—one observes an abnormal increase in mortality due to COVID-19. Then, an increase in vaccinations may be driven by excess mortality and not by the announcements relative to the COVID-19 Certificates. To rule out mortality as a driver for our results, we check for potential discontinuities around the cutoff in this variable. We retrieved two datasets. The first include Italian daily data about excess mortality from the National Institute of Statistics (computed as the difference between daily deaths in the years starting from the COVID-19 outbreak and the average deaths in those same days in years 2015-2019, which is standard in Italian COVID literature - Armillei et al., 2021; Borri et al., 2021; Buonanno et al., 2020; Cerqua et al., 2021; Depalo, 2021; Di Porto et al., 2022). The second dataset involves all Spanish daily deaths tracked to be linked to COVID-19 between 2021 and 2022 at the autonomous community level, registered by the National Institute of Statistics. Although these variables follow the heterogeneous regional evolution of the pandemic, are assumed not to jump (either in slope or level) at the cutoff. We first offer graphical evidence in Figure A4 for Italy and Figure A5 for Spain, where mortality variables are plotted around each cutoff and fitted through a second-degree polynomial, consistent with our baseline RKD specification. Visual inspection does not reveal systematic and contemporaneous slope changes, feasibly aligned with the announcement dates that could plausibly account for the measured effects on vaccination rates.

To provide the visual inspection with additional robustness, we re-estimate the RKD specification using mortality as the outcome variable, maintaining the same bandwidth (21 days) and the alternative specifications adopted in the baseline (uniform kernel, donut

⁸In this case, rather than a standard RD robust estimate we apply an OLS regression with dummy treatment at the threshold and the local second-degree polynomial as covariate, as such methods are econometrically equivalent.

⁹For the third Spanish announcement, the estimates are negative and insignificant, as the 21-day threshold is the only one delivering imprecisely estimated negative results. The donut specification is arguably inflating the coefficient, although in the same direction as the baseline, possibly due to the particular pattern of the data, which can also be observed from the behavior of the fitted polynomial.

exclusion around the cutoff, the combination of the former, and two-way clustering). The results are reported in Tables A5 and A6. For Italy (Table A5), the estimated slope changes in excess mortality around the three temporal thresholds do not document a trajectory consistent with a potential confounding epidemiological pattern. Around the first and third announcements, the estimated coefficients are negative and in some specifications even statistically significant, indicating that the excess mortality slope was not increasing discontinuously at the cutoff. Around the second announcement, coefficients are small and imprecisely estimated across models. Therefore, there is no evidence of a positive mortality shock coinciding with the cutoff dates able to explain the vaccination uptake documented in the main estimates.

For Spain (Table A6), the placebo exercise delivers even weaker evidence of relevant discontinuity in mortality: slope estimates are small in magnitude and generally statistically insignificant across both announcements and specifications. The absence of systematic variations in mortality dynamics around the thresholds further corroborates the validity of our identification.

4.3 Difference in Differences

We finally discuss the results of the dynamic Staggered Difference-In-Differences analysis limited to Spain. Figure A6 shows the results from the main OLS TWFE analysis.¹⁰ Our results indicate that the reintroduction of the COVID-19 Certificate generated a modest yet statistically significant increase in vaccinations in the immediate aftermath of the decision, amounting to several dozen additional doses per 100,000 unvaccinated individuals. The effect starts getting larger and is still significant after the 6-th lag, jumping to 200 additional doses (per 100,000) and growing even more thereafter. The dynamic effects are all significant after the 14-th lag, and they amount to about 600 additional doses after 25 days. While the estimates are sensibly smaller in the first lags of the time frame, we can interpret this result as a lower bound of the actual effect of the reintroduction of the COVID-19 Certificate, which may have had indirect effects also in terms of vaccinations on the control regions due to salience, as emerging from

¹⁰Complete coefficient estimates, for both the OLS TWFE results and the other estimation techniques, are reported in Tables A7 and A8 in the Appendix. In order to simplify the visualization of the coefficients, the outputs of the same DiD estimations are reported in two separate tables, one for the Pre-coefficients, and one for the Post-, although belonging to the same analyses.

the oscillating significance of the coefficients after the treatment date. Furthermore, the visual inspection of the pre-trend pattern clearly validates the Parallel Trend assumption, leading to the conclusion that the shift in the daily doses cannot be reasonably linked to pre-existing paths in the treatment communities, nor to anticipatory behaviors.

While the OLS estimates deliver reasonably credible results, the risk of contamination between comparison groups in a staggered design with heterogeneous treatment may still bring about doubts about the validity and unbiasedness of the estimated coefficients. Our second main specification, performed via the interpolation method proposed by Borusyak et al., 2024, delivers similar outcomes, which are still to be interpreted with some caveats (Figure A7). As a matter of fact, the estimated confidence intervals are generally narrower, and the interpolated trend constructed using not-yet-treated units yields similar, but statistically significant, effects in the immediate aftermath of the repeal dates. However, while statistically significant, the magnitude of these initial coefficients remains small. This confirms that the recovery in vaccinations was initially sluggish and suggests that our previous estimates for the periods immediately following the judicial decisions were potentially downward-biased due to similar behavioral responses among not-yet-treated units, pointing to the presence of spillover dynamics.

In addition, we clearly observe the violation of the Parallel Trend assumption, with treated units showing a significant, decreasing trend before the COVID-19 Certificate reintroduction. This may actually lead to the conclusion that the certificate reintroduction was epidemiologically motivated at the policy level, as soon-to-be-repealed regions may have been displaying dropping rates of first doses. This could have triggered policymakers (in this case, the holders of judicial power) to nudge back the population towards more responsible behaviors in terms of healthcare, by shifting them back to vaccines through the repeal decisions. In any case, the falling trend, although significant, goes in the opposite direction of the estimated effects, hence suggesting again for lower bound-results. Furthermore, the sudden change in slope and pattern is hard to be identified as the follow-up of an ongoing trend dynamics.

The more restrictive specifications proposed by Sun and Abraham, 2021 (Figure A8) and Gardner et al., 2025 (Figure A9) confirm our interpretation. While the parallel trend assumption, in the latter two cases, is clearly resumed in validity, as the anticipatory falling trend never grows different from zero in terms of statistical significance, the estimated

effects are greatly downgraded after the treatment, both in terms of size and significance. In the Sun and Abraham specification, statistical significance is only reached after 20 lags, with a size almost 20% smaller than the estimated one observed in the baseline OLS. In the second specification, the strictness of the confidence interval is loosened, and the last coefficients gained in magnitude (amounting to about 600 in the last lags), although all coefficients previous to the 17-th lag are statistically null.

5 Discussion and Conclusion

Overall, the present paper highlights that vaccination-based access restriction policies can be effective tools to increase vaccine uptake, but their effectiveness crucially depends on institutional design and credibility. By comparing Italy and Spain—two countries with similar decentralized healthcare systems but markedly different approaches to the EU Digital COVID-19 Certificate—we provide novel evidence on the mechanisms through which such policies operate.

Our SRKD estimates show that in Italy, the first announcement (i.e., the introduction of the standard Green Pass) and the third announcement (i.e., mandatory vaccinations for individuals over 50) reported significant effects across different bandwidth specifications, increasing vaccinations among unvaccinated individuals in a range spanning from 3.12 to 11.03 percent. The opposite direction in terms of magnitude across announcements—increasing for the first and decreasing for the third—can be interpreted as a measure of the speed with which each announcement affects vaccination decisions. The first announcement was able to increase vaccinations slowly, with the estimated effect growing as the bandwidth increases, whereas the third announcement implied a faster response. This pattern is consistent with the different nature of the two interventions: the former required the digital COVID-19 certificate to access selected public places, while the latter prohibited access to the workplace for unvaccinated individuals over 50, imposing severe income consequences in addition to a fine of 100 euros.

