INPS
Istituto Nazionale Previdenza Sociale

agosto 2024 – numero 81

WorkINPS *Papers*

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Anna Cecilia Rosso

ISSN 2532 -8565

In copertina: uno storico "Punto cliente" a Tuscania
INPS, Direzione generale, Archivio storico

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Tommaso Nannicini

Industrial Policy and the Decision to Emigrate

Anna Cecilia Rosso

(Università degli Studi dell'Insubria, LdA and CEP)

Industrial Policy and the Decision to Emigrate*

Anna Cecilia Rosso†

July 11, 2024

Abstract

I investigate how a policy framework aimed at supporting innovative startups affected the decision of young Italian citizens to emigrate. Leveraging a matched difference-in-differences design, I find that municipalities that had at least one innovative startup registered in the framework show a lower share of emigrants to other European countries with respect to the control municipalities. The decrease in the share of emigrants is likely driven by an expansion in the employment of firms and sectors more likely to register in the framework in the treated municipalities, which is of the same magnitude of the decrease in the number of emigrants. The firm-level results confirm that treated firms are larger than observationally equivalent control firms.

Key words: International migration; Selection; Innovation; Industrial policy
JEL codes: F22; J61; O15; O38

*I gratefully acknowledge the financial support from the EIEF 2019 grant. I thank Massimo Anelli, Gaetano Bassi, Matthieu Bouvard, Maria De Paola, Edoardo Di Porto, Tommaso Frattini, Giuseppe Ippedico, Camille Landais, Francesca Lotti, Max Nathan, Paolo Natticchioni, Michele Pellizzari, Anna Raute, Michel Serafinelli, Daniela Sonedda, and Jing Sophia Xue, Anna Valero. I am also grateful to the participants of the VisintInps scholar seminar, the 37th AIEL conference, the 19th North American Urban Economic Association conference, the Brucchi Luchino Workshop, the 2022 Annual VisintInps Conference, the University of Parma , the 64th SIE and the 13th European UEA Conference for useful suggestions. I am indebted to Francesco Manaresi for giving me access to the Innovative Startups Registry data and to Andrea Alemanno and Lucio Formigono from the team at IPSOS for giving me access to their microdata. I also thank Camilla Andretta and Tommaso Fonti for their excellent research assistance and all members of the administrative and technical staff of the VisitINPS program who provided invaluable support and help. I also thank Prefetto Lucia Volpe, Vittoria Sabatini and Anna Maria Grispo (Ministry of the Interior) for their help with data access. The findings and conclusions expressed are solely mine and do not represent the views of the INPS. The realization of the present article was possible thanks to the sponsorship and the donations in favour of the ?VisitINPS Scholars? program.

†Università degli Studi dell'Insubria, LdA and CEP . Email: annacecilia.rosso@uninsubria.it

1 Introduction

Low productivity, low innovation and high emigration rates have been topics of discussion for the Italian economy in the last 20 years. The low productivity growth in Southern Europe was partly due to inefficient management practices, which limited IT adoption and reduced the demand for IT-related skills and, thus, job opportunities for young and high-skilled individuals, inducing them to move abroad (Schivardi and Schmitz, 2020). Their high emigration rate increase the risk of missing out on either potential innovators with the consequence of slowing the creation of innovative firms (Anelli et al., 2023) or missing out on many young, highly qualified workers who can contribute with their skills to the growth of new or existing firms [1] Low innovation rates can also have a major impact on future economic growth.[2] To support sluggish economies and foster productivity growth after the Great Recession, industrial policy and especially business support policies have been brought back into fashion, as they have been viewed as an opportunity for governments to address potential market frictions by providing high-potential firms at early stages with the right environment in which to experiment with various strategies and easily adapt their business to productivity shocks (Criscuolo et al., 2019). In Italy, young, innovative start-ups are also strongly affected by market frictions, such as red-tape costs, the length of judicial trials, and corruption, which often result in lower growth, high failure rates and low demand for innovative goods and services, in general.

In this paper, I study whether a policy aimed at supporting innovative firms in their early stages, by decreasing their operational cost, can be effective at preventing the emigration of high-skilled individuals— those individuals who can favor the spread of ideas and firm creation—eventually improving local employment opportunities. I investigate the role that the policy reform had on the probability of emigrating from an Italian local labor market through the creation of opportunities for young individuals in local economies. Specifically, I study a 2012 policy—the Start-up Act (SUA)—created to assist innovative entrepreneurs by providing

[1] Young managers, professionals and entrepreneurs are crucial drivers of economic growth by introducing new practices and technologies in production and eventually also making it more innovative and productive through the reallocation of inputs (Calvino, Criscuolo, and Menon, 2016; Liang, Wang, and Lazear, 2018; Acemoglu, Akcigit, and Celik, 2017; Bernstein et al., 2022).

[2] Recent research on the probability of being an inventor in the USA has found that individuals are more likely to become one if they are exposed to more innovation but also if they have grown up in areas densely populated by other inventors, with the main channel being role models and networking (Bell et al., 2019).

a bundle of complementary instruments available until the fifth year of operation (i.e., red-tape cutting, tax incentives to facilitate access to external finance, tailor-made labor laws, etc.) with the aim of creating a better ecosystem for them. The policy's features allow me to pin down the economic mechanism behind the decision and selection to emigrate.

I exploit rich administrative datasets on emigrants, workers, and firms using a municipality-level matching difference-in-differences design to estimate the effect of having at least one young firm registered in the SUA framework (hereafter 'innovative startup') on the municipality-level share of emigrants and employment in young firms for the years 2007 to 2017. A municipality is treated if I observe at least one innovative startup between 2013 and 2015. Control units are municipalities that have never experienced an innovative startup; therefore, they have never been locally exposed to the policy. To interpret the estimated effect as casual, I assume pre-treatment common trends in outcomes between control and treated units, meaning that in the absence of an innovative startup, the share of emigrants would have moved similarly in treated and control municipalities. By matching treated municipalities on baseline covariates, I am dealing with the challenge of selecting an appropriate control group with comparable (socio-economic) conditions in the years before the treatment and reducing the bias associated with these covariates, also potentially balancing on unobservables.

I find that having an innovative startup decreases the municipality-level share of emigrants to all destinations (-0.014 percentage points (p.p.), a 19% decrease with respect to the baseline mean in the treated municipalities); it decreases the share of emigrants to other European countries (-0.009 p.p., a 22.5% decrease) and to extra-EU countries (-0.005, a 15% decrease). When focusing on the cohort of municipalities that were treated by firms that already existed at the time the Act was passed, the effects are larger (-0.025 p.p. a 39% decrease in the share of emigrants to all countries) and are mainly driven by emigrants to other European countries (-0.014 p.p., a 46% decrease). Practically, for an average municipality in our treated sample with 14,008 inhabitants and a share of emigrants to other European countries of 0.03%, these results mean that 1.26 fewer individuals emigrated to other European countries. I argue that any contemporaneous shocks that could drive both outcomes and the creation of innovative startups should be sudden and large enough to invalidate the parallel trend assumption, which is unlikely in this context. When performing the matching algorithm, I use the incorporation year of the oldest innovative startup that registered in the framework to control for any endogeneity caused by the choice in the timing of the application as the firm could have made some adjustments to meet the selection criteria or waited for the right local conditions before applying.[3] Moreover, the effect is estimated within municipalities and controlling for local economic shocks at the provincial level. This latter control is important given that the registration to the framework was made by the board of the provincial Chamber of Commerce, as these shocks could drive both the Chamber's decision to accept an innovative startup and the decision to emigrate. To dismiss any further concerns linked to possible issues of reverse causality, I show that results are there also with an alternative approach to the matching strategy based on instrumenting municipality-level participation in the framework. I address any possible heterogeneity in the treatment effects across different cohorts that may still arise despite the matching algorithm using alternative estimators, specifically the one by Sun and Abraham (2021). Results are robust to some final checks on the existence of unobserved confounders affected by the policy such as the municipality entrepreneurial propensity, which may mechanically drive framework participation. Finally, my results survive alternative municipality-level matching algorithms and additional controls for the sociopolitical changes created by the recently appointed new technical government.

Next, I study the mechanism. In a simple setting in which individuals compare the gains of migrating to the gains of staying in the home country, net of the cost of moving, individuals do not migrate if the opportunity cost of migrating increases. By creating new job opportunities for potential economic migrants in their current location, the opportunity cost increases, and thus, the probability of emigrating decreases. By the same token, if the Act facilitated the hiring process of workers in innovative startups, I expect an expansion in employment in this type of firms/sectors. The presence of an innovative startup in the municipality could have an equilibrium effect in the local labour markets of treated municipalities by increasing the

[3]As explained in Section 2, there were certain criteria firms had to comply with to be accepted in the policy. To some extent, the procedure and the fact that the decision to accept the firm into the framework were deliberated by the Chamber of Commerce, a local public entity that supports firms in their administrative activities within the same territory of competences of provinces, created some initial randomness in the provision of the framework. However, the decision to put the application forward was made by the firm and therefore depended on its judgment on the right conditions.

demand for workers in innovative startups. However, this increase was not accompanied by an increase in wages, at least in the short run, because of the documented over-supply IT-savvy workers (Schivardi and Schmitz, 2020) who are more likely to work for innovative startups.[4] I study the impact of the policy at the municipality and firm level, with two alternative research design where I exploit the policy features (i.e. age eligibility). I estimate a municipality level employment effect using the matching difference-in-differences designed but with a specific focus on the cohort of municipalities that were treated by innovate firms incorporated before the Act was passed. There are two related reasons for this focus. First, it is the cohort where I find the largest effect on the share of emigrants. Second, it is also the cohort where I can cleanly identify the effect of the SUA on employment and avoid confouding it with any possible direct effect of firm creation. I then estimate a firm level effect, by exploiting the variation in eligibility across firms as an instrument for the probability to be treated: age and propensity to innovate. The two approaches provide some complementary evidence of the economic mechanisms. The firm level analysis narrows down the impact of being an innovative startup, providing compelling evidence of the casual impact of the SUA on startups' employment.

At the local level, I find evidence consistent with the fact that municipalities that were treated by an innovative startup experienced an expansion in the number of workers in firms younger than age 5, in those sectors that were more likely to have firms that participated in the framework (+3 p.p., an increase of 0.8% with respect to the baseline mean number of workers in treated municipalities). This result is comparable to the decrease in the number of emigrants experienced by an average municipality and corresponds to 1.10 extra headcounts (0.41 as full-time equivalent employees) in firms younger than age 5 in the most treated sectors. At the firm level, the results on the expansion in employment of treated firms are confirmed: treated firms are larger than observationally equivalent noneligible firms, and they have a larger number of blue collars but also of apprentices hired in the local market (i.e., same municipality where the firm is headquartered). Given that the apprentices' contracts are only used for younger workers, who are also the demographic group with a higher probability of emigrating, SUA participation could have contributed to the expansion of these contracts, ultimately contributing to the

[4]As stated by ISTAT (2018), more than 45% of employees in treated firms were from the technological-engineering area. Over-supply implies that there was no market tightness, even in the absence of firm entry and exit dynamics (Beaudry, Green, and Sand, 2018)

decrease in the probability of emigrating for this group of the population.

Recent studies have focused mainly on the firm-level effect of the SUA, yet little is known about the possible effects that are generated in the local economy, especially on retaining workers. Whether this intervention was successful in preventing the migration of young and highly skilled individuals is ultimately an empirical question. Manaresi, Menon, and Santoleri (2022) showed that firms that applied to the program had greater numbers of employees, assets, equity and intangibles. Given the problematic existence of financial constraints for this type of firm (young and innovative) that is also documented in the literature (Hall and Lerner, 2010; Kerr and Nanda, 2015), frameworks such as this one provide support for this dimension, and the Act introduced also two instruments to ease access to external finance.

I contribute to two strands of the literature. I first focus on emigrant selection; specifically, I focus on the role that public policies could play in preventing workers from emigrating to other countries. I do this by looking at a country (Italy) that experienced large outflows of young and high-skilled individuals. The literature has shown that training programs to boost self-employment can have small but significant effects on the probability of emigrating by decreasing it for individuals who receive them. The estimated effects found are relatively small because, in the majority of cases, these policies are not specifically targeting the group of the population with a higher probability of emigrating (Giambra and McKenzie, 2021).[5] The policy under study directly affected the group of the population with the highest probability of emigrating, making it an interesting case study. Therefore, I contribute to the literature by focusing on a developed country a very different context from the ones previously studied, by using a unique large dataset that merges together different administrative sources covering the universe of individuals living abroad and the universe of employees subject to social security in Italy, allowing me to have precise estimates of the effects of the policy. This is an improvement compared to previous studies that answered similar questions (McKenzie, 2017; Giambra and McKenzie, 2021) that mainly rely on survey data. Second, I also contribute to the literature

[5]These programs are considered more effective than active labor market programs, especially in developing countries (McKenzie, 2017). For a review on the effectiveness of Italian active labor market policies, see Albanese, Cappellari, and Leonardi (2021). Other papers have found, for example, that unrestricted cash transfers to poorer individuals increase migration by overcoming credit constraints and providing insurance against risk Adhikari and Gentilini (2018). A more recent study in India revealed that a rural public works program that increased public employment also decreased seasonal migration from rural districts (Imbert and Papp, 2019).

on how entrepreneurs, innovative firms and clusters can positively affect local labor demand Moretti (2021). Therefore, I focus on how innovative clusters affect an alternative dimension—migration—which has not yet been studied.

The paper is organized as follows. I first describe the Startup Act and the main features of emigration from Italy (Section 2). I then describe the data (Section 3), the research design (Section 4) and the main results on the probability of emigrating (Section 5). In the last part of the paper, I study the possible mechanism of the labor market (Section 6).

2 Institutional background

The Startup Act

To address the lack of business dynamism of the Italian economy, the Monti government introduced the Startup Act, a new policy framework aimed at reducing any frictions in the creation and survival of innovative firms with high growth potential, as these types of firms are more likely to contribute to aggregate economic dynamism and to job creation (Criscuolo, Gal, and Menon, 2014; Haltiwanger, Jarmin, and Miranda, 2013).[6] The aim was to create a better ecosystem by supporting firms in the post-incorporation period (first 5 years of life) with numerous complementary instruments that firms admitted to the framework could use. In addition to firms' support, the policy also explicitly aimed at retaining talented workers. The Ministry of Business Development was in charge of its implementation.[7]

Eligibility criteria. To qualify to participate in the framework, firms had to be limited companies (hereafter LCs) and not publicly listed; they had to be operational for less than 5 years, have an annual turnover of less than 5 million euros and not be the result of branch splits or mergers. In addition, being defined as "innovative-oriented" meant meeting at least

[6] The Monti government took up office on 16th November 2011 until 28th April 2013. It was an expert cabinet, mainly formed by independent technocrats. During the 18 months of government, a 30 billion austerity plan was approved with the aim of helping the country out of financial difficulties, and to control public spending and reduce public debt. The new government's purpose was to change the structure of the Italian economy by, for example, improving competitiveness; opening certain professions, such as taxi drivers, pharmacists, doctors, lawyers and notaries; and strengthening public finances and social policies by increasing the labor market participation of young and female workers.

[7] For more details on the policy tools, refer to Appendix Section D. In the analysis, I focus on the startups that registered between 2012 and 2015. The policy was amended several times; see Appendix D Figure D.1 for several changes to the policy.

1 of the 3 criteria: (i) spend more than 15% (of the lowest of revenues or costs) on R&D; (ii) 1/3 of employees hold a PhD and/or 2/3 hold a Master's degree; (iii) to be the holder, depository or licensee of a patent or the owner/author of registered software. The decision to admit firms in the framework was deliberated by the board of the Chambers of Commerce, local public entities with their own operational autonomy within the territory of competences that precisely overlaps with the provincial territory. Chambers support firms in their administrative activities; they promote local economic development and monitor local economies. Their board is in power for 5 years. At the time, there were as many Chambers as were provinces (105).

Some policy data. For monitoring purposes, the Ministry of Business Development collected information on the firms admitted to the framework in a publicly available registry that can be merged with other administrative sources (see Section 3). I focus only on LCs, as being in this legal form was a requirement for applying. Panel (a) of Figure 1 graphically reports the number of firms younger than age 5 over the total number of firms in the same age group that were admitted into the framework by incorporation year, whereas Panel (b) reports the equivalent share but for firms that report positive intangible assets.[8] The figures clearly show that, for both types of firms, with or without intangible assets, the share of firms that used the policy framework is different from zero also for firms incorporated before the policy. It increased over time until 2015, when 0.08% (Panel a) and 2.5% of the firms (Panel b) were registered in the framework. The share of firms in the framework was initially modest because of not only the restrictions on the type of firms that had to be "innovative oriented" but also the difficulties that local institutions had in judging firms' proposition on being innovative and deliberating their admission. Each young firm that applied to use the framework had to present itself with an innovative mission, whereas the Chamber of Commerce of the province in which the firm was headquartered was responsible for the screening process for this admission. This process created, at least initially, some uncertainty in how the policy was implemented, which differed across provinces and partially contributed to the geographical differences in the use of the framework, as shown in Figure 2 and documented by (Manaresi, Menon, and Santoleri,

[8] Both innovative startups and other young firms included in Panel (b) are selected on having positive intangible assets by age 2. The administrative data provide no additional information on the level of innovativeness of young firms, despite the information from the balance sheet, which is used as a proxy of the firm-level propensity to innovate. For more details, see Section 6.2.

2022).[9] In this figure, I report the provincial share of firms that registered over all firms younger than age 5, averaged for years 2012 and 2015; in Panel (a), all firms are included. In Panel (b), I include firms with positive intangible assets.[10] Finally, I also document differences in the use of the policy framework across sectors. Again, these differences are partly explained by, first, the clustering of the "innovative-oriented" firms in some sectors, second, by how sectors are geographically distributed. Figure 3 shows the distribution of all firms aged 5 or younger (grey bars) and innovative startups (blue bars) across industry sectors. Innovative startups are overrepresented in Manufacturing (Section C), Information and Communication Activities (Section J) and Professional and Scientific Activities (Section M).[11] The clustering of firms in these sectors is not surprising given that these sectors are also more likely to patent: a simple correlation analysis between share of firms in sectors J&M and the share/number of patents at the provincial level show positive results (Figure A.1).

Emigrants from Italy

Italian emigrants are generally young, highly skilled and more likely to come from northern Italian regions, from urban areas with good levels of human capital (Anelli et al., 2023; Anelli and Peri, 2017). Much attention has been given to the consequences that Italian emigration had on the economy, where higher levels of emigrants in the population have been associated with fewer high-tech firms or within firms to lower productivity and wages in high-skill, intensive sectors (Dicarlo, 2022; Anelli et al., 2023).[12]. Our data confirm these findings: younger individuals (20–40) are more likely to emigrate; however, while until 2010 the share of emigrants across all age groups followed very similar trends, after the Great Recession, younger individuals show

[9]After 2015, a clarification note ensured greater homogeneity across Italian provinces. Please refer to the following: `https://www.mimit.gov.it/images/stories/normativa/Circolare-startup-e-PMI-innovative-14-02-2017.pdf`.

[10]I correlate the presence of firms younger than age 5 in the period before the policy (2007-2012) with the share of firms that used the framework to find a negative and significant correlation: *coef=-0.239* and *p value=0.000*, hinting that the policy was also used in provinces that were initially less dynamic in terms of presence of new firms.

[11]It is plausible that there were local differences in the visibility of the policy across different provinces. However, quantifying these factors with the existing data is difficult. Ideally, one would measure an 'attention rate' (i.e., the number of firms that used the framework over the number of firms aged 5 or younger that met one of the three innovation criteria) to capture it. Given that most of these business support policies generally advertised via Chambers of Commerce, by controlling for provincial shocks I am accounting for any existing difference in visibility across provinces and time.

[12]Other effects on outcomes such as the quality of elected politicians or the levels of fertility have also been documented (Anelli and Peri, 2017; Anelli and Balbo, 2021)

an even more pronounced increase (Figure A.2). Between 2013 and 2017, my focus period, I observe that emigration has heterogeneously changed across provinces, increasing everywhere (Figure A.3), however when I correlate the growth of emigrants and the share of innovative startups on total LCs younger than age 5 it gives me negative results, hinting at the fact that emigrants are less likely to move from provinces with a higher share of innovative startups. For this reason, I more deeply investigate the possible relation between innovative young firms and the share of emigrants, by performing the following descriptive exercise shown in Figure 4. On the x-axis of both panels I plot the quantiles (30) of the distribution of the share of LCs younger than age 5 out of total LCs in the sectors J&M, measured in year 2002 for all municipalities. On the y-axis I plot the average residual share of emigrants in the corresponding quantile.[13]. The picture clearly shows what seems to be a trend inversion. Municipalities with higher shares of innovative young firms in 2002 did not have lower shares of emigrants before the policy; however, the municipalities in the same quantiles experienced an inversion after 2012, and the share of emigrants decreased.

3 Data

The first challenge in estimating the effect of SUA on the probability of emigrating is measurement. I combine different sources of administrative data to directly measure: (i) the number of emigrants to other European or extra-European countries from the municipality; (ii) municipality-level employment; and (iii) firm-level employment; (iv) when and the municipality in which an innovative startup registered.

Emigrant data. The main source of data on emigrants is the Registry of Italian citizens residing abroad (*Anagrafe degli Italiani Residenti all'Estero*, AIRE). This dataset comes from the Minister of the Interior and represents the collection of all permanent Italian residents abroad between 1990 and 2019. For the main analysis, I focus on the years of migration from 2007 to 2017 and consider only individuals who were still abroad in 2019. My definition of em-

[13]The residual share of emigrants is obtained by regressing the share of emigrants to other EU countries (normalized by the 2002 population) on municipality and province-by-year fixed effects for the years 2002–2011 and then predicted for all years. I focus on sectors J&M as they are the ones that later in 2012 have firms that are more likely to participate in the SUA, as previously shown.

igrant includes individuals who declare that they are abroad as expats and live abroad.[14] The data contain information on the date of birth, gender, Italian municipality of origin, country of destination, and year of migration. The individual-level data are collapsed at the municipality, year of migration and destination (EU vs. extra-EU) levels, restricting the analysis to individuals in the age group 20 to 60 in the year of migration.[15]

Social Security data. My main source of employment data is the confidential matched employer-employee dataset from the Italian Social Security Agency (*Istituto Nazionale di Previdenza Sociale* - INPS) for the years 2002 to 2017. This dataset contains the universe of nonagriculatural firms with at least one employee from 1983 to 2019. The data include unique firm and worker identifiers that allow us to follow them over time. Each firm is identified through its tax identification number and each worker through his or her social security number (*codice fiscale*). This administrative dataset contains information on workers' start and end dates for each job, workers' earnings, annual days worked, occupation and apprenticeship status, contract type, and part- vs. full-time status, as well as demographic information such as gender, birth date, workers' residence and work location given by the firm location, which are reported as part of the administrative procedures and are mandatory. The frequency of reporting is once a year. This dataset does not include information on unemployed individuals, self-employed individuals, or individuals employed in the public sector. Firm-level information includes incorporation and dissolution data, as well as the firm's legal status (i.e., limited company, sole proprietor, etc.) and 5-digit industry codes.

Firm balance sheets. The main source of firms' balance sheet information for all of the LCs in Italy is *Cerved*. Data are available for approximately 700 thousand companies every year from 2005 to 2019, covering only LCs and representing approximately 50% of all firms present in the Social Security data.[16] The firm tax identification code (anonymized) allows me to match balance sheet information to the employer-employee data. I use balance sheets to measure the firm level propensity to innovate, as represented by R&D.

