The Impact of Italy's Strategy for Inner Areas on Depopulation and Industrial Growth: A Staggered Difference-in-Difference Analysis with Spatial Spillover Effects

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The Impact of Italy's Strategy for Inner Areas on Depopulation and Industrial Growth: A Staggered Difference-in-Difference Analysis with Spatial Spillover Effects

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This paper investigates the effects of a specific governmental place-based policy to fight depopulation: the Italian Strategy for Inner Areas (SNAI). Taking advantage of the most recent developments in the econometrics of policy evaluation, we apply a staggered difference-in-difference estimator to evaluate the impact of public policy in terms of population structure and the number of plants at municipal level. The analysis is made possible thanks to a detailed panel dataset containing information about the Italian municipalities over the years 2014-2020. The results show that, over the first two years, the policy did not affect the population structure, but it has generated a significant number of extra plants in the treated municipalities. A further key issue is whether the policy has generated spillover effects on neighbours which may either corroborate the encouraging result or invalidate it. To answer this question, we combine the baseline model with a spatial empirical strategy, and we find positive spillover effects for extra plants on neighbouring municipalities.

Questo lavoro indaga gli effetti di una specifica politica territoriale per combattere lo spopolamento: la Strategia Italiana per le Aree Interne (SNAI). Sfruttando i più recenti sviluppi dell'econometria della valutazione delle politiche, applichiamo uno stimatore di differenza nella differenza per valutare l'impatto in termini di struttura della popolazione e di numero di unità produttive a livello comunale. L'analisi è resa possibile grazie a un dettagliato dataset panel contenente informazioni sui comuni italiani negli anni 2014-2020. I risultati mostrano che, nei primi due anni, la politica non ha influenzato la struttura della popolazione, ma ha generato un numero significativo di attività nei comuni trattati, con effetti positivi di spillover sui comuni vicini.

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1. Introduction

Social scientists have been extensively showing that **the differential in labour productivity between agriculture and the rest of the economy, is one of the key factors in explaining the rural vs. urban differential** (Fields, 1979; Knight, 1976; Robinson, 1976). Urban growth and rural underdevelopment have also been explained by **other economic gains from agglomeration**: sharing, matching, and learning effects (Combes and Gobillon, 2015; Duranton and Puga, 2004), together with the technological spillovers, labour pooling and intermediate input linkages introduced by the seminal work of Marshall (1890). More recently, due to a combination of globalisation and technological change, many small-to-middle sized metropolitan areas, in addition to rural regions, have been characterised by lower labour force participation and income, while many large metropolitan areas have been more prosperous in terms of income and employment (Iammarino et al., 2019). This has increased the gap between the regions at the core and the regions at the periphery in many countries (Krugman, 1991). **This spatial heterogeneity is one of the major determinants of country differences in income inequality** (Bourguignon and Morrisson, 1998), **which in turn has a negative association with growth** (Alesina and Rodrik, 1994; Clarke, 1995; Persson and Tabellini, 1994).

To offset the imbalances that could benefit some areas in the core at the expense of areas at the periphery (Krugman, 1991), many national and international institutions, such as the European Union (EU), have, for some time, been developing a consistent cohesion policy framework (Albanese et al., 2023). Within this framework, **place-based policies aimed at targeting underperforming areas** are gaining momentum. **Prime examples include enterprise zones, EU Structural Funds, and industrial cluster policies** (Neumark and Simpson, 2015). These policies have the stated aim of reducing the economic disparities generally resulting from geographic remoteness (Farole et al., 2011), they are implemented by means of specific centralised instruments and, invariably, have attracted both supporters and opponents over time. On the one hand, a cohesion policy has been recognised as necessary to compensate the most backward regions for the negative effects that the reduction in barriers has had on their economies. On the other hand, it has been considered to be a profligate waste of resources, with high costs in terms of efficiency and, consequently, economic growth. In particular, regarding place-based policies, it has been highlighted that economic activities may move from other regions to the targeted areas, arbitraging away the benefits associated with the programme without improving local residents' welfare (Kline, 2010).

From an empirical perspective, there is a non-negligible amount of work aimed at evaluating the effectiveness of cohesion policies at different analytical levels, but there is no general consensus yet. **Overall, these policies seem to have a positive impact on growth, but the direction, size, and significance of the results appear to be highly heterogeneous according to the time frame and the level of territorial focus** (Becker et al., 2010; Mohl and Hagen, 2010; Rodrl´guez-Pose and Fratesi, 2004). Positive effects on investments, employment, productivity, and wages have been shown both in the EU and in China by Giua (2017), Becker et al. (2018), Lu et al. (2019), and Fattorini et al. (2020), among others. Alternatively, a limited impact of the EU structural funds on local development and total factor productivity has been highlighted by Ciani and De Blasio (2015) and Albanese et al. (2021). Furthermore, it has been shown in the US that policies targeting economically depressed areas produce negative spillovers to non-treated neighbouring areas which experience a decline in the number of firms and employment that offset the positive programme effects (Hanson and Rohlin, 2013).

This paper contributes to this literature by investigating the effect of a specific place-based policy implemented in Italy, called the **"National Strategy for Inner Areas"** (*Strategia Nazionale Aree Interne* - SNAI). The policy **was introduced in 2014** but due to administrative, bureaucratic, and financing problems, was first enacted only in 2018. The central goal of the policy is **to fight depopulation**, but it is quite unlikely to find significant effects in the short run, not necessarily implying a failure. Notwithstanding, given the amount of money at stake (**over 591 million euros of national funds in addition to the European structural funds**) it is still worth evaluating the policy in terms of "clues", i.e., indicators capable of telling us something about the possible future success or failure of the policy.

As a first test, we focus on the percentage of the population aged over 65. We selected this indicator as, in demography, it represents the main factor explaining survival or extinction of a community (Golini et al., 2000). **As a second test, we analyse the policy in terms of the number of business sites** of firms that, consistently with Criscuolo et al. (2019) and Gibbons et al. (2019), we refer to as "plants". We have chosen such an outcome to evaluate the effects of the policy, as it is one of the most sensitive to institutional changes in the short-run (Lu et al., 2019). Effects on the number of plants can be by far more immediate than those on population, i.e. detectable in the short-run. However, we stress that an increase in the number of plants is only a necessary but not sufficient condition for the success of the policy. Indeed, finding positive effects on the number of plants in the short-run generates a reasonable expectation of finding positive effects on the population in the long run. By the same token, if no effects are found on plants in the short-run, it is quite unlikely that depopulation can be restrained in the long run.

Inner areas are territories characterised by being located at a significant distance from the main centres providing essential services to inhabitants, i.e., health, education, and mobility. In this regard, inner areas are defined at municipal level according to a distance criterion. More precisely, a municipality is classified as an inner area if it is more than 20 minutes away from secondary schools, hospitals, and train stations. Consistently with the policy design, **our analysis is carried out at the municipality level**, the most disaggregated administrative level in Italy (Barca et al., 2014). Previous studies have analysed the impacts of regional policies at aggregated levels (Becker et al., 2010; Mohl and Hagen, 2010; Rodriguez-Pose and Fratesi, 2004), particularly by region (i.e., NUTS2), but currently, the increasing availability of detailed municipality-level data allows us to disentangle direct impacts of the policies that otherwise would not be observable at finer levels of aggregation. These fine pieces of information, in turn, allow for a fine-tuning of the policies that greatly benefit the reliability of the analysis. Our identification strategy leverages the pilot areas, i.e., a set of municipalities that received the treatment in advance before the policy is fully implemented in all the eligible municipalities. The non-synchronised treatment setting gave us the opportunity to use the treated municipalities, but not yet treated as a control group, that does not differ systematically from the previous treated group by construction. Hitherto, **the treated municipalities have been admitted into treatment at different points in time, in 2018, 2019, and 2020.** From a methodological point of view, **this staggered entry gives us the opportunity to take advantage of one of the most recent developments in the econometrics of policy evaluation**. Notably, we use **the estimator developed by Sun and Abraham (2021)** which generalises the difference-in-difference estimator accounting for staggered entry into treatment. Other similar staggered difference-in-difference estimators are also used, such as Gardner (2022), Callaway and Sant'Anna (2021) and Borusyak et al. (2021), to check that the results obtained still apply.