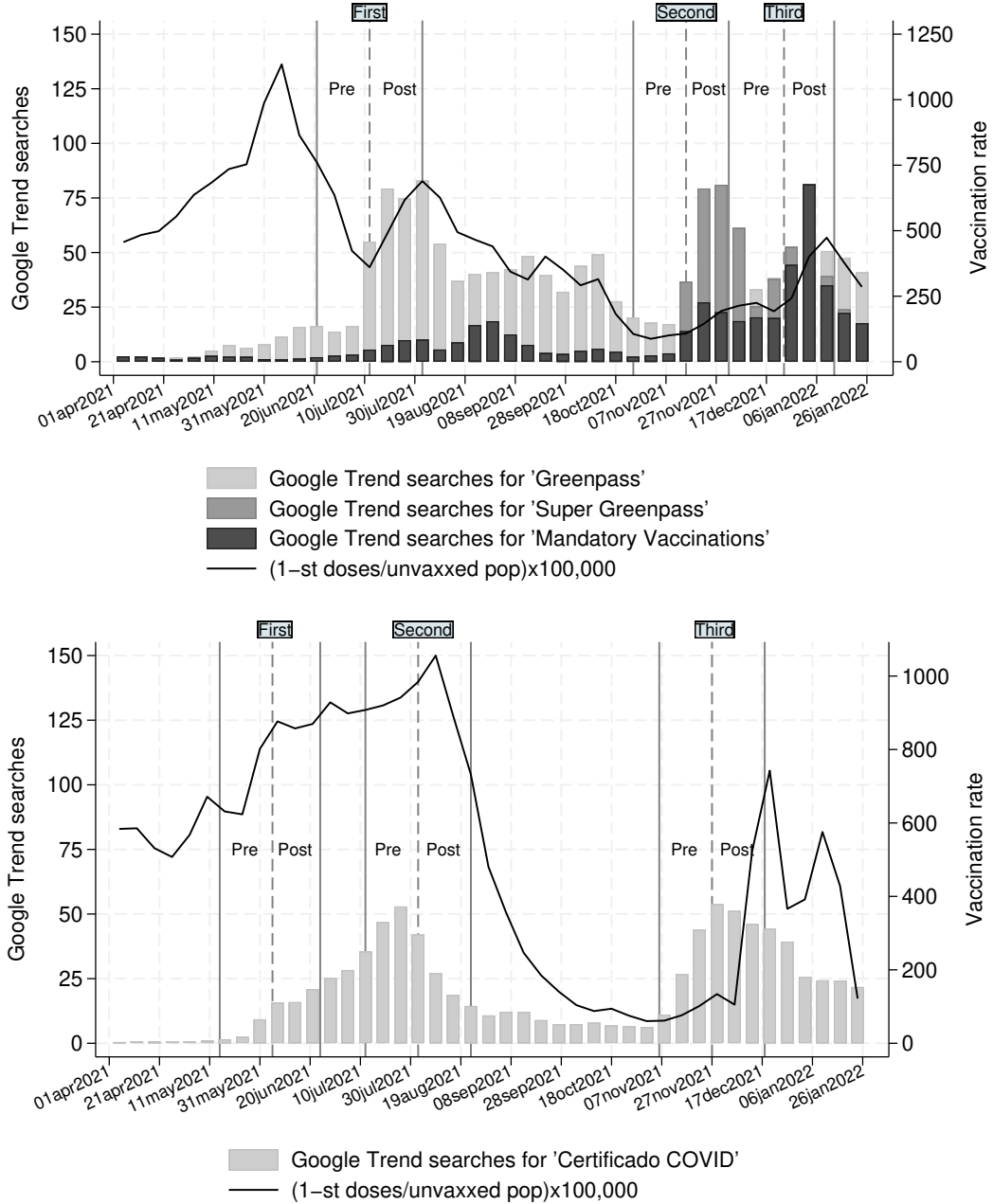
The results for Spain are strikingly different. Only the third announcement—the reintroduction of the COVID-19 Certificate by eleven autonomous communities in late November 2021—was effective in increasing vaccinations, with estimated increases between 2.15 and 9.60 percent. The first two announcements had no discernible effect. We argue that

this divergence is largely attributable to the institutional misalignment between the Spanish central government and the regional Superior Courts of Justice. When several courts abrogated the mandatory use of the certificate shortly after its initial introduction, this sent a contradictory signal to the public, effectively undermining the credibility of the policy.

These findings carry several implications for policy design. First, the Italian experience demonstrates that an incremental, centralized approach to vaccination-based access restrictions can effectively target different segments of the hesitant population over time. The first announcement likely reached moderately hesitant individuals for whom the inconvenience of not having the certificate tipped the balance in favor of vaccination. Once these individuals were vaccinated, only a stronger intervention—mandatory vaccination with tangible economic penalties—was capable of reaching more reluctant individuals. Second, the Spanish case underscores that the institutional coherence and credibility of the enforcement mechanism are as important as the policy itself. When the judicial system sends signals that contradict the executive’s policy intentions, the effectiveness of the intervention is severely compromised. This has practical implications for the design of future health policies in countries with multi-level governance structures, where the alignment between different layers of government is crucial for success.

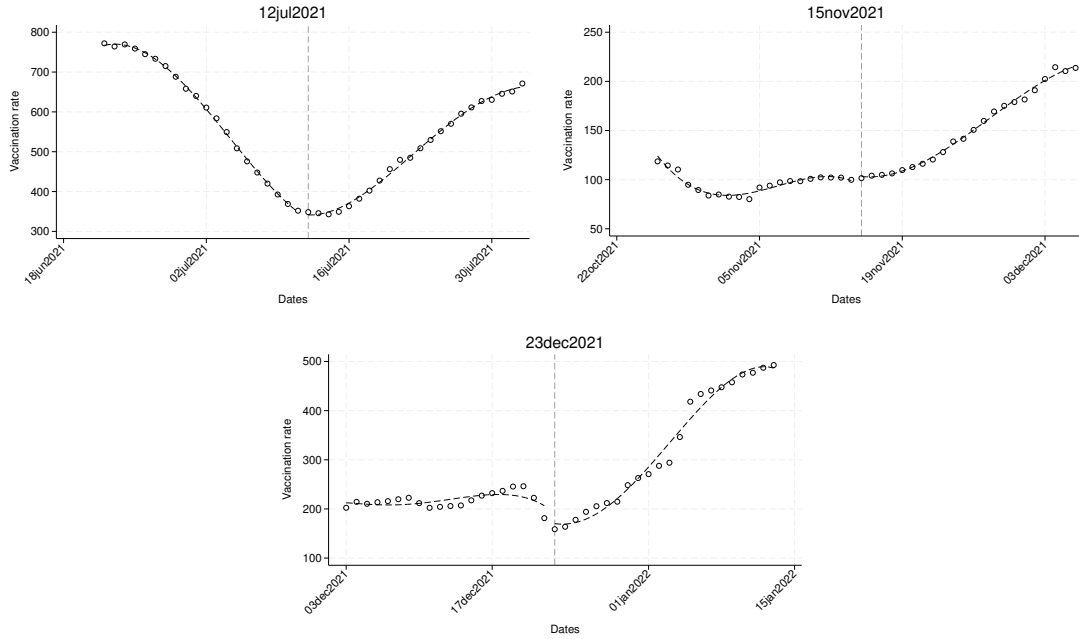
Third, while the COVID-19 Certificate occupies a gray area between nudging and mandatory vaccination—as noted by Congiu and Moscati (2022)—our results suggest that this intermediate position can be particularly effective when properly implemented. Unlike pure nudges, which rely on subtle changes in choice architecture, the certificate imposes a tangible cost on non-vaccination (exclusion from activities); unlike strict mandates, it preserves some degree of individual choice. The effectiveness of this approach, however, is contingent upon the credibility and consistency of its enforcement, as the Spanish experience clearly illustrates. Policymakers designing future interventions should therefore pay careful attention not only to the content of the policy but also to the institutional framework within which it is embedded, ensuring that potential judicial or political challenges are anticipated and addressed *ex ante*.