[14] I exclude second-generation immigrants and individuals born abroad who are registered because their parents are registered.

[15] This decision was made, for privacy reasons, by VisitINPS data-scientists who also created another dataset collapsed at the province, 5 year age-group, main destination, gender, and year of migration level used in Section 2 for the descriptive purposes.

[16] It is administered by the *Cervedgroup.ltd* but available to researchers on the INPS server.

Registry of innovative startups. This registry is a collection of firms admitted to the SUA framework. Publicly available information is kept and updated weekly by the Ministry to comply with its policy monitoring duties (Manaresi, Menon, and Santoleri, 2022). This dataset is merged with the Social Security data using the tax identification number, with some caveats, as not all innovative startups have a match (see below).[17] The final merged dataset provides access to the entire financial and labor market history of the innovative startups even after they exited the framework.

Sample selection. As emigrant information is available at the municipality-by-year level, I define policy participation at this level. As later explained in Section 4, a municipality starts being exposed and therefore treated by the SUA in the year of the incorporation of its oldest innovative startup.[18] Therefore, the main sample of analysis is a panel of municipalities. The construction of the dataset requires a match across different datasets as follows. From the startup registry, the data-scientists in the INPS identified 3,171 (86%) innovative startups in the social security data out of the 3,682 that registered between 2012 and 2015 and were still active at the time of the matching.[19] The matched innovative startup are headquartered in 827 municipalities, representing 92% of all the ever treated municipalities.[20]. Finally, I recover

[17]The reason for not appearing is linked to the fact that firms appear in the Social Security data only if they have ever formally hired at least one worker with any type of contract and INPS keep balance sheet information from *Cerved* only for firms that appear in their Social Security dataset.

[18]Emigrants are allocated to the municipality m of previous residence, and the information is available from *AIRE*. In line with this, all labor market outcomes are calculated for municipality m. Employment is therefore measured in municipality m, implying that for workers who live in m but work in municipality $m^{'}$, their labor market outcomes are allocated to municipality m. The risk of this choice is that, by using the municipality of residence, I possibly overestimate the labor demand in municipality m. As an example, I can think of a situation in which the municipality of residence is different from that of work. If the reason the worker has decided to commute to work every day is linked to lower wages in the municipality of residence, I incorrectly assign that wage and employment to the municipality of residence. In contrast, by using the municipality of work, I risk an overestimation of workers' supply. In the former case, the effect of the policy would be a lower bound of the overall effect on emigration, as workers have already exploited the commuting option and are less likely to emigrate. In the latter case, the effect of the policy would be an upper bound of the effect, as the effect is much greater. Among the two, I use the most conservative solution.

[19]The total number of companies that registered in the framework between 2012 and 2015 was 5,476, and 1,794 ceased activity between 2013 and 2020, leaving me with 3,682. As explained before, they are not present in the Social Security data because they never hired any workers. Nevertheless, because they participated in the framework, I investigate whether the reason for no longer being active could be explained by observable local-level conditions. A simple regression on the 5,476 observations on the probability that the innovative startup was matched to the administrative INPS data by year, month and region of registration (alternatively, number of patents in the region or even municipality) does not show any specific pattern, indicating that no coefficients on the region, year or month dummy or another variable are significant and therefore could drive this probability.

[20]I could have defined the treatment just by looking at all municipalities in which an innovative startup was present (891), therefore by dropping the initial step of the firm level merge with the Social Security data. This initial step is however necessary, as it allows me to identify firms that were active on the labor market, which

balance sheet information for 2,723 innovative startups from *Cerved*.

The period of analysis starts in 2007, 5 years before the introduction of the SUA, and ends in 2017. The data allow me to investigate the existence of prereform trends between the treated and control units of analysis. The panel of municipalities is balanced in the sense that I keep all municipalities that ever had an emigrant, and if there is no emigration in a specific year the municipality is kept in the sample with the number of emigrants set to zero. Emigrant shares are normalized by the 2002 municipality population.

4 Research design

The second challenge that I face in estimating the effect of SUA on emigration is its identification. My main strategy is based on a municipality experiencing the registration of an innovative startup. However, firms were not randomly allocated to municipalities and they could also decide to apply on the basis of some municipality and their own conditions. I therefore leverage a matching difference-in-differences estimation strategy with staggered adoption for which the year of adoption is the incorporation year of the oldest innovative startup in the municipality, for the following reasons.[21] First, the matching procedure allows me to address the challenge of selecting an appropriate control group for the treated municipalities, with comparable economic conditions in the years before the event. By matching on baseline covariates, I reduce the bias associated with these covariates, also potentially balancing unobserved confounders.[22] Second, the matching algorithm is performed using the incorporation year of the first innovative startup in that municipality and I rely on the staggering of incorporation year for identification. A young firm that wanted to benefit from the framework had to apply through the relevant Chamber of Commerce, which had to deliberate on its admission based on meeting the

had done some hiring, and therefore, enabling better identification of the possible channels. The two measures are however strongly and positively correlated as shown in Appendix Figure A.4. In this figure, I report on the x-axis the number of innovative startups in the municipalities identified as treated following the firm-level merge with the Social Security data, and the y-axis reports the number of innovative startups in municipalities ever exposed to the SUA irrespective of finding a firm merge with the Social Security data. I also report an additional exercise in the robustness checks to my estimates in Section 5.3.

[21] A municipality that experiences the registration of two innovative startups, the first one registered in 2014 and was incorporated in 2013 while the second one also registered in 2014 but was incorporated in 2012, will have as a year of adoption 2012.

[22] The matching procedure generally performs better with few periods and many units, as in the present case.

criteria.[23] While it is true that during the first three years of the policy (2013–2015), the uncertainty created by the lack of understanding of the law by the local authorities regarding the criteria partially induced some staggering participation across different provinces, the decision to apply was based on the firm, which could have adjusted to the criteria needed to become an innovative startup before putting forward an application. By matching by incorporation year, I can exclude any anticipation effect that may invalidate our results (Roth et al., 2023).[24] Although the matching and staggering adoption may circumvent part of the endogeneity of the registration, to support my argument and dismiss any further concerns, I present an alternative design based on an instrumental variable strategy on a sample of unmatched municipalities. The instrument is the local-level heterogeneity in the presence of young firms born in 2010 in sectors J&M interacted with the provincial propensity to innovate as alternatively measured by the number of patents in the years before the SUA passed or by the industry composition of the CC board as a proxy for board familiarity with sectors more likely to have innovative startups. I report these results in Appendix B.1. The two designs have advantages and disadvantages, and I view them as complementary evidence.

4.1 The matching algorithm to select the control group

A key challenge in my design is to find an appropriate comparison group for municipalities exposed to the policy. I use a matching sample procedure to identify a group of placebo municipalities in which no young firms were admitted to the framework but that have lagged characteristics similar to those in the treatment municipalities.

Notation. t denotes the calendar years, e denotes the municipality-level event year (see below), and $k = t - e$ denotes the periods before or after the event. I measure outcomes in each year t.[25] Finally, I perform the matching algorithm on the incorporation year, b, of firms in municipalities. Given that I am using calendar years, the incorporation must occur between January and December of that year. I construct a dataset as follows.

[23] Unfortunately, the Ministry does not collect rejection rates.

[24] Firms could have adjusted on several dimensions to be judged as "innovative oriented": R&D and patenting or a highly skilled workforce. According to a report by the Ministry, innovative startups took an average 125 days (approximately 4 months) to be registered in the startup registry since their incorporation (MISE, 2017).

[25] The *AIRE* reports the exact registration date for emigrants (day and month); however, labor market data are available yearly. Therefore, I use yearly observations.

Treated municipalities. Municipalities indexed by m that are treated in event time $e \in [2012, 2013, 2014, 2015]$ by innovative startups incorporated in year $b \in [2010, 2011, 2012, 2013, 2014, 2015]$. I define the event in the following way: for municipalities where the oldest innovative startup was incorporated after 2012, $b > 2012$, the event is $e = b$, in contrast, for municipalities where the oldest innovative startup was incorporated before 2012 ($b \leq 2012$), $e = 2012$, where 2012 is the year in which the SUA passed. For each municipality, I have a rich set of baseline characteristics used in the matching algorithm. I consider only the first treatment and do not consider multiple treatments if a municipality is treated more than once (i.e., more than one innovative startup was incorporated in that municipality).[26]

Control municipalities. For each year of event e, the control municipalities are sampled from the municipalities that *did not* have any innovative startups during the period of my analysis (i.e., *never-treated*). As in the treatment sample, I record all baseline characteristics. In every event year, I identify 5,283 potential control municipalities but require the control municipalities to be in a local labor market (LLM) other than the one of the treated municipalities to avoid contamination from spillover effects due to the creation of innovative startups (i.e., migration from control municipalities responding to the treatment within the same LLM). [27]

Sample matching to identify the control group. I implement a matching sample algorithm, *separately* for each incorporation year b. The procedure selects control municipalities that are similar to treated units by matching municipality-level preincorporation covariates.[28] The matched pairs are then used in the analysis. For each incorporation year (6), I create separate datasets and then stack them up. For each municipality m in the treatment group,

[26] Municipalities that received multiple treatments are only 3% of the treated sample. Given the small percentage of multiple treatments, I ignore this issue at this stage; however, an alternative would be to perform a similar exercise as in Bartanen, Grissom, and Rogers (2019), who, in their work on the effect of the principal on school performance, considers any principal's transition as a different unit of observation and arranges the data to have one observation per event per time.

[27] Local labor markets (LLMs) are defined using the 2001 definition of the National Statistical Office (ISTAT). According to this definition, LLMs are clusters of municipalities with commuting patterns within LLMs (and not across). They are used to proxy labor markets where people work and live. Municipalities are grouped into 686 LLMs.

[28] See, for example, Jäger and Jörg (2019) or, for a similar case, Italian municipalities (Fenizia and Saggio, 2022). The benefit of this method lies in the possibility of identifying a smaller control group—similar to a treated group—with respect to some observable characteristics. In fact, the cost of including all possible control units, instead of only the matched ones, is considered larger in terms of standard error gains (Rosenbaum and Rubin, 1985).

I can select a municipality in the control group with similar lagged characteristics. I follow Jäger and Jörg (2019) and do not match trends but only lagged covariates, allowing me to use pretrends themselves to evaluate the plausibility of the parallel trend assumption. In each year b, I select a placebo municipality from the set of control units to exactly match the following characteristics of treated municipalities: the log number of firms born in that year, the three- and two-year-lagged share of total emigrants, the one- and two-year-lagged log employment (full-time-equivalent) in young firms ($age \leq 5$) and the log population in 2002. Consider, for example, the first innovative startup that registered in the policy framework in 2014 in municipality A, born in 2011 in the same municipality. I match municipality A to another municipality in the control group, which had the same number of firms incorporated in 2011, the same share of emigrants in 2009 (2 years before) and in 2008 (3 years before), the same total employment in young firms ($age \leq 5$) in 2010 and the same population in 2002. The procedure estimates 6 separate logit regressions that relate the probability of having an innovative startup incorporated in b to the above baseline characteristics. The model provides the predicted probabilities to be treated (i.e., propensity scores). I match each municipality m in b with the municipalities in the control set with the closest score. I use a caliper propensity score matching algorithm without replacement to select the nearest control municipality based on the propensity score distance.[29] I only keep the matched municipalities and drop the municipalities that are out of the common support. The variables are chosen to create a comparison group that resembles as much as possible the observed characteristics of the treatment group. An exact match is found in 70% of the municipalities in the treatment group. When I do not find an exact match, the treated municipality is not included in the sample. Specifically, I match 581 treated municipalities to 581 control municipalities.[30] In Figure 5, I show the map with the matched treated (red) and control (blue) municipalities. Figure A.5 shows the distribution of

[29] The caliper matching method uses all of the comparison units within a predefined propensity score radius ("caliper"). I use a caliper of 0.05.

[30] In Table A.6, I report for each birth year b all of the treated and discarded units for each event year. To check for whether a bias is introduced by the matching algorithm, given that I am not able to match all of the treated municipalities, as a robustness check, I exclude from the logit model the emigration information (i.e., three- and two-year lagged local shares)—as the match on pretreatment outcomes could create an additional cost in terms of estimate bias—by undermining the second difference, forcing the treatment and control group's pretreatment outcome to also be equal on the main dimension of analysis(Ham and Miratrix, 2022). The results are not sensitive to the set of variables included in the matching algorithm and are reported in Section 5.3. Finally, I also perform the analysis using all treated and never-treated units in an IV settings and the results are comparable.

population (Panel A), share of emigrants (Panel B), startups (Panel C) and employment (Panel D) in the treated and control municipalities after matching. Once the stacked dataset with all of the matches is created, I define the event year e as explained before (if $b > 2012$, $e = b$; if $b \leq 2012$, $e = 2012$). I assign a placebo event value e to the matched control municipality as the one the municipality is matched with. Therefore, the variable identifying the cohort of treatment is defined for both treated and control units. [31]

4.2 Descriptive Statistics

In Table 1, I report the summary statistics for the matched municipality sample in the year before the event e for all cohorts. In Appendix Tables A.1 and A.2, I report the statistics for the 2012 cohort and for the 2013–2015 cohorts, respectively. This table assesses the extent to which the placebo sample represents a balanced comparison group for the difference-in-differences design, providing the right context to interpret the treatment effects; however, my research design allows for differences in the average levels of the outcome variables, whereas for identification, I need to assume that the characteristics of control and treated municipalities follow parallel trends before the treatment.

Column 1 reports the statistics for all of the municipalities in the sample, whereas columns 2 and 3 display the statistics for the treated and control municipalities, respectively, and column 4 shows the average difference. The average municipality in our sample had 14,140 inhabitants in 2002. The share of emigrants is 0.076%, the share of emigrants to other EU countries is 0.04% and the share of emigrants to extra-EU countries is 0.033%. These shares are statistically the same in the treated and control samples. I then look at the number of young firms, considering only LCs and their labor market. The average municipality in our sample has approximately 53.74 firms younger than the age of 5 years that employ a total of 710 individuals (365 full-time equivalent employees) in the municipality. The number of newly incorporated firms (i.e., those incorporated the year before the event) is 51, and they employ 102 individuals (37.30 as full-time equivalent). The average municipality has a total of 123 LCs of all ages. Finally, with respect to

[31] The same matching algorithm is also run at the LLM level allowing me to match 119 LLMs to 119 never-takers. As expected, the results with these units are noisier: the effect of innovative startups on emigrants is likely to be very local, attracting workers from the same municipality and therefore poorly estimated at higher geographical levels as diluted. This fact on hiring local workers is also documented by (ISTAT, 2018).

their location, 19% of the municipalities in our sample are in metropolitan areas[32] and 55% of the municipalities are located in the north of the country. The two samples are fairly balanced with respect to all characteristics. It should be noted that the similarity of the two samples is not a simple mechanical effect of the matched sampling, as the matching was relied on a few years before the event and not only the year before and not on all of the variables reported in the summary table. However, since I do not match exactly on the dummy for metropolitan areas and for being in the north, a potential concern is the level of imbalance in these dimensions that may drive our treatment status. To assess this concern, I follow Jäger and Jörg (2019) and regress our main regressor (the variable switches to 1 when the municipality becomes treated) on these variables, including province-by-year and municipality fixed effects. I find that these variables are jointly insignificant in predicting treatment status in our sample (p value=0.3034). I also report descriptive statistics for the 2012 and 2013–2015 cohorts in Tables A.1 and A.2. Although the results confirm that the two samples of treated and control municipalities are balanced within cohort groups, I observe differences across cohorts, with the municipalities treated in 2012 being larger in terms of population, number of young firms younger than age 5 years, number of newly incorporated firms and total number of firms, with more employees in these firms. These differences justify my choice of running also separate analyses for firms born before or after the year of the policy, including the use of alternative estimators that account for heterogeneous treatment effects driven by differences in characteristics across cohorts.

4.3 Econometric Specification and Identification

I estimate the effect of having an innovative startup incorporated in municipality m on the sample of treated and control municipalities using the following equation:

$$y_{mk} = \sum_{k=-6;k\neq-1}^{4} \gamma_k \mathbb{1}\{k = t - e_m\} + \sum_{k=-6;k\neq-1}^{4} \beta_k^T \mathbb{1}\{k = t - e_m\} \times T_m + \alpha_m + \lambda_{p(m),k} + r_{mk} \quad (1)$$

[32]These are areas around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. According to the official definition of metropolitan areas, territorial entities recognized by Article 114 of the Italian Constitution are composed of an aggregate of neighboring municipalities. Introduced with the reform of Title V of the Constitution in 2001, metropolitan cities are recognized as large-area territorial entities defined by the aggregation of neighboring municipalities, such as provinces.

where y_{mk} is an outcome variable for municipality m in period $k = t - e_m$ relative to event e_m, either the share of emigrants (all, EU or non-EU) or other possible outcomes measured at the municipality level. T_m is an indicator variable equal to 1 if municipality m is treated with an innovative startup. $\mathbb{1}\{k = t - e_m\}$ are the event time dummies (leads and lags around the event) that control for differences in outcomes across years, and e_m is the year of the event for municipality m. I omit the dummy for the year before the SUA event such that the coefficients of interest, β_k^T, identify the changes in the outcome y_{mk} between the treated and control units relative to the same difference in $k = -1$. The coefficients of interest capture the effect of the creation of an innovative startup in year $k = t - e_m$ in the treatment group and are normalized to zero in $k = -1$ ($\beta_{-1}^T = 0$). Differences in average outcomes between treated and control municipalities are absorbed by municipality fixed effects, α_m; therefore, I do not need to assume that the two groups have the same outcomes in the absence of treatment. In fact, α_m control for time invariant characteristics at the municipality level that may trigger both the creation of the innovative startup and the outcome variable, such as the average supply of certain types of workers or the presence of some specific sectors. $\lambda_{p(m),k}$ are province-by-year dummies that control for changes in provincial and regional policies, economic cycles or any other confounding factors that vary over time at the provincial level. Given that the competences of provinces perfectly overlap those of the Chambers of Commerce, controlling for any possible shocks at this level is crucial, as it controls, for example, for any decision or action made by the Chambers to support firms. Trends in emigration, firms and sectors are likely to differ geographically. By adding $\lambda_{p(m),k}$ to my estimation equation, I create more credible comparison groups and compare outcomes across municipalities that experienced the same provincial shocks.[33] Therefore, for identification, I use the variation within the same municipality to compare the changes in outcome relative to $k = -1$, *and* within the same province and time $p(m), k$ relative to the actual or placebo creation of an innovative startup to compare treatment group municipalities to municipalities in the control group. I cluster

[33] I also run a specification with province-specific trends, and the results are not affected. However, this inclusion is debatable, as controlling for trends does not allow for proper testing of the common trend assumption before the treatment. The omission of these trends should only bias the coefficients in the case of a systematic relationship between trends in emigration rates and the creation of an innovative startup in the same municipality. Moreover, if anything, emigration trends during the period in which the policy was implemented were moving upward, opposite the direction of the effect.

standard errors at the municipality level to address any concern about the serial correlation of outcomes across periods (Bertrand, Duflo, and Mullainathan, 2004). All of the regression results are weighted by the logarithm of the number of firms in sectors C (manufacturing), J (information and communication), and M (professional and scientific).[34] To facilitate the interpretation of the results, I also show a static specification for which the coefficients simply summarize the effects of the use of the framework on the outcomes for $k > 0$.[35]

4.4 The validity of the design

The identification of the effect relies on a dynamic difference-in-differences design (Fenizia and Saggio, 2022; Goldschmidt and Schmieder, 2017; Jäger and Jörg, 2019). The estimated effect comes from a comparison of treated municipalities to a sample of *never-treated* municipalities identified by the algorithm that act as a counterfactual. I need to assume that municipalities in the control and treatment groups would follow parallel trends in $k > 0$ in the absence of the creation of an innovative startup. This implies that the creation of an innovative startup is exogenous to the local economic situation *conditional* on the model controls: the timing and the scope for the creation of the innovative startup were not the result of an effort adopted by municipalities. Although I cannot directly test this assumption, I can assess the likelihood that the assumption holds by observing violations of the parallel trend assumption in the years leading to the event by simply looking at the coefficients for $k < 0$, because both treated and control units are observed in the period before the event. The absence of pre-event trends is taken as evidence of the strict exogeneity of the policy change (Freyaldenhoven, Hansen,

[34]There is one additional minor concern about data reliability, as there may be some unobservable delays in the registration of emigrants in the AIRE (i.e., measurement errors in the flow)(Anelli and Peri, 2017). However, in my estimation strategy, this could be an issue if the measurement error varies differently over time between municipalities; otherwise, it is net out by municipality fixed effects.

[35]I estimate the following equation:

$$y_{mk} = \sum_{k=-6;k\neq-1}^{4} \gamma_k \mathbb{1}\{k = t - e\} + \beta^T Policy_{mk} \times T_m + \alpha_m + \lambda_{p(m),k} + r_{mk} \tag{2}$$

I include in both my models (static and dynamic) calendar year fixed effects by controlling for $\lambda_{p(m),k}$. Notably, the calendar time is balanced only for part of the sample, as I am matching on incorporation year b but the event variable $e = b$ only if $b > 2012$, whereas $e = 2012$ if $b \leq 2012$. Therefore, I control for calendar year fixed effects, as not allowing for those effects may change the point estimates and standard errors of the effect.

and Shapiro, 2019). However, even in the presence of parallel trends, I may still worry about whether the control municipalities represent the right counterfactual situation, and I discuss some of the concerns below.

Unobserved sudden shock. One may worry about the existence of contemporaneous shocks that affect both the outcomes and the timing of the creation of the startup, which, for example, was incorporated in response to local economic shocks. If this is the case, then my estimates capture, at least in part, a selection effect. Given that the estimated effects are negative, a potential threat to identification arises, such as if the creation of an innovative startup comes from a positive period in the local economy that also results in lower emigration rates. Alternatively, the negative estimates are due to the liquidity constraints of potential emigrants resulting from a downturn, forcing individuals not to move but, at the same time, to a higher approval rate at the Chamber of Commerce level, which admits more startups to face this downturn. In my identification strategy, I allow the timing of the creation to depend on the unobserved time-*invariant* characteristics of the municipality, and I need to assume that pretrends are parallel (parallel trends assumption). If this assumption holds, this typology of shocks must be sudden and large enough to change individual decisions to emigrate. I argue the following. First, this potential threat to identification is unlikely, as both firm creation, especially of LCs (i.e., idea development, the search for money to be invested) and the decision to emigrate, which is a life-changing decision, require time. Second, in the estimated model, I include province-by-year fixed effects to remove provincial shocks in both the outcome variables and on the probability of having an innovative startup, with the identification coming from deviations from these shocks. The decision to include these fixed effects is crucial because, although the analysis is performed at the municipality level, it is the Chamber of the province—one administrative level up—that deliberates on whether the firm is an innovative startup. Any shocks affecting this process are captured by province-by-year fixed effects.[36] Third, by using the incorporation year (for firms born after 2012) or the year in which the law passed (for firms born before 2012), I address any concerns about the endogeneity of the timing of the registration of the innovative startup in the framework, which is linked to possible firm

[36] Another time-varying unobservable factor could have been policy visibility, and positive shocks could have been associated with greater visibility. As reported by ISTAT (2018), the visibility of the policy mainly occurred through the Chamber of Commerce; therefore, province-by-year fixed effects capture it.

anticipation behaviors in meeting the requirements (i.e., highly qualified workforce, patents or software, investments in R&D): firms could have invested more in these requirements during an upturn and therefore increased the probability of using the policy framework. Finally, I also provide results using an instrumental variable strategy in Section B.1 as an alternative approach to the matching strategy to remove any further endogeneity concerns where I instrument municipalities' participation in the framework.

