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# COLLEGES, BUSINESS DYNAMISM, AND LOCAL GROWTH

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# Colleges, Business Dynamism, and Local Growth

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**Abstract**

Can new colleges transform a local economy? This article studies the largest higher-education expansion in French history, a policy that opened hundreds of teaching-oriented colleges across 145 cities in the 1990s. Exploiting the staggered timing of openings, I find a persistent 17% increase in the share of college graduates. The policy induced creative destruction: firm creation rose by 16% while incumbent firms contracted. Entry was concentrated in skill-intensive and in tradable industries, led by experienced entrepreneurs. On net, value added grew by 5% and productivity by 6%. These gains were unevenly distributed: skilled employment rose among younger workers, while older, less-educated workers experienced lasting losses. Twenty-five years later, cohorts of enrollment age when the colleges opened have more education, higher employment rates, and more-skilled jobs. Overall, local effects were larger in initially less-developed areas, contributing to a narrowing of spatial disparities.

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# 1 Introduction

Universities are widely regarded as engines of economic growth. Much of the evidence supporting this view comes from large, long-established research universities (Moretti, 2004a; Kantor and Whalley, 2014; Hausman, 2021; Andrews, 2023; Babina et al., 2023). Since the mid-twentieth century, however, most of the growth in enrollment has come from smaller, teaching-oriented colleges (Trow, 1973; Kane and Rouse, 1999; Schofer and Meyer, 2005; Goldin and Katz, 2008). These institutions operate through a different channel: rather than training inventors or researchers, their primary role is to expand the local supply of skills valued by firms. Their effects therefore depend on whether students enroll and stay after graduation, how firms respond to the resulting increase in human capital, and how these adjustments affect other workers, those without a college degree. Quantifying these effects on firms, workers, and the places that host new colleges is central to assessing whether expanding access to college can serve as an effective tool for regional development.

This paper uses the largest expansion of higher education in French history to study the causal effects of new colleges on local business dynamism and growth. Starting in 1990, the French government opened hundreds of new colleges through the *Universities of the 21$^{st}$ Century* plan ("Plan Université 2000") with particular emphasis on two- and three-year degrees in scientific and technical fields. Following the policy, enrollment rose by more than 40% between 1989 and 1995. Although its primary objective was to raise educational attainment, the plan also sought to broaden access to higher education in more remote areas, with 145 cities receiving new capacity. Using detailed administrative data, I make three main contributions. First, I estimate the effects of new colleges on local economic development and firm dynamics. Second, I quantify their consequences for local labor markets and for individuals' long-run outcomes. Third, I examine how the impact varies across areas with different initial levels of development, and compare the policy's costs with its benefits.

My empirical strategy exploits the staggered rollout of colleges over a decade in a difference-in-differences design. Because colleges were not randomly located (targeted areas were larger and younger than non-targeted ones), I focus only on areas receiving colleges and compare early- to later-treated cities, using estimators robust to heterogeneous treatment effects. Identification therefore relies on the *timing* of openings, rather than their location, being unrelated to local trends. The institutional setting supports this assumption. The reform was among France's first decentralized higher-education policies: while the central government and local authorities negotiated which cities would receive new capacity, opening dates were not part of those nego-

tiations. Funding was instead disbursed in annual tranches to the projects that had completed the necessary administrative steps. Sites whose agreements with the State remained pending, whose land had not yet been secured, or whose permits were outstanding were carried over to subsequent years. As a result, opening dates often reflected idiosyncratic differences in implementation capacity. Some cities could move quickly by converting existing public buildings, such as former fire stations, whereas others first had to acquire land, obtain permits, and construct new facilities (DATAR, 1998). Three pieces of evidence further support the quasi-random timing assumption. First, treatment dates are uncorrelated with pre-policy characteristics, including city size, education, and business dynamism. Second, outcomes in early- and later-treated areas evolved in parallel before the policy. Third, effects emerge only two to three years after opening, once the first cohorts graduate, rather than at the opening itself, as they would if colleges had been timed to an anticipated upswing. Finally, I complement this approach with a triple-difference strategy comparing high- and low-skill industries in treated versus untreated areas, which differences out location-specific shocks common across industries. This design requires a weaker parallel trends assumption and directly tests the skilled-labor-supply channel.

I quantify the effects of the policy by combining administrative and survey data on colleges, firms, and workers. I leverage a registry of all public higher education establishments built in France since the $19^{\text{th}}$ century, complemented with hand-collected records, to identify the location, timing, and discipline of every new opening. A survey following graduates of each type of college into the labor market further reveals which occupations they enter and whether they remain where they studied. On the firm side, I draw on the national business register covering the universe of new firms since 1987, balance sheet data on the growth, value added, and productivity of firms, and a survey of entrepreneurs on founders' backgrounds and their firms' markets. On the worker side, I use linked employer-employee data covering a representative 4% sample of the workforce, merged with census information on workers' education, to track employment and wages by age and skill group. Finally, I exploit census records to estimate the long-run effects of the policy on individual educational attainment and labor market outcomes, twenty-five years after its start.

First, I find that the opening of new colleges led to a persistent increase in the educational attainment of the local workforce: the share of college graduates rose by 17% on average, and by 25% eight years after the first opening. This reflects both a surge in enrollment and high retention: more than 70% of two- and three-year graduates remain where they studied. The influx of educated workers set off a process of creative destruction. Firm creation rose by 16%, driven by the entry of new corporations rather than subsistence entrepreneurs (Schoar, 2010). Using

the construction of new colleges as an exogenous shifter of local education, I estimate that a 1% increase in the share of graduates raises firm creation by approximately 0.7% in manufacturing.

The evidence points to the supply of skilled labor as the primary driver of this entry response. Firm creation rose most in skill-intensive industries and expanded in tradable sectors, where local product demand plays little role; the new entrants, moreover, employed more educated workers seven years later. Survey evidence reveals that new firms were founded by experienced entrepreneurs who hired the new graduates, rather than by the graduates themselves. A local product demand channel (students and staff raising spending on local goods and services) likely operates as well in nontradable sectors, but it cannot account for the response of tradable sectors or the skill gradient across industries.

While firm creation increased following the construction of new colleges, incumbent firms experienced more exits and lower growth, leaving the total number of firms roughly unchanged but transforming its composition. Accounting for all margins (entry, exit, and growth of firms), I find a clear net gain: local value added rose by about 5% and productivity by about 6%.

I assess the robustness of these results in multiple ways. First, they are consistent across alternative specifications and samples, such as controlling for initial city size or excluding the Paris area. Second, they hold across estimation methods (Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021; Borusyak et al., 2024), and geographic definitions (urban areas, provinces, and commuting zones). Finally, the effects do not reflect local displacement. The city-level results survive aggregation to the province level, indicating genuine increases in education and firm creation rather than a reallocation of students and firms across cities within the same province. Untreated neighboring areas, moreover, gain modestly in education and employment, so the baseline estimates, if anything, understate the policy's overall effect.

Second, I investigate the implications for local labor markets and find a strong divide across generations. Skilled employment among younger workers rose by about 20%, while older, lower-skilled workers bore the cost of accelerated churn: their employment fell by 11% and their unemployment rose persistently. The policy thus benefited entrants (young workers and new firms alike) and had a significant negative impact on incumbents.

I then turn to the long-run implications, using a cohort approach combined with census data. Identification exploits two sources of variation. First, French students enter university around age 18, and very few enroll after their early twenties. This allows me to define "exposed cohorts," young enough to attend new colleges, and to contrast them with older cohorts unlikely ever to enroll. Second, the staggered rollout lets me compare exposed and unexposed cohorts across

treated and not-yet-treated areas, so that both groups grow up in similar locations and face the same macroeconomic conditions when making their educational and labor market decisions (Kahn, 2010; Oreopoulos et al., 2012; Charles et al., 2018). The identifying assumption is that outcomes across cohorts born in early- versus later-treated areas followed similar trends, which I test for older, unexposed cohorts.

The long-run gains for exposed cohorts are substantial. Twenty-five years after the policy, they were 10% more likely to hold a university degree, 3% more likely to be employed, and 11% more likely to work in a skilled occupation, consistent with the technical and scientific orientation of the new colleges. These gains did not appear to come through relocation: exposed cohorts were no more likely to migrate to other areas. The expansion thus achieved its stated objectives, durably increasing access to higher education and skilled employment for people born in targeted areas.

Third, I examine how the effects vary across cities. Interacting treatment intensity with initial city characteristics, I find that a given dose of treatment, defined as new seats relative to the initial workforce, raised education similarly regardless of initial development, measured by prior education, density, or value added per worker, but translated into significantly larger value-added gains in initially poorer and less dense areas. These results indicate that such expansions can narrow disparities across areas. The aggregate data point in the same direction: the policy period coincided with the sharpest episode of spatial convergence in education and income observed in France since the early 1980s, with initially less-educated provinces catching up fastest.

Finally, I study the policy's costs and benefits using the Marginal Value of Public Funds framework (Hendren and Sprung-Keyser, 2020). Despite high up-front construction and teaching costs, tax receipts on the induced growth turned the annual net cost negative within about five to eight years; cumulatively, the policy paid for itself in roughly eight to twelve years.

Taken together, these findings carry three lessons. First, large-scale investment in scientific and technical education can durably raise local growth, notably through the creation of new firms that employ the new skills. The gains are strongest where initial development is lower, making new colleges a potent place-based instrument. Second, although the outlays are large and immediate, tax receipts on the induced growth accumulated fast enough that the policy paid for itself in about a decade. Third, the gains are not evenly shared: the generations that gained access to the new colleges enjoy persistent benefits, while older, less-educated workers bear losses along the transition. These losses can persist, as geographic reallocation is slow, especially among less-educated workers (Blanchard and Katz, 1992; Autor et al., 2013; Yagan,

2019; Notowidigdo, 2020). The case for expanding higher education is therefore strongest when such investments are accompanied by policies that cushion these workers.