Figure 1: Vaccinations trend and Google Trends searches in Italy and Spain



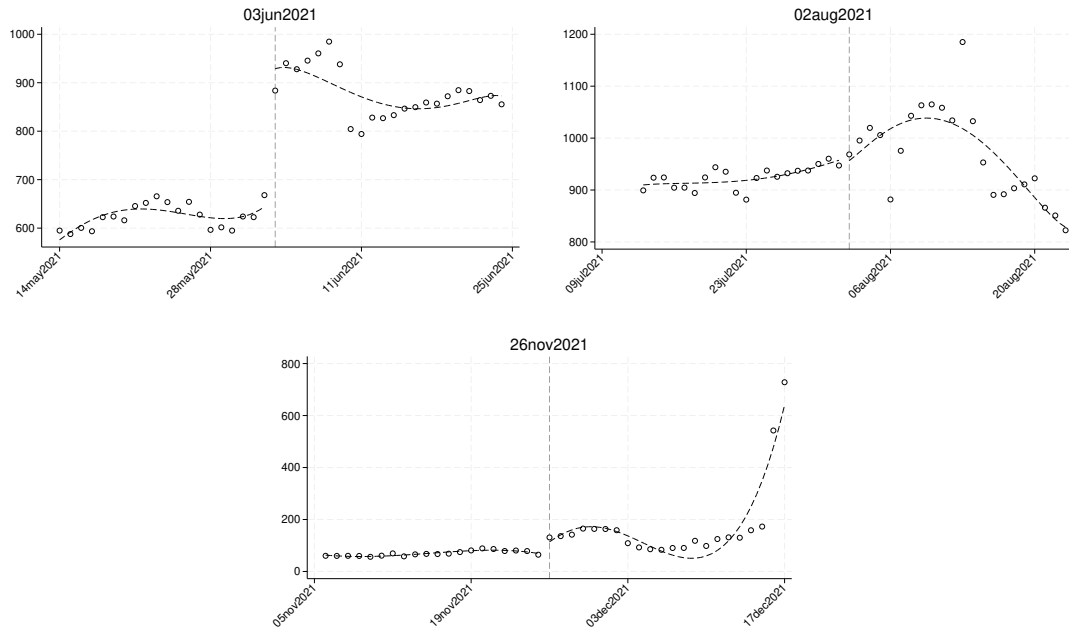
Notes: Panel A shows the average regional amount of Google Trends searches for the following terms: (i) 'Green Pass', (ii) 'Super Green Pass' and (iii) 'Mandatory vaccinations' (upper panel) and the number of first-doses per 100.000 individuals (lower panel) in Italy. Panel B shows the amount of Google Trends searches for the following terms: (i) 'Certificado COVID' (upper panel) and the number of first-doses per 100.000 individuals (lower panel) in Spain. Dashed lines correspond to weeks recording abnormal variations in the interest towards each term, solid lines identify pre- and post-treatment periods considering a six week window around the date of the announcement.

Figure 2: Green Pass effect on vaccination rates in Italy after key announcements



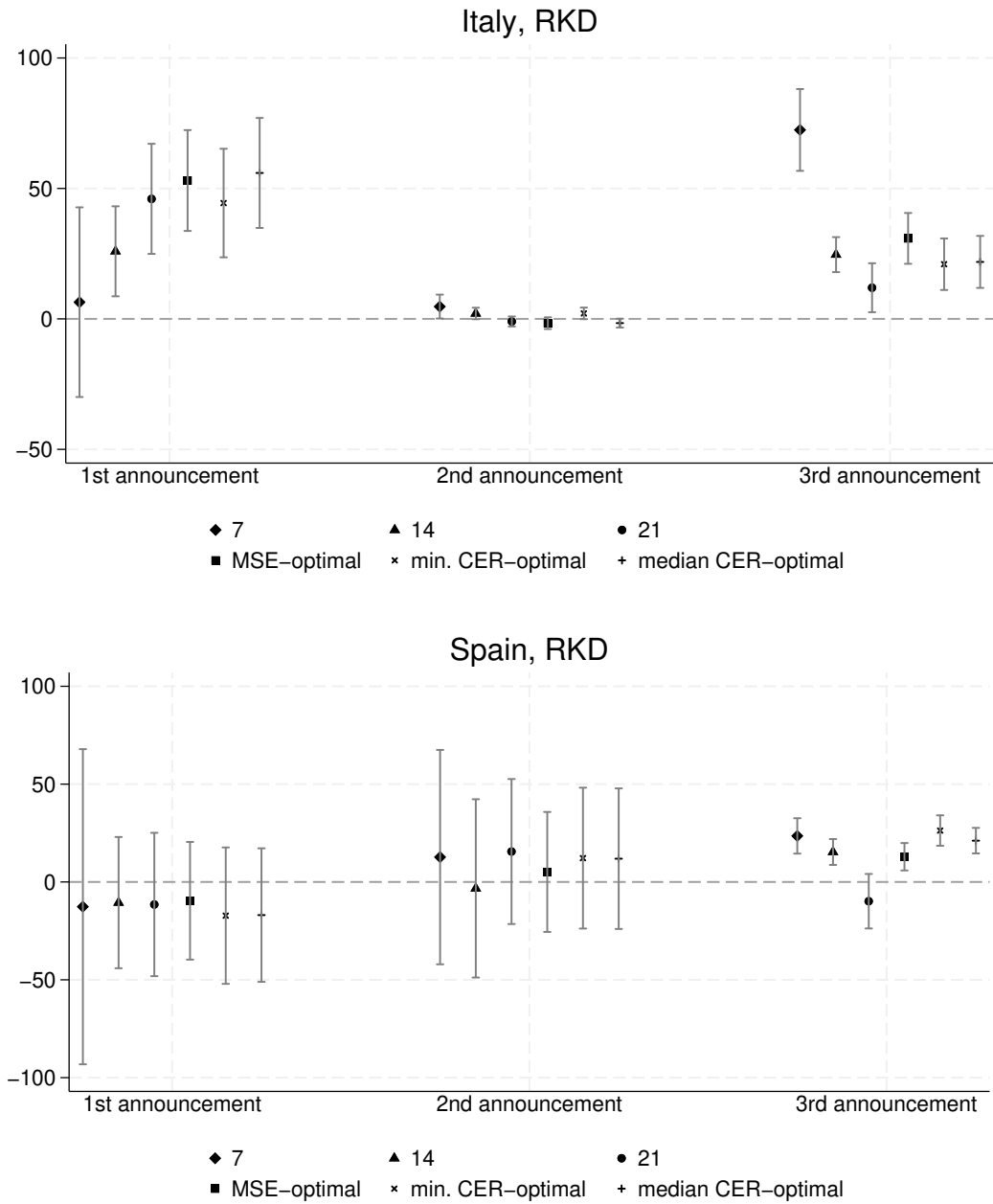
Notes: The vertical dashed line illustrates the threshold needed to identify days after which new information is available and it corresponds to the 12-th of July in the upper-left panel, 15-th of November in the upper-right panel, and 23-rd of December in the lower panel.

Figure 3: Green Pass effect on vaccination rates in Spain after key announcements



Notes: The vertical dashed line illustrates the threshold needed to identify days after which new information is available and it corresponds to the 03-rd of June in the upper-left panel, 02-nd of August in the upper-right panel, and 26-th of November in the lower panel.

Figure 4: Estimates of green pass announcements on vaccination trends in Italy and Spain - Kink Regression Discontinuity Design



Notes: This Figures plots a graphical overview of parameters computed adopting the Sharp RDD for different BWs. Diamonds, triangles and circles identify estimates computed using a BW of 7 14, and 21 respectively. Squares, x's and pluses identify the optimal bandwidths estimated following Calonico et al., 2014 and Calonico et al., 2020, using the MSE-optimal procedure, the minimum-threshold CER-optimal procedure and the median-threshold CER-optimal procedure respectively.

Table 1: Magnitude of Green Pass effect on vaccination rates in Italy and Spain after key announcements

Date	Vaccinated population	$\hat{\tau}_{RD,min-max}$	Magnitude $_{min-max}$	Magnitude $_{min-max}$ (excluding infected)
Italy				
12-jul-21	36,133,218	6.4-55.94	0.46-4.01	0.46-4.01
15-nov-21	46,930,060	0-0	0-0	0-0
23-dec-21	48,116,251	12-30.85	2.72-7.03	2.75-7.10
Total			3.12-11.03	3.21-11.11
Spain				
03-jun-21	19,038,135	0-0	0-0	0-0
02-aug-21	32,352,818	0-0	0-0	0-0
26-nov-21	38,288,327	5.89-26.28	2.15-9.60	2.15-9.60
Total			2.15-9.60	2.15-9.60

Notes: The number of positive unvaccinated individuals is computed by multiplying the number of current COVID-19 cases by the percentage of unvaccinated COVID-19 cases resulting in 527,286 on the 12nd July, 1,576,603 on the 15th November and 5,608,975 on the 23rd December in Italy and 0 across the three Spanish dates. The number of current COVID-19 cases are 40,426 on the 12nd July, 120,875 on the 15th November and 430,029 on the 23rd December in Italy and 23,368 on the 3rd June, 108,329 on the 2nd August and 38,729 on the 26th November in Spain.