Heterogeneous effect. The matching algorithm allows me to circumvent some formal issues that usually arise with event-study models that rely only on the variation in the timing of the treatment (Goodman-Bacon, 2021; Borusyak, Jaravel, and Spiess, 2023; Fenizia and Saggio, 2022), specifically its heterogeneity. In practice, heterogeneity in treatment effects across different cohorts may still exist if the cohorts differ in their covariates affecting their responses to the treatment. As shown in Section 4.2, the characteristics of the sample of municipalities in which the innovative startup was born before 2012 are different from those of later cohorts. For this reason, it remains important to show results using an estimator that accounts for heterogeneity of the effect across different cohorts. I use the approach suggested by Sun and Abraham (2021), as their suggested estimator is robust to treatment effect heterogeneity. The estimated coefficient, β_k^T, is a weighted average of the effects in each cohort, and weights are given by the relative size of the cohort. According to Sun and Abraham (2021), this source of heterogeneity is still compatible with the parallel trends assumption, as the latter only rules out the selection in initial treatment timing based on the evolution of the outcome before the treatment.[37]

Main confounding effect. The research design allows me to identify the effect of having at least one firm in the municipality that participated in the policy framework *conditional* on the firm being created exactly in that municipality. However, the new policy framework could have had a direct effect on the propensity to create new firms. In this case, I would not be able to separately identify the two effects: startup creation vs. framework participation. To address this issue, I perform three exercises. In the first one, as previously explained, I show the results

[37] I also run the dynamic difference-in-differences model on the sample of unmatched units, in which the control group is made of all never-treated units (5,283 municipalities). It should also be noted that, with a large, never-treated group, the setting becomes closer to that of a classical, nonstaggered difference-in-differences design and any (Borusyak, Jaravel, and Spiess, 2023; de Chaisemartin and D'Haultfuille, 2020).

for two groups of municipalities: those treated by startups incorporated *before* 2012 and those treated by startups incorporated *after* the introduction of the policy. For the former group, I am able to identify only the effect of framework participation, as these firms already existed before the SUA was passed in 2012. For the latter group, I am less likely to separately identify whether the effect is driven by the SUA affecting firm creation directly or the startup framework participation. Second, I run a placebo exercise in which I study the direct effect of firm creation on the probability of emigrating in the period before the SUA was passed. In an event study setting, I show that there is no correlation between the creation of any innovative firms and the share of emigrants in a placebo period, prior 2012. To make the exercise more credible, I focus only on firm creation in sectors J&M, the ones more likely to use the SUA. This placebo exercise allows me to exclude that the emigration behavior is not merely the direct effect of the creation of an innovative firm. Finally, I run the same exercise as in Freyaldenhoven, Hansen, and Shapiro (2019). The municipality-level propensity to create innovative firms could be affected by the policy by making individuals more prone to this activity in the treated municipality: municipalities mechanically become treated because the propensity to create an innovative firm increases more than in control municipalities. Instead of directly controlling for firm creation, Freyaldenhoven, Hansen, and Shapiro (2019) suggest examining the dynamics around the event period and using these dynamics to infer anything about the confounding factors. The effect of the policy is estimated using a 2SLS, with the closest lead to the event serving as an excluded instrument for firm creation.

5 The effect of the SUA on the probability of emigrating

This section shows the (static and dynamic) results on how the SUA affects the probability of emigrating. In the following Section 6, I examine the effects on different dimensions of the local labor market (i.e., municipality) and at the firm level.

5.1 Main Results

Figure 6 reports the event-study coefficients ($\hat{\beta}_k^T$) estimated using model 1 on the share of emigrants to all countries (Panels a–c), to other EU countries (Panels d-f) and to other extra-EU countries (Panels g-i). The share of emigrants is defined as $\frac{Emigrants^{dest}_{m,k}}{Population_{m,2002}}$, where $Emigrants^{dest}_{m,t}$ is the total number of emigrants who moved from municipality m in year k (the year of emigration, as reported in the *AIRE* data) to all destinations, EU or extra-EU countries, whereas the denominator does not vary over time and is fixed at the 2002 population level in the same municipality. Table 2 summarizes the effects of $k \geq 0$ of SUA on these outcomes. As explained in Section 4.4, I also show the results for municipalities treated by innovative startups incorporated before the SUA ("2012 cohort") and municipalities treated by innovative startups created after 2012 ("2013–2015 cohorts").

Figure 6a and, above all, Figure 6b show that the share of emigrants in treated municipalities closely tracks the control municipalities in the years before the arrival of an innovative startup.[38] This fact corroborates the validity of our research design, which mainly relies on parallel trends before the treatment to identify the effect. In the first year after the creation of an innovative startup, the average difference in the share of emigrants between the treated and control municipalities is negative but not statistically significant. In the following years, the share of emigrants decreases, and the difference is estimated more precisely. The estimated effect in Figure 6a is more likely driven by the effect on the share of emigrants to other EU countries (Figure 6b), which is observable up to period $k = 4$, whereas the effect on the share of emigrants to extra-EU countries disappears after $k \geq 3$. For example, in Panel 6b, the share of individuals who decide to emigrate to EU countries in all years $k > 0$ is 0.01 percentage points lower than that of the control municipalities. In Panels 6d to 6i, I split the sample by treatment cohort. The effect estimated for the 2012 cohort is more precise and larger than the effect estimated for later cohorts. These differences could be due to the heterogeneity of the effect or simply to the fact that for later cohorts, I could be confounding the effects of startup creation and startup framework participation, as explained in Section 4.4.

[38]The 4 placebo estimators assess whether each cohort group has parallel trends when untreated for at least $k+1$ periods, where $k+1$ is the number of periods over which parallel trends have to hold for the difference-in-differences estimator to be unbiased. For the results in Panels g to i, up to $k = 2017 - 2013 = 4$, β^T_{+k} is not estimated for all the same municipalities, as β^T_4 can only be estimated for $e = 2013$.

In Table 2, I report the results from model 2 on the share of emigrants to all countries (column 1), the share of emigrants to other EU countries (column 2) and the share of emigrants to extra-EU countries (column 3). This table confirms the dynamic effects shown in Figure 6 and shows that municipalities that participated in the SUA experienced a decrease in the share of emigrants to all countries (-0.014 p.p., a 19% decrease with respect to the mean baseline outcome of 0.075% measured in $k = -1$ in the treated municipalities), a decrease in the share of emigrants to other EU countries (-0.009 p.p., a 22.5% decrease) and to extra-EU countries (-0.005 p.p., a 15% decrease). The effect on the share of emigrants to extra-EU countries disappears when I consider cohorts separately in Panels B and C, while the effects are larger for the 2012 cohort for both emigrants to all countries (-0.025 p.p., a 39% decrease) and emigrants to other EU countries (-0.014 p.p., a decrease of 46%). For the most recent cohorts, the effect varies between -0.010 p.p. (12%) for emigrants to all countries and -0.007 p.p. (16%) for emigrants to other EU countries.

5.2 Heterogeneity analysis

This section examines whether the effect of the SUA is heterogeneous across municipalities depending on their characteristics. To assess the heterogeneity in the treatment effects, I estimate a variation in model 2, in which I interact the treatment dummy ($Policy_{mk} \times T_m$) with some municipality-level characteristics. I look at characteristics that could drive both emigration and participation. I interact the treatment dummy with the quintiles of the distribution of these characteristics measured 5 years before the introduction of the SUA, specifically in 2007. I analyze heterogeneity in the effect depending on the sociodemographic and industrial characteristics of the municipality. I focus on (a) the size of the municipality measured by its population, (b) the share of workers younger than 30, and (c) the *log* average real wages. Regarding industrial characteristics, I consider (d) the share of LCs in the innovative sectors (J&M) of the total number of LCs and (e) the share of newborn LCs of the total number of LCs. Finally, given that I do not measure patenting at the municipality level, I characterize municipalities using the (f) share of patents in the province over total patents in Italy averaged between 2007 and 2012 to measure the propensity to engage in innovation in the province and by (g) whether the municipality is part of a metropolitan area (see footnote 32).

In Figure 7, I show the results by pooling all cohorts and report the results by cohort in the Appendix Figures A.6 and A.7. In all panels, each bin shows the coefficient of the treatment interaction with the corresponding quintiles (x-axis) of the distribution of that municipality-level characteristic. I run separate regressions for each outcome variable: the share of emigrants to all countries (blue), to other EU countries (red) or to extra-EU countries (green). The black line shows the unconditional probability that a municipality in our sample participates in the framework at each quintile. The effects are more likely found in larger municipalities (Panel 7a) with a lower share of workers younger than age 30 (Panel 7b), and higher wages (Panel 7c). In terms of the industrial structure, the effect is driven by municipalities with a greater share of firms in the innovative sectors J&M (Panel 7d), higher levels of patenting (Panel 7f), lower shares of newborn LCs (Panel 7e) and nonmetropolitan areas. The results are confirmed for all cohorts, as shown in Figures A.6 and A.7.

5.3 Robustness

I conduct several robustness checks to evaluate the validity of my design.

Direct effect of startup creation on the probability of emigrating. As explained in Section 4.4, a possible concern regarding the validity of my empirical design is that the outcome variables are not directly affected by framework participation but rather by unobservable policy confounds that are related to both the outcome and the unobservable propensity to create new firms in the municipality. The concern is that municipalities are more likely to have an innovative startup participating in the framework during good economic times. This means that I observe participation conditional on an innovative startup being created. The absence of differential pretrends, shown in Figure 6, is often taken as evidence of the strict exogeneity of the policy change; however, it may simply be that pretrends are undetected due to the lack of statistical power. For this reason, I take this concern seriously and run several exercises to rule out the possibility that the effect is due to confounders and not to the casual effect of participation in the policy framework. I have already shown the results of the split sample, which reflects the assumption that this concern is less serious, as firms born *before* the introduction of the policy were already created. As a second check, I run a placebo exercise and observe the direct effect of firm creation in the years before 2012 on emigration. Specifically, the placebo exercise allows me to rule out that firm creation *per se* drives the estimated effect on migration, confounding the effect of the policy driven by the fact that the number of innovative startups that participate in the framework is higher in municipalities that do better economically and therefore have a higher number of new, innovative firms. I estimate the effect of creating a new firm in municipality m from 2002 to 2017 in a dynamic differences empirical model:

$$y_{mk} = \sum_{k=-6;k\neq-1}^{10} \gamma_k^{bf} \mathbb{1}\{k = t - e\} + \sum_{k=-6;k\neq-1}^{10} \beta_k^{bf} \mathbb{1}\{k = t - e\} \times T_m^{bf} + \alpha_m + \lambda_{p(m),k} + r_{mk} \quad (3)$$

y_{mk} is the share of emigrants (to all, EU or extra-EU countries). Control municipalities are identified by two conditions: zero startups in sectors J and M in 2002 and no change during the period. I alternatively define two types of events for which a municipality switches its treatment status. In the first case, the event that triggered treatment was a positive change in the number of new firms created in sectors J and M in the period before 2012. In the second case, the event is a 50% increase in the number of startups in J and M. In both cases, the treatment is staggered. To exclude the possibility that firm creation has a direct impact on the share of emigrants, we need the β_k^{bf} in $k+1$ to remain statistically equal to zero. I report the results from this exercise in Figure 8. Reassuringly, using both definitions of events, I do not see any change in the share of emigrants to any destination. The results, in fact, show that new firm creation in sectors that are more likely to participate in the framework does not directly change the probability of moving abroad.

Finally, I perform the exercise suggested by Freyaldenhoven, Hansen, and Shapiro (2019). This application implies that instead of directly controlling for firm creation in the empirical model, I observe its dynamics around the event time and use them to infer the dynamics of any confounds. Their methodology suggests a 2SLS estimator in which firm creation is instrumented with (a function of) the leads of the event. As suggested by Freyaldenhoven, Hansen, and Shapiro (2019), the closest leads are the most informative. For this reason, I decided to choose the first lead forward. Therefore, I define an instrument $z_{m1} = \mathbb{1}\{k = 1\} \times T_m$. I report the results in Table A.3, which confirm my baseline results, as in Table 2. The results are shown for all cohorts and for the 2013–2015 cohorts, as the confounding effect of firm creation could mainly affect these latter results.

Heterogeneous treatment effects. By matching on observable characteristics, I am able to partially address the heterogeneity (and endogeneity) of event timing; however, there may still be some differences in observable characteristics across cohorts, driving some unobservable selection, as reported in the tables in Section 4.2. Therefore, I use an estimator that accounts for the heterogeneity of the effect across different cohorts, as suggested by Sun and Abraham (2021). In Figure 9, I report the results on the share of emigrants to different countries and in different cohorts by using that estimator. The results are substantially comparable to our main dynamic results reported in Figure 6.

Effect of trust in a new government. A concern when estimating the effect of participation in the SUA on emigration is that the observed effect is not driven by the policy itself but simply by the fact that a new technical government was put in power during that period. In areas in which the trust in their work was higher, the probability of applying to the policy framework was also higher and simultaneously, the probability of emigrating was lower. By using IPSOS data on trust in the government at the municipality level, I run our baseline regression augmented by a control representing two alternative measures of trust in the government in 2012. Specifically, I count the number of college-educated individuals aged 18 to 40 years who declare that they have high trust in the government or are highly satisfied with the recent Monti government's actions.[39] I report these results in Table A.4. These results are comparable to those in Table 2, with the added control on trust for the government.

Excluding emigration in the matching algorithm. I exclude from the logit model run for the matching algorithm the variables measuring emigration (i.e., three- and two-year lagged local shares of total emigrants). The decision to match pretreatment outcomes may affect the results by creating an additional cost, as it forces the treatment and control group's pretreatment outcome to also be equal in the main dimension of analysis(Ham and Miratrix, 2022). Whereas matching on baseline covariates generally reduces bias, matching on pretreat-

[39]IPSOS collects data on opinions and Italians regarding the main political issues in the country (`https://www.ipsos.com/it-it/sondaggi-politici-oggi`). The data were collapsed at the municipality level for 2012 to 2013 in all municipalities. The raw data included 68,700 individual-level interviews conducted during the period when the Monti Government was in power.

ment outcomes (emigration in the context of the paper) can, on the one hand, mitigate the bias from latent confounders. On the other hand, this mitigation depends on the reliability of the outcome measurement.[40] At the same time, if parallel trends originally held, matching on pretreatment outcomes undermines an estimator that was initially already unbiased. Although I cannot measure reliability vs. the breakage of the parallel trends assumption, I simply check my results by excluding the pretreatment outcomes from the matching algorithm. The results are shown in Table A.5 and are comparable to the main results.[41]

Using all municipalities ever exposed to the SUA. As explained in Section 3, treatment is defined at the municipality level. In theory, treatment could have been defined without the initial merger of firms and labor market information from the Social Security data. As argued, the initial merger is necessary because it allows me to identify firms that were active in the labor market as they appear in the Social Security data. To confirm this and to show that my results are more likely to be driven by these firms, I report an additional exercise in which I match municipalities that have ever been exposed to the policy framework irrespective of the fact that I find a match for the innovative startup in Social Security data. The reason for not finding a match is likely that the firm never hired, formally, any employee; therefore, its profile never appeared in the INPS data. The results are reported in Figure A.8 and, as expected, show no effect of the SUA when I do not restrict to firms that are not active in the labor market.

6 Mechanisms at the municipality and firm level

The SUA facilitated the hiring process of workers in innovative startups, therefore in treated municipalities I expect to see an expansion in employment in these firms and sectors. As explained in Section 4.1, given that I exclude municipalities within the same local labor market, any observed increase in employment should not be due to workers from nearby municipali-

[40] Reliability is a measure of the degree to which outcome variables are coupled with latent covariates, such as how the measure is consistent over time.

[41] I also show that my results hold in a setting in which the treated units are not matched to a comparable set of control units by simply using all of the never-treated units. In this alternative estimation strategy reported in Appendix Section B.1, I control for possible confounders in an IV setting. The results, although larger in magnitude, are comparable in direction with the effect of municipality participation in the program negatively affecting the probability of emigrating, especially for other EU countries.

ties within the same LLM moving to the treated one, but to workers within the same treated municipality. I investigate the impact on the labour market of the SUA by looking at both municipality and firm level effects, with two alternative designs. At the municipality level, I study the changes in employment in firms younger than age 5 in sectors J&M, sectors with the highest probability of participating in the framework. I use the same matching difference-in-differences design used to study the effect of emigration, yet focusing only on the cohort of municipalities that were treated by an innovative startup incorporated before the SUA passed. It is in fact in this cohort where I find the largest effect on the share of emigrants, but it is also for this cohort that I can cleanly identify the effect of the SUA on employment and avoid confounding it with any possible effect of firm creation. The presence of an innovative startup in the municipality could have affected the equilibrium on the labour market of treated municipalities by increasing the demand for IT-savvy workers, the type of workers more likely to work for innovative startups ISTAT (2018).[42] I then estimate a firm level effect, by exploiting the variation in eligibility across firms as an instrument for the probability to be treated: age and propensity to innovate. The two approaches provide some complementary evidence mechanisms on the labour market. My results show that the presence of an innovative startup increased the employment of $Young\ firms_{age\leq 3,4,5}$ in treated municipalities through an increase in the number of workers in some occupations and in the number of local workers, who live in the same municipality in which the firm is headquartered. Firm-level analysis confirms this increase, showing a more precisely estimated effect of being an innovative startups hiring more workers as apprentices from the local labor market.[43]

[42]This increase in the demand was not however accompanied by an increase in wages, at least in the short run, because of the documented over-supply of IT-savvy workers (Schivardi and Schmitz, 2020). This implies that the market for workers in innovation-oriented firms was not tight even in the absence of firm entry and exit dynamics like in this case, partially explaining the lack of effect on wages.

[43]The apprentice contract is the most advantageous for young individuals, as it is automatically transformed into a fixed-term contract at age 30. It is the type of contract that allows young workers to learn a profession with a minimum length of 6 to 36 months. Additionally, there are fiscal advantages for firms because their cost is deductible (Maida and Sonedda, 2021).

6.1 Effect on municipality-level employment in young firms

Participation in the framework eased the hiring process through either tax credits for the highly skilled or simply by increasing the duration of fixed-term workers' contract.[44] I run model 1 on the log share of the total headcount by firm age in sector J&M. The results show an increase in the number of workers in young firms from the ages of 3 to 5 years. The results are reported in Figure 10.[45]. To facilitate the interpretation, I show the results for the same dependent variables in Table A.7 using the static model 2. This table confirms the dynamic effects in Figure 10 and shows that municipalities that participated in the SUA experienced an increase in the number of workers in young firms (Panel A) hired as blue and white collars and from the same municipality (Panel B).[46] Whereas some of these estimates are not significant, they still allow me to compute the number of additional workers who were hired in the treated municipality using the baseline outcomes. In the last row of each panel, I report the additional number of workers by type of firm and by the characteristics of the worker hired in the treated municipalities. These results show that on average, treated municipalities experienced an increase in the number of workers in firms younger than age 5 of 1.10 workers (0.41 FTE workers), or 0.8% (0.6%) of the baseline mean outcome in $k = -1$. This effect can be decomposed by occupation and translates into an increase of 0.5 blue collars, 0.9 white collars and 0.36 local workers.[47]

6.2 Firm-level effect

Research design

The eligibility requirements of the SUA created a sharp variation in a young firm's probability of applying the framework based on age and being "innovative oriented": firms older than 5

[44]In general, workers can be hired under a fixed-term contract for a maximum of 12 months, renewable only once. Fixed-term workers hired by innovative startups did not have a limit on duration, and contracts could be renewed an indefinite number of times within 36 months. Afterward, the contract could be renewed once more for a maximum duration of 12 months.

[45]The results for firms aged 0, 1 and 2 years are reported in Appendix Figure A.9

[46]To study the occupational composition and increase precision, I focus on firms aged 5 or younger, as employment in these firms is higher.

[47]I report the results for other remaining occupations (managers and apprentices) in Figure A.10 in the Appendix. I also check whether the framework changed the age composition of the workers in these firms. Although the results are barely significant in both the dynamic and static models, they suggest that managers in the treated municipalities are slightly older, whereas workers hired as apprentices are slightly younger.

years could not participate in the program even if they meet all of the requirements needed to be defined as innovative. As discussed in the previous sections, firms could make adjustments to meet the application requirements. Therefore, I restrict my analysis to the sample of firms born between 2007 and 2012, defined as eligible, firms born between 2010 and 2012.[48] As shown in Figure 1, the share of firms that participated in the framework and that were born before 2010 was not statistically different from zero. I also report the same share for young firms that have a propensity to innovate, which confirms this pattern.[49] I provide direct evidence of this variation in access to the framework by age and their propensity to be innovative in the Appendix C. Figure C.1 plots, among firms eligible by age in our sample, the evolution of the fraction of firms in the program in each age year for firms with positive intangible assets by age 2 and firms with zero intangible assets by age 2. Figure C.1a shows that for young firms with positive intangible assets, the probability of participating in the framework increases with age, from less than 1% in the first year to more than 3% after the age of 3 years. For firms without R&D, the probability of participating in the program was essentially zero for all ages. For noneligible firms, those born between 2007 and 2009 (Figure C.1b), the share of firms in the framework is null for all ages. My main sample of analysis is a panel of all private sector firms born between 2007 and 2012 and observed in the first 5 years of life.[50] I use a balanced panel. In case firms do not survive, I set their employment to zero, as is standard in this literature. In Appendix C, Table C.1 provides descriptive statistics on our sample of firms at age 2, which is the average age at which eligible firms in our sample started to use the framework. The average size of the firms in our sample is 7.21 employees (headcount), with an average weekly salary of €295.58; local workers and employees who live in the same municipality in which the firm is headquartered are on average 2; and employees' age is 36 years. Regarding firm characteristics, these firms have a similar survival probability by the age of 5 and have value added and revenues per week worked of 4 and 3.54, respectively. The table is also broken down by eligible and noneligible firms. Finally, Figure C.2 reports the

[48]This is also what the SUA allowed to be retroactive in the sense that firms born up to 48 months before the SUA could participate.

[49]Given the data limitations—no information on patents, precise R&D expenditures or education level of employees—I proxy their propensity to innovate using the log of intangible assets when younger than 2 years.

[50]I drop sectors that have a participation rate (share of firms that use the framework) below the $25th$ percentile of the distribution of the participation rate averaged across all sectors and all age groups. This choice is made to increase the precision of the point estimates.

sectoral distribution of firms in eligible and noneligible groups, showing that the former are more present in sectors C, J and M. The results are comparable when I restrict to a sample of firms with R&D. Despite these sectoral differences, the observable characteristics of eligible and noneligible firms are very similar before they are allowed into the framework, as shown in Table C.1. Figure C.3 also reports by age the average employment for innovative startups and for young firms that were eligible but did not use the framework. While it is difficult to test any parallel trend assumption, before age 2, firms in the two groups of innovative startups had similar yet slightly lower levels of employment, whereas after age 2, innovative startups diverged and were positioned on a higher level of employment.