We find no significant effects of the SNAI in terms of population structure, i.e., percentage of over 65. The absence of effects can be partially due to the **short time span of analysis** which does not allow to capture long-run effects of the policy. **Focusing on extra plants, the results show that, on average, the policy has produced results since its inception, generating a positive effect in the treated municipalities over the first two years**. However, this result, in turn, raises another crucial question about the presence of **spillover effects**. Positive spillovers generated on neighbouring municipalities would reinforce the effectiveness of the policy, but negative spillovers would bias the estimates upward raising doubts about its actual effectiveness. To investigate in detail this important issue, **we follow the empirical approach proposed by Kline and Moretti (2014) finding positive spillover effects.** This result corroborates the validity of extra plants generated by the policy and is in line with Kline and Moretti (2014).

The remainder of the paper is structured as follows: the next Section introduces the institutional setting. Section 3 presents the dataset, Section 4 describes the empirical strategy, Section 5 shows the results and Section 6 concludes the study.

2. Institutional Setting

Since the Treaty of Lisbon, **part of the EU policies is specifically aimed at hindering multidimensional poverty and - between and within - inequalities in the different territories and disadvantaged countries** (Atkinson and Piketty, 2007; Cannata et al., 2018; Carnazza et al., 2023; Liberati and Resce, 2022; Liberati et al., 2020; Mauro et al., 2018; Milanovic, 2016; Saraceno, 2015). In this regard, the role played by the European Rural Development Policies is gaining momentum (Crescenzi and Filippis, 2016; Shucksmith et al., 2005). For the purpose of targeting, within the larger EU rural development framework, **in 2012 the Italian Government introduced the definition of "inner areas": territorial areas burdened by social, economic, and environmental issues that have persisted for many decades** (Barca et al., 2014;

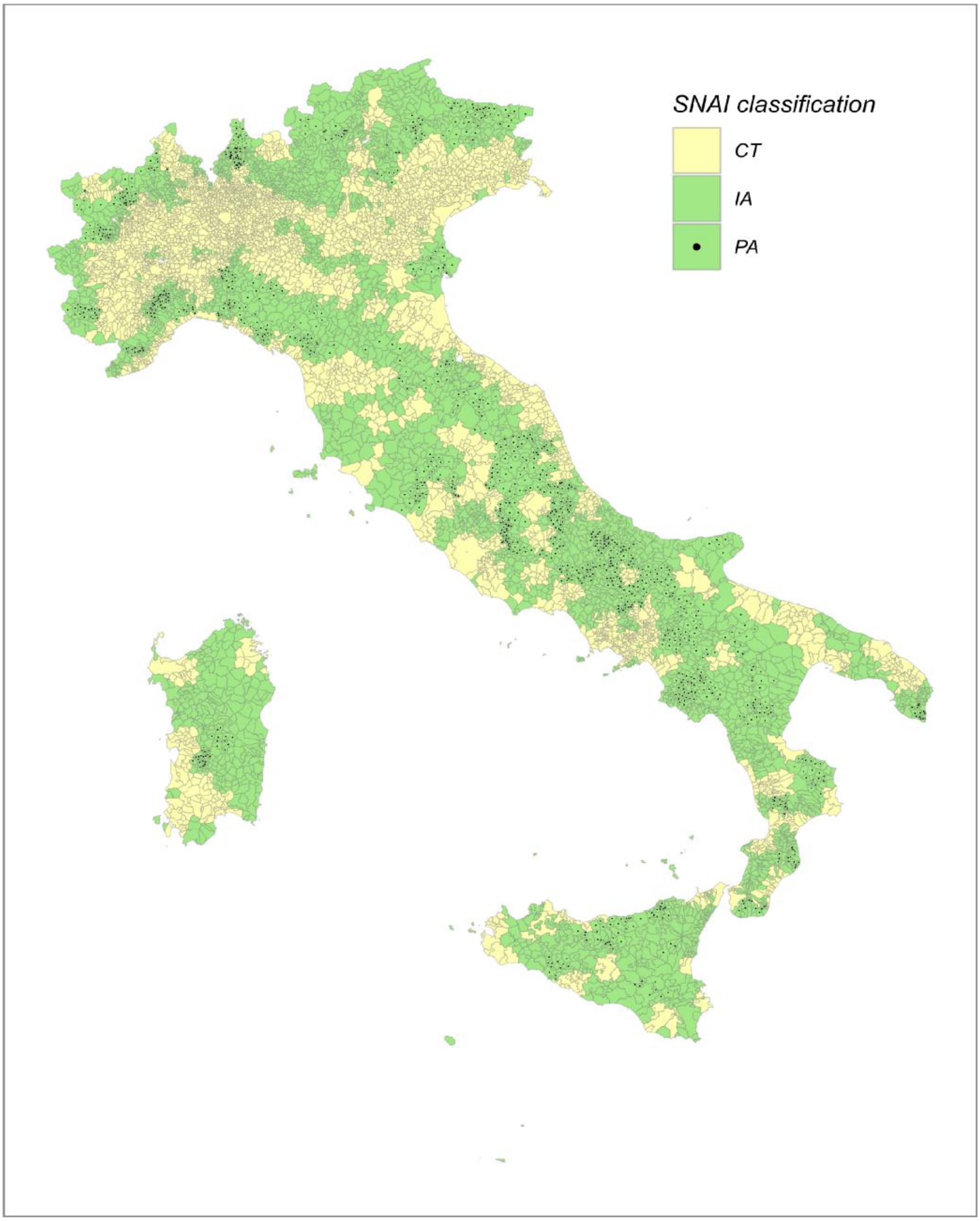
Raˆmniceanuab and Ackrill, 2020). In particular, inner areas are characterised by a distressing economic and productive weakness in terms of employment and entrepreneurship, low levels of income and wealth and relevant disparities in infrastructural and essential services, with respect to the centres. Furthermore, high rates of depopulation, youth emigration, limited birth rate and aging inhabitants are observed (Carrosio et al., 2018; Gallo and Pagliacci, 2019; Mastronardi and Cavallo, 2020; Urso et al., 2019). Despite their economic disadvantages, according to Barca et al. (2014) and Vendemmia et al. (2021), inner areas have capabilities which can be exploited as they are generally located in rich environmental and cultural systems and have resources.

For this reason, **in 2014, inner areas were organized in a specific cohesion policy framework focused on local development and citizenship rights: the SNAI** (Barca et al., 2014; European Commission, 2014). The aim of this policy is to hamper negative demographic trends, depopulation and emigration and fostering local well-being and resilience (Antolini and Grassini, 2020; Barca, 2009, 2019; Bianchi et al., 2021; Faggian and Ascani, 2021; Maggino and Alaimo, 2021; Romagnoli and Mastronardi, 2020; Sonzogno et al., 2022).

SNAI is conceived as a bottom-up strategy, favouring institutional cooperation at different levels, and promoting participation and collaboration with all the economic actors, both public and private (Barca, 2009, 2015; Cotella and Brovarone, 2020; Cuccu and Silvestri, 2019; Russo et al., 2016). In order to define the potential beneficiaries among the approximately 8,000 Italian municipalities, the policy first defines as non-beneficiaries the "centres", that is, municipalities, or a set of neighbouring municipalities, endowed with: (i) schools offerings education from primary to secondary level; (ii) a hospital facility for emergencies with rescue services and general medicine functions; (iii) at least one regional railway station (Vendemmia et al., 2021). Consequently, inner areas, i.e. potential beneficiaries, are classified residually, according to accessibility to these services. In particular, the second step of the policy consists in computing the distance, expressed in minutes by cars, from the remaining municipalities to the closest centre (Barca et al., 2014; Boscariol, 2017; Dipartimento per lo Sviluppo e la Coesione Economica, 2014). In order to be classified as inner, a municipality must be at least twenty minutes away from a centre.

The inner areas cover a large proportion of the Italian territory, accounting for the majority of Italian municipalities, i.e., 53%, with a residential population of 13.5 million inhabitants, accounting for more than 20% of the total. Figure 1 presents a view of the distribution of inner areas throughout the Italian territory (Barca et al., 2014).