These findings speak to three strands of the literature. First, this paper contributes to the literature on higher education and local development by showing that regional, teaching-oriented colleges reshape local labor markets through the creation of new firms. Human capital is among the strongest correlates of income and growth across countries and regions (Barro, 1991; Mankiw et al., 1992; Hanushek and Woessmann, 2008, 2012; Gennaioli et al., 2013; Gethin, 2025), and more educated cities grow faster (Glaeser et al., 1995; Shapiro, 2006). Yet the causal effect of education on local development is difficult to establish, as skills and prosperity reinforce each other (Bils and Klenow, 2000; Krueger and Lindahl, 2001; Acemoglu et al., 2014). A large literature has therefore turned to studying the effects of universities, documenting that proximity to them raises innovation (Furman et al., 2002; Abramovsky et al., 2007; Andersson et al., 2009; Toivanen and Väänänen, 2016; Azoulay et al., 2018; Che and Zhang, 2018; Bianchi and Giorcelli, 2020; Hausman, 2021; Andrews, 2023; Babina et al., 2023; Bergeaud et al., 2025) and income (Moretti, 2004a,b; Aghion et al., 2009; Kantor and Whalley, 2014; Liu, 2015; Glaeser and Lu, 2018; Valero and Van Reenen, 2019; Carneiro et al., 2023; Fan et al., 2026). These papers typically focus on large, historical, research-oriented universities.[1] I focus instead on *regional* colleges, which account for most of the recent expansion in higher education.

Teaching-oriented colleges are the most relevant margin for policy today: cheaper to build, easier to replicate, and far more numerous than flagship institutions, yet evidence on their effects remains limited. The construction of hundreds of colleges under the *Universities of the 21$^{st}$ Century* plan provides both the scale and the identifying variation needed to estimate causal effects. Finding a suitable control group for cities with historical universities has proven challenging, and existing approaches typically compare locations selected to receive a new institution with potential candidates or runner-up locations.[2] I sidestep this challenge with a different empirical strategy, exploiting the rollout of these colleges over a decade.[3] In addition, rich French administrative data allow me to assess the full set of margins through which colleges affect local growth, including the role of firms, and to trace the incidence of the shock across entrants and

[1] On historical institutions, see also Goldin and Katz (1999) on the formative years of American higher education, Cantoni and Yuchtman (2014) on the role of medieval universities in development and trade, and Howard et al. (2024) and Howard and Weinstein (2026) on the resilience and social mobility of American college towns.

[2] For example, Moretti (2004a) focuses on land grant colleges and argues that their location did not depend on natural resources or other factors that would make cities wealthier. Liu (2015) uses synthetic control methods to construct control counties comparable to those that received land grant colleges. Finally, Andrews (2023) and Russell et al. (2024) compare chosen sites with "runner-up" locations that were considered but not selected.

[3] Bleemer and Quincy (2026) builds on this approach to study agricultural productivity and technical change following the opening of junior colleges in the early twentieth-century United States.

incumbents, younger and older workers, and skill groups.

With these data, I identify a channel that the literature has largely overlooked: the response of firm creation to the local supply of educated workers. Firm entry is a central driver of local growth (Haltiwanger et al., 2013; Walsh, 2020), and more educated cities create more firms (Glaeser and Kerr, 2009; Doms et al., 2010; Glaeser et al., 2010).[4] While the literature has mostly focused on research institutions and star scientists (Zucker et al., 1998; Balsmeier et al., 2020; Tartari and Stern, 2021; Uhlbach et al., 2022), or on technology upgrading within existing firms (Che and Zhang, 2018; Carneiro et al., 2023), I show that expanding the educated *workforce* raises firm creation, led by experienced entrepreneurs who employ the new graduates.

Second, this paper contributes to the literature on the returns to education by measuring monetary returns that accrue beyond those who attend college. An extensive body of work identifies the private returns to schooling from individual-level variation in access to education, such as distance, cost, or admission to two- and four-year colleges (Card, 1993, 1999, 2001; Altonji et al., 2016; Mountjoy, 2022, 2026; Lovenheim and Smith, 2023). Besides the direct effects of education on students, higher education can shape economic activity through equilibrium adjustments in the relative supply of skills, which affects other workers and firms (Katz and Murphy, 1992). Whether these external effects are large remains unsettled: estimates of human capital externalities range from substantial (Moretti, 2004a,b,c) to negligible (Acemoglu and Angrist, 2000; Ciccone and Peri, 2006). I contribute to this literature in two ways. First, the staggered rollout allows me to study both margins: cohort-level exposure captures the gains accruing to the generations able to attend the new colleges, while the city-level design recovers the net local effect, including on residents who never went to those colleges. Second, I provide evidence on the firm-side response: the social returns materialize through the entry of skill-intensive firms, creative destruction, and productivity growth.

Third, this paper contributes to the literature on place-based policies by showing that investment in human capital can durably foster local development and expand opportunities for local residents. Large spatial disparities have been documented around the world, including in the United States and France (Moretti, 2012; Austin et al., 2018; Ehrlich and Overman, 2020; Gaubert et al., 2021; Kramarz et al., 2022; Carry et al., 2026). Whether governments should respond by targeting distressed *places* rather than people, however, remains theoretically debated. Canonical spatial-equilibrium analyses caution that worker mobility and land-price capitaliza-

[4]Prior work on the local determinants of entry emphasizes financial development (Black and Strahan, 2002; Cetorelli and Strahan, 2006; Bertrand et al., 2007; Michelacci and Silva, 2007; Ji et al., 2023; Fonseca and Matray, 2024), access to venture capital (Gompers and Lerner, 2001; Chen et al., 2010; Samila and Sorenson, 2011; Kerr et al., 2014; Lerner and Nanda, 2020), collateral values (Adelino et al., 2015; Schmalz et al., 2017), proximity to small suppliers (Chinitz, 1961; Glaeser et al., 2015), entry regulation (Djankov et al., 2002; Klapper et al., 2006), and labor market institutions (Acs et al., 2016; Hombert et al., 2020).

tion dissipate local gains (Glaeser and Gottlieb, 2008; Kline and Moretti, 2014). Yet frictions in student and worker mobility can preserve gains locally (Fabre, 2026), and recent work develops equity and efficiency rationales for targeting distressed areas (Bartik, 2020; Gaubert et al., 2025). The empirical evidence on the effectiveness of such policies is correspondingly mixed. Business tax incentives and enterprise zones, the most common in practice, are costly per job created and often displace activity from neighboring areas (Givord et al., 2013; Briant et al., 2015; Neumark and Simpson, 2015; Slattery and Zidar, 2020), although the federal Empowerment Zones raised the wages and employment of zone residents (Busso et al., 2013).[5] Even the archetypal big push, the Tennessee Valley Authority, generated local agglomeration gains that persisted for decades but were largely offset by losses elsewhere (Kline and Moretti, 2013). In contrast, the new colleges raised local economic activity without displacing it from neighboring cities, and the cohorts exposed to them were more educated and better employed decades later.

The paper is organized as follows. Section 2 presents the institutional background and the policy. Section 3 describes the data. Section 4 lays out the empirical strategy. Section 5 presents the effects of new colleges on local economic activity. Section 6 examines local labor markets and the long-run outcomes of exposed cohorts. Section 7 turns to the heterogeneity of the effects, spatial convergence, and the policy's fiscal returns. Section 8 concludes.

# 2 The Expansion of Higher Education in France

## 2.1 Background

Unlike in the United States, where higher education expanded in the early and mid-20$^{\text{th}}$ century (Goldin and Katz, 1999, 2008), France experienced a delayed but significant increase in enrollment at the end of the 20$^{\text{th}}$ century. This process unfolded in three successive waves of "massification," which collectively reshaped the country's educational landscape.

The first wave occurred in the aftermath of the student protests of May 1968. The government responded with the "Faure Law" (November 12, 1968), which established open admissions for high school graduates at their regional public universities and funded the construction of fourteen new institutions (Musselin, 2004).[6] Combined with low tuition fees, approximately €150 per year (less than 200 USD), these reforms led to a notable increase in university enrollment (Maurin and McNally, 2008), though the share of the population holding a university

[5] Beyond tax-based schemes, Criscuolo et al. (2019) evaluate direct investment subsidies in the UK and find employment gains confined to small firms, with no effect on larger recipients.

[6] These include, for example, the universities of Limoges, Picardie, and Saint-Étienne.

degree remained modest until the early 1990s.

A second wave followed in the 1980s, driven largely by local governments establishing satellite campuses in medium-sized cities (Filatre and Grossetti, 2003; Ferréol, 2010). This decentralized expansion was widely criticized as insufficient and fragmented: capacity failed to keep pace with demand, which was growing both from the large cohort born during the 1960s baby boom (Appendix Figure A.1, Panel (a)), and from a sharp rise in secondary school completion.[7] In 1985, the Ministry of Education set an ambitious target of 80% high school graduation (Baccalauréat) per cohort. Although this goal was met only in the 2010s, the completion rate rose sharply through the late 1980s, further enlarging the pool of university-eligible students.[8]

Third, following a decade of small-scale and decentralized initiatives, the French central government launched a major higher education expansion effort in 1990: the *Universities of the 21$^{st}$ Century* plan. As one of the most ambitious college expansion policies in French history, this nationwide construction plan aimed to close the widening gap between the demand for seats and available capacity, while extending geographic access beyond the major university cities. The next section describes this policy in detail.

## 2.2 Universities of the 21$^{st}$ Century

The plan was launched in 1990 by Socialist Minister of Education Lionel Jospin. Originally conceived as a five-year construction program (1991–1995), the initiative followed an emergency program (1989–1990) that had created only temporary teaching space, and was designed instead as a durable response to structural capacity constraints. The policy was then extended under the subsequent right-wing government, with additional facilities built through the early 2000s. In total, the program represented an investment of $10 billion (in 2018 U.S. dollars), making it one of the largest public investments in higher education in modern French history.

### 2.2.1 Objectives and Motivations

The plan pursued three main objectives. First, it aimed to expand the capacity of the French higher education system through the construction of hundreds of new establishments and departments (fields) within existing universities. Second, it sought to reduce territorial disparities in access: students were disproportionately concentrated in Paris prior to this initiative, while

[7] The demographic pressure proved only temporary: birth rates declined sharply following the 1973 oil shock and have remained lower since.

[8] The rise was driven both by the introduction of a vocational diploma (the "Baccalauréat Professionnel") and by increased enrollment in traditional tracks. The fraction of a cohort obtaining a "Baccalauréat Général" (the standard route to university) grew from 21.7% to 37.2% within eight years. By contrast, the United States had already reached a high school graduation rate of nearly 80% per cohort by the early 1970s (see Goldin (1998); Goldin et al. (2009) and Doxey et al. (2026) on high school expansion in the US).

many other cities lacked sufficient higher education infrastructure. Accordingly, the policy prioritized the development of small and medium-sized institutions distributed broadly across the country. Third, the government viewed the expansion of scientific and technical education as a long-run lever for economic growth.