My main identification strategy relies on using the interaction between being eligible and having some R&D as a source of quasiexperimental variation in SUA treatment. For each outcome y, the baseline specification underlying reduced-form graphical evidence is:

$$y_{ibsma} = \sum_j \delta^j \cdot \{\mathbb{1}[b \in \mathcal{E}] \cdot 1[RD_i^{a<2} > 0] \cdot \mathbb{1}[j = a]\} + \sigma_s + \omega_m + r_{ibsma} \tag{4}$$

where y_{ibsma} denotes outcome y for firm i, in age year a, in 3-digit industry s in municipality m. A firm in our sample can be in either the group of eligible firms $b \in \mathcal{E}$ that can use the framework or in the group of noneligible firms $b \in \mathcal{E}^C$. $RD_i^{a<2}$ is the log of intangible assets before age 2. Through our specification, j indicates the age of the firm in the year of the SUA. I control for sector fixed effects, σ_s, and municipality fixed effects, ω_m. My coefficients of interest are δ^j, and they trace the dynamics of the effect of eligibility on the outcome of interest over ages. By controlling for industry and municipality fixed effects, I exploit differences in eligibility within sectors and locations, partially controlling for the fact that eligible and noneligible firms are not evenly distributed across sectors. I provide graphical evidence of the probability of participation by plotting the estimated coefficients $\hat{\delta}^j$ in Figure 12. These coefficients capture the evolution across ages of the probability of participating in the framework for firms with or without R&D in the eligible group of firms compared to ineligible firms with or without R&D. The omitted age year in specification 4 is year 3; therefore, the results are expressed relative to the probability of participating in the framework for firms $a = 3$. My specification does not suffer from survival bias, as in each age year, I observe the effect of participation in that age year on contemporaneous outcomes in the same age year. Estimates of the effect of the SUA treatment are obtained from running IV models, where I instrument the probability of SUA treatment T by the interaction of being age eligible with having positive intangible assets. The following specification illustrates the IV model, and specification 6 is the corresponding first stage:

$$y_{ibsma} = \gamma^{IV} \cdot \{T_{ibsma}\} + \alpha_s + \eta_m + \nu_{ibsma} \tag{5}$$

$$T_{iasmb} = \lambda \cdot \{\mathbb{1}[b \in \mathcal{E}] \cdot 1[RD_i^{a<2} > 0] \cdot \mathbb{1}[j \leq 2]\} + \epsilon_s + \tau_m + \rho_{iasmb} \tag{6}$$

Identifying Assumptions

In this setting, by including age, I exclude the possibility of confounding the effect with outcome differences due to how firms behave at different stages of their life. My estimation happens within industry and municipality; therefore, I allow for the fact that innovative firms could perform differently in different industries (i.e., different technological adoption) but also across different locations (i.e., different human capital and infrastructure availability). The estimation is performed within the group of firms with or without intangible assets: by estimating the effect within these two groups, I am able to control for the differences that do matter across these types of firms (Khan and Manopichetwattana, 1989; Aghion et al., 2004). My identification strategy rests on the assumptions that there are no unobservable shocks that I am not able to capture that would be, within industry and location, specific to firms eligible for the framework *and* have intangible assets in the first years of life.

Results

Figure 12 provides a graphical representation of the variation used to identify the causal effect of SUA on firm outcomes. The graph plots the coefficients for all ages a from a regression that follows specification 4. The regression uses as an outcome the probability that a firm participates in the framework. This graph confirms the evidence from Figure C.1 discussed above that our instrument generates a sharp and significant first stage. Our instrument accounts for a 2.3 percentage point increase in the probability of participating in the SUA by firms born after 2010 ($a \leq 2$ in 2012), starting with a baseline of zero for firms born before 2010, as shown in Panel A of Table 3. Participation quickly increased and was 4 percentage points higher for firms born in 2012, the year of the policy (firms with $a = 0$). Table 3 displays estimates of the effect of SUA on employment outcomes. In the table, I also report in column (6) what I call contribution in terms of the number of extra workers in treated firms.[51]. Panel B shows that the treated firms are larger, as they have on average 5.5 more employees (4.2 if measured as FTE) than do similar noneligible firms of the same age. They also have more workers from all occupations, but specifically 0.88 more apprentices and, confirming results from the previous section, 1 more blue collar worker (yet not precisely estimated). I also investigate whether workers are more likely to be hired from the labor market of the municipality in which the innovative startup is headquartered (*local workers*) to determine that this is the case for apprentices for which 0.10 worker are local workers and for blue collar workers; however, the estimated effect is not significant.

7 Conclusions

I study how a policy aimed at supporting firms in the postincorporation period can affect emigration by supporting innovative firms. The policy aimed at decreasing the operational cost of young, innovative firms was effective at decreasing emigration by increasing employment in treated firms and municipalities. It assisted innovative startups by providing a bundle of complementary instruments available until the fifth year of operations (i.e., reducing red-tape, tax incentives to facilitate access to external finance, tailor made labor laws, etc.), with the aim of creating a better ecosystem for them. The policy also had the intended aim of retaining talent in the Italian economy. I find that for an average municipality in our treated sample,

[51]This contribution is measured by multiplying the level of the mean outcome by the estimated coefficient $\hat{\gamma}^{IV}$

there were 1.26 fewer individuals who wanted to move to other European countries. My results survive several robustness checks and alternative estimation strategies.

By creating new job opportunities for potential economic migrants in their current location, the opportunity cost of emigrating increases, and therefore the probability of emigrating decreases. The Act facilitated the hiring process of workers in innovative startups. For this reason, I observe that municipalities treated by an innovative startup experienced an expansion of employment in young firms (firms younger than 5 years) in the most innovative sectors—those more likely to have firms that participated in the framework. At the firm level, the results of the expansion in the employment of treated firms are confirmed.

This paper provides the first evidence in a developed country, Italy, of how, by exogenously changing economic incentives through *ad hoc* policies, governments can affect migration decisions. It has been shown that high emigration rates from Italy, especially from high-skilled individuals, create a cost for potential innovators and entrepreneurs because of slowing opportunities to improve productivity and innovation. Young managers and professional and, eventually, innovative firms with high potential are crucial drivers of economic growth.

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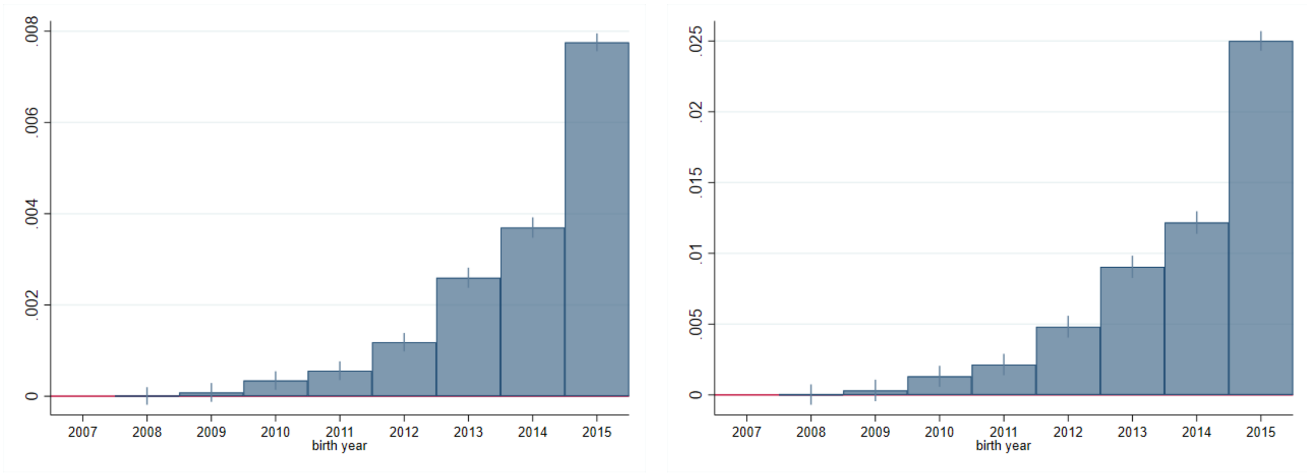
Tables and Figures

Figure 1: **Share of innovative startups by incorporation year**

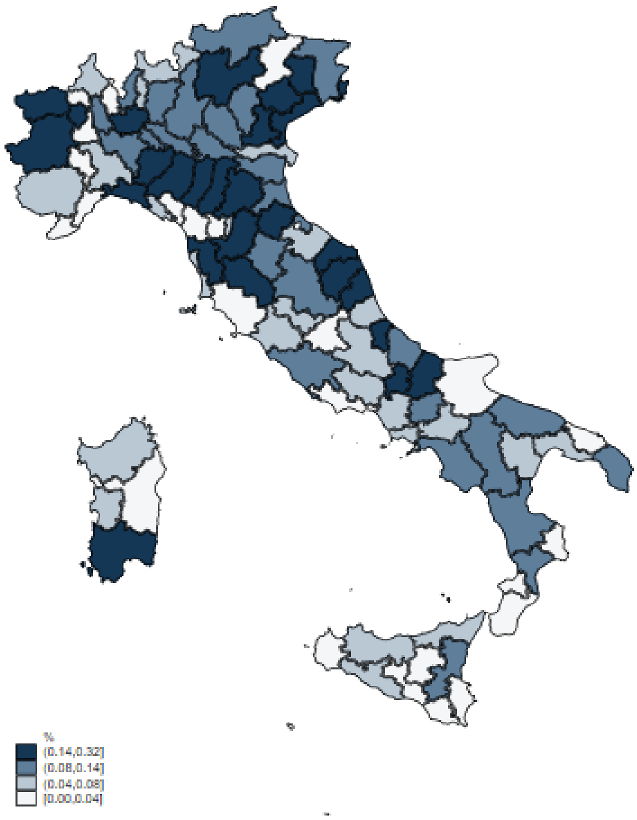


(a) LCs $age \leq 5$

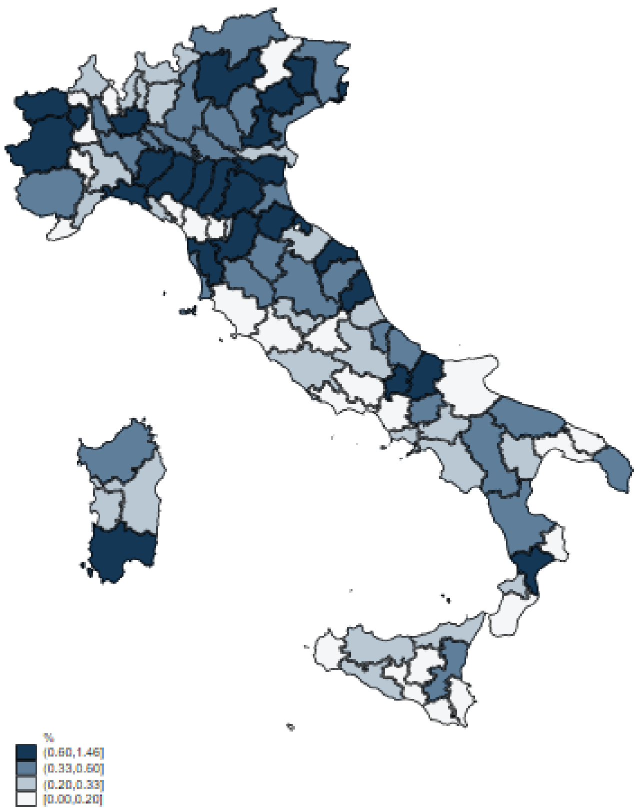
(b) LCs $age \leq 5$ with intangible assets

Notes: Administrative data and Innovative Startup Registry data for firms incorporated between 2007 and 2015. Number of innovative startups that registered in the framework over the total number of LCs younger than age 5 (Panel a) and with positive intangible assets by age 2 (Panel b) by incorporation year.

Figure 2: **Share of innovative startups across provinces**



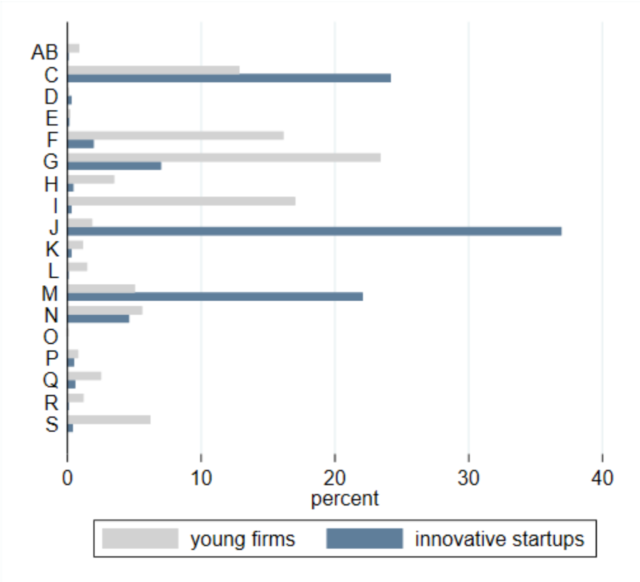
(a) LCs $age \leq 5$



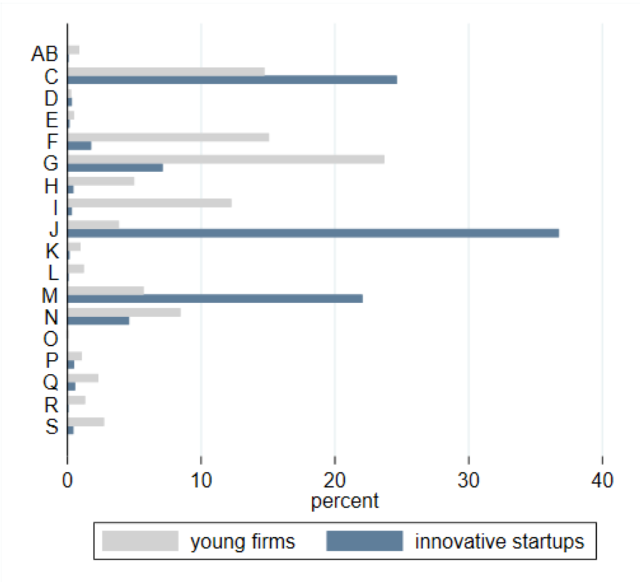
(b) LCs $age \leq 5$ with intangible assets

Notes: Administrative data and Innovative Startup Registry data for LCs incorporated between 2007 and 2015. Number of innovative startups over the total number of LCs younger than age 5 in the province (Panel a) and with positive intangible assets by age 2 (Panel b). Graphs show average percentages for years 2013 and 2015.

Figure 3: **Industry distribution of innovative and other startups**



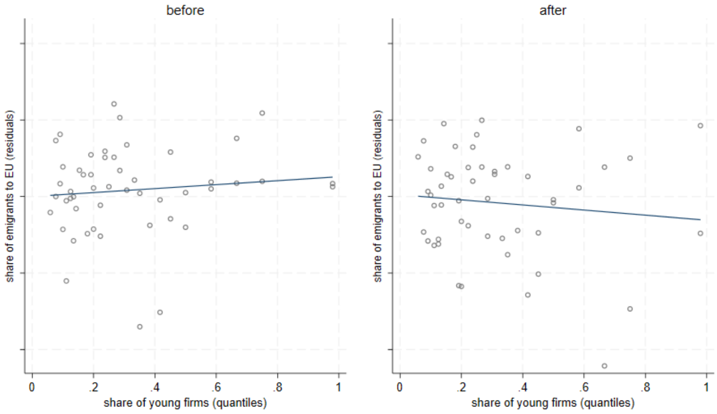
(a) LCs $age \leq 5$



(b) LCs $age \leq 5$ with intangible assets

Notes: Administrative data and Innovative Startup Registry data for firms incorporated between 2007 and 2015. Industry distribution for LCs younger than age 5 (grey bars) and for innovative startups (blue bars) reported as percentages. Panel (a) shows all LCs and Panel (b) LCs with positive intangible assets by age 2 Sectors include: A = Agriculture, forestry and fishing; B = Mining and quarrying; C = Manufacturing; D = Electricity, gas, etc.; E = water, waste, etc.; F = Construction; G = Wholesale and retail trade; H = Transportation and storage; I = Accommodation and food service activities; J = Information and communication; K = Finance and insurance; L = Real estate; M = Professional and Scientific; N = Administrative and support activities; P = Education; Q = Health; R = Arts, entertainment and recreation; other = residual sectors.

Figure 4: **Young LCs and emigration to other EU countries, before and after the SUA**



Notes: Administrative data for years between 2005 and 2015. The x-axis in both panels reports the quantiles (30) of the distribution of the share of LCs younger than age 5 out of the total number of LCs in sectors J&M in year 2002 in the same municipality. The y-axis reports the residual share of emigrants to other EU countries averaged by quantiles. The residuals share of emigrants is obtained by regressing the share of emigrants to other EU countries (normalized by the 2002 population) on municipality and province-by-year fixed effects for years 2005–2011, then predicted for all years.

Figure 5: **Treated and control municipalities matched with the algorithm**

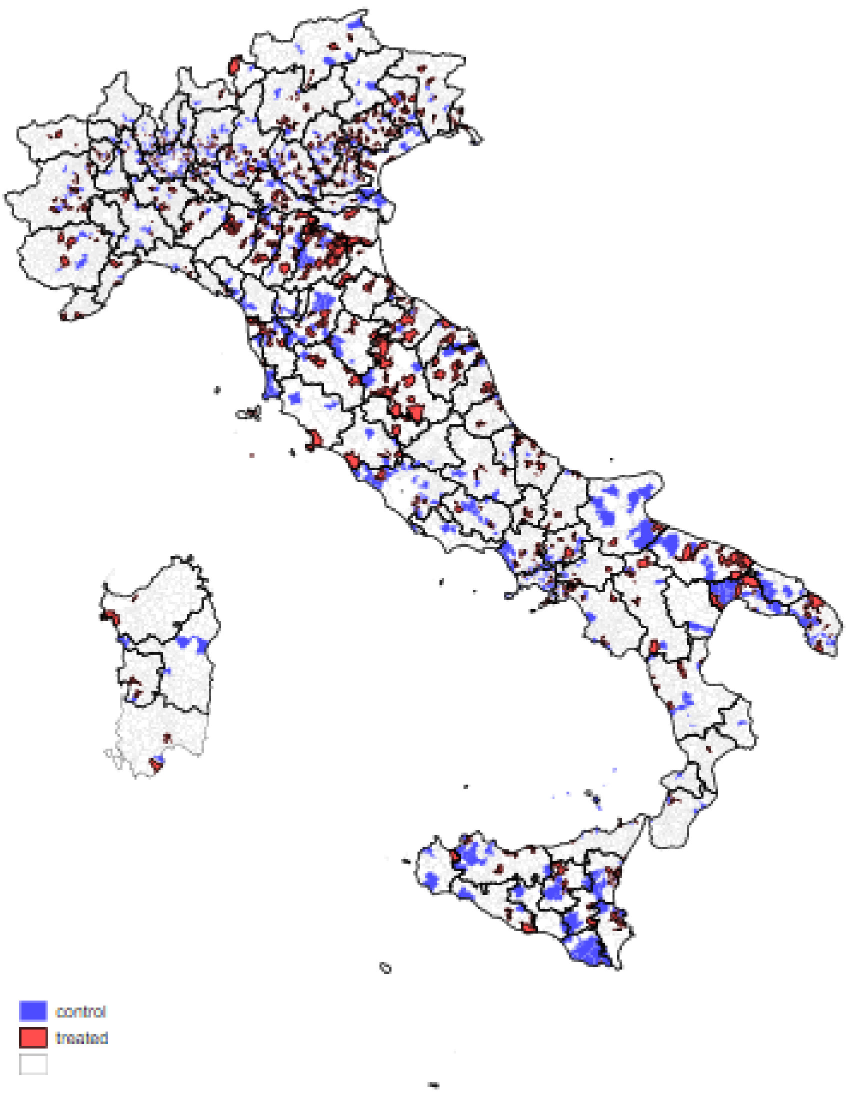
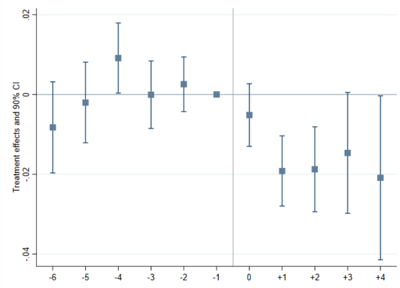
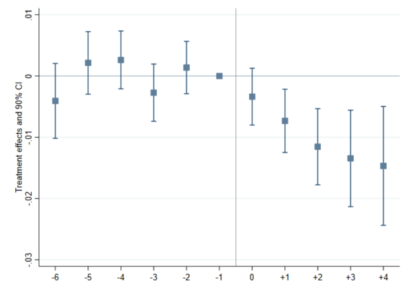


Figure 6: **Effect of SUA on emigrant shares (all, EU and extra-EU)**

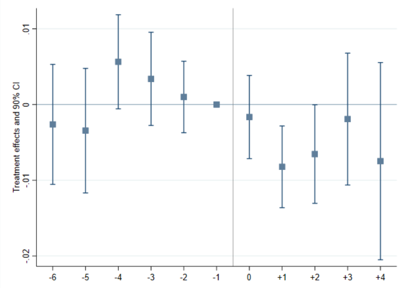
all cohorts



(a) all emigrants

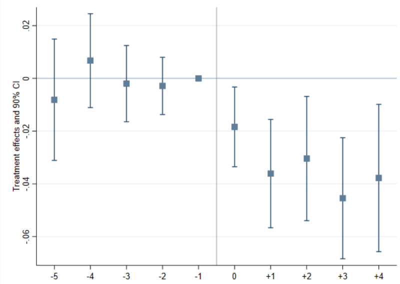


(b) emigrants to EU

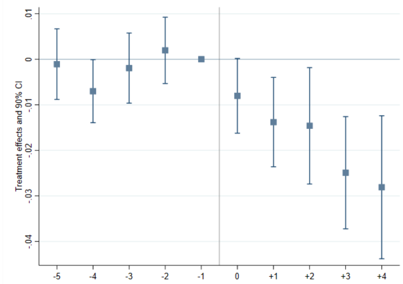


(c) emigrants to extra-EU

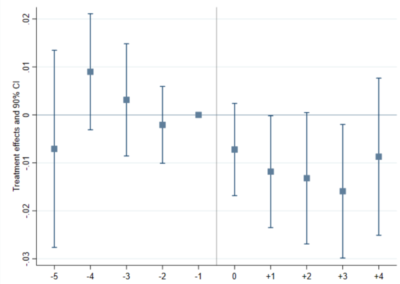
2012 cohort



(d) all emigrants

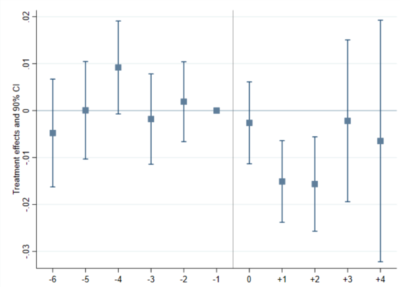


(e) emigrants to EU

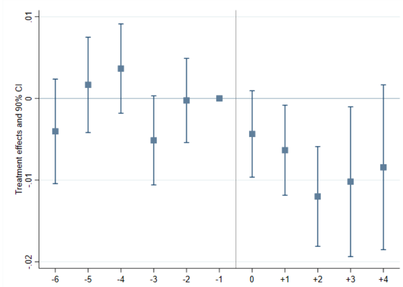


(f) emigrants to extra-EU

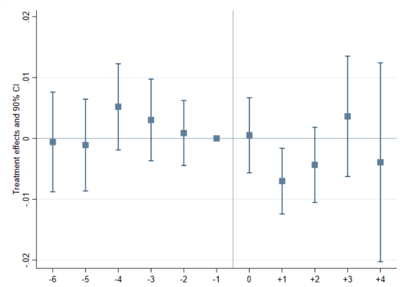
2013–2015 cohorts



(g) all emigrants



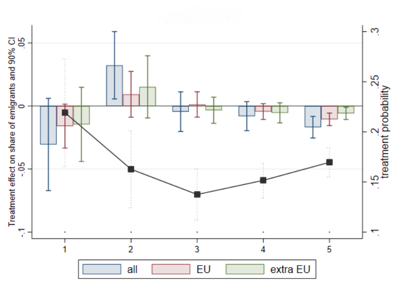
(h) emigrants to EU



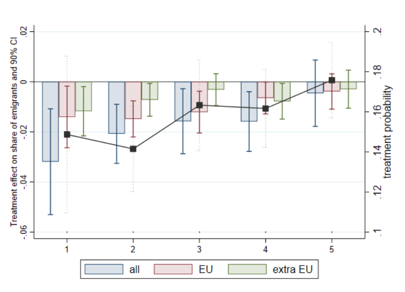
(i) emigrants to extra-EU

Notes:Panels a–i display the regression coefficients and the associated 90% confidence intervals for the difference between the treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level shares of all emigrants (panels a, d, g), shares of emigrants to other EU countries (panels b, e, h) and shares of emigrants to extra-EU countries (panels c, f, i). The shares of emigrants are with respect to the 2002 population and reported as percentages. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The x-axis indexes event time. The quantitative results are summarized in Table 2. I report the results for all cohorts (panels a–c), for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012 (panels d–f) and for cohorts 2013, 2014 and 2015 (panels g–i) for innovative startups incorporated after 2012. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