Figure 1: Spatial distribution of municipalities and pilot areas classified according to SNAI



Note: CT = centres Municipalities, IA = Inner areas municipalities, PA = Municipalities enclosed in pilot areas

Since the policy aims also at favoring institutional cooperation, eligible municipalities had to merge to form a consortium in order to apply for the benefits. For this reason, as a third step **in 2015, the "Technical Committee for Inner Areas" identified the so-called *pilot areas* or *project areas***, on the basis of the co-planning capacity of the municipalities, organised in consortia (Boscariol, 2017). This step is crucial, as for historical reasons, dating back to the end of the Middle Ages, municipalities in Italy, especially the smaller ones, were characterised by parochialism, that is, struggles, envy, and hatred between neighbouring municipalities. **To date, seventy-two pilot areas have been selected** (see Figure 1) **accounting for 1, 077 municipalities and more than two million citizens.** These municipalities are mostly located in the Southern regions, in mountain sites and suffer from aging and a significant loss of resident population and low levels of income (Cavallo, 2019; Lucatelli et al., 2019; Monaco, 2020).

Municipalities within the pilot areas are asked to act and work jointly, following an articulated and complex procedure until they receive approval in accordance with the "Programme Framework Agreements" (PFA). The PFA are technical planning documents summarising all the projects to be carried out and therefore include the financial and related details. They contain the references of the implementing bodies, financial resources, planning and timing of the interventions, as well as the expected results and the socio-economic benefits accruing from the individual projects (Boscariol, 2017; Dipartimento per le politiche di coesione, 2017). However, not all of the selected 1, 077 municipalities have been financed in 2020, the last observation of our sample. Accordingly, Table 1 reports information only on the pilot areas that have been financed, based on the monitoring carried out by the Department for Cohesion Policies, present in the "Annual Reports on the National Strategy for Inner Areas" (Dipartimento per le politiche di coesione, 2019, 2020, 2021). In particular, the 2020 Report notes that **as of December 31, 2020, 19 pilot areas, corresponding to 269 municipalities, have received payments, for an amount exceeding 29 million euros, over the year 2018-2020.** On average, **the treatment received by each municipality ranges from around 70k Euro in 2018 to 172k at the end of 2020**. Despite the small overall investment, as the treated municipalities are few and small, the payments can make a difference in the local economy (the average resident population of the municipalities in these areas does not exceed 2, 400 residents). **In the pilot areas, the payments are about 625 Euros per inhabitant** which are higher than what a municipality spends on average for relevant services such as local policing (Bucci et al., 2023). Furthermore, **the figures are expected to increase in the future, as the total funds' programmed amount to about 390m Euro**[1].

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[1] Our study does not consider the years after 2020 in order to avoid two main confounders that took place in 2020: the institutional changes that involved the SNAI for the expiry of the European cohesion programming for the years 2014-2020, and the outbreak of Covid-19 and its policy consequences. Undoubtedly, the Covid-19 pandemic has worsened social, economic, and territorial problems (Carillo and Jappelli, 2022; Cerqueti et al., 2022). Lockdowns and market closures have brought the entire world economy to its knees. The Italian GDP, in 2020, recorded a loss of 8.9% (ISTAT, 2021). Poverty and inequalities, especially health and education, have significantly increased (Brunori et al., 2021; De Falco and De Vogli, 2021). The EU's response to the recession has

The SNAI policy is financed by the government budget and by EU funds, as well as other public and private resources (Rossitti et al., 2021). According to institutional Annual Reports of SNAI (Dipartimento per le politiche di coesione, 2019, 2020, 2021), in the first years of implementation of the policy, **welfare practices and initiatives were activated in many municipalities of the pilot areas** in the economic and productive sectors that favor local development, such as **agriculture, tourism, the efficiency of the public administration, management, and recycling of waste, energy, and maintenance.** Moreover, some activities have been put in place to improve and make essential services more efficient, mainly with regard to **the socio-health sector, education and training systems, and transports** (Carrosio et al., 2020; Dipartimento per le politiche di coesione, 2020; Lucatelli et al., 2019; Mastronardi et al., 2020). Innovative professional figures, such as family nurses and community midwives, have been introduced in the personal care and assistance sector. With the resources of the SNAI, health facilities have also been opened for the elderly and the sick who need long-term care (Monaco, 2020; Tantillo, 2020). Many investments have also concerned the education sector. Old buildings have been modernized, and redeveloped, new school facilities have been created with advanced digital technologies, and innovative learning programs and training offers have been activated (Lucatelli et al., 2019; Vendemmia, 2021). Similarly, in the transport sector, important projects have been launched and tested in the spirit of a sustainable mobility sector, with on-call systems for workers and students and with cycle paths (Bacci et al., 2021; Monaco, 2020). In spite of this anecdotal information, to the best of our knowledge, no evidence has been provided on a scientific basis and our contribution attempts to fill this gap.

been powerful. With the European funds of the Next Generation EU (NGEU), the Community institutions have intended to promote robust and sustainable economic growth throughout the continent (Carta and De Philippis, 2021; Cerniglia et al., 2021; Watzka and Watt, 2020). The Italian National Recovery and Resilience Plan (PNRR) (Governo Italiano, 2021) detailed multiannual structural interventions, organized in a multilevel structure, which involves local and regional administrations (Maranzano et al., 2021). It pays considerable attention to the problems present in the inner areas. It refinances the Strategy with significant resources, which are combined with national funding approved during the most acute phase of the pandemic emergency (Dipartimento per le politiche di coesione, 2021). In addition to the funds for the recovery for the strengthening of the SNAI, in the coming years, other resources will be assigned, as part of the new European programming of cohesion policies, 2021-2027. The EU, therefore, recognises the importance of SNAI as an economic and cohesion policy capable of reversing negative trends. In this sense, Covid-19 for SNAI represented a sort of watershed between the new and old European programming and nationally oriented for marginal areas (ISTAT, 2022).

Table 1: SNAI financial progress. Treatments in the pilot areas (2018-2020)

Treatments	Total scheduled	Payments		
		2018	2019	2020
Total treatments	389,858,749	2,805,298	5,163,059	21,275,616
Per municipality	1,449,289	70,132	48,708	172,972
Per inhabitant	625	1,701	2,551	7,621
Number of pilot areas	**19**	**3**	**5**	**11**
Number of municipalities	**269**	**40**	**106**	**123**
Number of inhabitants in pilot areas	**623,892**	**65,984**	**214,517**	**343,391**
Average inhabitants in the municipalities	**2,319**	**1,650**	**2,024**	**2,792**

Note: our elaborations on data provided by the Department for Cohesion Policies

3. The dataset

Table 2 reports **descriptive statistics broken down by types of municipalities, centres, and inner areas. In turn**, the latter are divided with respect to the SNAI treatment, i.e., into municipalities eligible for treatment and municipalities actually treated in the three cohort years analyzed (2018-2019-2020). The variables are gathered into four groups obtained from three different sources and merged by means of the unique municipality identifier provided by the Italian National Institute of Statistics, Istat.

The first group is meant to capture economic conditions (source: Italian Ministry of Economics and Finance), the second demographic conditions (source: Istat), the third institutional characteristics (source: registry of local and regional administrators of the Ministry of the Interior), and finally the fourth pertains territorial morphology of the Italian municipalities (source: Istat).

It can be noted that the **inner municipalities show poor (average) performance on many of the indicators** studied. Especially in the treated municipalities, the results obtained reveal the greater pervasiveness of **multidimensional deprivations**, which negatively affect the quality of life and well-being of the territories, since the latter areas are characterised by **high socio-economic criticalities**, as well as by differences in the composition and in the structure of institutions and local administrations. Notably, these differences are made evident by the average number of plants (700 units on average more in the centres than in the inner areas), and by the level of income. Furthermore, despite a greater surface the inner areas have, on average, a population which is a quarter of the centres, and a **higher share of population over 65 years**, highlighting aging of municipalities as well as a higher proportion of population that is out of the labour market. These figures, capture the main problem afflicting the inner areas, namely **depopulation and abandonment**. This phenomenon is also partly attributable to the fact that these municipalities, are mainly located in mountain areas, far from industrial and commercial centres and from more accessible logistics and distribution centres, as evidenced by the altitude. Other noteworthy characteristics that emerge from the table concern the **lower**

proportion of female mayors and the greater proportion of municipalities administered by civic lists for the municipalities of the inner areas. These data show a **weak capacity to innovate** and adapt its organisational and governance structure. As regards the remaining institutional characteristics, such as age of mayor, intensity of graduated mayors and average age of municipal councilors, the two groups are similar. In this sense, in fact, in the last three columns of Table 2, there are fewer evident gaps between eligible inner areas and treated municipalities.