Two main factors motivated the initiative. The first was the Socialist government's political commitment to democratizing higher education. The second was a growing mismatch between student demand and available capacity: the Ministry of Education projected an additional 300,000 students by 1995, when universities were already operating near full capacity, as illustrated by the declining space-per-student ratio shown in Appendix Figure A.1, Panel (b). Enrollment ultimately rose by more than twice the projected increase, a surge the policy not only absorbed but appears to have encouraged. I provide causal evidence on this in Section 5.

### 2.2.2 Implementation

Between 1990 and 2003, 474 departments were opened, particularly in two-year technical colleges (IUT), and 399 new colleges were established, including 9 full universities, 37 technical colleges, 161 satellite campuses, 46 engineering schools, and roughly 100 "Instituts Universitaires Professionnalisés" (IUP). Figure 1, Panel (a) displays the cumulative number of openings by institution type; Appendix Table B.1 provides an annual breakdown. Hereafter, I refer to both newly created departments and new establishments as *colleges*.

These new institutions share four main characteristics. First, they are typically smaller than traditional universities, with enrollments ranging from several hundred to a few thousand students. Second, they largely focus on short-cycle education: satellite campuses typically offer two-year diplomas (*Diplôme d'Études Universitaires Générales*, DEUG), while technical colleges award two-year technical diplomas (*Diplôme Universitaire de Technologie*, DUT) combining coursework and internships.[9] Third, they are predominantly "local," serving students from the surrounding area, many of whom remain in their region after graduation (Section 3.2). Fourth, they emphasize scientific and technical fields. Appendix Figure A.2 illustrates the distribution of newly opened departments by field of study, with Panel (a) including all colleges and universities and Panel (b) focusing on technical colleges. Consistent with the policy's objectives, scientific and technical disciplines dominate (approximately 53% of all new departments), followed by economics, business, and law (around 29%), while humanities account for only 11%. In technical colleges specifically, most new departments offer business-related majors (e.g., mar-

[9]Students educated in IUT typically work as skilled technicians or middle managers under the supervision of engineers. In contrast, IUPs and engineering schools offer longer programs, awarding four- and five-year engineering diplomas, respectively.

keting, management, accounting) or scientific and technical fields (e.g., telecommunications, IT, computer engineering, chemistry).

In line with the decentralization objective, new colleges were distributed broadly across the French territory (not only in large cities but also in many small and medium-sized urban areas), generating substantial variation in their location. Figure 1, Panel (b) highlights the urban areas that received new capacity between 1990 and 2003. While higher education was highly concentrated prior to the reform (over 27% of students lived in the Paris region in 1990, 17.6% in Paris alone), the policy broadened access considerably. The share of students in the municipality of Paris fell to 14% in 2000, while that of the Paris region (excluding Paris) increased due to the creation of four new universities. To examine the allocation of new colleges more systematically, I estimate probit and logit models where the dependent variable is an indicator equal to one if an area received any new higher education capacity during the policy period, and zero otherwise. Covariates include 1990 area-level characteristics such as population, age distribution, educational attainment, employment structure, and business dynamism (Appendix Table B.2). The results show that larger areas and those with greater potential student demand (a larger youth population and a higher share of high school graduates) were significantly more likely to receive new colleges. Almost no new colleges were built in cities with fewer than 20,000 inhabitants.[10] Areas with higher public sector employment shares were similarly more likely to receive new capacity. In contrast, initial economic conditions, including employment, industry composition, average wage, firm entry rate, and local political orientation, show little correlation with treatment. These results suggest that allocation was driven primarily by demographic rather than economic considerations.

Alongside physical expansion, the higher education budget rose significantly: spending grew from around 1.1% of GDP in the mid-1980s to 1.34% in 1995 (from €16 billion to €22 billion in 2018 euros), stabilizing around 1.3% thereafter (Appendix Figure A.1, Panels (c) and (d)). The construction program itself was financed through multi-year commitment authorizations, with funds disbursed in annual tranches through appropriations voted in each year's budget act, an arrangement the government described as "rolling multi-year programming." This staggered disbursement reflected two constraints: the program's cost could not be absorbed in a single budget year, and constructing hundreds of new facilities simultaneously was deemed infeasible. As a result, new colleges opened gradually between 1990 and 2003.

In step with college openings and spending, faculty numbers increased sharply across all categories (Appendix Figure A.1, Panel (e)), with hiring beginning in 1989 and continuing steadily

[10]The 20,000-inhabitant threshold corresponds to the $5^{th}$ percentile of the distribution of treated areas, but the $68^{th}$ percentile of the French city-size distribution.

throughout the 1990s.[11] The government supplemented regular faculty through two channels: first, qualified high school teachers are authorized to teach university freshmen and sophomores, and their number increased significantly from 1992; second, technical colleges require half of their faculty to be professionals employed in the private sector. These flexible labor margins enabled a rapid scale-up in instructional capacity. Evidence from the National Archives reveals no substantial deliberation over faculty recruitment, suggesting that the supply of potential faculty exceeded initial demand.

Finally, the 1990s expansion far surpassed those of adjacent decades. Appendix Figure A.1, Panel (f) shows the cumulative number of new establishments (i.e., excluding new departments) constructed between 1980 and 2010. During the 1980s, about 100 new colleges were built, a figure that more than tripled in the 1990s. Construction continued into the 2000s but at a lower pace. The successor plan, *University of the Third Millennium* ("Université du 3e millénaire," U3M), shifted funding from new institutions to renovations and support facilities such as libraries, dormitories, and cafeterias.

### 2.2.3 Allocation Process

The number and location of new colleges were determined through a process involving both national and local officials, unfolding in three phases. First, local elected officials, *Préfets* (the State's representatives), and school superintendents identified local needs and made preliminary recommendations on the number and location of new institutions. Second, expert working groups from multiple ministries (Education, Economy, and Spatial Planning) synthesized these proposals into a nationally coherent plan, guided by projections from the French National Institute of Statistics and Economic Studies (INSEE) of expected enrollment by 2000 in each of France's 22 metropolitan regions. Third, a final round of local consultations preceded submission to the central government for approval. The allocation of the national budget and the final location of new institutions across regions were based on three primary criteria: (i) projected student demand, (ii) the existing stock of higher education establishments, and (iii) geographic access. *Ex ante* economic assessments were rare, consistent with decisions being primarily shaped by demographic considerations. Descriptive statistics on areas ultimately selected are provided in Section 3.2. Notably, this process determined the location of new colleges but not the pace at which each site opened: Section 4 examines the drivers of this timing, which underpins the identification strategy.

[11] The traditional French higher education system recognizes only two ranks: Assistant Professor ("Maître de conférences") and Full Professor ("Professeur des Universités"), both tenured civil-service positions.

### 2.2.4 Educational Attainment

Between 1989 and 1995, enrollment in higher education grew by approximately 655,000 students, a 44% increase (Figure 2, Panel (a)). The rise in the stock of graduates was equally marked: the share of 25–34-year-olds holding a college degree rose from 18% in 1990 to 31% in 2000 and 43% in 2010 (Panel (b)).[12]

The *Universities of the 21st Century* plan was a central driver of the massification of higher education in France and a rare example of a modern, large-scale reform that durably raised educational attainment. Its scale and the variation in the timing and location of new institutions make it a valuable setting for studying the causal effects of higher education on individuals and local economies.

# 3 Data and Descriptive Statistics

To assess the impact of new colleges on local economic activity and individual labor market outcomes, I combine multiple data sources: (i) administrative, archival, and survey data on higher education establishments and new graduates, (ii) an administrative registry of firms, balance sheet data, and a survey of entrepreneurs, (iii) linked employer-employee data (LEED), the labor force surveys (LFS), and census data. Further details on the data and sample selection process are provided in the Data Appendix (Section C).

## 3.1 Geography

The empirical analysis primarily relies on cross-city comparisons over the period 1985–2003, aggregating data at the level of urban areas (*aires urbaines*). Unlike municipalities, which follow administrative borders, they provide a functional definition of cities based on economic activity and commuting patterns. Urban areas cover the vast majority of the French population and economic activity, and nearly all new colleges (all but three) were built within them. This makes them the natural unit for the question at hand, since new colleges shape both where students can study and where their graduates subsequently work. By contrast, commuting zones partition the territory on the basis of commuting flows alone, and can therefore split the catchment area of a single institution across several units, attenuating measured effects; I nonetheless verify robustness to this alternative definition, as well as to provinces. I restrict the sample to the 759 urban areas located in continental France, as consistent data for Corsica and overseas territories

[12] The respective figures were 15% in 1990, 24% in 2000, and 33% in 2010 when focusing on people aged 25–55. See Appendix Figure A.3.

are not available for the full period.

### 3.2 Higher Education

To identify new colleges opened during the policy, I exploit administrative data on all public universities and colleges constructed in France since 1880. This registry, maintained by the Ministry of Higher Education, provides detailed information on the date of opening, precise location, and academic disciplines offered by each public institution. It includes universities, satellite campuses, two-year technical colleges (IUT), and engineering schools.[13] The registry also documents new departments opened within existing universities (e.g., humanities at the University of Dunkirk), though not those within two-year technical colleges. To address this gap, I complement the registry with hand-collected data on newly opened fields in technical colleges, sourced from the official gazette of the French Republic ("Journal Officiel"). Finally, the registry does not report capacity at the time of opening; I therefore proxy each establishment's size with its 2016 enrollment, which captures the capacity ultimately put in place. Details on the data and imputation procedure are provided in Appendix Sections C.1 and C.2, respectively.

To better understand the post-graduation trajectories of students, I draw on data from the 1994 "Enquête de Cheminement: sortants de l'éducation supérieure," a survey that tracks higher education graduates for up to three years after graduation. Two- and three-year colleges primarily train skilled technicians and middle-skilled white-collar workers (see Table 1). For instance, graduates from technical colleges commonly work as accountants, lab technicians, or IT technicians, particularly in manufacturing and skilled service sectors such as telecommunications. By contrast, engineering schools and IUPs tend to train future managers and engineers, including IT specialists, research engineers, and analysts. The data show high local retention rates across all diploma types: a large share of graduates work in the area where they studied. Retention is especially high among graduates of shorter programs: over 70% for two- and three-year university degrees and 64% for two-year technical college diplomas. Moreover, these institutions primarily attract local students: three-quarters of new graduates earned their high school diploma in the same region where they pursued higher education.[14] Finally, they educate students from relatively lower socio-economic backgrounds. While almost 50% of graduates from engineering schools have a father with a university diploma, the figure is only 20% for graduates of two-year technical colleges. These figures underscore the role of these colleges in serving the local

[13] Although the data cover only the public sector, the private sector accounts for a small and stable share of total enrollment over the period: 12.88% in 1990 and 12.84% in 2000 (DEPP and SIEP, 2010, 2021).