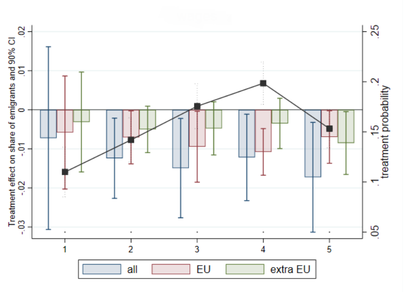
Figure 7: **Heterogeneity of the effect of SUA on emigration, all cohorts**



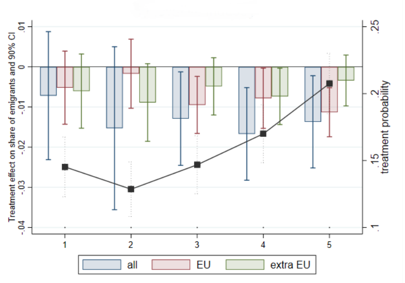
(a) population



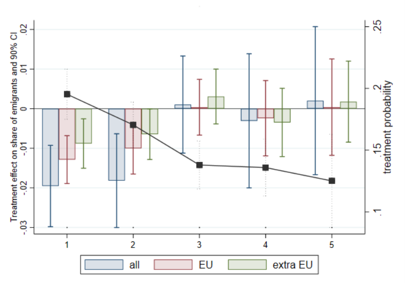
(b) share of workers ≤ 30



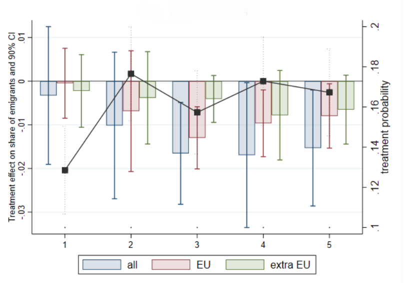
(c) *log* wages



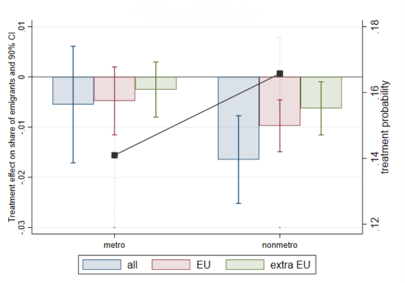
(d) share of LCs in J&M



(e) new LCs



(f) share of provincial patents

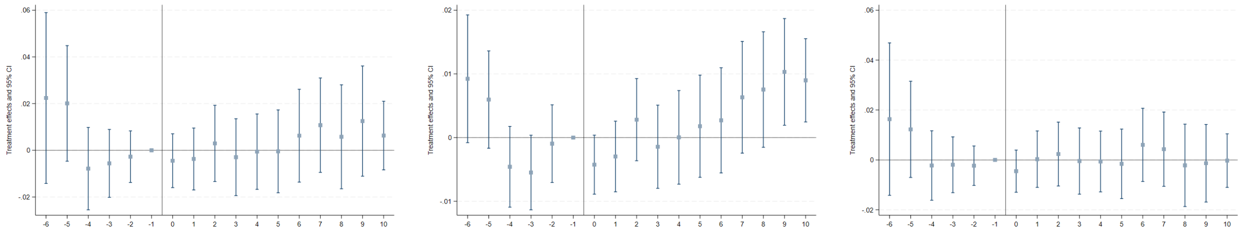


(g) metropolitan area

Notes:The Panels display the treatment effects of the SUA on the share of emigrants estimated using a variation of model 2, where $Policy_{mk} \times Treated_m$ is interacted with the quintiles of the distribution of corresponding municipality level characteristics. The bins shows the treatment effects in each quintile in which the outcome variable is the municipality-level share of all emigrants (blue), share of emigrants to other EU countries (red) or share of emigrants to extra-EU countries (green). I also report the associated 90% confidence intervals, and the standard errors are clustered at the municipality level. I report the results for all cohorts. The shares of emigrants are with respect to the 2002 population and reported as percentages. The black line shows the unconditional probability that a municipality in each quintile participates in the framework. Panel (a) shows the heterogeneity with respect to the 2007 population of the municipalities in our sample, Panel (b) with respect to the 2007 share of workers aged ≤ 30, Panel (c) with respect to the 2007 *log* average wages, Panel (d) with respect to the 2007 share of firms in the innovative sectors (J&M) out of the total number of firms, Panel (e) with respect to the 2007 share of LCs incorporated in that year over the total number of LCs, Panel (f) with respect to the share of patents in the province over the total number of patents in Italy averaged between 2007 and 2012 and Panel (g) shows the coefficients of the treatment dummy interacted with a dummy that takes the value of 1 if the municipality is part of a metropolitan area. Metropolitan areas are around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

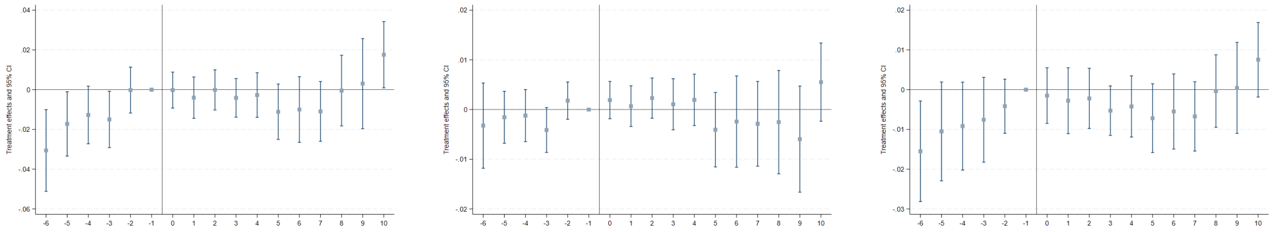
Figure 8: **Placebo effect of new firms on emigrant shares before 2012**

New firm in sectors J&M



(a) all emigrants (b) emigrants to EU (c) emigrants to extra-EU

%50 increase in number of new firms in sectors J&M

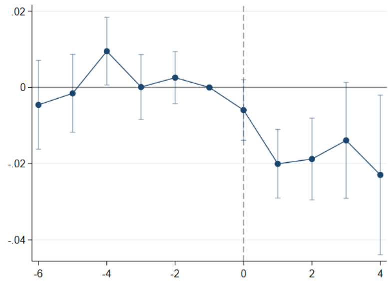


(d) all emigrants (e) emigrants to EU (f) emigrants to extra-EU

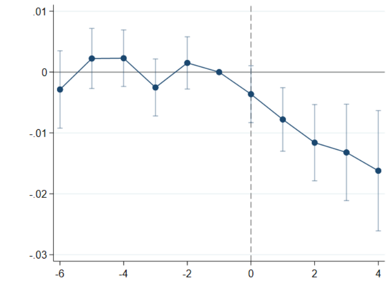
Notes:Panels a–f display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which a firm in sectors J&M was incorporated, i.e., the $\hat{\beta}_k^{bf}$ from equation 3. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level shares of all emigrants (panels a, d), shares of emigrants to other EU countries (panels b, e) and shares of emigrants to extra-EU countries (panels c, f). The x-axis indexes event time. I report the results for the first version of the placebo (panels a–c) where a municipality is treated when at least one new LC is incorporated in sectors J&M. The results in panels d–f shows the second version of the placebo treatment for which a municipality is treated if there is at least a 50% increase in the number of new LCs in sectors J&M. The shares of emigrants are with respect to the 2002 population and reported as percentages. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure 9: **Effect of SUA on emigrant shares using estimator by Sun and Abraham (2021) (all, EU and extra-EU)**

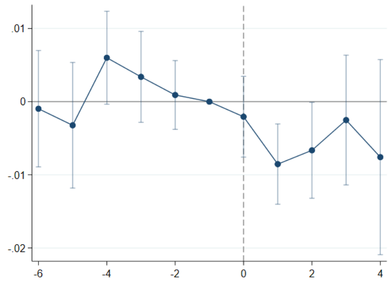
all cohorts



(a) all emigrants

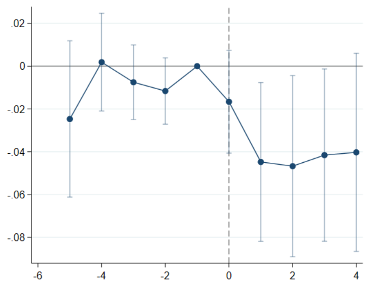


(b) emigrants to EU

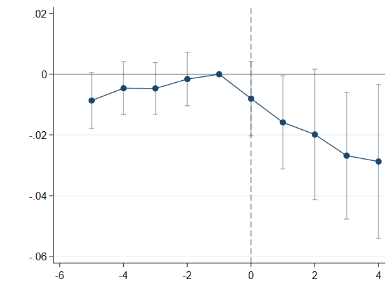


(c) emigrants to extra-EU

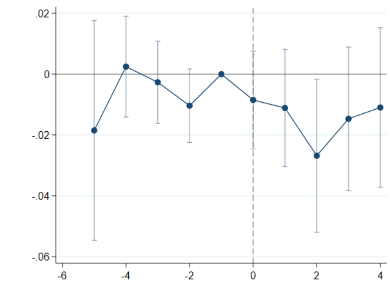
2012 cohort



(d) all emigrants

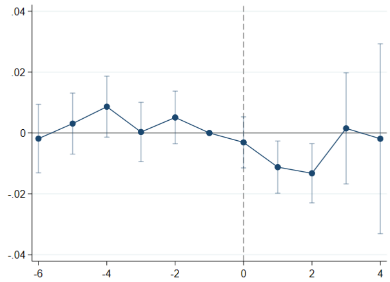


(e) emigrants to EU

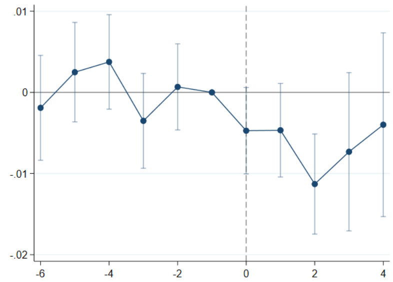


(f) emigrants to extra-EU

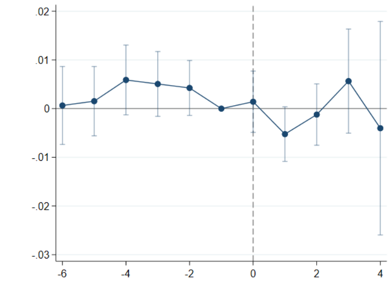
2013–2015 cohorts



(g) all emigrants



(h) emigrants to EU

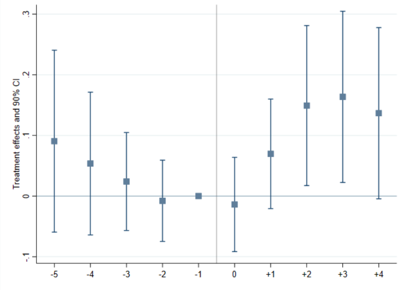


(i) emigrants to extra-EU

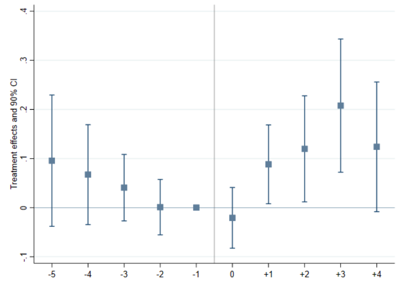
Notes:Panels a–i display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation 1. The estimator by Sun and Abraham (2021) is used. The outcome variables are municipality-level shares of all emigrants (panels a, d, g), shares of emigrants to other EU countries (panels b, e, h) and shares of emigrants to extra-EU countries (panels c, f, i). The shares of emigrants are with respect to the 2002 population and reported as percentages. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The x-axis indexes event time. The quantitative results are summarized in Table 2. I report the results for all cohorts (panels a–c), for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012 and event year is $e = 2012$ (panels d–f) and for cohorts 2013, 2014 and 2015 with event year $e = b$ (panels g–i). Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure 10: **Effect of SUA on employment of young LCs in sector J&M**

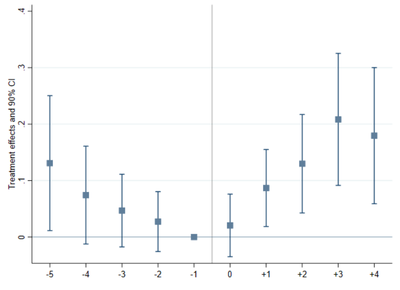
total employment (headcounts) in:



(a) LCs younger than 3

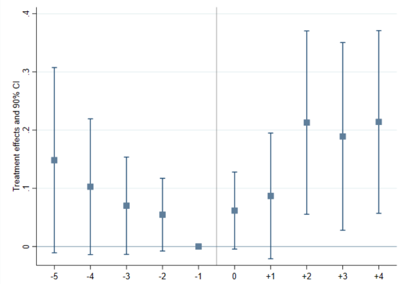


(b) LCs younger than 4

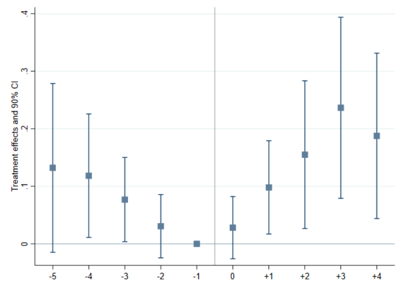


(c) LCs younger than age 5

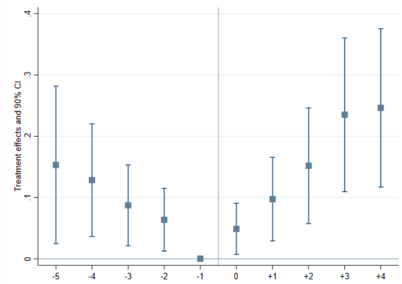
total employment (FTE) in:



(d) LCs younger than 3



(e) LCs younger than 4

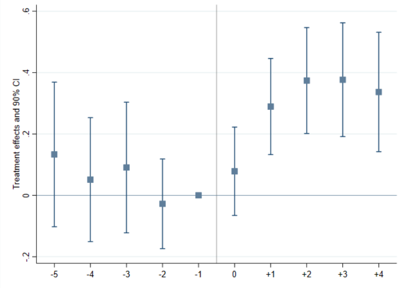


(f) LCs younger than age 5

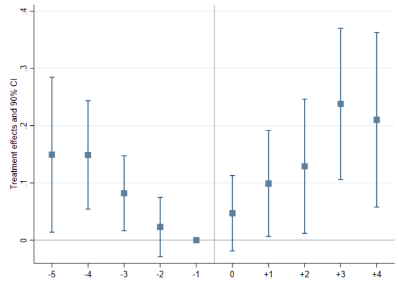
Notes:Panels a–f display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation 1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality $log(headcount + 1)$ in LCs younger than 3, 4 and 5, respectively in panels a–c and $log(FTE + 1)$ in panels d–f, where FTE are full-time equivalent workers measured using hours worked. Sectors J&M are only considered. The x-axis indexes event time. The quantitative results are summarized in Table A.7. I report the results for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012 and event year $e = 2012$. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure 11: **Effect of SUA on type of employment of LCs in J&M**

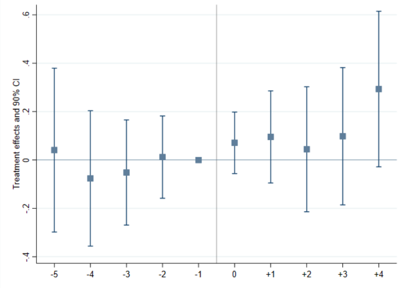
headcounts characteristics in firms age 5 and younger



(a) blue collars

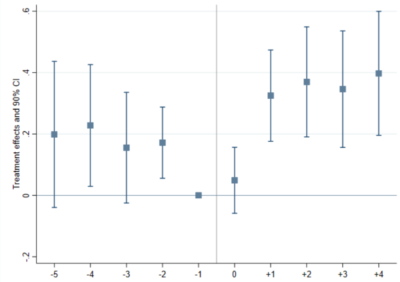


(b) white collars

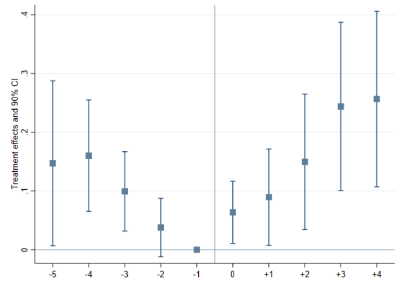


(c) local workers

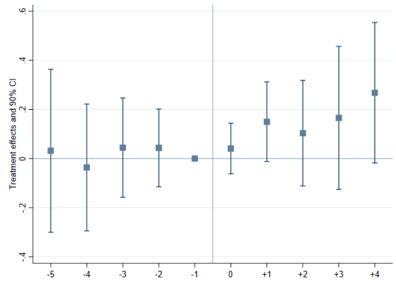
FTE characteristics in firms age 5 and younger



(d) blue collars



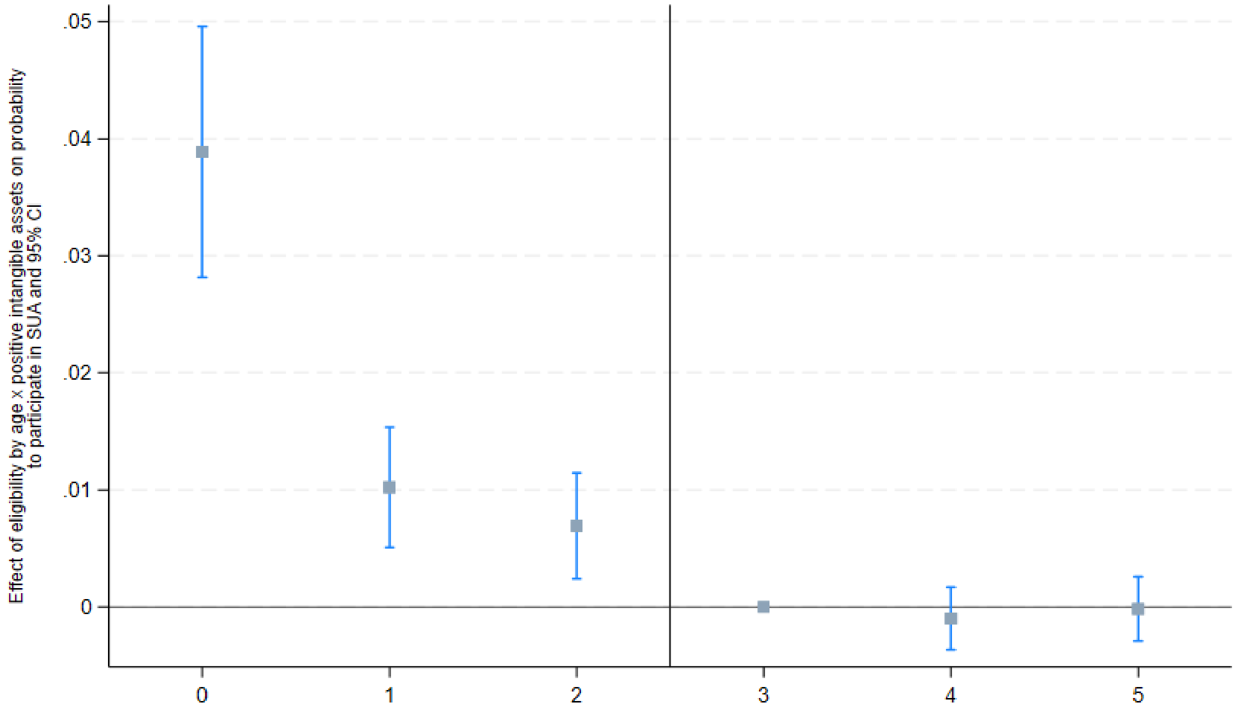
(e) white collars



(f) local workers

Notes:Panels a–c display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality $log(headcount + 1)$ in firms younger than age 5, respectively in panels a–c and $log(FTE + 1)$ in panels d–f, where FTE are full-time equivalent workers measured using hours worked. Local workers live in the same municipality where the LC is headquartered. Sectors J&M are only considered. The x-axis indexes event time. The quantitative results are summarized in Table A.7. I report the results for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012 and event-year $e = 2012$. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure 12: **Effect of being eligible × positive intangible assets on the probability to participate in the framework**



Notes: The graph reports the coefficients $\hat{\delta}_1^j$ estimated from equation 4 for all ages $a \in [0, 5]$ using the probability of being treated in each age year as outcome. The omitted age year is 3; therefore, all of the results are relative to that age year. The vertical bars indicate the 95% confidence interval based on standard error clustered at the municipality level.