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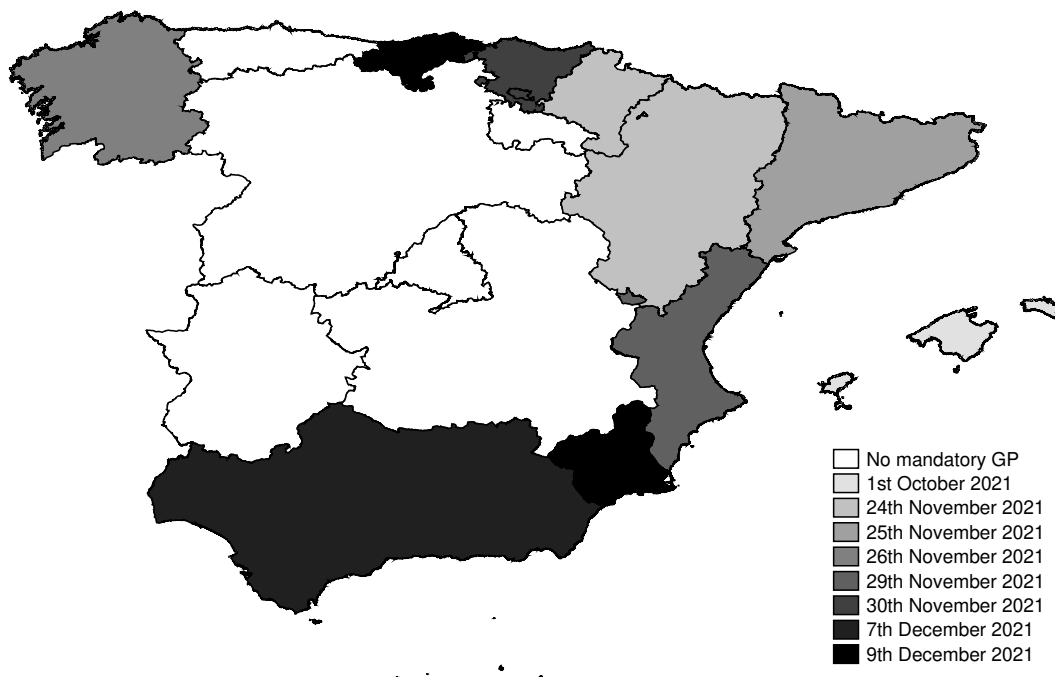
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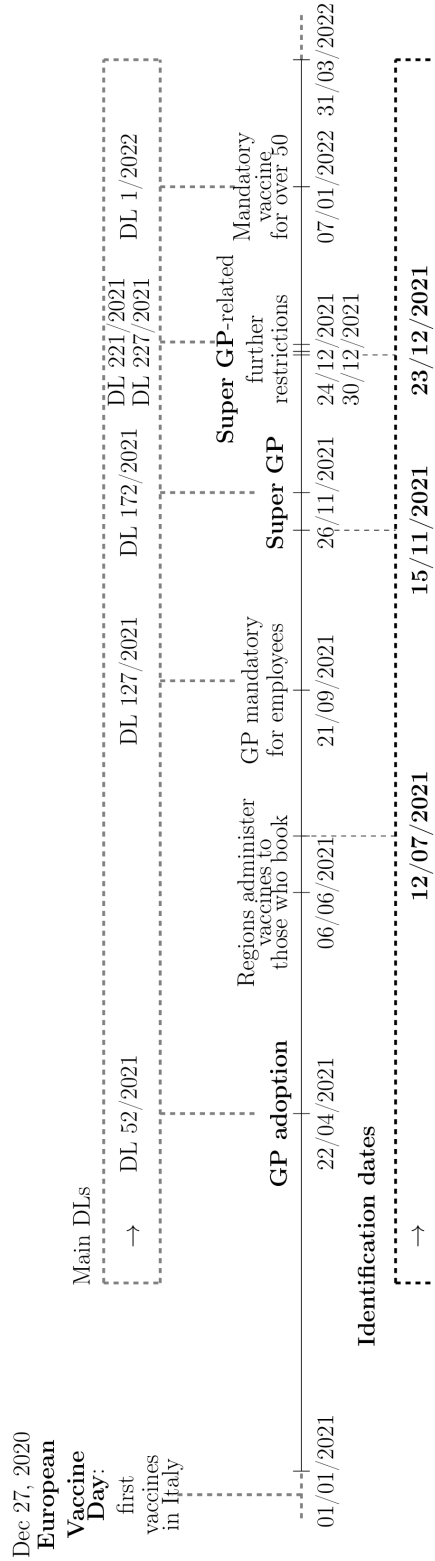
Online Appendix

Figure A1: Spanish Court repeals
Spanish provinces' court repeals by date



Notes: Visual representation of the staggered court decisions concerning the adoption of the COVID-19 certificate in Spain.

Figure A2: Vaccination and GP-related restrictions in Italy



Notes: Timeline of the main GP-related events in Italy, overlapped with the identification dates identified from Google trends in the scope of the RKD.

Table A1: Timeline of COVID-19 Green Pass-related measures in Italy (Apr 2021 – Mar 2022)

Date	Measure tied to the Covid certificate (“Green Pass”, GP)
22 Apr '21	Establishment of the COVID-19 Green Certification.
26 Apr '21 — 15 Jun '21	Travel in areas with high contagion rates allowed only for GP holders, except for work, health, or necessity reasons.
26 Apr '21 — TBD	Shows open to the public (movies, theaters, concerts) are allowed in low-contagion areas, with distanced seats and limited capacity. Access may be reserved to GP-holders only.
1 Jun '21 — TBD	Nationally relevant events and sports competitions are opened to the public in low-contagion areas, with limited capacity. Access may be reserved to GP-holders only.
15 Jun '21 — TBD	Fairs, congresses, conferences are allowed in low-contagion areas. Access may be reserved to GP-holders only. Religious and civil celebrations allowed through GP utilization.
6 Aug '21 — TBD	GP required for indoor restaurant services, cultural events, sports events, museums, gyms, swimming pools, wellness centers, fairs, congresses, theme parks, and entertainment venues.
1 Sep '21 — TBD	GP mandatory for school staff, university personnel and university students, and for long-distance public transport.
21 Sep '21 — TBD	GP mandatory for all public and private sector employees.
6 Dec '21 — 15 Jan '22	Establishment of the “Super Green Pass” (vaccinated or recovered only), required for indoor dining, public events, theaters, cinemas, and ceremonies. The validity of vaccination certificates is reduced to 9 months.
24 Dec '21 — 31 Jan '22	Super GP requirement extended to bars, gyms, museums, spas, theme parks, and social centers; validity of vaccination certificates reduced to 6 months.
7 Jan '22 — 15 Jun '22	Base GP requirement extended to personal care services (e.g., hairdressers), public offices, banks, and most shops (except essential services). Vaccination mandatory for those aged 50+.
10 Jan '22 — 31 Mar '22	Super GP requirement extended to outdoor restaurants, feasts, fairs, congresses, conferences, religious and civil celebrations; previous restrictions extended to 31 Mar 2022.