[14] Consistent with these patterns, Fabre (2026) documents that French students' demand for higher education is highly sensitive to distance.

population, both for enrollment and for labor market integration. Appendix Section C.3 and Appendix Table B.3 provide additional descriptive statistics on graduates and their labor market outcomes, disaggregated by type of education.

Finally, Table 2 summarizes key characteristics across three groups of cities: (i) areas that received new capacity, (ii) areas that already had at least one college prior to the policy, and (iii) all 759 urban areas. In line with the policy's goal of expanding access to education, treated areas are on average smaller and less educated than university towns. Along other dimensions, including age distribution, unemployment, and firm entry rates, the three groups are very similar.

### 3.3 Firms and Entrepreneurs

I measure firm entry, survival, and exit using the National Enterprise and Establishment Register Database (*Sirene register*) maintained by INSEE. This comprehensive registry covers the universe of private-sector firms in France since 1987, excluding those in agriculture and finance. It records the creation of new firms and establishments on a monthly basis.[15] For each new unit, the dataset provides the precise date of creation (defined as the registration date), the location of the headquarters at the municipality level, a four-digit industry code, and the number of employees at creation. The analysis focuses on the creation of incorporated firms, which are most likely to contribute to economic growth and hire skilled labor.[16] In contrast, sole proprietorships often reflect "subsistence entrepreneurship" and are less likely to expand. In Section 5, I show that most new firms created following the opening of a new college were indeed incorporated firms. Appendix Table B.4 provides descriptive statistics on firm creation in France over the period of study. I exclude construction firms from the main sample to avoid capturing the mechanical effect of college construction itself, though they are included in industry-specific analyses.

I complement the *Sirene register* with the *SINE* ("Système d'Information sur les Nouvelles Entreprises") survey on French entrepreneurs. Conducted every four years by INSEE, the survey covers a representative sample of approximately one-third of all firms created in the first half of each survey year, starting in 1994.[17] It provides detailed information on both entrepreneurs (age, education, professional background) and firms (industry, size, primary clientele). I use this survey to examine whether new firms are founded by recent graduates, experienced workers, or serial entrepreneurs (Appendix Table B.5 reports founder characteristics across the three survey

[15] Establishments correspond to new locations opened by existing firms while new firms are defined as new registrations, created *ex nihilo*.

[16] "Société Anonyme" (SA) and "Société à Responsabilité Limitée" (SARL) correspond to firms incorporated with the legal categories "54, 55, 56" in the INSEE classification. The results are unchanged when I consider all corporations (legal categories starting with "5") and not only "SA & SARL."

[17] See Landier and Thesmar (2009) for a detailed description of this survey.

waves). Additionally, SINE identifies firms' main clients (e.g., individuals, businesses, or public administrations) and the geographical scope of their market (local, national, or international). This allows me to assess whether new firms primarily serve local markets (including potentially new colleges themselves) or operate at a broader scale.

Finally, I use firms' balance sheet data (FICAS, 1985–1999) to quantify the effects of new colleges on firm growth. Like the Sirene sample, the data cover for-profit firms outside agriculture and finance. The data report employment, wage bill, turnover, value added, investment, capital, and profits. Because some firms operate establishments in areas treated at different times, I restrict the analysis to single-establishment firms and multi-establishment firms located within the same urban area. The sample covers most firms in the economy, except the largest ones.[18] As a robustness check, I compare employment and wage results with those from linked employer-employee data (LEED), available at the establishment level, and show that estimates are consistent.

### 3.4 Workers and Local Labor Markets

I rely on three main data sources to study local labor markets and the level of education of the workforce. First, I draw on the French linked employer-employee data (DADS, 1985–2003) to investigate the effects of new colleges on employment, wages, and education. The data are based on mandatory employer reports of the earnings of each employee subject to French payroll taxes. They contain information on employees' demographics, jobs (e.g., length of the spell, gross wage, occupation), and their firms and establishments. They cover a random sample of about 4% of the workforce. I merge the DADS with census data (the "Échantillon Démographique Permanent," hereafter EDP) to recover workers' highest level of education. I restrict the sample to mainland France and focus on workers aged 18–60, excluding from the main analysis public sector and construction workers to avoid mechanical effects from new colleges. This provides a conservative estimate of the effect of colleges on local employment. Finally, I keep all individuals who earn at least the equivalent of a month of full-time minimum wage during the year to focus on economically meaningful jobs.

I complement the linked employer-employee data with the French labor force surveys. While the LEED include only employed individuals, the LFS cover all residents, allowing me to track local population, student enrollment, and unemployment in areas receiving new colleges. Although the national sample is large (∼75,000 households), representativeness is limited at the urban area level. Consequently, the analysis using LFS data is conducted at the province (*dé-*

[18] Single-establishment firms represent approximately 92% of firms in the sample and 41% of employment.

*partement*) level, covering 94 provinces in mainland France.[19]

Finally, I draw on the population censuses, which provide detailed demographic and labor market information, including age, place of residence, education, occupation, and industry. First, I use the 1990 census to recover pre-reform characteristics at the urban area level. Second, I exploit the 2015 census to study the long-run effects of new colleges on educational attainment and labor market outcomes at the individual level, twenty-five years after the policy began. The census records both the date and municipality of birth, which I use to identify cohorts directly exposed to new colleges, i.e., individuals young enough to enroll when the colleges opened.

# 4 Empirical Strategy

This section describes the two empirical strategies used to identify the causal effect of new colleges on local economic activity. I begin by estimating the effects at the urban area level, exploiting the staggered rollout of the policy in an event-study design. I then use variation across industries in their pre-reform skill intensity to implement a triple-difference framework.

## 4.1 Staggered Event Study Design

The main empirical exercise compares early- and later-treated areas. As shown in Section 3, treated areas differ from untreated ones in their 1990 characteristics: they tend to be larger and younger. Using untreated areas as a control group could therefore lead to biased estimates. The *timing* of treatment, however, is unlikely to reflect economic conditions for two reasons.

First, the *Universities of the 21$^{st}$ Century* plan was one of France's first decentralized higher education policies: although higher education falls under central government authority, the plan was negotiated at length with local governments. The resulting agreements fixed the location of new sites, not the calendar of openings, alleviating concerns about strategic timing. Second, the timing that emerged was governed by administrative and budgetary steps. A site could open only once local governments had signed an agreement with the central government, land had been secured, and building permits obtained, all conditional on the availability of the plan's funds, which, as described in Section 2.2, were disbursed on a rolling basis. Because each year's envelope went to the operations that were ready, a site that missed a single step was carried over to a later tranche, and small frictions translated into delays of a year or more.[20] Table 3

[19] Provinces were created during the French Revolution and cover roughly equal land areas.

[20] Parliamentary exchanges illustrate how multilevel coordination produced such delays. In 2001, a deputy from Lorraine complained that feasibility studies had been completed a year earlier and local governments had honored their commitments, yet the central government was slow to release its share of funding. The ministry

confirms this in the aggregate: total construction closely matched the government's target, but the annual path diverged substantially from the schedule, especially at the beginning and end of the period. These deviations trace to the individual steps of the process. The complexity of multilevel decision-making meant the final plans were not signed until 1992 in many areas. In addition, archival evidence (DATAR, 1998) indicates that some cities opened colleges quickly by repurposing existing public buildings (such as fire stations), whereas others faced longer delays due to the lack of available public land and the time required to obtain permits and construct the new colleges. This introduces quasi-random variation in treatment *timing*.

I conduct two direct tests to verify that later-treated areas serve as credible counterfactuals for early-treated ones. First, I examine whether the timing of treatment correlates with cities' pre-policy characteristics. While this is not required for the validity of difference-in-differences, finding no statistically significant relationship provides additional support for the empirical strategy. I regress the year of treatment on a range of 1990 demographic and economic characteristics. Panel (a) of Figure 3 and Appendix Table B.6 show that none of the 22 coefficients is statistically significant at conventional levels, and magnitudes are modest, within plus or minus one year. There is no systematic relationship between timing and population (both in levels and in growth), age structure, distance to Paris, education, industry composition, local labor market conditions (employment, wages, public sector), firm creation, or political orientation. This holds whether the treatment is defined as the first year an area received new capacity or as any year in which new capacity was added. Second, I perform placebo tests to assess pre-trends in education, employment, wages, firm creation, and firm growth (Sections 5 and 6). Pre-treatment coefficients are consistently small and not significant, confirming that early- and later-treated areas were evolving at a similar pace before treatment. The dynamics of the response further argue against strategic timing: effects on local economic activity appear only two to three years after colleges open (once the first cohorts graduate), not immediately, as they would if openings had been timed to accompany an anticipated upswing. Together, these findings support the validity of using later-treated areas as a control group. In addition, the triple-difference design presented in the next section relaxes the parallel trends assumption: exploiting variation in skill intensity across industries within areas, it differences out time-varying, location-specific shocks common to all industries, so that identification requires only that industries within an area share parallel trends.

Finally, Section 6.3 runs a series of additional tests to ensure that the results are not driven

replied that local partners had settled their contributions and the assignment of construction management only progressively, which had spread the completion of operations over time (Assemblée nationale, written question no. 68205, response of 17 December 2001). Neither account relates the delays to local economic conditions.

by systematic differences between early- and later-treated areas, including controlling for initial city size, excluding Paris, and restricting the control group to only the last cohorts of treated areas, rather than all not-yet-treated areas.

**Empirical model.** I estimate the city-level impact of new colleges using the following staggered event-study specification:

$$y_{at} = \alpha_a + \delta_t + \sum_{\substack{i=-7 \\ i \neq -1}}^{8} \gamma_i \, \mathbb{1}\{t = \text{Year treatment}_a + i\} + \epsilon_{at} \tag{1}$$

where $y_{at}$ denotes the outcome of interest in area $a$ and year $t$, including the local college graduate share, firm creation, firm growth, employment, wages, and population, with $\epsilon_{at}$ the error term. Variables are expressed in logs, with the exception of the college share.[21] $\alpha_a$ are area fixed effects controlling for time-invariant characteristics. $\delta_t$ are year fixed effects capturing time-varying shocks that affect all areas in a similar way. *Year treatment*$_a$ refers to the first year in which area $a$ received new higher education capacity. Panel (b) of Figure 3 plots the number of newly treated areas by year. The main coefficients of interest, $\gamma_i$, trace the dynamic effect of the policy $i$ years after an area receives its first college; coefficients for $i < 0$ serve as placebo tests for differential pre-trends. These parameters reflect the causal impact of new colleges under the standard parallel trends assumption: in the absence of the policy, early- and later-treated areas would have followed similar trajectories.