Table 1: **Descriptive Statistics**

	All	Treated	Control	Diff.(3)-(2)
	(1)	(2)	(3)	(4)
Population in 2002	14,140.34	14,008.66	14,272.02	263.35
	(11985.43)	(11795.74)	(12180.90)	
Share of all emigrants(%)	0.076	0.075	0.076	0.001
	(0.089)	(0.094)	(0.083)	
Share of non-EU (%)	0.035	0.033	0.036	0.003
	(0.057)	(0.055)	(0.059)	
Share of EU (%)	0.040	0.040	0.040	0.000
	(0.047)	(0.047)	(0.046)	
N of LCs $age \leq 5$	53.75	55.16	52.34	-2.821
	(53.86)	(56.81)	(50.75)	
HC in LCs $age \leq 5$	710.72	698.89	722.56	23.67
	(637.66)	(619.15)	(655.97)	
FTE in LCs ($age \leq 5$)	365.98	363.86	368.11	4.249
	(319.87)	(316.29)	(323.67)	
N of in newly incorporated LCs	51.40	50.44	52.363	1.921
	(42.18)	(42.15)	(42.23)	
HC in newly incorporated LCs	102.54	100.84	104.25	
	(115.95)	(127.31)	(103.43)	3.410
FTE in newly incorporated LCs	37.30	37.94	36.67	-1.275
	(59.60)	(74.99)	(38.56)	
N of LCs	123.43	128.59	118.27	-10.313
	(109.92)	(114.61)	(104.87)	
Metropolitan areas	0.197	0.172	0.222	0.050**
	(0.398)	(0.378)	(0.416)	
North	0.551	0.587	0.515	-0.072**
	(0.498)	(0.493)	(0.500)	
Observations	1,162	581	581	

Notes: Matched municipalities, AIRE and INPS data 2007–2017:Treated municipalities are matched to out-of-LLM potential control municipalities. All statistics are computed across municipality-year observations in the year before the event (defined as $e = 2012$ for municipalities in which the innovative startup was incorporated before the SUA and as $e = b$ where b is the incorporation year of the oldest innovative startup for the 2013–2015 cohorts). Column 1 reports the statistics for the full matched sample, while columns 2 and 3 reports statistics for the treated and control municipalities, respectively. The difference in column 4 is calculated as (3)-(2). Standard errors are reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. I report municipality-level shares of emigrants with respect to the 2002 population as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Only limited companies (LCs) are considered. Newly incorporated LCs are incorporated in the year before the event. Metropolitan areas shows the share of municipalities in metropolitan areas around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. According to the official definition, metropolitan areas' territorial entities recognized by Article 114 of the Italian Constitution are made up of an aggregate of neighboring municipalities. Introduced with the reform of Title V of the Constitution in 2001, metropolitan cities are recognized as large-area territorial entities defined by the aggregation of neighboring municipalities, similar to the provinces. HC stands for headcount.

Table 2: **Effect of SUA on the share of emigrants**

Dependent variable	Share of emigrants		
	all	EU	extra-EU
	(1)	(2)	(3)
Panel A: all cohorts			
Policy	-0.014***	-0.009***	-0.005**
	(0.005)	(0.003)	(0.003)
Mean outcome (%)	0.075	0.040	0.033
Observations	12,782	12,782	12,782
Panel B: 2012 cohort			
Policy	-0.025*	-0.014*	-0.007
	(0.015)	(0.008)	(0.008)
Mean outcome (%)	0.064	0.030	0.030
Observations	2,464	2,464	2,464
Panel C: 2013–2015 cohorts			
Policy	-0.010**	-0.007**	-0.004
	(0.005)	(0.003)	(0.003)
Mean outcome (%)	0.078	0.042	0.034
Observations	10,388	10,388	10,388
Municipality FE	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes

Notes: Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. All cohorts include all municipalities in which a startup that registered was born between 2010 and 2015. Cohort 2012 includes only municipalities in which the innovative startup was incorporated before 2012 with event year $e = 2012$. The 2013–2015 cohorts include all the other cohorts with event year $e = b$. Standared errors are clustered at the municipality level and reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Table 3: **Effect of SUA on firm level outcomes**

	Estimate	s.e.	*log*	levels	N	contr.
	(1)	(2)	(3)	(4)	(5)	(6)
	A. First Stage					
Prob. to participate	0.023***	(0.003)			64598	
	B. Employment outcomes (IV)					
All wokers						
log HC	1.437	(1.701)	1.581	3.860	64598	5.55
log FTE	1.990*	(1.160)	1.143	2.136	64598	4.25
by occupation:						
log Blue collars (HC)	0.981	(1.032)	0.71	1.034	64598	1.01
log white collars (HC)	0.205	(1.941)	0.974	1.649	64598	0.34
log managers (HC)	0.002	(0.743)	0.067	0.069	64598	0.00
log apprentices (HC)	2.482***	(0.709)	0.303	0.354	64598	0.88
log Blue collars (FTE)	0.699	(0.659)	0.494	0.639	64598	0.45
log white collars (FTE)	1.113	(1.243)	0.701	1.016	64598	1.13
log managers (FTE)	0.034	(0.618)	0.048	0.049	64598	0.00
log apprentices (FTE)	1.910***	(0.469)	0.182	0.200	64598	0.38
Local wokers						
log HC	-0.005	(1.731)	0.612	0.844	64598	0.00
log FTE	0.607	(1.051)	0.425	0.530	64598	0.32
by occupation:						
log Blue collars (HC)	0.367	(0.545)	0.272	0.313	64598	0.11
log white collars (HC)	-0.898	(2.129)	0.335	0.398	64598	-0.36
log managers (HC)	-0.593	(0.428)	0.015	0.015	64598	-0.01
log apprentices (HC)	1.041***	(0.368)	0.088	0.092	64598	0.10
log Blue collars (FTE)	0.402	(0.438)	0.183	0.201	64598	0.08
log white collars (FTE)	-0.012	(1.200)	0.236	0.266	64598	0.00
log managers (FTE)	-0.522*	(0.313)	0.011	0.011	64598	-0.01
log apprentices (FTE)	0.719***	(0.242)	0.052	0.053	64598	0.04

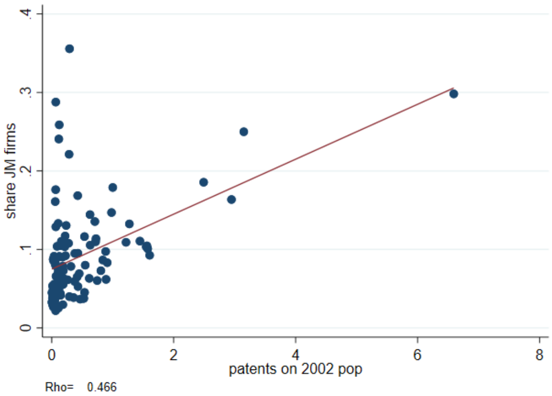
Notes: Panel A reports the estimates of the coefficient λ from specification 6 and its associated municipality-level cluster standard error in parenthesis. Panels B reports the γ_{IV} coefficients estimated from equation 5 and their associated standard errors clustered at the municipality level in parenthesis for a set of different firm-level employment outcomes. Mean outcome is the firm-level average outcome (measured in *log* or in levels) in nontreated firms at age $a \leq 2$. contr. is the contribution in number of extra workers in treated firms computed by multiplying the mean outcome levels by $\hat{\gamma}^{IV}$. HC stands for headcount. *log* refers to mean outcome, while levels refers to the outcome before the log-transformation. Standard errors are clustered at the municipality level and reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%.

A Appendix

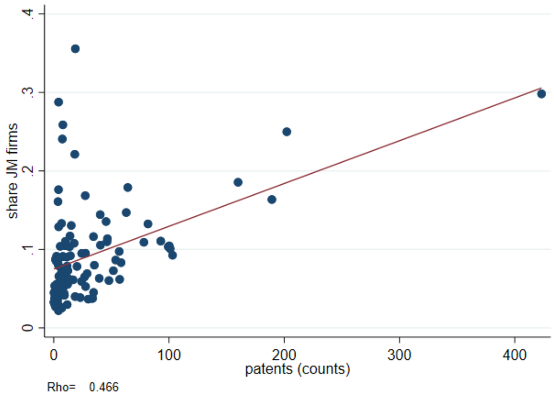
This Appendix provides additional tables and figures that are also discussed in the paper.

A.1 Appendix Figures

Figure A.1: **Correlation between share of firms in sectors J&M and patenting activity in the province**



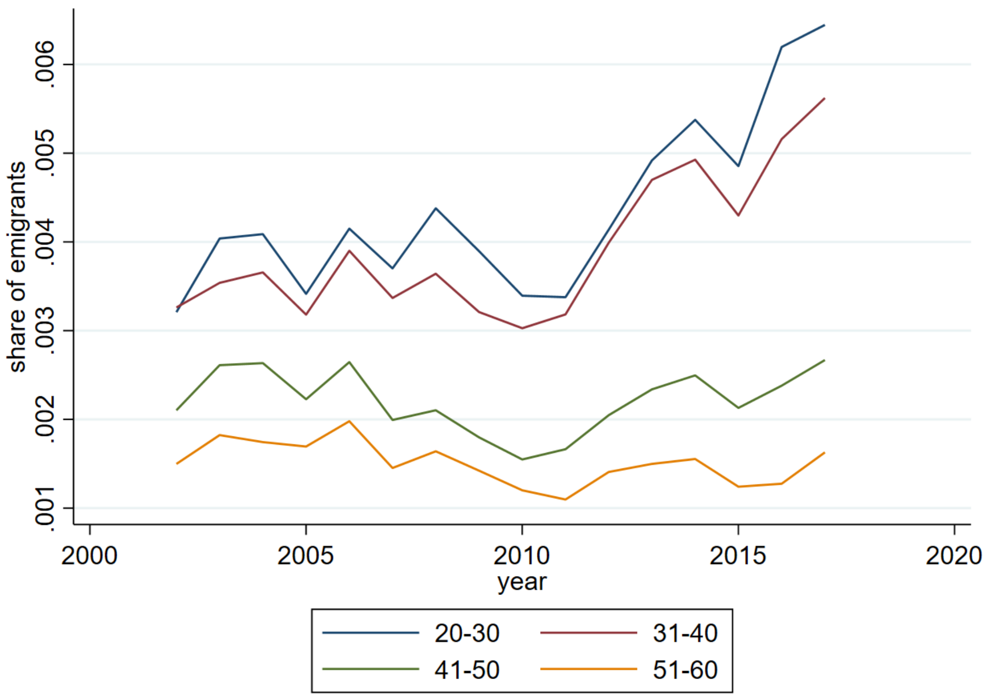
(a) Share of Firms in J&M and number of patent per inhabitant in the province



(b) Share of Firms in J&M and number of patents in the province

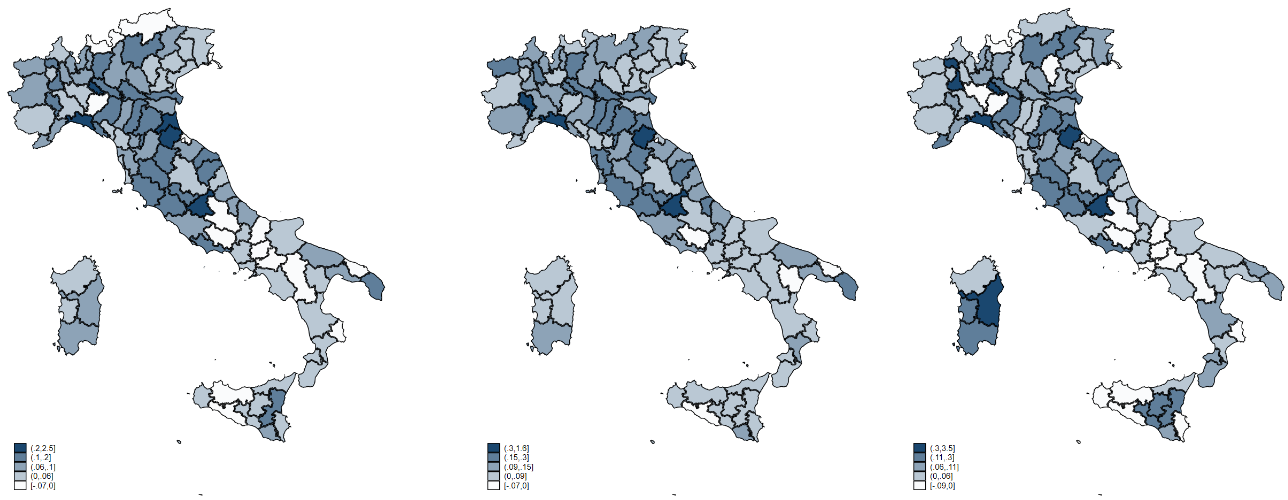
Notes: Administrative data on LCs between 2007 and 2017. Share of firms is the number of firms in sectors J&M on total number of firms in the province, while the patenting activity is measured as share of patents is the number of patents in the population (Panel a) or the total number of patents (Panel b).

Figure A.2: **Share of emigrants by age-group**



Notes: Registry of Italian Citizens Abroad (AIRE) Population data from ISTAT database. Years 2002 to 2017. Share of emigrants defined as the $\frac{Emigrants_{at}}{Pop_{at}}$ for each age group a and year t.

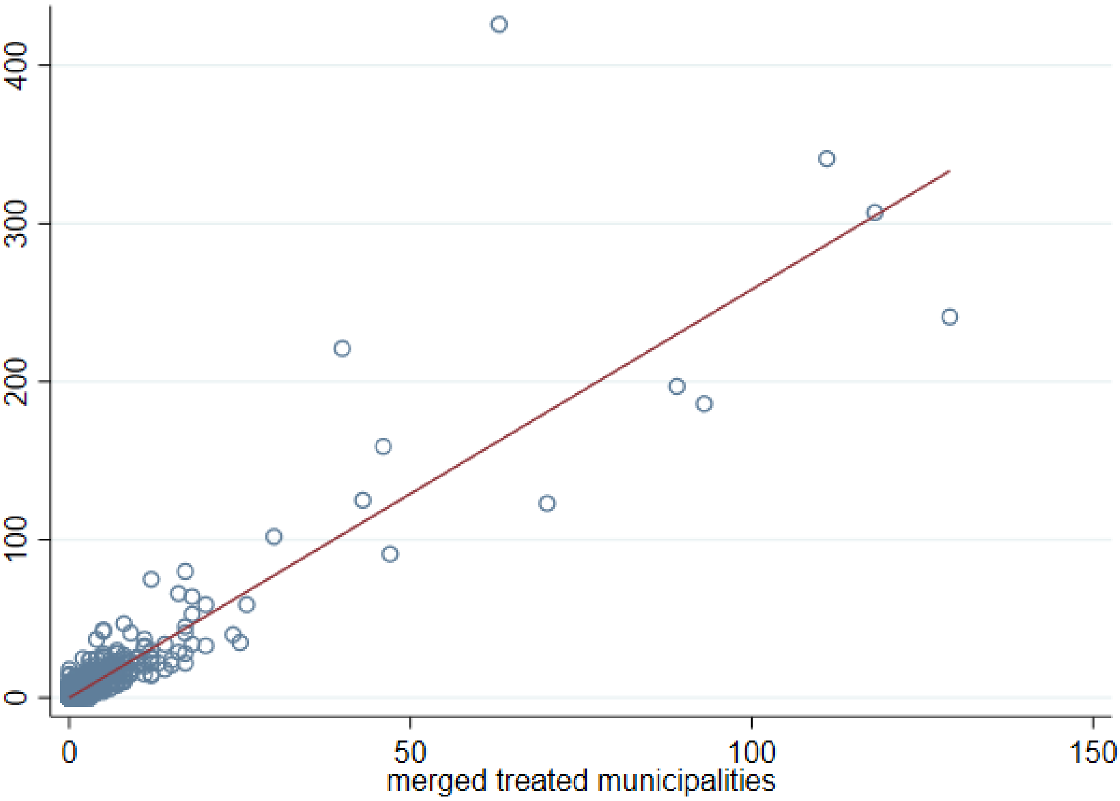
Figure A.3: **Change in the flow of emigrants**



(a) all emigrants (b) emigrants to EU (c) emigrants to extra-EU

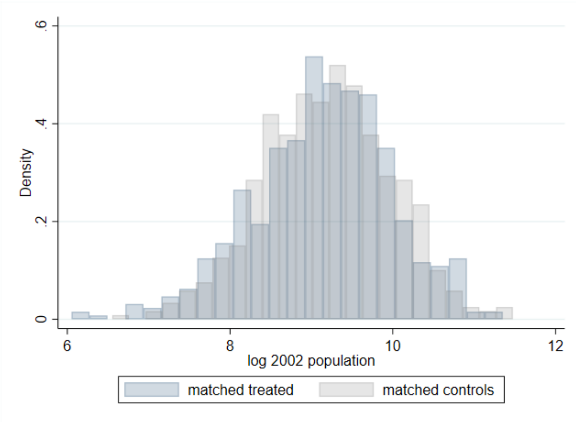
Notes: Registry of Italian citizens residing abroad, years 2013 to 2017. Percentage growth in the number of emigrants by destination who moved between 2013 and 2017 by province of origin.

Figure A.4: **Correlation between municipalities where innovative startup is merge with INPS data and all municipalities**

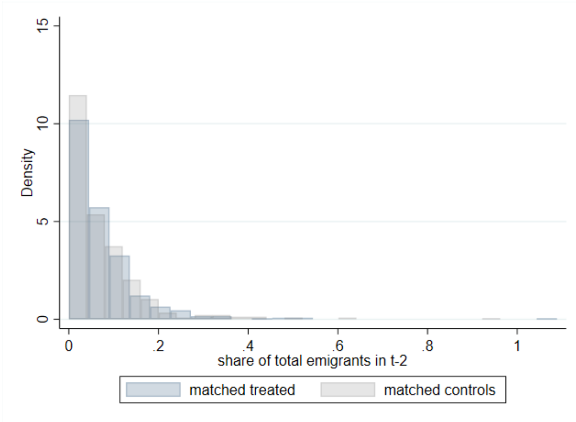


Notes: INPS data and Registry of Innovative startups. The x-axis reports the number of innovative startups in the municipalities identified as treated following the firm-level merge with Social Security data, while the y-axis reports the number of innovative startups in the municipalities that were ever exposed to the SUA irrespective of the merge with the Social Security data.

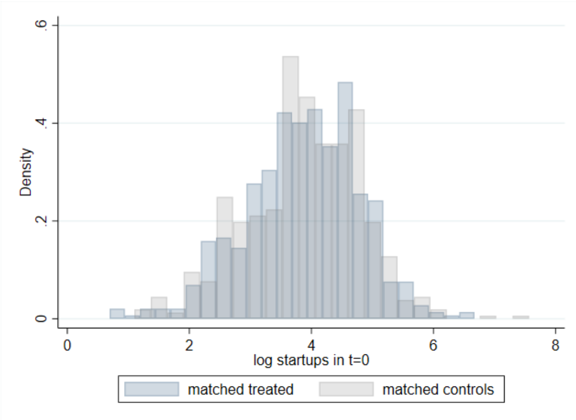
Figure A.5: Distribution of Log Wages and Log Size at t-1



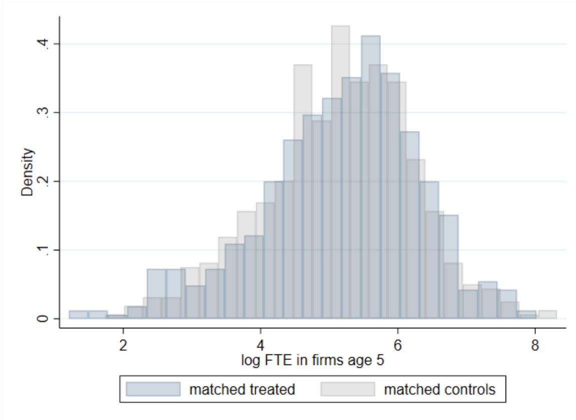
(a) *log* Population in 2002



(b) Share of total emigrants in $t-2$



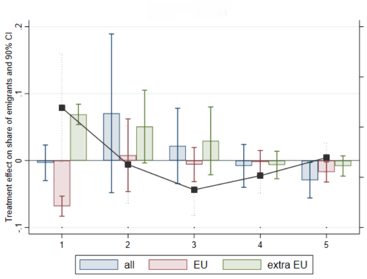
(c) New LCs in $t = 0$



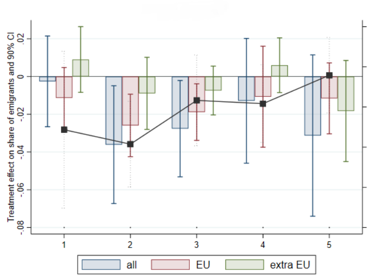
(d) *log* of FTE in LCs $age \leq 5$ in $t-1$

Notes: Matched municipality sample, INPS and AIRE data years 2007–2017. Panel a show the distribution of *log* population in 2002, Panel b shows the share of total emigrants in $t-1$, Panel c shows the the distribution of *log* of startups in $t-1$, Panel d shows the *log* of full-time equivalents employed in LCs $age \leq 5$ in $t-1$.