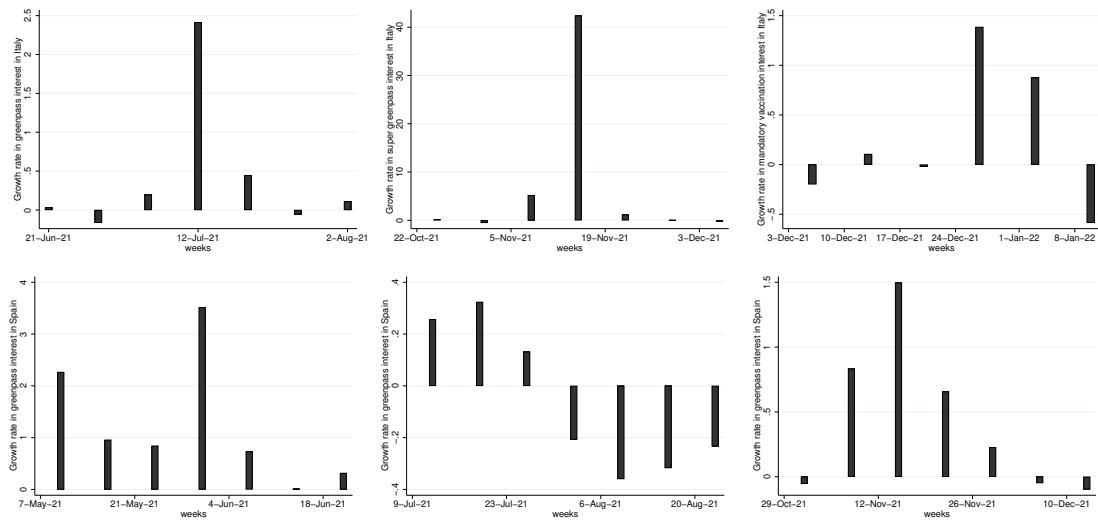
Notes: Timeline of the main GP-related events in Italy. “TBD” refers to restrictions introduced without a predetermined end date.

Table A2: Timeline of COVID-19 certificate-related measures in Spain (Jun 2021 – Spring 2022)

Date	Measure tied to the COVID certificate (Certificado COVID Digital UE)
7 Jun '21 — TBD	Spain begins issuing the EU Digital COVID Certificate (vaccination, recovery, or negative test) prior to formal EU implementation. Certificate valid primarily for cross-border mobility within the EU.
1 Jul '21 — 30 Jun '23	EU Digital COVID Certificate enters into force across the European Union for cross-border travel. Regulation later extended until 30 June 2023.
22 Jul '21 — 12 Aug '21	Galicia introduces a regional COVID certificate requirement for access to indoor hospitality venues (bars and restaurants). Measure subsequently annulled by the regional High Court.
25 Nov '21 — 22 Feb '22	Catalan High Court authorizes the COVID certificate for access to indoor bars, restaurants, gyms, sports centers, and nursing homes. Certificate becomes compulsory from 2 Dec '21 and remains in force until lifted on 22 Feb '22.
2 Dec '21 — 22 Feb '22	COVID certificate mandatory in Catalonia for indoor hospitality, nightlife, gyms, and residential care facilities (vaccination, recovery, or test).
Dec '21 — TBD	Several Autonomous Communities (including Navarra, Basque Country, Aragón, Balearic Islands, and Andalusia) introduce or expand COVID certificate requirements for hospitality venues, nightlife, cultural events, and large gatherings, subject to judicial authorization.
Jan '22 — Spring '22	Progressive lifting of regional COVID certificate requirements across Autonomous Communities as infection severity declines and judicial rulings limit proportionality of the measure.

Notes: Timeline of the main COVID certificate-related measures in Spain. Unlike Italy, domestic use of the certificate was implemented at the regional (Autonomous Community) level and often subject to judicial authorization. “TBD” refers to measures introduced without a predetermined end date or whose duration varied across regions.

Figure A3: identification of pre- and post-announcement periods in Italy and Spain



Notes: Panel A depicts the percent variation from a week to another in the interest towards Green Pass, Super Green Pass and mandatory vaccination in Italy. Panel B depicts the percent variation from a week to another in the interest towards Green Pass introduction, abolition, and re-introduction in Spain.

Table A3: Green Pass effect on vaccination rates in Italy across announcements

	(1)	(2)	(3)	(4)	(5)
	Baseline	Uniform Kernel	Donut: $\Delta(\text{date}) < 1 $	Donut + Uniform K.	Two-way Clustering
Panel A: First announcement (12 Jul 2021)					
$\hat{\tau}_{RD}$	46.0116*** (10.765)	56.7616*** (11.859)	58.4311*** (15.766)	67.1871*** (14.723)	46.0116*** (7.101)
Observations	881	881	818	818	860
R-squared					0.64
Mean of Y	545.2	545.2	560.4	560.4	545.2
SD of Y	232	232	232	232	232
Panel B: Second announcement (15 Nov 2021)					
$\hat{\tau}_{RD}$	-0.9928 (0.976)	-1.8409 (1.512)	-1.4553 (1.348)	-2.0523 (2.052)	-0.9928 (1.438)
Observations	875	875	813	813	854
R-squared					0.82
Mean of Y	125.5	125.5	127.3	127.3	125.5
SD of Y	53.57	53.57	53.57	53.57	53.57
Panel C: Third announcement (23 Dec 2021)					
$\hat{\tau}_{RD}$	11.9431** (4.787)	16.7209*** (6.110)	4.2153 (6.569)	15.4658* (7.913)	11.9431* (5.966)
Observations	878	878	815	815	857
R-squared					0.61
Mean of Y	273.4	273.4	281.5	281.5	273.4
SD of Y	142.9	142.9	142.9	142.9	142.9

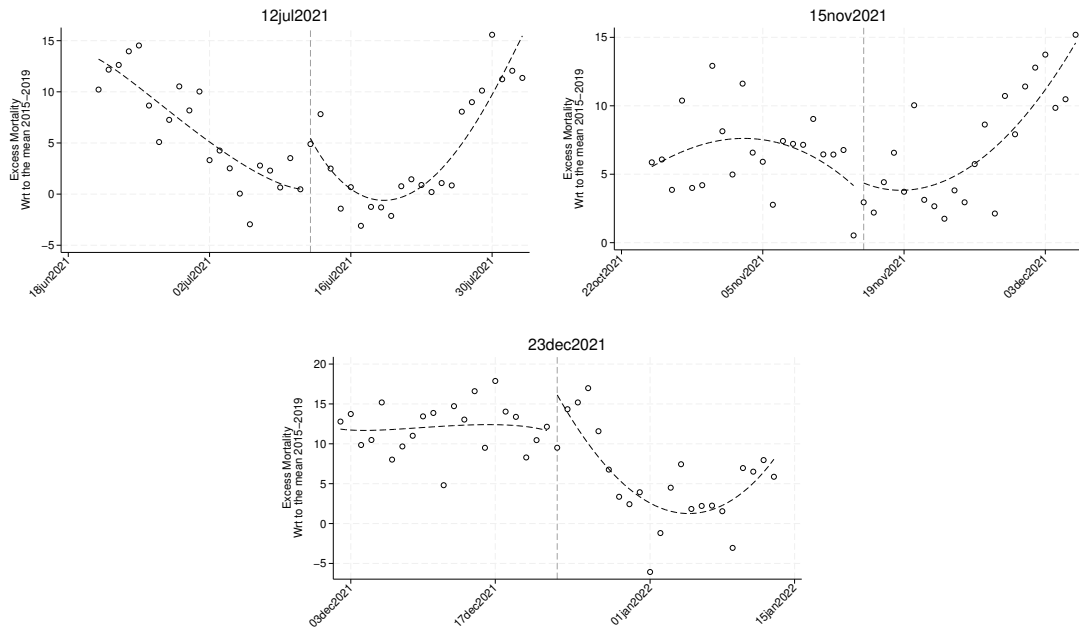
Notes: Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Bandwidth fixed at 21. Column (1) uses the baseline specification (triangular kernel, second-degree polynomial, no removal around the cutoff); column (2) uses a uniform kernel; column (3) is a donut RKD excluding observations with $|\Delta(\text{date})| \leq 1$; column (4) combines donut exclusion with a uniform kernel; column (5) reports the OLS-equivalent specification with two-way clustered standard errors. Standard errors are in parentheses. Columns (1)–(4) cluster at the regional level; column (5) clusters two-way (region and day/date).