I estimate $\gamma_i$ using estimators robust to heterogeneous treatment effects, as such heterogeneity can bias two-way fixed-effects estimates in staggered designs (de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). Specifically, I use the estimators of de Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2021), which compare newly treated areas to not-yet-treated ones.[22] I show that the results are robust to the imputation estimator of Borusyak et al. (2024) and to restricting the control group to the last-treated cohorts (Sun and Abraham, 2021). Regressions are weighted by 1990 area size to ensure that estimates reflect the economic contribution of each area.[23] Standard errors are clustered at the urban area level.

I complement the binary treatment model with continuous specifications that account for variation in the number and size of new colleges across areas, using as treatment either the

[21] For outcomes with zeros, I use the inverse hyperbolic sine transformation, which approximates the logarithm for large values while handling zeros. This is the case, for example, for firm creation in some area-industry cells.

[22] I estimate event studies only through 2001, as identification requires not-yet-treated areas and all areas are treated by 2003.

[23] Regressions on firm outcomes are weighted by the number of firms in 1990, and labor market regressions are weighted by the 1990 workforce size.

number of new colleges or the number of new seats (in levels or relative to the population aged 25–55). These specifications rescale the reduced form by the dose of treatment, so that coefficients measure the effect of an additional college or seat.

### 4.2 Exploiting Variation Across High- and Low-Skill Industries

While the event study design exploits variation across space and time, it does not leverage differences in exposure based on skill demand. If new colleges affect the local economy primarily through the supply of educated workers, effects should be stronger in skill-intensive industries. I test this hypothesis using a triple-difference strategy, comparing outcomes across higher- and lower-skilled industries in treated versus untreated areas. This specification leverages *within-area* variation to difference out location-specific characteristics and shocks. I estimate the following model:

$$y_{ajt} = \psi \text{ Exposure}_j \times \text{After}_t \times \text{ New Colleges}_{at} + \alpha_{at} + \delta_{jt} + \lambda_{aj} + \epsilon_{ajt} \tag{2}$$

where $Exposure_j$ measures the share of higher-skilled workers in the two-digit industry $j$ computed *before* the policy using the French LEED. I proxy industry skill intensity using either (i) the share of workers with a university degree, or (ii) the share of workers in high-skilled white-collar occupations.[24] Appendix Figure A.4 presents the top and bottom ten two-digit industries ranked by the share of university graduates. High-skill industries include R&D activities; office machinery and computer manufacturing; IT; telecommunications; and business services. In contrast, lower-skill industries include textiles, leather, and clothing; personal services; waste management; and ground transportation. New Colleges$_{at}$ is either a binary indicator for whether area $a$ received new higher education capacity before time $t$, or a continuous measure of the number of higher education establishments constructed. After$_t$ equals one in the post-period. $\psi$ captures the additional impact of new colleges on the outcome for industries that employ a 1 percentage point higher share of skilled workers. The model is estimated in long differences. To reduce noise from small area-industry cells, I average outcomes over two three-year periods centered on 1988 and 2000. The regressions are weighted by the number of firms in the area-industry cell observed in 1988.

Finally, the specification is saturated with fixed effects to address potential confounders. Area-by-industry fixed effects, $\lambda_{aj}$, absorb all time-invariant characteristics of each area-industry pair, such as an area's specialization in particular industries. Area-by-year fixed effects, $\alpha_{at}$, ab-

[24] High-skilled white-collar occupations include engineers and managers. Unlike the level of education, the occupation code is available for all workers.

sorb all time-varying shocks at the area level, so that identification no longer requires parallel trends across areas, but only that high- and low-skill industries would have evolved similarly within an area absent the policy. Industry-by-year fixed effects, $\delta_{jt}$, capture nationwide changes in industry dynamism over time, ensuring that the estimated effect of new colleges is not driven by nationwide differential trends between higher- and lower-skilled industries. These controls further alleviate concerns that policymakers may have strategically timed the opening of new colleges based on expected growth in certain areas or industries.

# 5 Human Capital and Business Dynamism

## 5.1 Local Human Capital

I begin by examining the impact of new colleges on the educational attainment of the local workforce. Although the policy expanded access to higher education, this does not necessarily imply a corresponding rise in the local supply of skilled workers, as graduates may choose to relocate. Figure 4 suggests otherwise. Cities that received new colleges experienced a steady increase in higher education capacity (Panel (a)), followed about three years later by a rise in the share of workers with a college degree (Panel (b)).[25] The lag matches the minimum duration of university programs in France, and the effect strengthens over time as successive cohorts enter the labor market. On average over the post-treatment period, the local share of college graduates rose by 2.4 percentage points (s.e. = 0.72), reaching 3.6 percentage points eight years after the first opening. These gains correspond to increases of 17% and 25%, respectively, relative to the 1989 baseline of 14.3%.[26] Finally, placebo coefficients are small and not statistically significant, indicating that early- and later-treated areas were on similar educational trajectories before the colleges opened. This supports the validity of the parallel trends assumption.

Using data from the Labor Force Surveys and a 1994 survey of higher-education graduates, I find that the rise in educational attainment stemmed from both a substantial increase in enrollment and a high local retention rate of new graduates. Column (1) of Table 4 displays the results for enrollment at the province level. Early-treated areas experienced a 5.4 percentage point increase in the enrollment rate of 18–24-year-olds, which translated into a 1.7 percentage point rise in the share of college graduates (column (2)). A key contributor to this effect was

[25] Year 0 corresponds to the first year new colleges were active (i.e., started providing education).

[26] Accounting for treatment intensity, a continuous specification implies that adding new college seats equivalent to 1% of the population aged 25–55 raises the local share of college graduates by 1.84 percentage points on average (Appendix Table B.7). In contemporaneous work, Charruau (2024) studies eight of the newly opened sites and similarly documents a persistent rise in the local college share and skilled employment. In contrast, my analysis covers the full policy, encompassing several hundred new facilities.

the high retention of graduates: in 1994, over 70% of two- and three-year university graduates remained in the region where they studied (see Appendix Section C.3 for more details). Consequently, the expansion of colleges significantly increased the local supply of educated workers, thereby expanding the talent pool available to firms. I now turn to the economic consequences of this supply shock, beginning with business dynamism.

## 5.2 Business Dynamism and Growth

### 5.2.1 Firm Creation

Young firms are key drivers of employment growth, GDP, and innovation (Haltiwanger et al., 2013; Decker et al., 2014; Adelino et al., 2017; Walsh, 2020). In this section, I examine whether the construction of new colleges spurred business formation.

As a first step, I consider the creation of new establishments, which include both newly registered firms (i.e., firms created *ex nihilo*) and new branches of existing firms. Figure 5 presents event-study estimates for the log number of new establishments (Panel (a)) and the establishment entry rate (Panel (b)). The effect is substantial: eight years after opening, the number of new establishments had risen by 16%, with the entry rate up by 1.5 percentage points. Coefficients are small and not significant before the policy and in the first two to three years afterward; they then turn positive and rise steadily, tracking local educational attainment. This pattern is informative in two respects. The flat pre-treatment coefficients again support the parallel trends assumption and indicate that the timing of the policy was not driven by local dynamics already under way. The lag suggests it was not driven by anticipated growth either: colleges opened to accompany an imminent upswing would have seen entry rise at the opening. Instead, entry rose only once the first cohorts graduated, as expected if firm creation responds to the inflow of new graduates into the workforce, or if graduates themselves start new firms. I examine these mechanisms in the next section.

Second, I decompose business formation into new sole proprietorships, new corporations, and new establishments opened by existing firms. Appendix Figure A.5 reports event-study estimates for each of these three margins. The increase is driven almost entirely by new corporations, which rise by 16% on average over the period (Figure 6, Panel (a)). By contrast, the effect on sole proprietorships is close to zero, suggesting that the results are not driven by "subsistence entrepreneurs," small businesses with limited growth or job-creation potential (Schoar, 2010; Hurst and Pugsley, 2011). The opening of new establishments by existing firms rises only slightly and temporarily. For the remainder of the section, I therefore restrict attention to newly created corporations, excluding sole proprietorships and establishments opened by

existing firms.

To characterize the firms created, I extend the analysis to the industry level. Table 5 presents estimates for each one-digit industry, averaged across treatment periods, and Appendix Figure A.6 plots year-by-year estimates for four aggregated industries. Firm creation increased across most industries, including manufacturing, agrifood, retail, transportation, hospitality, and household services. Section 5.2.2 exploits differences across industries, in particular their skill intensity and whether they serve local or national markets, to identify the channels behind this response.

**Potential displacement effects.** A natural concern is that the rise in firm creation reflects displacement rather than net entry: entrepreneurs might simply have established their businesses in treated areas instead of untreated ones, leaving aggregate firm formation unchanged. Testing for displacement at the national level would require a "super" control group (areas unaffected by both the direct and indirect effects of new colleges). I instead reproduce the analysis at a more aggregated geographic unit, the province level. If firm creation is merely reallocated across cities within the same province, the estimated effect should weaken or disappear at this broader level. The province is a natural unit for this test, since most entrepreneurs are "local": according to the SINE survey, 83% of entrepreneurs in 2006 established their business in the same province where they resided, and 54% in 1998 did so in their own municipality or a neighboring one.

Because provinces contain multiple cities, there is less variation in the timing of openings than at the urban-area level, so I focus on effects up to five years after the first opening. Firm creation at the province level remains significantly positive, rising by approximately 20% after five years (Appendix Figure A.7), evidence of a genuine net gain in business formation rather than local reallocation.[27]

**Quality of new firms.** While the number of new firms increased significantly, the overall impact on economic activity depends on their composition. I first use employment at registration, a proxy for initial quality that is independent of subsequent growth. Despite the substantial rise in entry, the size of new firms did not decline, so that total employment in new firms rose by about 15% (Figure 6, Panel (a)). Average quality therefore does not appear to have deteriorated, as one would expect if the additional entrants were marginal, less productive firms.