Overall, a wide heterogeneity in economic, demographic, and institutional features can be observed, calling for support and public policies aimed at generating development in the most marginal municipalities and reducing inequalities between urban and rural areas.

Table 2: Descriptive statistics for centres and Inner areas (eligible and treated in 2018, 2019 and 2020)

	Centres	Inner areas			
		Eligible	Treated in 2018	Treated in 2019	Treated in 2020
Plants	934.25	218.37	134.67	120.52	198.11
	(5704.45)	(416.58)	(149.17)	(206.75)	(215.83)
Average Income	23325.53	20264.14	22066.05	18974.59	19231.03
	(3161.25)	(2854.08)	(2034.38)	(1993.40)	(2147.06)
Demographic					
Population	11724.21	3264.73	1636.38	1746.88	2828.89
	(57077.89)	(5713.51)	(1521.16)	(2171.11)	(2760.52)
Over 65	21.69	25.34	22.10	28.80	26.92
	(4.27)	(5.68)	(3.10)	(6.57)	(4.64)
Institutional characteristics					
Mayor's age	52.05	52.18	48.79	52.94	51.72
	(10.64)	(10.89)	(12.40)	(10.70)	(10.57)
Mayor's education (degree)	0.45	0.43	0.24	0.39	0.48
	(0.50)	(0.49)	(0.42)	(0.48)	(0.49)
Female mayor	0.16	0.12	0.14	0.10	0.09
	(0.37)	(0.32)	(0.34)	(0.29)	(0.28)
Average age of council	47.36	46.38	45.77	46.24	45.31
	(4.54)	(4.91)	(4.39)	(4.52)	(4.41)
Civic list	0.60	0.60	0.33	0.61	0.58
	(0.49)	(0.49)	(0.47)	(0.48)	(0.49)
Geographical variables					
Altitude	208.51	476.31	752.72	655.14	602.68
	(181.56)	(305.49)	(434.28)	(226.49)	(280.91)
Surface	30.86	43.52	54.39	49.74	69.14
	(51.98)	(46.27)	(50.11)	(47.14)	(50.56)

Note: average values at municipal level and standard deviation in parenthesis. Plants = number of plants; Average income = inhabitants' average income; Population = number of inhabitants; Population density =share of inhabitants; Over 65= percentage population over 65; Mayor's age = age of the mayor; Mayor's education = 1 if mayor has a degree; Female mayor = 1 if mayor is *female; Age of council = average age of municipal council members; Civic list = 1 if the municipality is administrated by a civic list; Altitude =altitude in meters; Surface = surface in km^2*

4. Empirical Strategy

4.1 Recent developments in the difference-in-difference

Differences-in-difference (DID) is a widely used estimator among applied researchers. Under the assumption of parallel trends between treated and non-treated units and no anticipation effects, this estimator can consistently estimate the causal effects in non-experimental settings. However, a recent body of literature has shown that the estimator suffers from serious drawbacks under staggered entry. When units enter into treatment at different points in time, i.e. by cohorts, the canonical two-way fixed effects (TWFE) coefficients may even take the opposite sign with respect to the true one, essentially due to negative (or at least incorrect) weights attached to each cohort, according to Callaway and Sant'Anna (2021) and Sun and Abraham (2021) the most prominent among the authors. Moreover, the inclusion of treatment lags and leads in the TWFE regression as a test for pre-trends can be very misleading due to heterogeneous treatment effects across cohorts. Indeed, the coefficients of lags and leads can be contaminated by effects from other periods and the test is inconclusive. Among the different solutions offered by the new strand of the literature, **Sun and Abraham (2021) proposes a method that corrects the incorrect weights attached by the TWFE to each coefficient in such a manner as to retrieve unbiased estimates of the average treatment effect for the treated (ATT) at different relative times, i.e., at different treatment time for each cohort.** The distribution of these coefficients is commonly referred to as event study analysis and is satisfactorily reported in a plot that gives an immediate impression of the policy effects unfolding over time.

The policy we are considering falls exactly within this occurrence, as its implementation, hitherto, has occurred in three subsequent cohorts, 2018, 2019, and 2020. Therefore, taking advantage of the recent advances in econometric theory, we estimate the effects of the policy by applying the estimator proposed by Sun and Abraham (2021). Essentially, this method is based on the idea of using the proportion of cohorts as weights, because these are more interpretable than the weights implied by the TWFE and sum up to unity. The resulting estimates are robust to treatment effect heterogeneity.

The identifying assumptions are the same as the well-known assumptions of DID, such as (i) parallel trends in baseline outcomes; (ii) no anticipatory effects prior to treatment. The main building block of the estimates is the cohort average treatment effect on the treated, (CATT), defined as the cohort-specific average difference in outcomes relative to a suitable comparison group. It follows that the entire procedure can be synthesised in three steps. First, estimate each CATT using an interacted TWFE regression, where interactions take place between cohort dummies and relative time dummies.

Estimate the weights and finally, average the CATT's using the weights. A more formal description of the estimator is given in the next section and for a complete guide we refer the interested reader to Sun and Abraham (2021).

As for the comparison group, one can either use the never treated units or the last cohort to be treated. In order to make the treated and the non-treated group as similar as possible we first restrict our attention to eligible units only, and then we chose the last treated cohort as the most similar. Indeed, as noted by Sun and Abraham (2021) never treated are likely to behave differently from the last treated; for this reason, **we chose the last treated cohort, i.e., 2020, as a control group**[2]. In our specific setting, **the outcome variable is the number of plants in each municipality** and the parallel trend is plausible for at least two reasons; (i) treated and last treated were chosen on the basis of common characteristics; (ii) the municipalities in these cohorts are affected by serious and structural development problems, due to being located in mountain regions and far from vital connections to market outlets. These time-invariant characteristics strongly affect the outcome variable and cannot be changed, whence the need for intervention. In other words, in the absence of intervention, all the cohorts would keep following (unfortunate) analogous paths in entrepreneurship. As far as anticipatory effects are concerned we claim that they are very unlikely to hold again on the basis of two structural features. Firstly, due to historical and cultural heritage Italian local realities are characterised by strong inertia, even inactivity, and it is difficult to think of a (positive) reaction to the policy before its actual implementation. Just to provide some figures supporting this claim, consider that only around 40% of the mayors of the treated municipalities hold a degree and only 10% of mayors are females, pointing to a conservative cultural heritage. Secondly, the policy was completely new and to a certain extent, its effects were unexpected. In environments characterised by resistant attitudes to change and substantial uncertainty about the benefits, it is unlikely to expect agents to behave strategically such as by anticipating the unknown benefits.

Notwithstanding, uncertainty about future benefits raises strong suspicions of heterogeneous causal effects across cohorts where the impact of the policy unfolds over time as the agents, and therefore the cohorts, become acquainted with the policy and its potential benefits. In other words, there are reasons to argue that treatment heterogeneity would be in place, invalidating the TWFE estimates. Indeed, cohorts may differ in their covariates, which affects how they respond to treatment. For instance, if treatment effects differ with municipal average income or the share of poverty at the municipal level, we will have heterogeneous effects. Furthermore, treatment effects may vary across cohorts due to cyclical macroeconomic conditions. Nonetheless, these sources of heterogeneity are still compatible with our parallel trend assumption, which only rules out selection into treatment timing based on the evolution of the baseline outcome.

[2] When the last treated are used as control group one needs to drop periods beyond the date at which the last cohort was first treated, i.e. 2020 in our case. Fortunately, we do not lose any information as 2020 time-unit observations for outcome and covariates are not available at the estimation time.