Table A4: Green Pass effect on vaccination rates in Spain across announcements

	(1)	(2)	(3)	(4)	(5)
	Baseline	Uniform Kernel	Donut: $\Delta(\text{date}) < 1 $	Donut + Uniform K.	Two-way Clustering
Panel A: First announcement (03 Jun 2021)					
$\hat{\tau}_{RD}$	-11.5051 (18.690)	-9.2097 (20.022)	-21.5600 (25.959)	-12.6046 (24.769)	-11.5051 (18.625)
Observations	798	798	741	741	779
R-squared					0.45
Mean of Y	757	757	751.3	751.3	757
SD of Y	250.9	250.9	250.9	250.9	250.9
Panel B: Second announcement (02 Aug 2021)					
$\hat{\tau}_{RD}$	15.5543 (18.905)	13.5177 (22.903)	23.9673 (25.756)	16.2373 (30.468)	15.5543 (13.647)
Observations	798	798	741	741	779
R-squared					0.48
Mean of Y	948.1	948.1	946.4	946.4	948.1
SD of Y	343.9	343.9	343.9	343.9	343.9
Panel C: Third announcement (26 Nov 2021)					
$\hat{\tau}_{RD}$	-9.8179 (7.107)	-44.6333*** (8.730)	-26.8371** (11.674)	-70.1811*** (12.023)	-9.8179 (7.782)
Observations	798	798	741	741	779
R-squared					0.39
Mean of Y	123.9	123.9	125	125	123.9
SD of Y	161.1	161.1	161.1	161.1	161.1

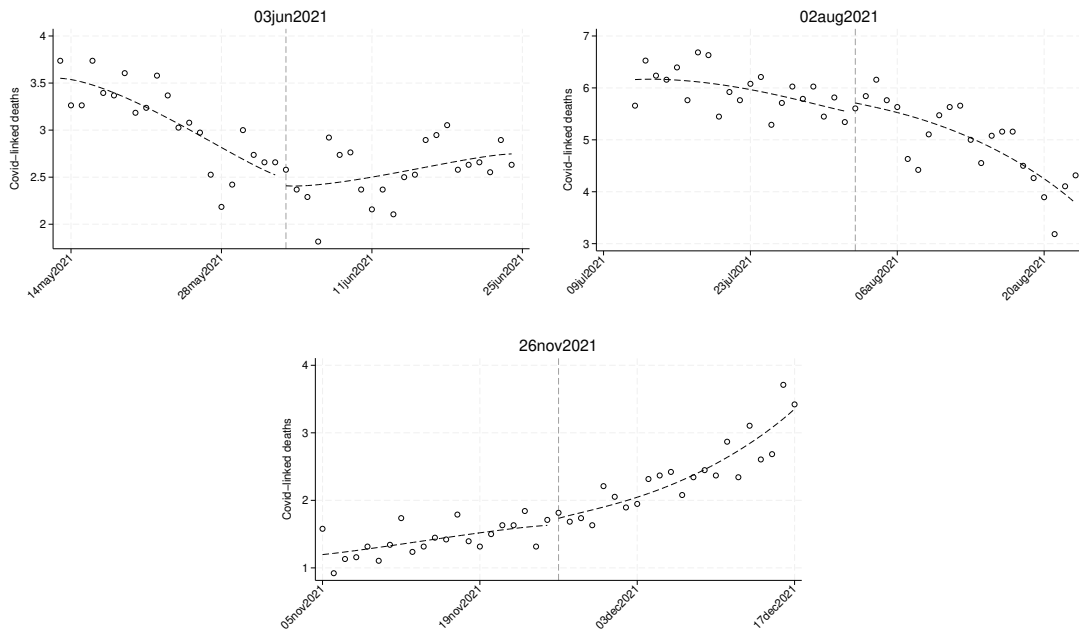
Notes: Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Bandwidth fixed at 21. Column (1) uses the baseline specification (triangular kernel, second-degree polynomial, no removal around the cutoff); column (2) uses a uniform kernel; column (3) is a donut RKD excluding observations with $|\Delta(\text{date})| \leq 1$; column (4) combines donut exclusion with a uniform kernel; column (5) reports the OLS-equivalent specification with two-way clustered standard errors. Standard errors are in parentheses. Columns (1)–(4) cluster at the regional level; column (5) clusters two-way (region and day/date).

Figure A4: Italian daily excess mortality plotted around the identified threshold



Notes: The vertical dashed line illustrates the threshold needed to identify days after which new information is available and it corresponds to the 12-th of July in the upper-left panel, 15-th of November in the upper-right panel, and 23-rd of December in the lower panel.

Figure A5: Spanish daily deaths linked to COVID-19 plotted around the identified threshold



Notes: The vertical dashed line illustrates the threshold needed to identify days after which new information is available and it corresponds to the 12-th of July in the upper-left panel, 15-th of November in the upper-right panel, and 23-rd of December in the lower panel.

Table A5: Green Pass effect and excess mortality in Italy across announcements

	(1)	(2)	(3)	(4)	(5)
	Baseline	Uniform Kernel	Donut: $\Delta(\text{date}) < 1 $	Donut + Uniform K.	Two-way Clustering
Panel A: First announcement (12 Jul 2021)					
$\hat{\tau}_{RD}$	-2.1272*** (0.787)	-0.8493 (0.755)	-1.8018** (0.884)	-0.1886 (0.911)	-2.1272*** (0.529)
Observations	860	860	800	800	820
R-squared					0.24
Mean of Y	5.105	5.105	5.159	5.159	5.105
SD of Y	16.59	16.59	16.59	16.59	16.59
Panel B: Second announcement (15 Nov 2021)					
$\hat{\tau}_{RD}$	0.5872 (0.675)	0.4692 (0.489)	-0.9986 (0.666)	-0.3470 (0.476)	0.5872 (0.515)
Observations	860	860	800	800	820
R-squared					0.21
Mean of Y	6.771	6.771	7.137	7.137	6.771
SD of Y	12.32	12.32	12.32	12.32	12.32
Panel C: Third announcement (23 Dec 2021)					
$\hat{\tau}_{RD}$	-1.5493** (0.654)	-2.2588*** (0.589)	-3.1485*** (1.078)	-3.1826*** (0.868)	-1.5493* (0.876)
Observations	860	860	800	800	820
R-squared					0.40
Mean of Y	8.693	8.693	8.445	8.445	8.693
SD of Y	14.79	14.79	14.79	14.79	14.79

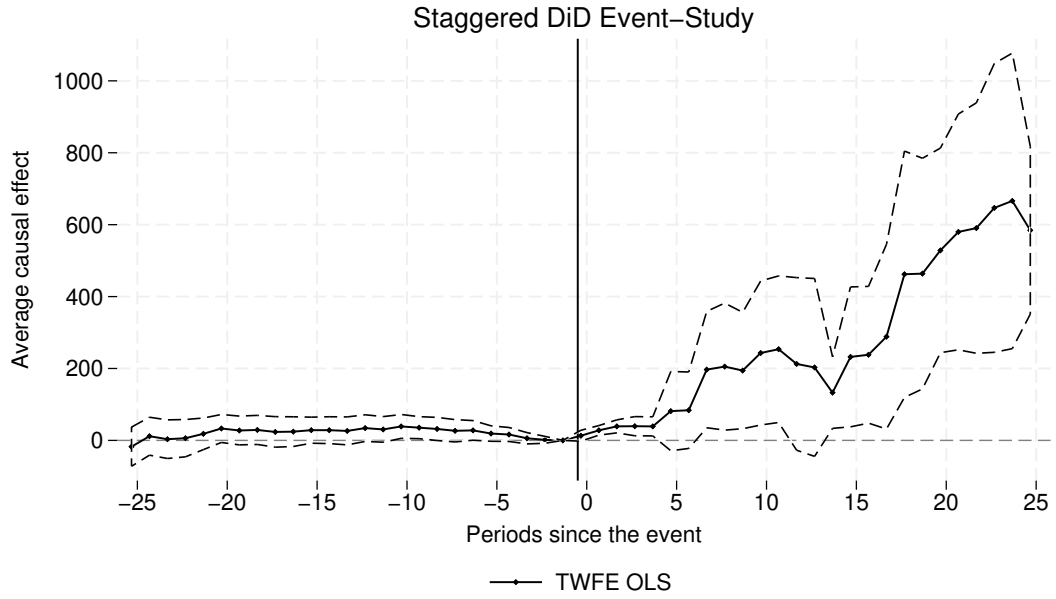
Notes: Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Bandwidth fixed at 21. Column (1) uses the baseline specification (triangular kernel, second-degree polynomial, no removal around the cutoff); column (2) uses a uniform kernel; column (3) is a donut RKD excluding observations with $|\Delta(\text{date})| \leq 1$; column (4) combines donut exclusion with a uniform kernel; column (5) reports the OLS-equivalent specification with two-way clustered standard errors. Standard errors are in parentheses. Columns (1)–(4) cluster at the regional level; column (5) clusters two-way (region and day/date).