Initial size is only an imperfect proxy, however, since new firms typically start small. I

[27]Displacement could still operate through national competition if, for example, more productive entrants in early-treated areas crowded out entry elsewhere. Such effects are unlikely to be quantitatively meaningful in the short or medium run, however: new firms typically start small and need time to grow large enough to exert competitive pressure at the national level.

therefore turn to their subsequent performance, matching the registry of firms with linked employer-employee and balance sheet data observed seven years after creation.[28] I compare firms established before and after the opening of a new college, in both early- and later-treated areas. Because firms created before the policy were nonetheless exposed to new colleges later in their life cycle, albeit for a shorter period, the estimates likely represent a lower bound. Appendix Table B.8 presents the results. The policy had a small and insignificant effect on survival at age seven (column (1)), though the large increase in entry implies a much higher absolute *number* of surviving firms. Point estimates suggest that these firms are also somewhat larger, employing about 5% more workers, both young and older, though the effects are imprecisely estimated (columns (2)–(4)). They are, however, more skilled: the number of managers and engineers rose by nearly 5%, with a slightly larger effect among young skilled workers (columns (5)–(7)). New firms also show higher profits and total factor productivity, with positive but imprecise estimates for sales. Overall, these results indicate that the upskilling of the local workforce, caused by the new colleges, triggered the creation of higher-quality firms.

**Education and firm creation.** Evidence from U.S. cities shows that a more educated workforce is associated with higher rates of firm formation (Glaeser and Kerr, 2009; Glaeser et al., 2010; Doms et al., 2010). Establishing causality is challenging, however, as entrepreneurial cities may attract skilled workers rather than the reverse. The policy I study provides a shifter of the local level of education that can be used to study this relationship. Results presented above show a positive impact of new colleges on both local educational attainment and firm entry. I now examine the relationship directly, asking whether a larger shift in the level of education is associated with a larger response in firm entry. As a first step, Appendix Table B.7 reports estimates from a continuous specification that accounts for treatment intensity. On average, each new college raised the share of college graduates by 0.07 percentage points and firm creation by 0.4% over the study period. Expanding capacity by 1% of the population aged 25–55 raises local educational attainment by 1.8 percentage points and firm creation by 13%.

Second, I turn to the semi-elasticity of firm creation with respect to education, using the construction of new colleges as a shifter of workforce education. This quantification exercise, detailed in Appendix Section D, rescales the effect on firm entry by the effect on education, under the assumption that the entry response operates entirely through improved educational attainment. To mitigate potential demand effects from local students and professors, I focus

[28] The gap corresponds to the shortest interval between the start of the Sirene registry (1987) and the first available year of the comprehensive Linked Employer-Employee Data (1994). Consequently, firms created between 1987 and 2003 are observed in the LEED from 1994 to 2008. See Appendix Section C.1 for more details on the comprehensive LEED.

on firms in tradable industries, specifically manufacturing, where production is geared toward national and international markets. Consistent with this assumption, there is little effect on firm creation in manufacturing in the first few years after treatment, when education gains are small but local demand effects should be strongest. A positive effect emerges thereafter, and the resulting ratio provides a benchmark for the strength of the education–entry link.

Appendix Table B.9 presents the results. Column (1) reports the estimates from an ordinary least squares (OLS) regression of the number of firms created on the share of workers with a university degree, controlling for area and year fixed effects. Consistent with prior literature, the OLS coefficient indicates a positive and statistically significant relationship between education and firm creation, with a semi-elasticity of about 2.6%. Column (2) presents the first-stage regression and column (3) the reduced-form impact of new colleges on firm creation; the F-statistic of 114 indicates a strong first stage. Column (4) reports the 2SLS estimate: a one percentage point increase in the share of workers with a university degree raises firm creation by approximately 4.6% (significant at the 1% level). Evaluated at the mean graduate share, this semi-elasticity corresponds to an elasticity of about 0.7: increasing the share of workers with a university degree by 1% would raise firm creation by about 0.7% in the manufacturing sector.

Finally, Section 5.2.2 shows that the firm entry effect increases with industry skill intensity, reinforcing the interpretation that entry responds to the local supply of skills.

### 5.2.2 Mechanisms

I consider three channels that could explain the rise in firm creation: (i) new colleges may raise the supply of skilled workers available to firms and entrepreneurs, (ii) they may boost local consumption through the arrival of students, graduates, and university staff, or (iii) they may interact directly with firms, through procurement or research collaborations. The first is a supply-side channel operating through the labor market, while the latter two reflect local demand.

**Channel (i): Supply of skilled workers.** The policy may have spurred firm creation by relaxing a human capital constraint: a larger pool of educated workers facilitates the adoption of advanced technologies and the implementation of new ideas, an advantage particularly valuable for young firms (Bartel and Lichtenberg, 1987; Doms and Lewis, 2006; Acemoglu et al., 2024). Three pieces of evidence support this channel. First, the dynamics of firm entry closely track those of local educational attainment, both responding about three years after colleges open. Second, entry rose in tradable industries such as manufacturing, where production serves national markets and local consumption is unlikely to drive firm creation. Third, if the supply of educated

workers drives entry, the effect should be stronger in industries intensive in skilled labor. I test this prediction using the triple-difference specification of equation (2), comparing firm entry in higher- and lower-skilled industries across treated and untreated areas. Table 6 reports the triple-interaction coefficient, defining industry skill intensity by the share of college-educated workers (columns (1) and (2)) or of high-skilled white-collar workers (columns (3) and (4)) prior to the policy. Across both measures, firm creation increased significantly more in skill-intensive industries. In line with these results, firms created after the policy employ more managers and engineers seven years later (Appendix Table B.8).

New graduates could contribute to this effect as employees or as entrepreneurs themselves: education correlates strongly with entrepreneurship and business success (Praag et al., 2008), and university training may provide the balanced skill mix that Lazear (2004) argues entrepreneurs require. The SINE survey allows me to separate these two channels. Appendix Table B.10 reports difference-in-differences estimates for entrepreneurs' characteristics in areas treated between 1994 and 2003.[29] The policy *reduced* the share of founders who were students before creating their firm by 4.4 percentage points, and the share aged 25 or younger by 4.7 percentage points. Instead, new entrepreneurs were less likely to have been out of the labor force and more likely to have been top managers or to have previously founded a business, though the latter is imprecisely estimated. Consistent with these results, founders of incorporated firms average 39–40 years of age over the period of study, and only 3–5% were students before creating their firm (Appendix Table B.5). The supply channel therefore operates through the workforce rather than through founders: experienced entrepreneurs created firms that employ more young skilled workers, echoing the finding that successful founders are typically experienced, not recent graduates (Azoulay et al., 2020).[30] These findings suggest that a shortage of human capital in treated cities constrained firm entry.

**Channel (ii): Local product demand.** Firm creation also responded in nontradable industries (retail, hotels and restaurants, household services) and did so immediately after colleges opened, consistent with a local consumption channel (Figure 6, Panel (b), and Appendix Figure A.6). Notably, this demand response did not rest on population growth: the policy had no detectable effect on local population or on the share of young residents (the point estimate for population is $-0.2\%$ and I can reject effects larger than 4% at the 95% level; Table 4, columns (3) and (4)). The rise in demand thus reflects students and staff already residing in the area, in

[29] The first survey wave was conducted in 1994, which rules out studying areas treated earlier.

[30] By contrast, Bernstein et al. (2022) find that local *demand* shocks in Brazil generate entry in nontradable industries driven by young skilled entrepreneurs.

line with the policy's objective of educating local youth.[31] The channel nonetheless complements rather than drives the overall entry response: it cannot account for entry in tradable industries such as manufacturing or for the skill gradient across industries. In addition, the composition of new firms did not shift toward locally oriented businesses: they were no more likely to serve local or regional markets, if anything less so, while the coefficient for national markets is positive but imprecise (Appendix Table B.11, columns (4)–(7)).

**Channel (iii): Direct interactions with colleges.** Finally, new colleges could foster entry directly, through procurement or research spillovers generating spin-off companies (Hausman, 2021; Babina et al., 2023; Bergeaud et al., 2025). Three considerations make this channel unlikely here. First, the establishments built under the policy were overwhelmingly teaching institutions with limited research activity, unlike the research universities studied in prior work; even there, spin-offs are typically too few to account for an increase in entry of this magnitude. Second, research and patenting take many years to materialize, while firm entry responded within three years of the policy. Third, new firms were no more likely to serve public administrations as main clients (Appendix Table B.11, column (3)), suggesting limited direct procurement effects.

Taken together, the evidence points to the supply of educated workers as the main driver of the rise in firm creation, complemented by a local-demand response in nontradable sectors.

### 5.2.3 Incumbent Firms and Local Growth

Having documented the rise in firm creation and its mechanisms, I now turn to incumbent firms and to the overall impact of the policy on local economic activity. Panel (a) of Figure 7 plots event-study estimates for the log number of firms: the coefficients are positive but not statistically significant, indicating little change in the total stock of firms. Since the entry of young firms increased, this implies offsetting exits, confirmed in Panel (b), which shows a rise in the number of firms leaving the market. Entry thus crowded out existing firms, leaving the stock roughly unchanged.

I next turn to surviving incumbents, using a balanced panel of firms active from 1989 (i.e., one year before the policy began) onward. This specification captures the effect for the average incumbent firm. Table 7 shows that incumbents grew more slowly following the policy: sales and value added fell by about 3%, profits by 5.6%, and employment by 1.4%. Average wages also declined slightly (−0.9%), suggesting a second margin of adjustment beyond employment.

[31] The results are also not driven by a mechanical effect of campus construction, as the construction industry and the public sector are excluded from the analysis.

Finally, both labor productivity and total factor productivity decreased.

With entrants gaining and incumbents losing on average, the net effect on local activity is ambiguous. To estimate it, I aggregate the firm-level data at the urban area level, using an unbalanced sample of single-establishment firms to account for entry, exit, and firm growth. Table 8 shows that the positive effects dominate: sales rose by 5.1% and value added by 4.8% (event study in Figure 8). These effects are larger than the Mincerian benchmark implied by reclassifying workers from high school to college, holding everything else constant. The gap reflects the response of firms to the new supply of skills. The rise in value added is driven by a large increase in capital (10.1%), consistent with the firm entry documented above, and by higher productivity: output per worker rises by 7% and TFP by 6.2%. Employment does not change significantly, while average wages and profits both increase, by 4.3% and 7.5%, respectively. The policy hence resulted in creative destruction with positive effects on local economic activity. In the next section, I investigate the incidence of these effects across worker types and the long-run impact on labor market outcomes.