4.2 The model

This section is **an abridged version of the model by Sun and Abraham (2021**), we present, hereafter, only the parts of the formal model that strictly pertain to our setup, referring the reader to the original paper for a full-length presentation. Let us consider a panel data set up with $T + 1$ time periods and N units. Let us suppose to observe the outcome variable Y_{it} and the binary treatment status D_{it} with $i = 1, \ldots, N$ and $t = 1, \ldots, T + 1$. The treatment status takes the value 1 if and when unit i is treated. We also assume the treatment status to be of the absorbing type, i.e. once a unit is treated, it remains treated in the following periods. It follows that it is possible to group units by cohorts, namely according to the first treatment time, $E_i = min\,\{t : D_{it} = 1\}$, and denoting by $E_i = \infty$ never treated units. Thus, units in the same cohorts e for $e \in \{0, \ldots, T, \infty\}$ are first treated at the same time $\{i : E_i = e\}$. As for the potential outcome, we define Y_{it}^{e} the potential outcome in period t when unit i is first treated in time period e. By analogy, Y_{it}^{∞} is the potential outcome under no treatment. In order to make identification possible the following assumptions are made:

Assumption 1 *Parallel trend in potential outcome:*
$E\left[Y_{i,t}^{\infty} - Y_{i,s}^{\infty}\right]$ *is the same for all admissible e and for all* $s \neq t$

Assumption 2 *Absence of anticipation effects:*
$E\left[Y_{i,e+l}^{e} - Y_{i,e+l}^{\infty} | E_i = e\right] = 0$ *for all admissible e and* $l < 0$

Using the notation to hand and the event study setup, we can formally define the building block of even study analysis, as the average of unit-level treatment effects at relative period l for units belonging to cohort $E_i = e$ as:

$$CATT_{e,l} = E\left[Y_{i,e+l} - Y_{i,e+l}^{\infty} | E_i = e\right] \tag{1}$$

In plain English, $CATT_{e,l}$ represents the ATT of units first treated at time e, l periods since treatment. Usually, l is referred to as the relative period $l \in [-e; T - e]$. Under the two assumptions above, Sun and Abraham (2021) propose to estimate a weighted average of $CATT_{e,l}$ with convex weights given by the share of cohorts that have been treated at least for l relative periods. In addition, such a weighted average is normalized by the number of relative periods included in the estimate. Formally the estimator is given by

$$v_g = \frac{1}{|g|} \sum_{l \in g} \sum_{e} CATT_{e,l} Pr\left\{E_i = e | E_i \in [-l, T - l]\right\} \tag{2}$$

where g is the set of relative periods included in the estimate.

From a practical point of view, the estimator by Sun and Abraham (2021) can be implemented in three steps which aim at estimating the sample analogue of each component of (2) and to feed them back into the population equation. Hence, one must first estimate $CATT_{e,l}$

using a TWFE augmented by the interactions between relative period indicators and cohort indicators excluding at least one cohort indicator from the set C:

$$Y_{it} =_i +\lambda_t + \sum_{l \notin C} \sum_{l \neq -1} \delta_{e,l} \left(1 \{E_i = e\} D^l_{i.t}\right) + \epsilon_{i,t} \tag{3}$$

in (3) the excluded period is −1 and the estimated CATT's can be interpreted as the effects of the policy with respect to the previous relative time, e.g. $CATT_{e,l}$ is the average effect at relative time l with respect to l −1, for cohort e. The cumulative effect of the policy at time m can be obtained as $\sum_{e,l=0}^{m} CATT_{e,l}$. As a comparison group if there are never treated we may set C = ∞, otherwise, we may set C = {max {E_i}} i.e. the latest treated cohort and restrict the estimation sample to t = 0, . . . , max {E_i} − 1.

$$\hat{v_g} = \frac{1}{|g|} \sum_{l \in g} \sum_{e} \hat{\delta}_{e,l} \hat{Pr} \{E_i = e | E_i \in [-l, T-l]\} \tag{4}$$

This estimator is referred to as an "interaction-weighted" estimator, IW, and is a consistent estimator of a weighted average of CATT's. The absorbing treatment state is actually the case of our empirical application because the treated municipalities are treated up to the end of the sample; however, the effect of being ever treated may still be interesting in many contexts, such as the effect of minimum wage on employment (Callaway and Sant'Anna, 2021) or the effect of hospitalization on future labour earnings (Sun and Abraham, 2021). Moving from the theory to the policy under estimation, our outcome variables are the percentage of over 65 in the population and the number of plants per municipality. In addition, in order to corroborate the results, we test the baseline model controlling for the following sets of covariates:

1. a first set is intended to capture the economic conditions of inhabitants, *eco*, containing the average income of inhabitants. This covariate is included to control for municipal heterogeneity in the economic conditions that may contribute to the creation of both the demand and the supply side (Gallo and Pagliacci, 2019). Data for *eco* are taken from the fiscal declaration data set available at the municipal level from the Italian Ministry of Economics and Finance[3];
2. demographic, *demo*, consisting in the first lag of (log)population. We include population to control for factors connected to the agglomeration, which is widely recognized as a key explanatory factor for the concentration of industrial activity by location theorists (Carlino, 1980). Data for *demo* are taken from Istat[4];
3. institutional characteristics of the local government, *gov*, such as mayor's age, gender, and education (dummy for degree or more), the average age of municipal council members, and a dummy if the municipality is ruled by a civic list. We include *gov* since it has been largely shown that some institutional factors have an impact on the local creation of plants. Regarding the age of the mayor and councilors, Alesina et al. (2019) noted the tendency

3 https://www1.finanze.gov.it/finanze/pagina_dichiarazioni/public/dichiarazioni.php

4 Istat makes available the most recent official data on the population in Italian municipalities derived from surveys conducted at the Offices of Registry and Civil Status of Municipalities and from the Census of Population. Link: https://demo.istat.it/

of younger politicians to behave strategically, increasing spending and obtaining more transfers from higher levels of government, and these factors can somehow affect our outcome variable. We also control for gender, as women in politics are usually more concerned about peoples' well-being, show higher cooperation and team working skills, and are less likely to engage in corruption, compared to their male counterparts (Chattopadhyay and Duflo, 2004; Hernández-Nicolás et al., 2018). Consequently, a higher female political participation may affect the policies implemented and the allocation of resources across different programmes (Funk and Gathmann, 2015). Education is used to control the local politicians' formal human capital which is likely to affect government quality (Geys, 2017). Data for *gov* are taken from the registry of local and regional administrators of the Ministry of the Interior[5].

4. geographical variables, *geo*, altitude, and surface in squared kilometers are included to account for the differentiation in logistic costs that can be generated by the heterogeneity in these features (Hesse and Rodrigue, 2004). Data for geo are taken from Istat[6].

5. Results

This section presents the estimated effect of SNAI on long and short-run outcomes, i.e. population structure and the number of plants, respectively. Section 5.1 reports the results for the over 65 population estimates showing that there is no effect on this outcome. Concerning the number of plans, the focus is on three aspects: the effects on the treated municipalities (Section 5.2), robustness checks (Section 5.3), and possible spillover effects on the neighbourhood (Section 5.4).

5.1 The effects on the population structure in the treated municipalities

As a first step, we assess whether the policy has generated any effect on the main outcome of interest, i.e., aging and depopulation. To this purpose, we use the percentage of population over 65 as outcome, since it has been shown to be the main factor explaining the survival or extinction of a community by demographic studies (Golini et al., 2000). In this regard, Table 3 reports the estimates from the TWFE and IW estimator.

Table 3 reports one marginally statistically significant result at 10% in the case of IW estimator without covariates (Column 1), the result is not robust to covariates inclusion. In general, **the table shows no statistically significant effects on the outcome variable** and the two estimators agree on the result. **This is quite an expected result considering the short time span of our analysis** which does not allow to capture the longer-run effects of the policy. The estimates have also been repeated by

5 This registry consists of the information on elected officials in municipalities, provinces, metropolitan cities and regions concerning biographical data, the list or group to which they belong or are connected, educational qualification and the profession exercised. Link: https://dait.interno.gov.it/elezioni/ anagrafe-amministratori

6 https://www.istat.it/it/archivio/156224

including covariates and the same result applies (columns 2-4). Therefore, we move on to analyse possible short-run effects repeating the analysis on the number of plants.

5.2 The effects on plants in the treated municipalities

Table 4 reports the results from the estimation of the TWFE and the IW estimator showing substantial differences between the two. Looking at TWFE pre-treatment coefficients, i.e., from -5 up to -2, if we were confident that the TWFE provided valid inferences, we would conclude that the anticipatory effects condition was violated. Fortunately, recent developments have proven that test to be invalid. The estimates from the IW estimator tell another story.