Table A6: Green Pass and excess mortality in Spain across announcements

	(1)	(2)	(3)	(4)	(5)
	Baseline	Uniform Kernel	Donut: $\Delta(\text{date}) < 1 $	Donut + Uniform K.	Two-way Clustering
Panel A: First announcement (03 Jun 2021)					
$\hat{\tau}_{RD}$	0.0114 (0.050)	0.0876 (0.055)	0.0626 (0.081)	0.1355* (0.073)	0.0114 (0.038)
Observations	1,634	1,634	1,520	1,520	1,558
R-squared					0.55
Mean of Y	2.822	2.822	2.843	2.843	2.822
SD of Y	3.791	3.791	3.791	3.791	3.791
Panel B: Second announcement (02 Aug 2021)					
$\hat{\tau}_{RD}$	-0.0159 (0.122)	0.0392 (0.105)	-0.0858 (0.137)	0.0263 (0.121)	-0.0159 (0.096)
Observations	1,634	1,634	1,520	1,520	1,558
R-squared					0.66
Mean of Y	5.443	5.443	5.432	5.432	5.443
SD of Y	7.120	7.120	7.120	7.120	7.120
Panel C: Third announcement (26 Nov 2021)					
$\hat{\tau}_{RD}$	0.0419 (0.062)	-0.0054 (0.043)	0.0791 (0.092)	-0.0075 (0.058)	0.0419 (0.052)
Observations	1,634	1,634	1,520	1,520	1,558
R-squared					0.58
Mean of Y	1.905	1.905	1.917	1.917	1.905
SD of Y	2.653	2.653	2.653	2.653	2.653

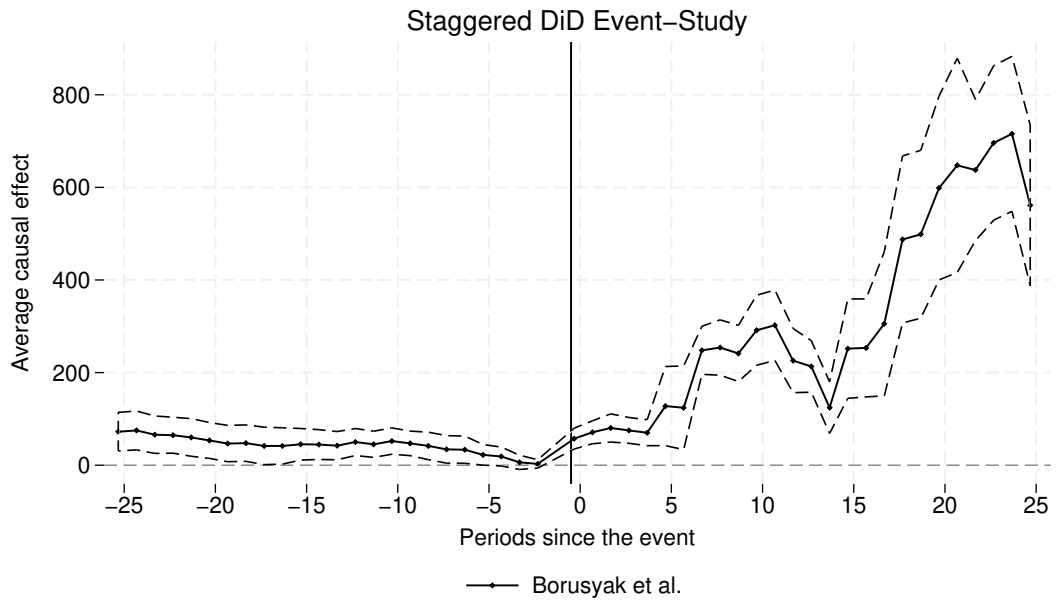
Notes: Significant levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Bandwidth fixed at 21. Column (1) uses the baseline specification (triangular kernel, second-degree polynomial, no removal around the cutoff); column (2) uses a uniform kernel; column (3) is a donut RKD excluding observations with $|\Delta(\text{date})| \leq 1$; column (4) combines donut exclusion with a uniform kernel; column (5) reports the OLS-equivalent specification with two-way clustered standard errors. Standard errors are in parentheses. Columns (1)–(4) cluster at the regional level; column (5) clusters two-way (region and day/date).

Figure A6: Green Pass reintroduction effect on vaccinations in Spain



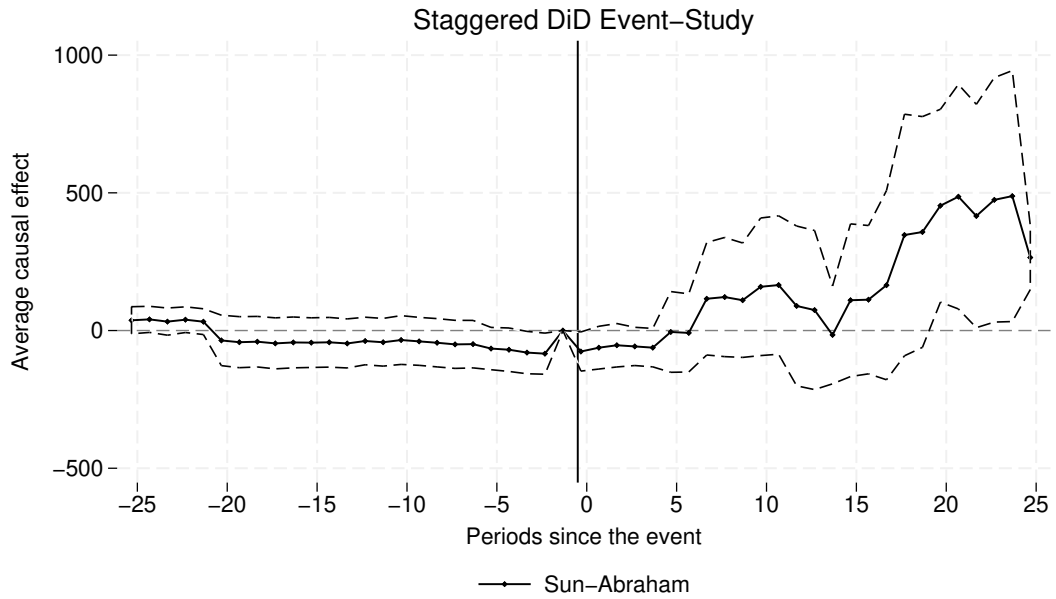
Notes: Event study estimates of the effect of the Green Pass reintroduction on vaccinations in Spain - control group of Spanish regions. Estimation procedure: TWFE. Confidence intervals at 95%

Figure A7: Green Pass reintroduction effect on vaccinations in Spain



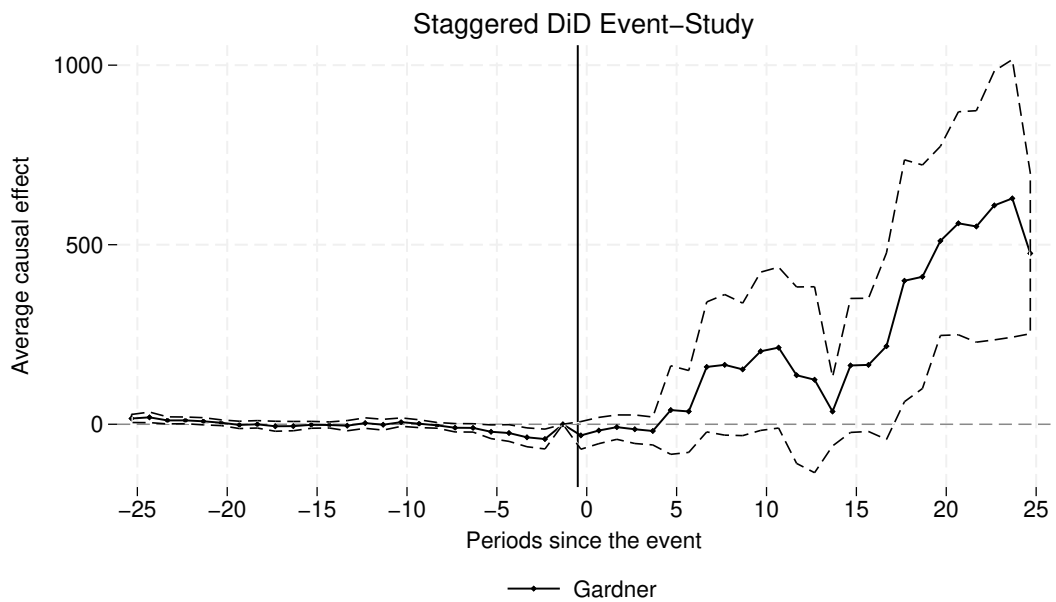
Notes: Event study estimates of the effect of the Green Pass reintroduction on vaccinations in Spain - control group of Spanish regions. Estimation procedure: Borusyak et al., 2024. Confidence intervals at 95%