# 6 Local Labor Markets and Long-Run Individual Outcomes

This section investigates the labor market effects across worker groups using linked employer-employee data. This approach offers two advantages. First, establishment-level data allow me to estimate impacts for all firms rather than only single-establishment firms. Second, they provide individual-level information that can be used to study heterogeneity across age and skill groups. I then investigate the long-run effects of gaining access to these colleges for cohorts exposed to the policy using census data.

## 6.1 Local Labor Markets

To shed light on the direct and indirect impacts of the policy, I divide the sample into "younger" workers (aged 18–34), who were of enrollment age or entering the labor market when new colleges opened, and "older" workers (aged 35–60), who had already completed their education, and distinguish between higher- and lower-skilled occupations. Lower-skilled occupations include blue-collar workers and clerks, while higher-skilled occupations include technicians, supervisors, engineers, managers, and executives.

**Wages.** Figure 9 displays the average wage separately for younger and older workers (Panel (a)). Following the opening of a new college, wages slightly increase for younger workers, consistent with the rise in educational attainment, whereas those of older workers remain largely

flat, with a small temporary dip in the years immediately following the policy. To account for workforce composition, Panel (b) reports a measure of residualized wages controlling for age and education, separately by gender. The results show a decrease of about 2%, consistent with an increase in the supply of skilled workers reducing the college premium (Katz and Murphy, 1992; Card and Lemieux, 2001). While the magnitude is relatively small compared to the size of the supply shock, the entry of skill-intensive firms constituted an endogenous local demand response that absorbed the new graduates, muting the wage adjustment.

**Employment.** Panels (c) and (d) of Figure 9 present employment results by skill group. For younger workers, employment in higher-skilled occupations rises by about 20% over eight years. The impact is also positive, though smaller and not statistically significant, for lower-skilled occupations (consistent with some graduates entering clerk or blue-collar roles after completing two- or three-year programs and the rise in firm entry in local service industries). In contrast, employment among older workers declines overall, with the decrease particularly pronounced for lower-skilled occupations (about 11% after eight years). For higher-skilled workers, the decline is small and not statistically significant. The positive employment effect for younger workers, coupled with the negative one for older workers, mirrors the patterns of business dynamism documented above and is consistent with displacement effects: new firms disproportionately hire younger workers (Ouimet and Zarutskie, 2014), while exiting firms tend to employ predominantly older workers. As a result, younger workers benefit more from firm entry, while older workers are more affected by firm exit. Overall, the gains and losses nearly offset each other, resulting in limited net employment effects at the city level (Panels (e) and (f) of Figure 9).

Finally, while the decline in employment for older workers may reflect various mechanisms (e.g., retirement or relocation), I find an increase in their unemployment rate at the province level (Table 4, columns (5)–(7)). This suggests a persistent negative impact for older cohorts that is not offset by reallocation across cities. These results echo evidence that mobility only partially offsets negative local labor shocks (Blanchard and Katz, 1992; Autor et al., 2013; Yagan, 2019).

### 6.2 Long-Run Impact on Exposed Cohorts

I now turn to the impact on individuals born in these areas, investigating two main questions: (i) did the policy create new opportunities for the *locals* (i.e., individuals originally from these locations), and (ii) what are the *long-run* effects of college openings on individuals' labor market outcomes? These questions are central to the place-based policy debate, as such policies aim to durably improve the prospects of people growing up in places with fewer opportunities.

Combining an event-study design with the 2015 census, I trace the effects on education and employment twenty-five years after the policy began.

**Empirical strategy.** I exploit variation in both the timing of the policy across areas and the degree of exposure across cohorts born in the same city. Using an event-study framework, I compare "young" and slightly older cohorts born in early- versus later-treated areas. The idea is that new colleges improved access to higher education for cohorts about to complete high school when the policy was implemented, but not for older ones who had already made their schooling decisions.[32] The staggered rollout thus generates variation in exposure among individuals from the same cohort born in different cities, enabling comparisons across people facing similar macroeconomic conditions.

I define "young" cohorts as those aged 21 or younger at the time of the policy, since most French students complete high school between ages 17 and 19, and typically enroll in higher education immediately afterward. Among 1994 higher-education graduates, 81% had finished high school at those ages, and 99% before age 22 (*Enquête de Cheminement*). Cohorts aged 22 at the time of the policy therefore serve as the omitted reference group. I use area of birth rather than place of residence because birthplace is pre-determined and a good proxy for location during adolescence (63% of 16-year-olds live in their urban area of birth).[33] Because identification comes from variation across cohorts rather than over time, the staggered rollout constrains the number of cohorts in the analysis but not the horizon between treatment and observation, permitting a long-run analysis of individual outcomes.[34]

I use the following model:

$$Y_{i,t} = \alpha_a + \delta_c + \mu_t + \sum_{\substack{j=13 \\ j \neq 22}}^{28} \gamma_j \, \mathbb{1}\{j = \text{Yr. of treatment}_a - \text{Yr. of birth}_i\} + X'_{i,t}\beta + \epsilon_{i,t} \qquad (3)$$

where $Y_{i,t}$ denotes the outcome of individual $i$, born in area $a$, belonging to cohort $c$, and observed at time $t$. $\alpha_a$ and $\delta_c$ capture area and cohort fixed effects, respectively, while $\mu_t$ controls for contemporaneous shocks in year $t$. The parameters of interest, $\gamma_j$, measure the effect of new colleges on individuals aged $j$ when their area of birth received its first new college, relative to those aged 22. $X_{i,t}$ includes gender, age, age-squared, and a linear time trend by area of birth. Outcomes are measured in the 2015 Census, restricting the sample to individuals aged 25–55. As

[32] See also Duflo (2001) and Currie and Moretti (2003) for similar identification strategies.

[33] This figure rises to 71% and 84% at the province and region level, respectively (Census 2006).

[34] The city-level analysis instead compares "not yet treated" areas to those already treated, so its length is limited by the timing of the policy rollout. There is a trade-off between the number of periods used in the analysis and the size of the control group. At the individual level, however, variation in exposure occurs across cohorts rather than over time, avoiding this trade-off.

in the city-level analysis, identification requires that, absent the policy, outcomes across cohorts would have evolved similarly in early- and later-treated areas. I estimate the parameters using estimators robust to heterogeneous treatment effects in staggered designs.

**Results.** I examine in turn the effects on higher education access, labor market outcomes, and geographic mobility.

While Section 5.1 shows a large increase in the share of college graduates in the local labor force, this may partly reflect diversion: students who would have attended a historical university may instead study at a local college (Rouse, 1995; Mountjoy, 2022). I test this directly by estimating the effect of new colleges on individuals' educational attainment, regardless of where they studied, but instead based on where they were born. Panel (a) of Figure 10 shows a large increase in the probability of obtaining a college degree: 3.8 percentage points (+9.9%) for individuals aged 13 at the time of the reform, relative to those aged 22. Placebo cohorts (those older than 22 at the time of the policy) show small and non-significant effects, supporting the validity of the research design. By contrast, the effect on high school graduation is small and not statistically significant (Panel (b)), confirming that the gains come from access to higher education rather than from higher completion at lower levels.

The same pattern holds for labor market outcomes. The probability of being employed in 2015 rises by about 2.6 percentage points (+3%) for cohorts aged 13 at the time of the policy, with no effect for placebo cohorts (Panel (c)). Exposed individuals also hold more skilled jobs: conditional on employment, the probability of holding a skilled position rises by 5.2 percentage points (+10.8%) (Panel (d)). The gains are concentrated in occupations such as skilled technicians and middle managers, consistent with the type of education provided in local colleges (see Appendix Section C.3). They are mirrored almost entirely by a decline in lower-skill white-collar occupations such as clerks (−4.7 percentage points).

Finally, Panel (e) reports the probability that an individual lives in their province of birth. The coefficients are small and insignificant for all cohorts, indicating no detectable effect on migration. While there is typically a positive relationship between education and geographic mobility (Wozniak, 2010; Machin et al., 2012; Malamud and Wozniak, 2012), three factors may explain why new colleges did not increase mobility: (i) local colleges make it possible to study near home, (ii) the policy targeted a population less mobile than the average student, and (iii) studying locally likely strengthens lasting ties to the area.

Taken together, these results show that the policy enhanced local human capital while retaining beneficiaries within the area, suggesting that place-based higher education expansions can generate persistent gains for local populations.

### 6.3 Robustness Tests

In this section, I assess the robustness of the results to several alternative specifications: excluding the largest area in the sample (Paris), controlling for initial city size, using alternative definitions of local labor markets, and employing different estimation methods. Finally, I examine whether new colleges generated "spillover" effects on neighboring areas. The corresponding results are reported in Appendix Section E. I summarize the main findings below.

**Excluding Paris.** The distribution of French city sizes is heavy-tailed. The Paris urban area, which roughly corresponds to the Paris region, had about 11 million inhabitants in 1990 (19.5% of the French population). To ensure that the results are not driven solely by the French capital, I replicate the main analysis excluding the urban area of Paris. The results, shown in Appendix Figure E.1, are consistent with the baseline estimates but smaller in magnitude, reflecting the Paris region's substantial economic weight and the large number of higher education institutions established there during the policy period.[35] New colleges continue to raise local educational attainment and firm creation (Panels (a) and (b)): the share of college graduates increased by an average of 1.6 percentage points over the period, and firm creation by 8.4%. Residualized wages declined significantly, consistent with the increased supply of skilled workers (Panel (d)). Employment rose among younger workers and fell among older ones, largely offsetting each other and leaving a limited but growing effect on total employment (Panels (c), (e), and (f)). Excluding the three largest urban areas (Paris, Lyon, and Marseille) yields comparable results. Beyond excluding these cities, I control directly for initial city size and use finer geographies that split the Paris urban area into several units; both approaches confirm the main patterns.

**Controlling for initial location size.** As an alternative to excluding the largest areas from the sample, I re-estimate the main specification controlling for initial city size (1990 population).[36] In this specification, the parallel trends assumption is required to hold only conditional on size, allowing cities with different population levels to be treated at different dates. The results, presented in Appendix Figure E.2, confirm the baseline findings. Treated areas experienced a substantial increase in local educational attainment and firm creation, accompanied by a decline in residualized wages, a sizeable rise in employment among young workers, and a

[35] Although the municipality of Paris itself was not targeted by the policy, several suburban municipalities benefited from the construction of large higher education establishments. In particular, four of the nine full universities were built in the Paris metropolitan area. These suburban municipalities are included in the broader Paris urban area.