Table 3: Event study. Estimates of the effect of SNAI on over 65 percentage

relative time	TWFE	IW (1)	IW (2)	IW (3)	IW (4)
-5	0.0360	0.232	0.150	0.110	0.110
	(0.107)	(0.226)	(0.209)	(0.212)	(0.212)
-4	-0.0340	0.036	-0.011	-0.001	-0.001
	(0.116)	(0.163)	(0.152)	(0.154)	(0.154)
-3	-0.0569	0.013	-0.03	-0.051	-0.051
	(0.112)	(0.127)	(0.119)	(0.121)	(0.121)
-2	0.0313	0.082	0.045	0.036	0.036
	(0.0854)	(0.098)	(0.091)	(0.092)	(0.092)
0	0.0218	-0.071	-0.065	-0.067	-0.067
	(0.0645)	(0.107)	(0.107)	(0.109)	(0.109)
1	-0.195	-0.419*	-0.366	-0.368	-0.368
	(0.131)	(0.232)	(0.224)	(0.223)	(0.223)
N	2,087	1,788	1,746	1,714	1,714
eco			✓	✓	✓
gov				✓	✓
geo					✓

*Note: Robust Std. Err. clustered at the municipal level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. TWFE stands for Two Way Fixed Effects; IW - Columns (1)-(4) - stands for the interaction-weighted estimator. The bottom of the table indicates the sets of covariates included in each column. eco=average income of inhabitants; gov characteristics of the municipal council; geo geographic characteristics.*

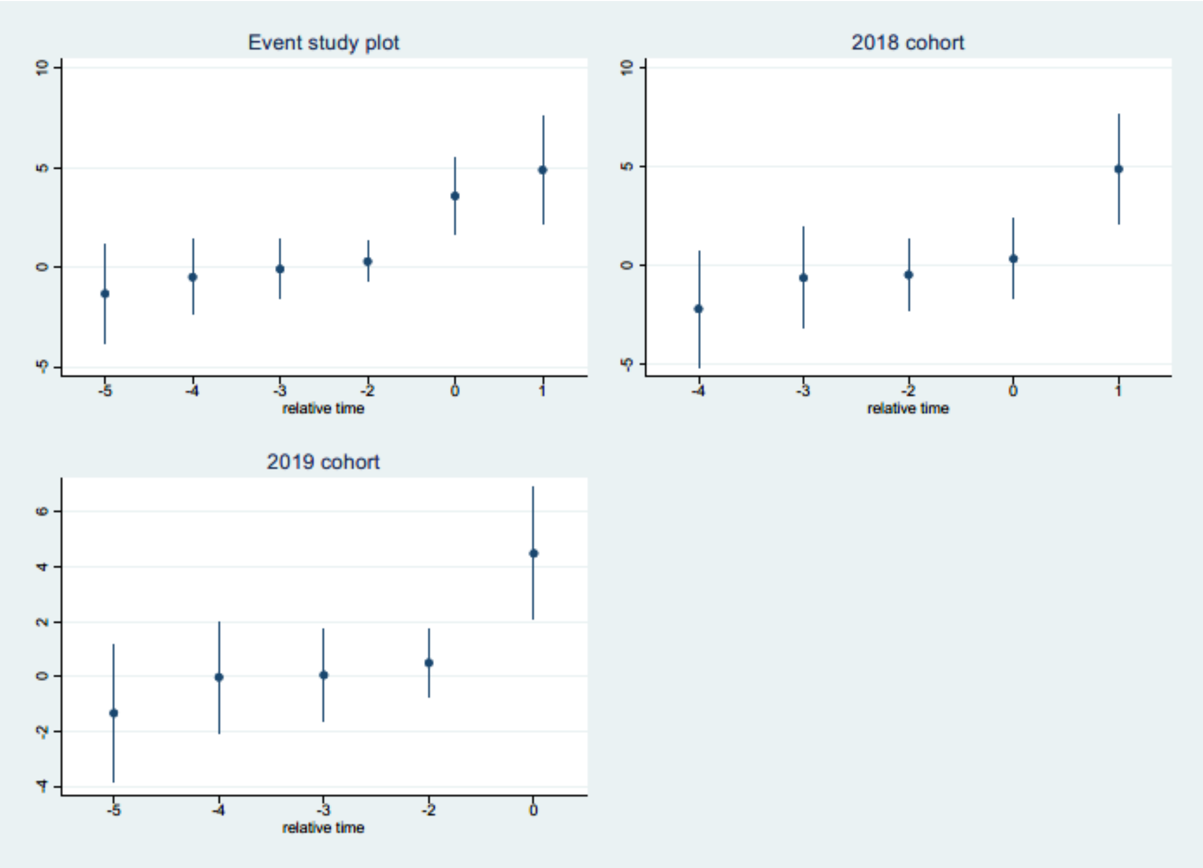
The IW estimator signals **a positive effect of the policy since its inception**. The coefficients associated with the treatment are positive and significant **both in the year the policy was introduced and in the following year**. Thus, there is a positive and persistent increase in plants (Pval=0.002 of the joint hypothesis of both coefficients statistically significant), on average in treated municipalities. This result can be decomposed by looking at the CATT's. Interestingly, significant results $\hat{\delta}_{2018,l}$ start accruing to municipalities since the second calendar time. Indeed, the fourth column of the table, reports the CATT for the 2018 cohort and no statistical effects at relative time 0 corresponding to calendar time 2018, the first year the policy was ever introduced. After one year, l = 1, the policy produced positive and significant effects for this cohort corresponding to the calendar year 2019. Symmetrically, the cohort first treated in 2019 exhibits positive and significant effects at l = 0, i.e. in 2019 and very close in magnitude to those of cohort 2018 at l = 1. This joint reading of the effects of the policy unfolded over time is made immediate by a visual representation shown in Figure 2. The upper left picture reports the estimates from the IW column, along with 95% confidence interval, while the remaining two pictures report the ATT's in columns (4) and (5) of Table 4, respectively in the upper right and lower left panel.

Table 4: Event study. Estimates of the effect of SNAI on the number of plants

relative time	TWFE	IW	$CATT_{e,l}$ $\hat{\delta}_{2018,l}$	$\hat{\delta}_{2019,l}$
-5	7.246***	-1.325	-	-1.325
	(1.026)	(1.507)		(1.507)
-4	5.362***	-0.493	-2.195	-0.019
	(0.809)	(1.142)	(1.786)	(1.225)
-3	3.530***	-0.091	-0.626	0.058
	(0.732)	(0.911)	(1.552)	(1.003)
-2	2.136***	0.29	-0.471	0.502
	(0.535)	(0.611)	(1.076)	(0.74)
0	1.418	3.576***	0.339	4.477***
	(0.947)	(1.174)	(1.232)	(1.448)
1	1.604	4.884***	4.884***	-
	(1.604)	(1.661)	(1.661)	
N	1,788	1,788	1,788	1,788

*Note: Robust Std. Err. clustered at the municipal level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. TWFE stands for Two Way Fixed Effects; IW stands for interaction-weighted estimator; the column $\hat{\delta}_{2018,l}$ reports the treatment effect on 2018 cohort at different relative times. Similarly, for $\hat{\delta}_{201p,l}$. The values in column IW are a weighted mean of $\hat{\delta}_{e,l}$ for e = 2018, 2019*

Figure 2: Event study plot. The effect of SNAI on the number of plants



Note: the upper left picture reports the event study plot of the policy. The upper right figure reports the CATT's for 2018 cohort, whereas the lower left panel reports the CATT's for the 2019 cohort. Vertical bars represent the 95% confidence intervals

These results are in line with the previous literature providing evidence of a positive effect of the cohesion policies (Becker et al., 2018; Fattorini et al., 2020; Giua, 2017) and in contrast with the results by Ciani and De Blasio (2015), who noted a limited impact of the EU structural funds (programming period 2007–2013) on local development. Possible pieces of explanation for this difference can be found in the different time span considered, in the outcomes measured, and in the specific programme analysed (Albanese et al., 2021; Ciani and De Blasio, 2015).

5.3 Robustness checks of municipal effects

In order to corroborate the results presented in the previous section, we report in Table 5 a number of robustness checks in which we re-estimate the IW model by adding covariates. The results displayed in each column of Table 4 are virtually identical and confirm the main result, that is, **the policy shows a positive and significant impact on the number of plants at the municipal level**.