Figure A8: Green Pass reintroduction effect on vaccinations in Spain



Notes: Event study estimates of the effect of the Green Pass reintroduction on vaccinations in Spain - control group of Spanish regions. Estimation procedure: Sun and Abraham, 2021. Confidence intervals at 95%

Figure A9: Green Pass reintroduction effect on vaccinations in Spain



Notes: Event study estimates of the effect of the Green Pass reintroduction on vaccinations in Spain - control group of Spanish regions. Estimation procedure: Butts and Gardner, 2022, from Gardner et al., 2025. Confidence intervals at 95%

Table A7: Event-study coefficients: pre-treatment periods

	FDVR (TWFE)	FDVR (Borusyak)	FDVR (Sun-Abraham)	FDVR (Gardner)
Pre 25	-17.483 (27.752)	72.344*** (21.298)	37.220 (25.110)	16.101*** (5.712)
Pre 24	11.667 (26.935)	75.137*** (21.423)	40.567* (24.166)	19.300** (7.513)
Pre 23	3.353 (27.369)	65.890*** (20.622)	32.409 (25.192)	10.824** (5.028)
Pre 22	6.125 (26.481)	64.729*** (19.778)	39.435* (23.782)	10.919** (4.799)
Pre 21	18.309 (22.438)	59.974*** (20.739)	32.162 (23.878)	8.305 (5.105)
Pre 20	33.191* (19.850)	53.400*** (19.746)	-35.896 (46.714)	4.222 (4.089)
Pre 19	27.584 (20.423)	46.753** (20.023)	-42.269 (47.290)	-1.741 (5.201)
Pre 18	29.147 (20.638)	47.636** (20.070)	-40.497 (46.833)	-0.150 (5.254)
Pre 17	23.544 (21.673)	41.597** (20.646)	-46.591 (47.266)	-5.527 (7.095)
Pre 16	24.425 (21.073)	41.572** (20.034)	-43.146 (47.064)	-5.135 (6.618)
Pre 15	28.406 (18.498)	45.177*** (17.390)	-43.982 (46.008)	-1.504 (4.866)
Pre 14	28.425 (19.038)	44.626*** (16.388)	-42.673 (46.049)	-1.992 (4.304)
Pre 13	26.383 (19.860)	42.164*** (15.558)	-46.883 (45.438)	-3.935 (7.261)
Pre 12	34.182* (18.921)	50.030*** (14.874)	-37.949 (44.039)	3.310 (7.475)
Pre 11	30.516* (18.185)	45.148*** (14.453)	-42.604 (44.170)	-1.240 (7.669)
Pre 10	38.822** (16.924)	52.103*** (14.482)	-34.459 (45.218)	6.001 (5.973)
Pre 9	35.481** (15.625)	47.198*** (13.503)	-39.344 (44.421)	1.626 (5.575)
Pre 8	31.847* (16.377)	41.640*** (15.158)	-44.338 (44.744)	-2.986 (4.052)
Pre 7	26.749* (15.854)	34.148** (15.168)	-50.204 (44.526)	-9.701 (5.952)
Pre 6	27.850** (13.921)	33.465** (14.960)	-49.391 (43.981)	-10.075* (5.945)
Pre 5	19.042* (10.971)	22.370** (11.346)	-65.459* (39.218)	-20.841** (9.750)
Pre 4	16.420* (9.910)	18.792* (10.393)	-69.658* (40.281)	-24.325** (11.867)
Pre 3	5.898 (8.038)	6.151 (7.866)	-80.024** (39.255)	-36.341*** (13.407)
Pre 2	1.611 (4.880)	2.818 (4.598)	-84.001** (38.066)	-41.102*** (14.078)
Pre 1	0.000	0.000	0.000	0.000

Notes: Pre-treatment event-study coefficients by estimator. Standard errors are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The omitted period is Pre 1.

Table A8: Event-study coefficients: post-treatment periods

	FDVR (TWFE)	FDVR (Borusyak)	FDVR (Sun-Abraham)	FDVR (Gardner)
Post 0	13.005* (7.716)	57.410*** (11.513)	-76.158** (36.284)	-30.985 (19.644)
Post 1	27.958*** (6.804)	71.056*** (12.655)	-62.214 (39.630)	-17.387 (18.674)
Post 2	39.035*** (9.220)	80.430*** (15.502)	-53.405 (40.238)	-7.970 (17.340)
Post 3	39.527*** (13.591)	75.131*** (14.219)	-57.701 (35.408)	-13.839 (20.280)
Post 4	38.978*** (13.554)	70.132*** (14.323)	-62.125* (35.597)	-18.639 (19.841)
Post 5	81.434 (56.263)	127.681*** (43.600)	-5.098 (74.813)	39.564 (62.904)
Post 6	83.867 (54.233)	124.015*** (46.088)	-8.635 (72.335)	35.705 (58.149)
Post 7	196.957** (82.585)	248.143*** (26.484)	115.451 (104.228)	159.715* (92.327)
Post 8	205.310** (90.140)	254.114*** (30.516)	121.602 (110.365)	165.500* (99.909)
Post 9	194.334** (82.580)	241.377*** (30.913)	110.083 (105.958)	152.857 (94.098)
Post 10	243.096** (102.213)	291.426*** (38.575)	158.850 (127.440)	203.245* (112.445)
Post 11	253.632** (104.023)	302.282*** (38.758)	165.183 (128.227)	213.542* (114.290)
Post 12	212.743* (122.466)	225.926*** (35.378)	89.325 (148.008)	136.635 (125.381)
Post 13	202.914 (126.204)	213.233*** (28.332)	74.362 (147.379)	124.068 (132.104)
Post 14	132.866*** (50.673)	124.344*** (28.398)	-15.834 (90.601)	35.599 (48.929)
Post 15	232.231** (99.275)	251.730*** (54.728)	110.116 (141.407)	163.802* (95.164)
Post 16	238.172** (97.036)	253.326*** (53.977)	112.025 (137.398)	165.397* (94.660)
Post 17	288.492** (131.039)	305.408*** (79.481)	164.677 (175.224)	217.480 (132.541)
Post 18	462.172*** (174.793)	487.574*** (92.006)	346.866 (223.776)	399.645*** (171.810)
Post 19	463.904*** (163.676)	498.673*** (92.519)	357.802* (213.744)	410.744*** (158.816)
Post 20	528.487*** (145.179)	598.578*** (101.206)	453.330** (178.593)	510.650*** (134.365)
Post 21	579.866*** (167.298)	647.560*** (118.011)	486.054** (207.962)	559.671*** (158.417)
Post 22	590.509*** (177.700)	637.559*** (77.673)	415.947** (206.848)	550.903*** (164.423)
Post 23	646.665*** (204.935)	696.138*** (85.146)	474.521** (226.445)	609.482*** (191.015)
Post 24	666.189*** (209.684)	715.678*** (85.351)	488.218** (232.453)	629.022*** (197.230)
Post 25	584.387*** (118.944)	561.237*** (88.605)	265.068*** (59.396)	475.891*** (114.144)

Notes: Post-treatment event-study coefficients by estimator. Standard errors are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Borusyak coefficients are normalized to the estimated pre-treatment coefficient at event time $= -1$, so that Pre 1 is set to zero for comparability with the other estimators.

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