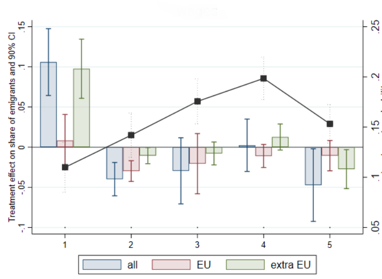
Figure A.6: Heterogeneity of the effect of SUA on emigration, 2012 cohort



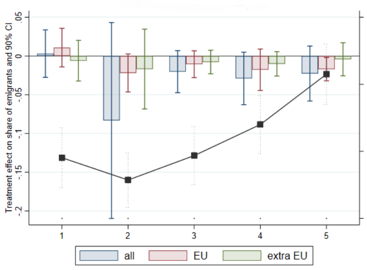
(a) population



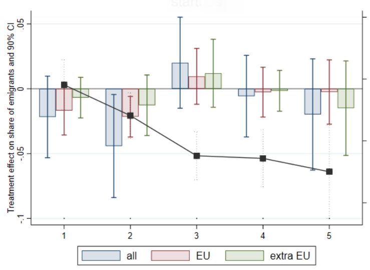
(b) share of workers ≤ 30



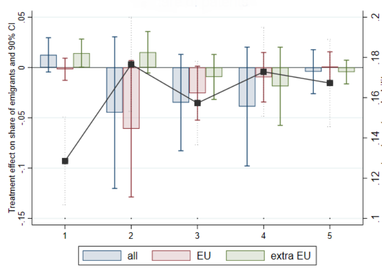
(c) *log* wages



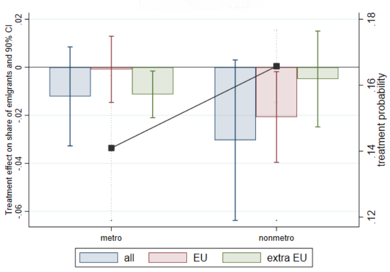
(d) share of LCs in J&M



(e) new LCs



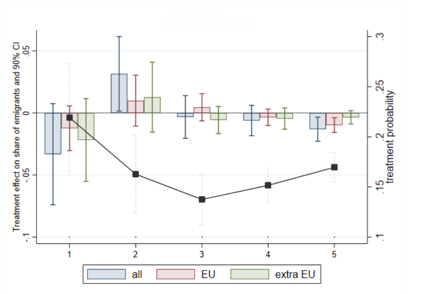
(f) share of provincial patents



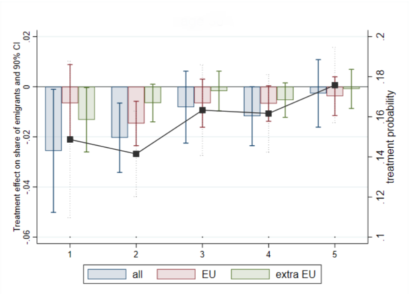
(g) metropolitan area

Notes:The Panels display the treatment effects of the SUA on the share of emigrants estimated using a variation of model 2, where $Policy_{mk} \times Treated_m$ is interacted with the quintiles of the distribution of corresponding municipality level characteristics. The bins shows the treatment effects in each quintile where the outcome variable is the municipality-level share of all emigrants (blue), share of emigrants to other EU countries (red) or share of emigrants to extra-EU countries (green). I also report the associated 90% confidence intervals, and standard errors are clustered at the municipality level. I report the results for the 2012 cohort. The shares of emigrants are with respect to the 2002 population and reported as percentages. The black line shows the unconditional probability that a municipality in each quintile participates in the program. Panel (a) shows the heterogeneity with respect to the 2007 population of the municipalities in our sample, Panel (b) with respect to the 2007 share of workers aged ≤ 30, Panel (c) with respect to the 2007 *log* average wages, Panel (d) with respect to the 2007 share of LCs in the innovative sectors (J&M) out of the total number of LCs, Panel (e) with respect to the 2007 share of LCs incorporated in that year over the total number of LCs, Panel (f) with respect to the share of patents in the province over the total number of patents in Italy averaged between 2007 and 2012 and Panel (g) shows the coefficients of the treatment dummy interacted with a dummy that takes the value of 1 if the municipality is part of a metropolitan area. Metropolitan areas are around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

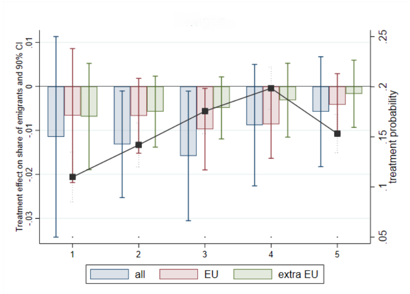
Figure A.7: Heterogeneity of the effect of SUA on emigration, 2013–2015 cohort



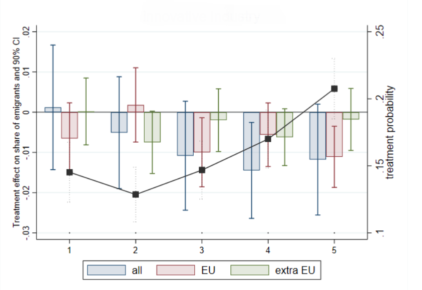
(a) population



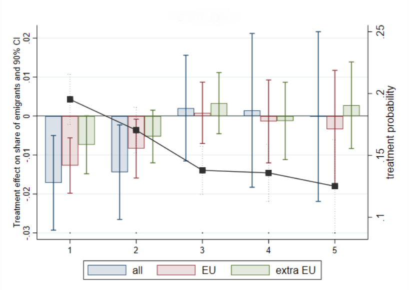
(b) share of workers ≤ 30



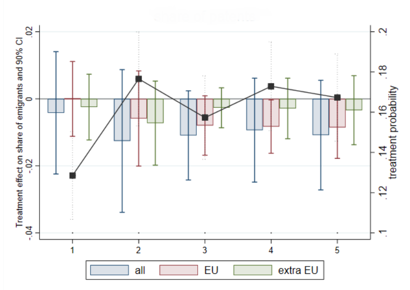
(c) *log* wages



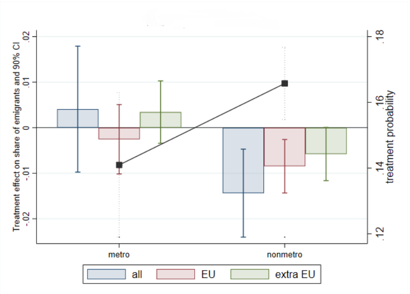
(d) share of LCs in J&M



(e) new LCs



(f) share of provincial patents

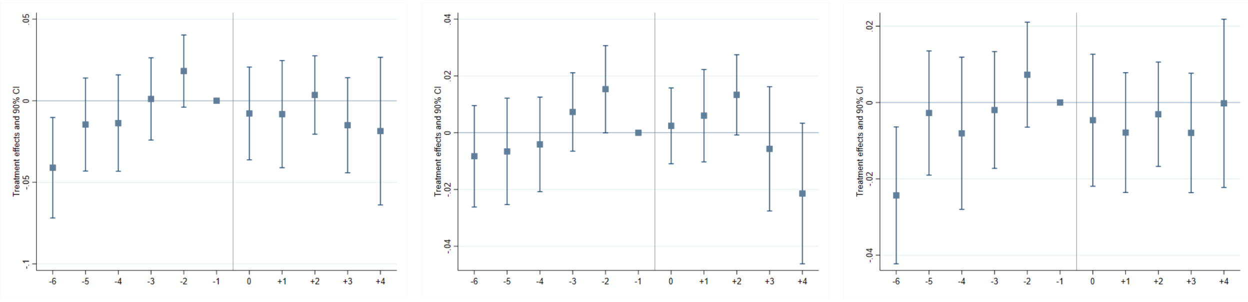


(g) metropolitan area

Notes:The Panels display the treatment effects of the SUA on the share of emigrants estimated using a variation of model 2, where $Policy_{mk} \times Treated_m$ is interacted with the quintiles of the distribution of corresponding municipality level characteristics. The bins shows the treatment effects in each quintile where the outcome variable is the municipality-level share of all emigrants (blue), share of emigrants to other EU countries (red) or share of emigrants to extra-EU countries (green). I also report the associated 90% confidence intervals and standard errors are clustered at the municipality level. I report the results for the 2013–2015 cohorts. The shares of emigrants are with respect to the 2002 population and reported as percentages. The black line shows the unconditional probability that a municipality in each quintile participates in the program. Panel (a) shows the heterogeneity with respect to the 2007 population of the municipalities in our sample, Panel (b) with respect to the 2007 share of workers aged ≤ 30, Panel (c) with respect to the 2007 *log* average wages, Panel (d) with respect to the 2007 share of LCs in the innovative sectors (J&M) out of the total number of LCs, Panel (e) with respect to the 2007 share of LCs incorporated in that year over the total number of LCs, Panel (f) with respect to the share of patents in the province over the total number of patents in Italy averaged between 2007 and 2012 and Panel (g) shows the coefficients of the treatment dummy interacted with a dummy that takes the value of 1 if the municipality is part of a metropolitan area. Metropolitan areas are around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure A.8: **Effect of SUA on emigrant shares (all, EU and extra-EU)- including all municipalities ever exposed to the SUA**

all cohorts

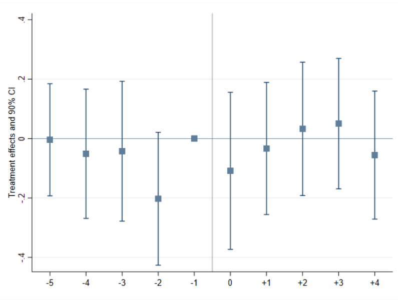


(a) all emigrants (b) emigrants to EU (c) emigrants to extra-EU

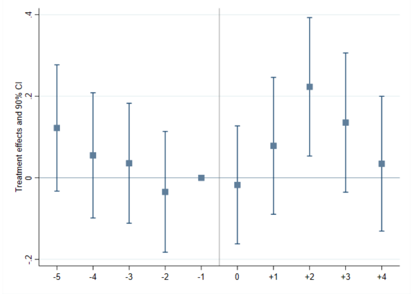
Notes:Panels a–c display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level shares of all emigrants (panels a, d, g), shares of emigrants to other EU countries (panels b, e, h) and shares of emigrants to extra-EU countries (panels c, f, i). The shares of emigrants are with respect to the 2002 population and reported as percentages. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. The x-axis indexes event time. I report the results for all cohorts and include all municipalities that have ever been exposed to the framework irrespective of finding a match in the Social Security data for the innovative startup. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Figure A.9: Effect of SUA on employment of young LCs in sectors J&M

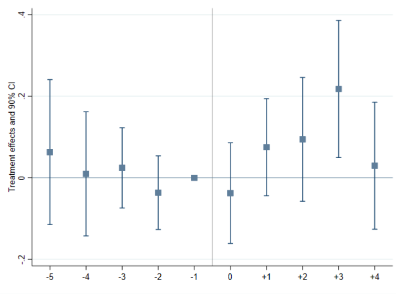
total employment (headcount) in:



(a) new LCs

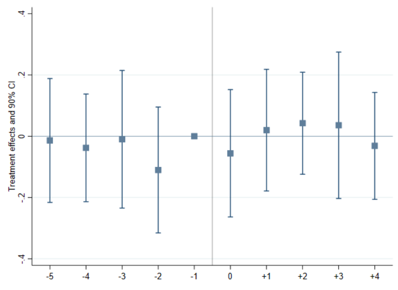


(b) LCs younger than 1

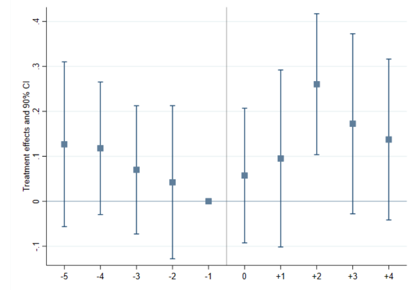


(c) LCs younger than 2

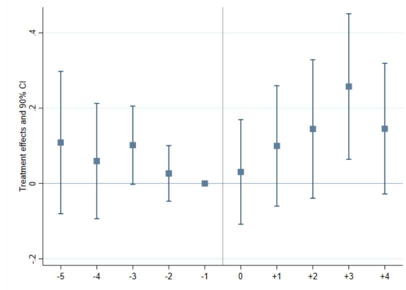
total employment (FTE) in:



(d) new LCs



(e) LCs younger than 1

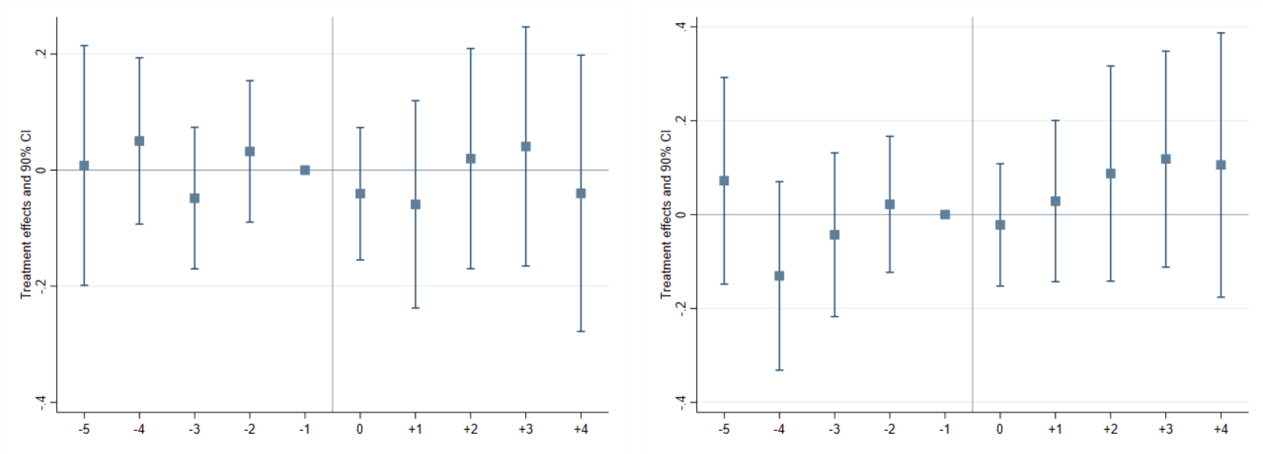


(f) LCs younger than 2

Notes:Panels a–c display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level total employment measured as either headcount or full-time equivalent. Specifically, I use $log(headcount + 1)$ in LCs aged 0, 1 and 2 and younger, respectively in panels a–c and $log(FTE + 1)$ in LCs aged 0, 1 and 2 and younger, respectively in panels d–f, where FTE are full-time equivalent workers measured using hours worked. Sectors J&M are only considered. The x-axis indexes event time. I report the results for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012.The x-axis indexes event time. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

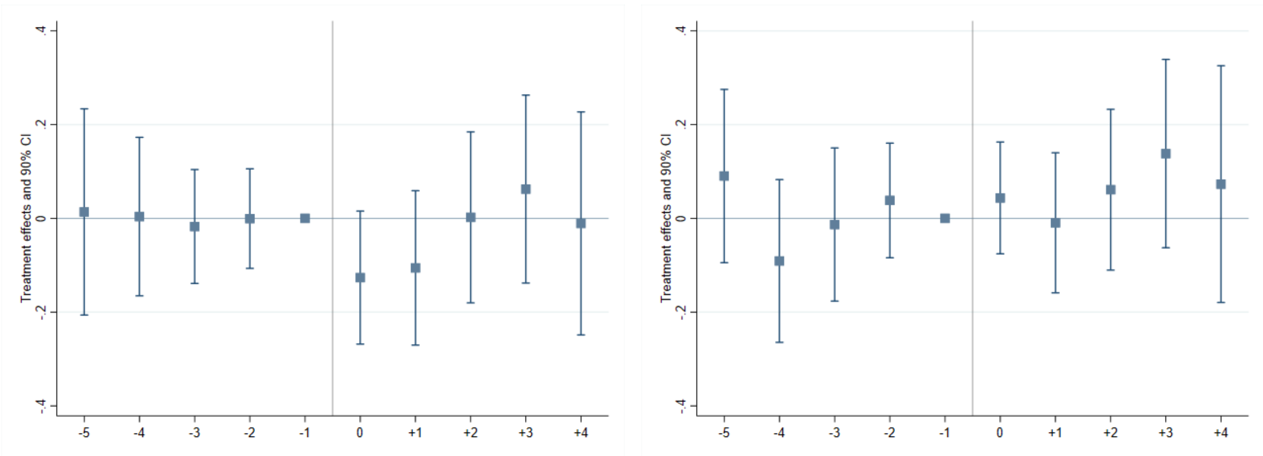
Figure A.10: Effect of SUA on employment of young LCs in sectors J&M

headcounts in LCs $age \leq 5$ by occupation:



(a) managers (b) apprentices

FTE in LCs $age \leq 5$ by occupation:



(c) managers (d) apprentices

Notes:Panels a–c display the regression coefficients and the associated 90% confidence intervals for the difference between treated and control municipalities relative to the event-year in which an innovative startup was incorporated, which is set to 2012 for firms incorporated before 2012, i.e., the $\hat{\beta}_k$ from equation1. The coefficients at $k = -1$ are normalized to zero. The outcome variables are municipality-level total employment measured as either headcount or full-time equivalent. Specifically, I use $log(headcount + 1)$ in LCs younger than age 5 ($age \leq 5$) by occupation in panels a-b and $log(FTE+1)$ in LCs younger than age 5 ($age \leq 5$) by occupation in panels c–d, where FTE are full-time equivalent workers measured using hours worked. Sectors J&M are only considered. I report the results for the 2012 cohort, including only municipalities in which the innovative startup was incorporated before 2012.The x-axis indexes event time. Standard errors are clustered at the municipality level. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

A.2 Appendix Tables

Table A.1: **Descriptive Statistics (2012 cohort)**

	All	Treated	Control	Diff.(3)-(2)
	(1)	(2)	(3)	(4)
Population in 2002	21495.31	21616.47	21374.15	-242.32
	(18636.07)	(18427.30)	(18924.59)	
Share of all emigrants (%)	0.058	0.064	-0.014	0.051
	(0.071)	(0.075)	(0.065)	
Share of non-EU (%)	0.030	0.030	0.029	-0.002
	(0.050)	(0.045)	(0.055)	
Share of EU (%)	0.026	0.030	0.022	-0.009**
	(0.031)	(0.035)	(0.027)	
N of young LCs $age \leq 5$	90.1cm	93.66	86.64	-7.02
	(88.58)	(95.56)	(81.29)	
Headcount in young LCs ($age \leq 5$)	1147.35	1132.14	1162.56	30.42
	(973.63)	(918.10)	(1030.08)	
FTE in young LCs ($age \leq 5$)	592.1	591.83	592.37	0.54
	(477.19)	(463.59)	(492.50)	
N of newly incorporated LCs	73.62	72.68	74.56	1.88
	(54.46)	(54.70)	(54.44)	
Headcount in newly incorporated LCs	158.15	147.99	168.30	20.31
	(146.38)	(127.91)	(162.71)	
FTE in newly incorporated LCs	57.96	53.94	61.99	8.05
	(54.65)	(45.62)	(62.34)	
N of LCs	199.59	207.88	191.30	-16.60
	(177.88)	(184.82)	(171.07)	
Metropolitan areas	0.241	0.179	0.304	0.125**
	(0.429)	(0.385)	(0.462)	
North	0.558	0.571	0.545	-0.027
	(0.498)	(0.497)	(0.500)	
Observations	224	112	112	224

Notes: Matched municipalities, AIRE and INPS data 2007–2017. Treated municipalities are matched to out-of-LLM potential control municipalities. All statistics are computed across municipality-year observations in the year before the event ($e = 2012$). Column 1 reports the statistics for the full matched sample, while columns 2 and 3 reports statistics for the treated and control municipalities, respectively. The difference in column 4 is calculated as (3)-(2). Standard errors are reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. The share of emigrants (all, EU and non-EU) are with respect to the 2002 population. Firms are only LCs (limited companies). Newly incorporated LCs are incorporated in the year before the event. Metropolitan areas shows the share of municipalities in metropolitan areas around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. According to the official definition, metropolitan areas' territorial entities recognized by Article 114 of the Italian Constitution are made up of an aggregate of neighboring municipalities. Introduced with the reform of Title V of the Constitution in 2001, metropolitan cities are recognized as large-area territorial entities defined by the aggregation of neighboring municipalities, similar to the provinces.

Table A.2: **Descriptive Statistics (2013–2015 cohorts)**

	All	Treated	Control	Diff.(3)-(2)
	(1)	(2)	(3)	(4)
Population in 2002	12383.93	12191.87	12575.91	384.11
	(8905.92)	(8645.28)	(9164.36)	
Share of all emigrants (%)	0.080	0.078	0.082	0.004
	(0.092)	(0.098)	(0.086)	
Share of non-EU (%)	0.036	0.034	0.038	0.004
	(0.058)	(0.057)	(0.060)	
Share of EU (%)	0.043	0.042	0.044	0.003
	(0.049)	(0.050)	(0.049)	
N of young LCs $age \leq 5$	45.06	45.97	44.15	-1.82
	(36.54)	(37.345	(35.73)	
Headcounts in young LCs $age \leq 5$	606.46	595.43	617.49	22.07
	(470.92)	(468.54)	(473.53)	
FTE in young LCs ($age \leq 5$)	311.99	309.42	314.57	5.13
	(239.68)	(240.01)	(239.58)	
N of in newly incorporated LCs	46.10	45.13	47.06	1.93
	(36.78)	(36.68)	(36.89)	
Headcount in newly incorporated LCs	89.27	89.58	88.95	-0.63
	(103.17)	(124.69)	(75.92)	
FTE in newly incorporated LCs	32.37	34.12	30.62	-3.50
	(59.71)	(80.00)	(27.04)	
N of LCs	105.24	109.65	100.83	-8.82*
	(75.67)	(79.47)	(71.47)	
Metropolitan areas	0.187	0.171	0.203	0.032
	(0.390)	(0.377)	(0.402)	
North	0.549	0.591	0.507	-0.083**
	(0.498)	(0.492)	(0.500)	
Observations	938	469	469	

Notes: Matched municipalities, AIRE and INPS data 2007–2017. Treated municipalities are matched to out-of-LLM potential control municipalities. All statistics are computed across municipality-year observations in the year before the event (defined as the incorporation year of the oldest innovative startup that registered, $e = b$). Column 1 reports the statistics for the full matched sample, while columns 2 and 3 reports statistics for the treated and control municipalities, respectively. The difference in column 4 is calculated as (3)-(2). Standard errors are reported in brackets. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. The share of emigrants (all, EU and non-EU) are with respect to the 2002 population. Only LCs (limited liabilities) are considered. Newly incorporated LCs are incorporated in the year before the event. Metropolitan areas shows the share of municipalities in metropolitan areas around the cities of Turin, Genoa, Milan, Venice, Bologna, Florence, Rome, Naples and Bari. According to the official definition, metropolitan areas' territorial entities recognized by Article 114 of the Italian Constitution are made up of an aggregate of neighboring municipalities. Introduced with the reform of Title V of the Constitution in 2001, metropolitan cities are recognized as large-area territorial entities defined by the aggregation of neighboring municipalities, similar to the provinces.

Table A.3: **Effect of innovative startups on the share of emigrants, controlling for firm creation**

Dependent variable	Share of emigrants		
	all	EU	extra-EU
	(1)	(2)	(3)
Panel A: all cohorts			
Policy	-0.014**	-0.006**	-0.008
	(0.007)	(0.003)	(0.005)
Mean outcome (%)	0.075	0.040	0.033
Observations	12,782	12,782	12,782
Panel B: 2013–2015 cohorts			
Policy	-0.010*	-0.007*	-0.005
	(0.006)	(0.003)	(0.004)
Mean outcome (%)	0.078	0.042	0.034
Observations	10,388	10,388	10,388
Municipality FE	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes

Notes: Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. All cohorts include all municipalities in which the innovative startup was incorporated between 2010 and 2015. The 2013–2015 cohorts include municipalities in which the startup that registered was born after 2012 with event year ($e = b$). Standard errors are reported in brackets and clustered at the municipality level * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. Regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Table A.4: **Effect of innovative startups on the share of emigrants, controlling for trust in Monti Government**

	High trust			Government's actions		
Dependent variable	Share of emigrants					
	all	EU	extra-EU	all	EU	extra-EU
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: all cohorts						
Policy	-0.014***	-0.009***	-0.005**	-0.014***	-0.009***	-0.005**
	(0.005)	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)
Trust	0.016*	0.008	0.008*	-0.007	-0.002	-0.005
	(0.009)	(0.006)	(0.004)	(0.008)	(0.004)	(0.005)
Mean outcome (%)	0.075	0.040	0.033	0.075	0.040	0.033
Observations	12,782	12,782	12,782	12,782	12,782	12,782
Panel B: 2012 cohort						
Policy	-0.024*	-0.014*	-0.007	-0.025*	-0.014*	-0.007
	(0.014)	(0.008)	(0.008)	(0.015)	(0.008)	(0.008)
Trust	0.027***	0.015**	0.014**	-0.008	0.002	-0.01
	(0.010)	(0.007)	(0.006)	(0.021)	(0.009)	(0.014)
Mean outcome (%)	0.064	0.030	0.030	0.064	0.030	0.030
Observations	2,464	2,464	2,464	2,464	2,464	2,464
Panel C: 2013–2015 cohorts						
Policy	-0.010**	-0.007**	-0.004	-0.010**	-0.007**	-0.004
	(0.005)	(0.003)	(0.003)	(0.005)	(0.003)	(0.003)
Trust	-0.003	-0.004	0.002	0	0.007	0.002
	(0.014)	(0.008)	(0.008)	(0.013)	(0.010)	(0.006)
Mean outcome (%)	0.078	0.042	0.034	0.078	0.042	0.034
Observations	10,388	10,388	10,388	10,388	10,388	10,388
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. All cohorts include all municipalities in which the innovative startup was incorporated between 2010 and 2015. Cohort 2012 includes only municipalities in which the innovative startup was incorporated before 2012 with event year $e = 2012$. The 2013–2015 cohorts include all the other cohorts, with event year $e = b$. In Columns 1–3, the added control on the government is the number of college graduate aged 18–40 that highly trusted the Monti government, while in columns 4—7, the control is number of college graduate aged 18–40 that was highly satisfied with the Monti government's actions. Standard errors are reported in brackets and clustered at the municipality level * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Table A.5: **Effect of SUA on the share of emigrants, no emigrant share in the matching algorithm**

Dependent variable	Share of emigrants		
	all	EU	extra-EU
	(1)	(2)	(3)
Panel A: all cohorts			
Policy	-0.014***	-0.007***	-0.007***
	(0.005)	(0.002)	(0.003)
Mean outcome (%)	0.075	0.039	0.033
Observations	12,782	12,782	12,782
Panel B: 2012 cohort			
Policy	-0.02	-0.009	-0.011
	(0.013)	(0.006)	(0.008)
Mean outcome (%)	0.064	0.031	0.03
Observations	2,464	2,464	2,464
Panel C: 2013–2015 cohorts			
Policy	-0.010**	-0.007**	-0.004
	(0.005)	(0.003)	(0.003)
Mean outcome (%)	0.077	0.041	0.034
Observations	10,388	10,388	10,388
Municipality FE	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes

Notes: In the matching algorithm, the emigration information (i.e., three and two-year lagged local shares of total emigrants) is excluded from the logit model. Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as a percentage. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. All cohorts include all municipalities in which an innovative startup was incorporated between 2010 and 2015. Cohort 2012 includes only municipalities in which the innovative startup was born before 2012 with event year $e = 2012$. The 2013–2015 cohorts include all the other cohorts with event year ($e = b$). Standard errors are reported in brackets and clustered at the municipality level. * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. Regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Table A.6: **Matching algorithm: Number of Matched Units**

	incorporation year						total
	2010	2011	2012	2013	2014	2015	
No. of municipalities							
All	31	50	80	219	195	252	827
Matched	18	35	59	151	140	178	581
Discarded	13	15	21	68	55	74	246
Matching rate	0.58	0.70	0.74	0.69	0.72	0.71	0.70

Notes: Municipalities by year in which the first innovative startup was incorporated. Matched municipalities are treated ones that found corresponding control municipalities. The matching rate is given by the number of matched on all municipalities.