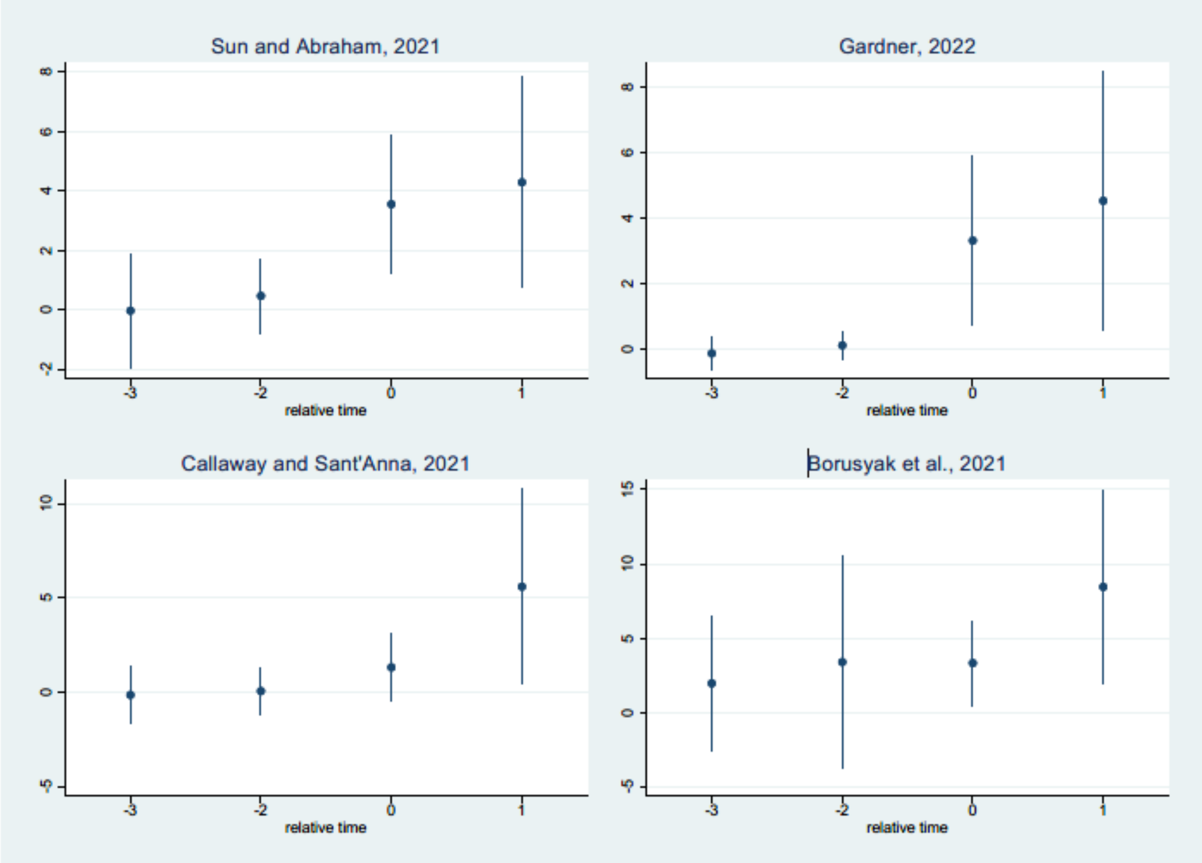
Table 5: Robustness checks

relative time	(1)	(2)	(3)	(4)
-5	-1.173	-	-	-
	(1.547)			
-4	-0.266	0.165	0.516	0.516
	(1.171)	(1.249)	(1.34)	(1.34)
-3	-0.024	-0.07	-0.033	-0.033
	(0.934)	(0.942)	(0.976)	(0.976)
-2	0.394	0.374	0.463	0.463
	(0.624)	(0.624)	(0.639)	(0.639)
0	3.838***	3.843***	3.547***	3.547***
	(1.197)	(1.19)	(1.183)	(1.183)
1	4.66***	4.659***	4.281**	4.281**
	(1.721)	(1.802)	(1.803)	(1.803)
N	1,746	1,456	1,428	1,428
eco	✓	✓	✓	✓
demo		✓	✓	✓
gov			✓	✓
geo				✓

*Note: Robust Std. Err. clustered at municipal level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. The bottom of the table indicates the sets of covariates included in each column. eco=average income of inhabitants; demo (log) population; gov characteristics of the municipal council; geo geographic characteristics*

Moving from this result, another check consists of using alternative estimators that recently appeared in the literature of event study analysis. In particular, we have re-estimated column (4) of Table 5 using Gardner (2022), Callaway and Sant'Anna (2021) and Borusyak et al. (2021). Evidence is provided in Figure 3 which clearly shows that the same result is attainable independently of the estimator. In particular, the policy starts producing effects at relative time 0 and no anticipatory effects are detected. Finally, the same exercise has been repeated without covariates in order to match the column "IW" of Table 4 and the result still applies, see Figure A1 in the Appendix.

Figure 3: Event study plot. Four alternative estimators



Note: the picture reports the event study plots obtained by applying four different estimators. Sun and Abraham (2021) upper left, Gardner (2022) upper right, Callaway and Sant'Anna (2021) lower left and Borusyak et al. (2021) lower right. Vertical bars represent the 95% confidence intervals

5.4 The effects of the policy in the neighbourhood

The results obtained so far show that, in the treated municipalities, the number of plants increased significantly over the first two years. Moving from this piece of information, it is relevant to ask **whether there have been spillover effects** on neighbouring municipalities. In principle, positive spillovers may have taken place as more active and thriving municipalities may somehow engender a sort of positive dynamics on the firms located in the neighbourhood. By the same token, one can argue that firms may have relocated their productive activity to neighbouring treated municipalities, so as to benefit from treatment. Under this last scenario, the policy would generate negative spillovers. Both views are legitimate and the final verdict lies only on empirical grounds.

To answer this relevant question, **we have followed the approach by Kline and Moretti (2014)** and the ensuing literature. Notably, the authors evaluating the effects of the Tennessee Valley Authority, a place-based policy consisting of the construction of dams and transportation canals, on a variety of outcomes such as employment in agriculture and manufacturing, raise the problem of potential spillovers. They notice that keeping in the non-treated sample counties on the other side of the Authority border, the effects were likely underestimated. As a solution, the authors remove counties that share borders with the Authority. However, a potential limit to this analysis is that we do not know *a priori* to what extent spillovers are in order, if any. As a solution, we will remove units located in rings of increasing radius. To make the strategy as clear as possible let us suppose a situation in which spillovers are present only up to d kilometers from the treated units and we run the estimate removing units located once up to d km and once up to f km, with $f > d$. The two estimates are expected to produce similar results because in both cases the spillovers have been removed, not being present in the $f - d$ ring. As a corollary, and also as a check to that claim, by repeating the estimates without the units in the $f - d$ ring we expect the estimates to be close to the one obtained without discarding any unit from the sample because we are still keeping units affected by spillovers among the non-treated units, i.e, in the radius d. Table 6 implements this strategy. For ease of reference and to see how and if the estimates change, Column (4) of Table 4 is reported in Column (1) of Table 6. It represents the case in which no potential spillovers are detected. Column (2) of Table 6 reports the estimates obtained by discarding neighbours located within 10km from the treated units. Similarly, in Column (3), where the radius is set at 20km. Finally, Column (4) removes from the sample the units located in the ring 10 − 20km from the treated municipalities. In the notation used above $d = 10$, $f = 20$.

Table 6: Event study. Estimates of the effect of SNAI on the number of plants removing neighbours

relative time	(1) 0km	(2) 10km	(3) 20km	(4) 10-20km
-4	0.516	0.432	0.515	0.595
	(1.34)	(1.38)	(1.38)	(1.34)
-3	-0.033	-0.017	0.05	0.033
	(0.976)	(0.999)	(1.002)	(0.979)
-2	0.463	0.425	0.495	0.528
	(0.639)	(0.661)	(0.662)	(0.64)
0	3.547***	7.991***	8.017***	3.415***
	(1.183)	(1.63)	(1.719)	(1.218)
1	4.281***	9.998***	9.959***	4.034**
	(1.803)	(2.273)	(2.387)	(1.842)
N	1,428	1,378	1,337	1,423
eco	✓	✓	✓	✓
demo	✓	✓	✓	✓
gov	✓	✓	✓	✓
geo	✓	✓	✓	✓

*Note: Robust Std. Err. clustered at the municipal level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1.*
The bottom of the table indicates the sets of covariates included in each column. eco=average income of inhabitants; demo (log) population; gov characteristics of the municipal council; geo geographic characteristics.
The estimates have been carried out by using the interacted-weighted (IW) estimators. Column (1) coincides with (4) of Table 4, the others have been obtained by removing from the control sample the units located within the radius of 10km, 20km, 10-20km from treated units in column (2), (3) and (4), respectively

Interestingly, in Column (2) the estimated coefficients at relative time l = 1 and l = 0 are statistically significant at 1% and higher than the corresponding values in Column (1), where no neighbours were dropped, whereas no pre-trends are observed. As far as Column (3) is concerned, we do not observe appreciable differences with respect to Column (2). This joint evidence supports the positive spillover hypothesis for units located within 10km from the beneficiaries. In other words, **the policy seems to have generated positive effects also on the number of plants of neighbouring municipalities located within a radius of 10km.** Eventually, Column (4) confirms the result just claimed that the estimated coefficients are close to those in Column (1), where no units were dropped. These results are in line with Kline and Moretti (2014).