Table A.7: **Effect of SUA on employment of young firms in sectors J&M**

	Headcounts in firms:			FTE in firms:		
	(1)	(2)	(3)	(4)	(5)	(6)
	age 3	age 4	age 5	age 3	age 4	age 5
Policy	0.04	0.027	0.03	0.03	0.022	0.018
	(0.038)	(0.036)	(0.035)	(0.036)	(0.035)	(0.034)
Mean outcome	3.288	3.489	3.633	2.797	3.014	3.171
contribution	1.3%	0.8%	0.8%	1.1%	0.7%	0.6%
extra workers	1.03	0.86	1.10	0.46	0.43	0.41
	blue collars	white collars	local	blue collars	white collars	local
Policy	0.156*	0.035	0.075	0.116	0.028	0.069
	(0.080)	(0.047)	(0.122)	(0.078)	(0.046)	(0.121)
Mean outcome	1.451	3.324	1.773	0.877	2.932	1.477
contribution	11%	1%	4%	13%	1%	5%
extra workers	0.51	0.94	0.37	0.16	0.50	0.23
Observations	2,464	2,464	2,464	2,464	2,464	2,464
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The outcome variables are municipality $log(headcount + 1)$ in LCs younger than 3, 4 and 5 in columns 1 to 3, respectively, and $log(FTE + 1)$ in LCs younger than 3, 4 and 5 , in columns 4 to 7, where FTE are full-time equivalent workers measured using hours worked. Sectors J&M are only considered. Mean outcome is the municipality-level average outcome in $k = -1$ in treated municipality in the 2012 cohort. Only 2012 cohort is included: municipalities in which the innovative startup was incoroporated before 2012 with event year $e = 2012$. Local workers live in the same municipality where the LCs is headquartered. Contribution shows the size of the coefficient with respect to the mean outcome, while extra worker shows the number of extra workers hired in treated municipalities measured as $headcount \times \beta^T$ or $FTE \times \beta^T$ where β^T are estimated from model 2. Standard errors are reported in brackets and clustered at the municipality level * indicates significance at 10%, ** indicates significance at 5%, ***indicates significance at 1%. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

B Appendix: Alternative estimation strategies

B.1 Instrumental Variable Strategy

As explained in Section 4 in the main text, my research design based on matching treated municipalities and a staggering adoption help solve some of the endogeneity issues of firms self-selection into the framework.To further support my results, I present an alternative design based on an instrumental variable strategy on a sample of unmatched municipalities.[52] The use of an instrumental variable identification strategy allows me to deal with unexpected unobservable shocks that could simultaneously increase the municipality-level probability to have innovative startups and decrease the share of emigrants. The instrument is defined as the municipality-level heterogeneity in the presence of young firms born in 2007 interacted with the provincial propensity to innovate measured, alternatively, by the industry composition of the Chamber of Commerce (CC) board or by the number of patents filed in the province in the period before the introduction of the SUA. I have a balance sample of 827 treated and 5,283 control municipalities, 6,110 in total that are observed throughout the period and for which I record the emigrant information at least once in the period between 2007 and 2017. When not observed, these characteristics are set to zero. I instrument the variable $Innov_{mk} = Policy_{mk}^{unmatched} \times T_m$ with IV_{mt}. The variable $Policy_{mk}^{unmatched}$ in this design is a dummy that switches to 1 during the year in which the first startup *registers* in the framework. T_m is an indicator variable equal to 1 if the municipality m is treated.[53]

$$IV_{mt} = Share\ New\ firms_{m,birth} \times Propensity\ Innovate_p \times After_t \tag{7}$$

The instrumental variable is the interaction between the share of new firms incorporated in 2007 in municipality m ($Share\ New\ firms_{m,birth}$), the propensity to innovate at the provincial level p ($Propensity\ Innovate_p$) and a period dummy. The $Share\ Young\ firms_{m,birth}$ is measured as

[52] By unmatched, I refer to the fact that the procedure explained in 4.1 is not run, and I do not constraint control municipalities only to the ones identified by the algorithm with similar observable prepolicy characteristics to the treated ones but use as control municipalities all the never-treated.

[53] See 4.3.

the number of LCs born in 2007 normalized by the total number of LCs in the same municipality. The $Propensity\ Innovate_p$ is either a dummy equal to 1 when the share of provincial board members of the Chamber of Commerice in the manufacturing sector is above the median share across all provinces or a dummy equal to 1 if the share of provincial patents is above the $25th$ of the distribution of the share of patents across all provinces.[54] The time dummy $After_t$ is equal to 1 if $year \geq 2013$. The idea of using board composition lies in the role of the board, especially in the first years in which the policy was implemented: the board had to decide on whether a firm that applied to the program was "innovative oriented" and therefore use the framework. While there were objective criteria to be used to identify this level of innovativeness, their application created some uncertainty that varied across provinces.[55]. By using the composition of the board, I measure the possible constraints that a firm encountered in the acceptance process as measured by the board familiarity with innovative oriented sectors, also affecting the timing of firm participation. Municipalities with a similar share of new firms could therefore face different constraints due to differences in the characteristics of the local institutions. I estimate the following model with specification 8 to illustrate the IV model and specification 9 being the corresponding first stage:

$$y_{mk} = \beta^{IV} \cdot Innov_{mk} + \alpha_m + \lambda_{p(m),k} + r_{mk} \tag{8}$$

$$Innov_{mk} = \kappa^{IV} \cdot IV_{mk} + \eta_m + \theta_{p(m),k} + \nu_{mk} \tag{9}$$

[54]In the former case, the share of board members that belong to the manufacturing sector is computed using data from the boards of all Chambers in the 105 Italian provinces in 2012. Each board member is associated with either a sector (agriculture, banking and insurance, manufacturing, transportation, tourism, crafts, trade, other sectors) or a service (consumer's protection associations, trade unions, professionals, business services). I then compute the share of the number of board members in the manufacturing sector on the total number of board members. This share has an average of 0.15, a minimum value of 0.04 and a maximum value of 0.25. In the second measure of propensity to innovate, the share is measured by taking the total number of patents created in the province between 2007 and 2012 and dividing it by the total number of patents created during the same period in all Italian provinces.

[55]As explained in Section 4, evidence exists of the fact that the board of the CC could have delayed registration of the applying firms because of inexperience on the topic (`https://www.mimit.gov.it/images/stories/normativa/Circolare-startup-e-PMI-innovative-14-02-2017.pdf`)

The relevance of the instrument is assessed using the F-statistics, as reported in the Table, whereas my exclusion restriction is the share of new, young, innovative firms born in 2007. Both measures of propensity to innovate do not directly affect the share of emigrants in the municipality. While this restriction is not testable, I can run a placebo exercise for the period before the policy, where I check whether the share of new firms and either the board composition or patenting activity directly affected the share of emigrants. The share of new firms is measured in 2002 ($Share\ New\ firms_{m,birth}$). I focus on years before 2012 and report these results in Table B.2. I report two different prepolicy variables. The first one is the interaction of the share of new firms and the propensity to innovate in the province (IV prepolicy 1). The second one is the interaction between the share of new firms and the propensity to innovate with an additional simulation of a before and after period in 2005.

B.1.1 Results

Table B.1 show the results for the two instrumental variables. In both cases, the results are comparable to those in Table 2 in both direction and magnitude, confirming that having an innovative startup in the municipality negatively affects the share of emigrants, especially to other EU countries by decreasing it. I observe slightly larger IV results. It is in fact plausible to believe that the use of the framework happened before in those municipalities with better institutional quality, including more dynamic labour markets and availability of young high skilled workers/entrepreneurs. These factors are also more likely to reduce push factors, with the implication that our baseline results are downward biased. In Table B.2, I report the results of the placebo exercise. Reassuringly, the results show no direct effect of the instruments in the period before the policy

Table B.1: **Effect of SUA on the share of emigrants - Instrumental variable**

	(1)	(2)	(3)	(4)	(5)	(6)
		all years			$t \leq 2013$	
Dependent variable	Share of emigrants					
	all	EU	extra-EU	all	EU	extra-EU
Panel A: Chamber of Commerce board composition						
Policy	-0.020	-0.014*	-0.006	-0.023*	-0.013*	-0.011
	(0.013)	(0.008)	(0.007)	(0.013)	(0.007)	(0.007)
F-stat	18.860	18.860	18.860	14.530	14.530	14.530
Panel B: Patenting activity						
Policy	-0.007	-0.013*	0.000	-0.011	-0.013*	-0.003
	(0.013)	(0.008)	(0.008)	(0.012)	(0.007)	(0.008)
F stat	56.802	56.807	56.807	50.792	50.798	50.798
Mean outcome	0.058	0.028	0.030	0.058	0.028	0.030
obs	67210	67210	67210	62293	62293	62293
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Province × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. Panel A reports results where the $Propensity\ Innovate_p$ in IV_{mt} is a dummy that is equal to 1 if the provincial board of the Chamber of Commerce has share of board members that belong to the manufacturing sector that is above the median share across all provinces, while in Panel B it is measured with a dummy that is equal to 1 if the share of provincial patents is above the $25th$ of the distribution of the share of patents across all provinces. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

Table B.2: **Placebo IV**

	(1)	(2)	(3)	(4)	(5)	(6)
	placebo 1			placebo 2		
Dependent variable	Share of emigrants					
	all	EU	extra-EU	all	EU	extra-EU
Panel A: Chamber of Commerce board composition						
IV prepolicy (1)	0.016	0.0152	0.0075			
	(0.0400)	(0.0197)	(0.0226)			
IV prepolicy (2)				0.0244	0.0146	0.0146
				(0.0435)	(0.0216)	(0.0242)
F-stat	11.305	11.083	12.527	11.305	11.083	12.527
Panel B: Patenting activity						
IV prepolicy (1)	0.0241	0.0117	0.0226			
	(0.0287)	(0.0107)	(0.0203)			
IV prepolicy (2)				0.0417	0.0177	0.0327*
				(0.0275)	(0.0111)	(0.0192)
F stat	11.305	11.083	12.527	11.305	11.083	12.527
Mean outcome	0.08	0.11	0.058	0.08	0.109	0.058
obs	40073	40076	40076	40073	40076	40076
Region × Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Mean outcome is the municipality-level share of emigrants with respect to the 2002 population in $k = -1$ in that cohort and reported as percentages. Emigrants are either to all countries or to EU or extra-EU countries. EU countries include Austria, Belgium, Bulgaria, Cyprus, Croatia, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, United Kingdom, Czech Republic, Romania, Slovakia, Slovenia, Spain, Sweden, and Hungary. Years 2007 to 2017. Panel A reports results where the *Propensity Innovate*$_p$ in IV_{mt} is a dummy that is equal to 1 if the provincial board of the Chamber of Commerce has share of board members that belong to the manufacturing sector that is above the median share across all provinces, while in Panel B it is measured with a dummy that is equal to 1 if the share of provincial patents is above the $25th$ of the distribution of the share of patents across all provinces. The regression results are weighted by the logarithm of the number of firms in sectors C (Manufacturing), J (Information and communication), M (Professional and Scientific).

C Appendix: Firm level analysis

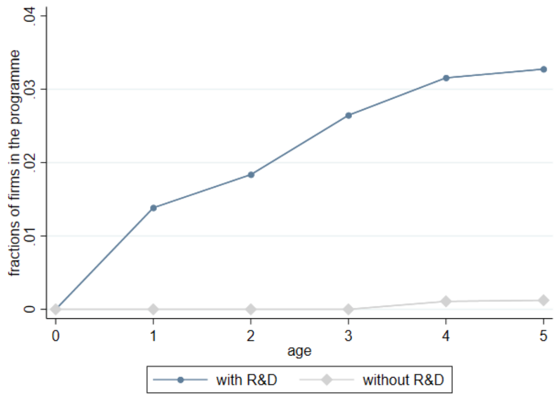
C.1 Descriptive Statistics

Table C.1: Average firms' characteristics in the main sample by birth cohort eligible and noneligible firms

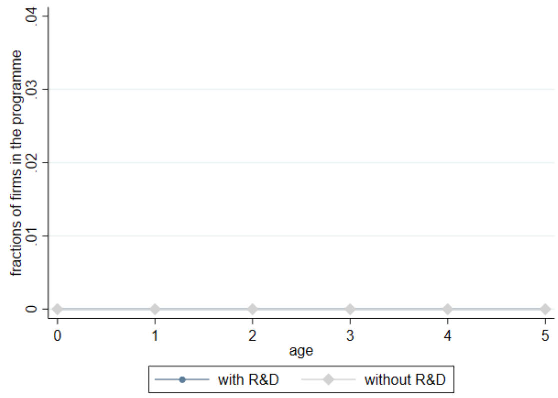
	(1) All	(2) Eligible	(3) Non-Eligible
Employees (FTE)	4.3	4.14	4.40
	(6.55)	(5.67)	(7.24)
Employees (headcount)	7.21	6.94	7.46
	(17.87)	(12.76)	(21.46)
Average weekly real salary	295.58	284.01	305.99
	(188.72)	(187.04)	(189.64)
Local workers (headcount)	2.03	1.96	2.10
	(3.92)	(3.50)	(4.25)
Local workers (FTE)	1.29	1.25	1.33
	(2.28)	(2.20)	(2.36)
Average workers'age	36.53	36.99	36.09
	(8.16)	(8.22)	(8.07)
Survival probability after age 5	0.66	0.66	0.65
	(0.47)	(0.47)	(0.48)
Value added per week worked	3.99	4.00	3.99
	(32.60)	(17.04)	(41.89)
Revenues per week worked	3.54	3.48	3.59
	(31.13)	(15.89)	(40.18)
Observations	14921	7077	7844

Notes: This table reports the mean and standard deviation in brackets of a set of firm variables characteristics for firms in our sample at age 2. Column 1 refers to both eligible and noneligible firms, where eligibility is defined by birth cohort. Column 2 restricts the sample to eligible firms—those born between 2010 and 2012. Column 3 restricts the sample to firms born between 2007 and 2009. Value added and revenue per week come from CERVED data. Value added is measured as total revenues plus unsold stocks minus cost of goods and services used in production. All monetary figures are expressed in 2010 euros. Registry of Italian citizens residing abroad, CERVED and INPS data years 2007–2017. Startup registry for years 2013 to 2017.

Figure C.1: Share of firms younger than age 5 in the program



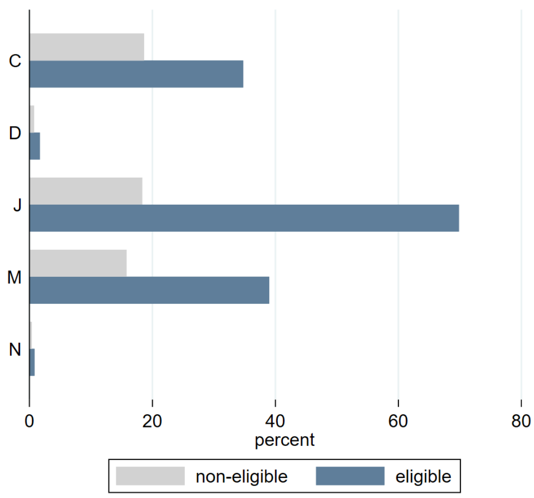
(a) firms born between 2010 and 2012



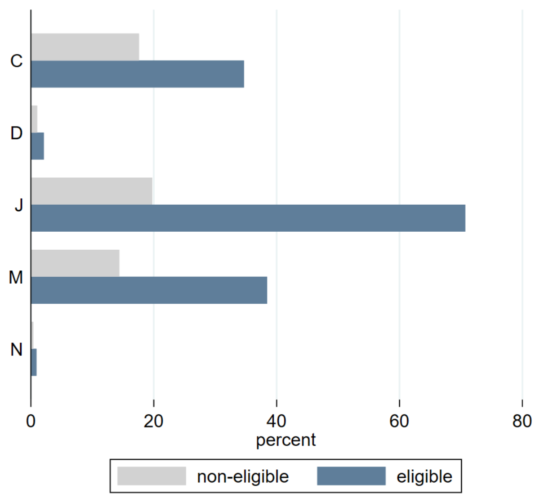
(b) firms born between 2007 and 2009

Notes: The graphs report the share of younger firms that participated in the program in each age year $a \in [1-5]$ by eligibility status and presence of intangible assets during the eligibility period. Panel C.1a plots, among eligible firms born between 2010 and 2012 in our sample, the evolution of the share of firms participating in the program between the age of 1 and 5.

Figure C.2: Share of eligible and noneligible firms by sector



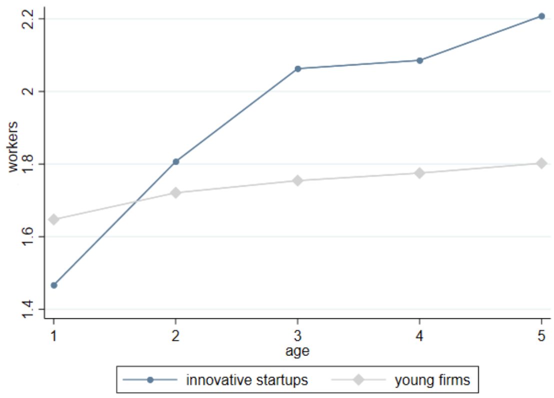
(a) all firms



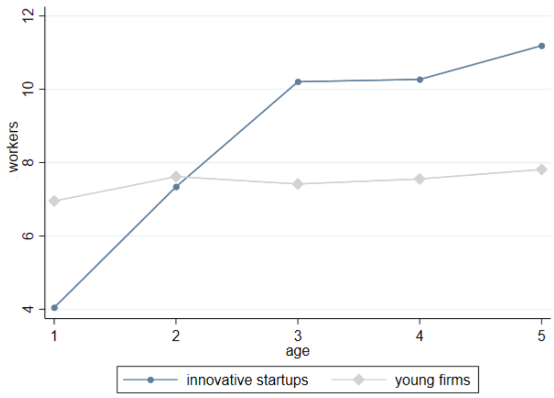
(b) firms with positive intangible assets

Notes: The graphs report the sectoral distribution of all eligible and noneligible firms (Panel a) and eligible and noneligible firms with positive intangible assets.

Figure C.3: Average employment in innovative startups and young firms



(a) *log* employment (headcounts)



(b) employment

Notes: The graphs report in Panel A the average *log* employment (headcounts) and in Panel B the average number of employees of innovative startups and young firms (those who did not enter the program among the eligible).

D Appendix: Policy details of the SUA

The policy has been considered as a European best practice and different from previous policies. The aim of the policy was to support startups' survival rate using the following tools. Admitted firms could use digital technologies to reducethe red-tape, receive support during market entry and eventual exit, use tax incentives for corporate and private investors and access credit guarantee schemes, flexible remuneration schemes, and tailor-made rules. They were in fact exposed to less restrictive rules for hiring fixed-term workers for any duration, and these contracts could be renewed an indefinite number of times for 36 months[56] In addition, standard regulations on rates of fixed-term employees over open-ended employees did not apply because innovative start-ups could hire as many fixed-term employees as they wanted. For the first years of the policy, the hiring of highly qualified individuals was subject to a tax credit up to 35% of the cost incurred by the firm (Menon et al., 2018). One of the aim of the program was to facilitate equity and debt capital access via tax incentives or credit guarantee schemes. Its ultimate goal was in fact to tackle financial frictions in both credit and equity capital that are known to play a major role in preventing startup growth and survival, especially for innovative firms(Hall and Lerner, 2010; Kerr and Nanda, 2015; Revest and Sapio, 2012) Hence, acquiring external financing is critical to startup performance. The policy introduced two instruments used to ease financial frictions: (1) a 30% fiscal incentive for venture capitalists and private investors who invest in equity capital; (2) privileged access to public scheme bank loan programs through free of charge access to Fondo di Garanzia, a government fund that acts as a guarantor for bank loans. The results of access to external finance for these startups are positive: the policy had in fact alleviated the financial frictions that characterize innovative startups, also thanks to the fact that it provided a comprehensive set of measures that eased both debt and equity financing, allowing the startups to choose the financing instruments more appropriate for their strategy, with positive trickle down effects on employment and intangible assets (Manaresi, Menon, and Santoleri, 2022). In addition, the Act supported the development of the Italian venture capital industry, which lagged that of other OECD countries.

According to the start-up registry, by the end of 2017, 10,556 firms in 1,731 municipalities

[56] After that, the contract could be renewed once more for a maximum duration of 12 months.

had registered for the program, and this number increased over time. The Decree-law 179/2012 of the Act passed on October 18, 2012 and was then converted into Law no.221 on December 17, 2012. In the analysis, I focus on only the startups that registered between 2013 and 2015. The policy was amended several times, as reported in Figure D.1, where I show the several changes to the policy. Recent work by the Minister of Development shows that the fiscal cost of the policy for the period 2013–2016 was approximately 30 million euros. During this period, approximately 9,000 registered startups were admitted to the program, implying a reasonable cost of 3,300 euros per registered startup (Menon et al., 2018). In 2018, the Italian Statistical Office conducted a survey on 2,250 startups that registered in the program and revealed interesting statistics on their average characteristics. Approximately 81% of the founders are male, 43 years-old of age on average and have either a degree in STEM or Management/Economics. Most of them have at least one employee (59.4%) who is on average approximately 25–34 years of age, hired with a project-based contract, and is more likely to have technical skills. Interestingly, they are more likely to be hired from the local labour market and have a strong attachment to the territory. Similar characteristics are found in shareholders being more likely to be from the same area in which the startup is headquartered (ISTAT, 2018).

Figure D.1: Policy Timeline