Having not found any effect on the treated units in terms of the over 65 percentage, it is implausible to expect spillovers on neighbours, however, for sake of completeness the exercise has been repeated also on this outcome finding no effects. Evidence is given in Table A1 in the Appendix.

6. Conclusions

This paper analyses the role of the SNAI in tackling depopulation and in increasing business activities in treated municipalities. To this aim, **we have exploited a detailed panel dataset containing information about the Italian municipalities over the years 2014-2020, and the treated municipalities located in the pilot areas**. From a methodological perspective, we have taken advantage of one of the most recent developments in the econometrics of policy evaluation: the IW estimator developed by Sun and Abraham (2021), which generalises the DID estimator accounting for staggered entry into treatment. In a non-synchronized treatment setting, the IW estimator uses the last treated as a comparison group, greatly decreasing unobservable heterogeneity, as the comparison group is treated itself. In addition, the causal effects are aggregated by using sensible weights to obtain an event study analysis.

The results show that the policy did not significantly affect the population structure, but it has generated extra plants in the treated municipalities over the first two years. An additional issue tackled in the empirical application is the question whether the policy has generated spillovers over the neighbouring municipalities, thus reinforcing (in case of positive spillovers) or crowding out (in case of negative spillovers) the result obtained. To answer this question an empirical solution has been implemented by following the approach of Kline and Moretti (2014) who suggest discarding potentially affected units, by concentric rings. Results show that positive spillover effects were occurring, hence corroborating the positive effect of the policy. A caveat in the interpretation of the results is in order. Since the results pertain to a counterfactual situation, **the extra plants must not necessarily be construed in terms of new business sites that start operating. Indeed, they could be plants that would have shut down without treatment.**

The current institutional framework on which the policy is implemented creates a mediation mechanism within which **the heterogeneity in the quality of the local institution plays a crucial role**. **The complexity of the decision-making process combined with the reduced administrative capacity of some municipalities could compromise the effectiveness of the SNAI.** Nevertheless, this paper shows that **the policy is promising** in the treated municipalities **but**, given its primary goal to fight depopulation, **it would be desirable to repeat a similar analysis in the future once the policy is at its full implementation and after a certain time lapse** in order to test for possible effects on other crucial outcomes such as population, employees, and income. We leave this task to further research should the data become available. Of course, the number of plants can only partially reveal the trend of economic activity at the local level, but the limitations of empirical exercises should be considered in light of the many benefits of having timely empirical evidence on the effectiveness of a policy.

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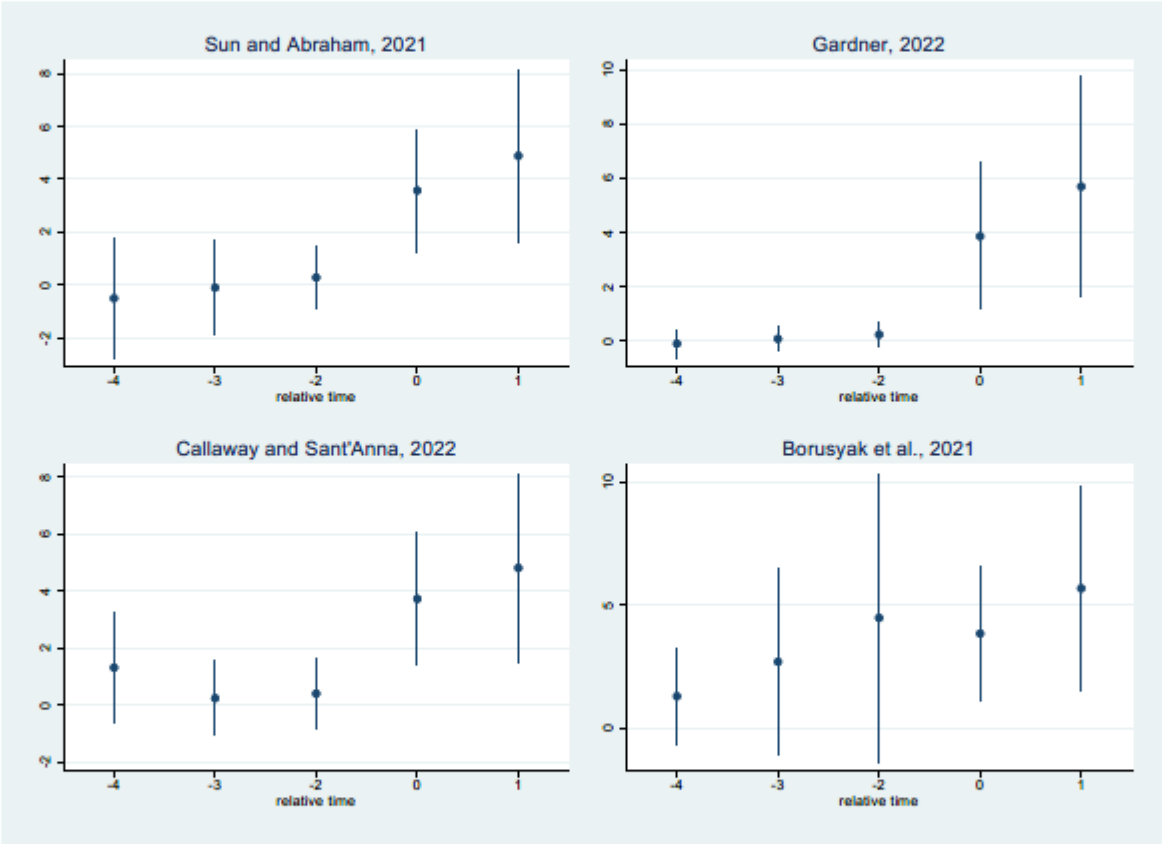
Appendix

Table A1: Event study. Estimates of the effect of SNAI on over 65 percentage

relative time	(1) 0km	(2) 10km	(3) 20km	(4) 10-20km
-5	0.11	0.112	0.112	0.11
	(0.212)	(0.209)	(0.209)	(0.213)
-4	-0.001	0.001	0.001	0
	(0.154)	(0.15)	(0.151)	(0.154)
-3	-0.051	-0.045	-0.045	-0.051
	(0.121)	(0.119)	(0.119)	(0.122)
-2	0.036	0.042	0.043	0.036
	(0.092)	(0.09)	(0.09)	(0.092)
0	-0.067	0.092	0.1	-0.068
	(0.109)	(0.118)	(0.122)	(0.111)
1	-0.368	-0.181	-0.171	-0.37
	(0.223)	(0.227)	(0.231)	(0.225)
N	1,714	1,664	1,659	1,709
eco	✓	✓	✓	✓
gov	✓	✓	✓	✓
geo	✓	✓	✓	✓

*Note: Robust Std. Err. clustered at the municipal level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. The bottom of the table indicates the sets of covariates included in each column. eco=average income of inhabitants; gov characteristics of the municipal council; geo geographic characteristics. The estimates have been carried out by using the interacted-weighted (IW) estimators. Column (1) coincides with (4) of Table 3, the others have been obtained by removing from the control sample the units located within the radius of 10km, 20km, 10-20km from treated units in column (2), (3) and (4), respectively*

Figure A1: Event study plot. Four alternative estimators - baseline model with no covari-ates



Note: the picture reports the event study plots obtained by applying four different estimators. Sun and Abraham (2021) upper left, Gardner (2022) upper right, Callaway and Sant'Anna (2021) lower left and Borusyak et al. (2021) lower right. The estimates have been obtained without covariates inclusion. Vertical bars represent 95% confidence intervals

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