[36] One appealing feature of the estimator developed by Callaway and Sant'Anna (2021) is that it can control for initial characteristics. In particular, it incorporates and extends the stabilized Inverse Probability Weighting method of Abadie (2005).

moderate negative impact on older workers.

**Definition of cities.** The main analysis defines cities as urban areas. Appendix Figures E.3 and E.4 present the main results at the province (*département*) and commuting zone (CZ) levels, respectively. Metropolitan France comprises 94 provinces and 279 CZs. Unlike urban areas, these units form a complete partition of the French territory and therefore include rural areas. They also provide a more disaggregated representation of the Paris region, which comprises 8 provinces and 25 CZs. The results remain consistent across the three geographies, though employment estimates are attenuated at the province level. This likely reflects dilution: provinces pool treated cities with untreated areas, so that coefficients average over both. Overall, the main conclusions are robust to the choice of geographic unit.

**Alternative estimators.** Appendix Figure E.5 reproduces the main analysis using the estimators of Callaway and Sant'Anna (2021) (baseline), de Chaisemartin and D'Haultfœuille (2020), and Sun and Abraham (2021). As expected, the first two yield very similar results, as both rely on comparable methods and use all not-yet-treated areas as a control group. The Sun and Abraham (2021) estimator, which uses only the last-treated cohorts (areas receiving new colleges after 1999) as the control group, produces qualitatively similar results and slightly larger effects. This indicates that the results are not driven by the composition of the control group. The main results are also robust to the imputation method proposed by Borusyak et al. (2024). Appendix Table E.1 reports the average post-treatment effect for the four estimators.[37] Estimates for education, firm creation, and employment are consistent in magnitude across the four methods, with the imputation estimator being the most precise, particularly for employment. For residualized wages, the imputation estimate is positive but imprecise, in contrast with the declines implied by the other three estimators.

**Spillovers on surrounding areas.** Finally, I examine whether the effects of new colleges diffused beyond the treated cities. Appendix Table E.2 reports the results for urban areas located within 50 km of a treated area but which did not receive a college themselves. These neighboring areas are considerably smaller than those considered so far, with an average population below 10,000 inhabitants, so that even modest absolute inflows translate into large percentage effects. Surrounding areas experienced a rise in the share of college graduates (2.8 percentage points), consistent with graduates spreading across nearby labor markets, and an increase in employment

[37] The imputation method yields an average treatment effect over the post-treatment period when observations are weighted, but not year-by-year estimates. It is therefore reported in Appendix Table E.1 but omitted from the year-by-year comparisons of Appendix Figure E.5.

of 5.6%, driven by higher-skilled jobs (+13.7%). The effect on firm creation is small, though imprecisely estimated, suggesting that the response of business formation was concentrated in the larger cities hosting the new colleges. Overall, these results indicate that the benefits of the policy diffused, at least partly, to neighboring communities. They also imply that baseline estimates may understate the overall effect of new colleges on local development.

# 7 Heterogeneity, Convergence, and Fiscal Costs

The preceding section estimated the average effects of new colleges and their incidence across firms, workers, and cohorts. Two additional questions remain to inform policy. First, how are the gains distributed across places? If effects are largest in initially less developed places, expanding colleges can narrow territorial disparities; if they concentrate in already-strong cities, it would widen them. Second, at what cost were these gains obtained? Construction and teaching outlays were large and immediate, and whether the induced growth repaid them determines whether this investment generated value for public funds. I address the first question by estimating how the effects vary with initial city characteristics, and the second by comparing the policy's costs with the tax receipts it generated.

## 7.1 Heterogeneity and Convergence

Reducing territorial disparities in access to higher education was an explicit objective of the plan: while some capacity was built in large cities such as Paris, many colleges opened in small and medium-sized urban areas (Section 2). Whether such an expansion succeeds as a place-based policy depends on where its effects are largest (Kline and Moretti, 2013; Neumark and Simpson, 2015). If local returns to human capital are concave, so that a first college in a lightly educated area matters more than an additional one in a saturated market, effects should be strongest where initial attainment and development are low, and the expansion should compress spatial disparities. If instead complementarities with density and the existing skill base dominate, effects should concentrate in already-strong areas and the expansion should widen them. I test these competing predictions by estimating whether the policy produced larger effects in some types of cities than in others, considering initial development, educational attainment, and population density, as well as the share of college-age residents, which proxies for the potential demand for the new seats.

To do so, I interact a measure of treatment intensity with initial city characteristics, esti-

mating the following variant of the baseline model in long differences:

$$\begin{aligned} y_{a,t} \;=\; & \alpha_a + \delta_t + \gamma\,\text{Intensity}_a \times \text{After}_t + \phi\,\text{Intensity}_a \times X_{a,1990} \times \text{After}_t \\ & + \theta\, X_{a,1990} \times \text{After}_t + \epsilon_{a,t} \end{aligned} \tag{4}$$

where $\text{Intensity}_a = \frac{\text{Seats}_a}{\text{Workforce}_a}$ is the cumulative number of new university seats in area $a$, averaged over the period of analysis and scaled by the area's 1990 workforce. This measure accounts for three features of the rollout: areas can be treated several times with establishments of different sizes; averaging over the window gives more weight to seats delivered early; and scaling by initial workforce allows one seat to matter more in a small city than in a large one. The city characteristics $X_{a,1990}$ are standardized to mean zero and unit variance across areas, so that $\gamma$ measures the effect of intensity in a city with average characteristics while $\phi$, the coefficient of interest, captures the differential effect associated with a one-standard-deviation higher characteristic. $\theta$ absorbs differential growth trajectories related to city characteristics, so that $\phi$ is not confounded by, say, denser areas growing faster over the period. The sample comprises treated urban areas only: parameters are identified from variation in the dose of treatment across treated areas. With only two periods, the design sidesteps the negative-weighting problems of staggered comparisons. As in the baseline specifications, estimates are weighted by initial area size.

Table 9 reports the interaction coefficients $\phi$ for four area characteristics measured in 1990: (1) log value added per employee, (2) college share, (3) population density, and (4) the fraction of the population aged 15–24. Starting with educational attainment (Panel A), no interaction is statistically significant except that with the fraction aged 15–24 (at the 10% level). This indicates that the same dose of treatment produced similar education gains in more or less developed, educated, or dense areas. Hence, along these dimensions, the first stage of the policy appears homogeneous. Gains were slightly larger where young residents were more numerous, consistent with a larger potential demand for the new seats. Turning to local growth (Panel B), the gains were significantly larger in initially less productive and less dense areas. The interaction with education is also negative but not statistically significant. Because the education gains did not vary with initial productivity or density, this heterogeneity reflects differences in how a comparable education shock translated into economic activity, not differences in the shock itself. These results suggest a larger impact on local development in lagging places, making higher education a particularly relevant tool for place-based policy.

These estimates suggest that the construction of colleges facilitated the catch-up of initially less developed areas. I complement this local analysis with descriptive evidence on convergence over time. Figure 11 examines $\beta$-convergence (Barro, 1991) across French provinces over four

sub-periods bracketing the policy: before (1982–1990), during (1990–1999), after (1999–2010), and a decade after (2010–2015).[38] Panels (a) and (b) plot the annualized growth of the college-graduate share and of income per adult, respectively, against the initial college share; Panel (c) plots income growth against its own initial level. Before the policy, France was diverging: provinces with higher initial college shares gained graduates *and* income faster than the rest, while Panel (c) shows no convergence in income over the same period. This pattern reversed during the policy decade: convergence was sharpest in 1990–1999 among the four sub-periods, with initially less-educated provinces catching up in both education and income. The same reversal appears when ranking provinces by initial income rather than education (Panel (c)). Convergence continued at a slower pace over 1999–2010, consistent with treated cohorts gradually entering the labor market, and flattened in the final period. These aggregate patterns are descriptive, but their timing, together with the heterogeneity estimates above, is consistent with the expansion having operated as an instrument of territorial convergence.

### 7.2 Costs and Benefits of the Educational Expansion

The higher education expansion durably raised local educational attainment and growth. A natural question for policymakers is whether these gains justify the considerable resources the policy absorbed. I provide evidence on this question using the Marginal Value of Public Funds (MVPF) framework of Hendren and Sprung-Keyser (2020), the ratio of beneficiaries' willingness to pay to the net cost to the government, inclusive of fiscal externalities. Three general caveats apply. First, like any welfare quantification, this exercise rests on judgment calls and simplifying assumptions. Appendix F details the assumptions, sources, and computations. Second, higher education generates benefits beyond those I measure: civic participation (Dee, 2004; Milligan et al., 2004), reduced crime (Lochner and Moretti, 2004), and the consumption value of education itself (Delavande and Zafar, 2019). Willingness to pay should therefore be read as a lower bound. Third, the framework was designed for marginal policy changes, whereas the expansion I study is large. Confronting the costs of one of the largest public investments in higher education in modern French history with its benefits nonetheless provides a useful benchmark for policy. I summarize the approach and results below, expressing all flows in 2018 U.S. dollars.

In the early years, the government bears construction and operating costs while the economic effects have not yet materialized. As value added responds, tax receipts grow rapidly such that the annual net cost turns negative in the fifth year. Appendix Figure F.1, Panel (a), illustrates the magnitude of the costs in that year. Perhaps surprisingly, the construction program itself

[38] Period breaks follow census years. Income per adult is available only at the province level from Bonnet et al. (2021).

accounts for a relatively small fraction of the total: the bulk consists of the recurring cost of educating the additional students ($10,837 per student per year). Tuition receipts offset only a tiny fraction of this cost. Forgone tax revenue on the earnings students give up while studying is likewise modest, because taxes are low near the minimum wage. Against these outlays, the government recoups taxes on the additional GDP generated by the policy (estimated in Section 5.2). Beneficiaries' willingness to pay follows a similar pattern: initially negative, as the additional students forgo net-of-tax earnings, it turns positive after four years, once the net-of-tax GDP gains accruing to workers and firm owners dominate (Appendix Figure F.1, Panel (b)).

Cumulatively, the policy pays for itself within eight years. Willingness to pay is positive and the net government cost negative, so the MVPF is infinite under the convention of Hendren and Sprung-Keyser (2020). As a sensitivity check, I reproduce the exercise using the log value-added estimates minus one standard error. The annual net cost turns negative after eight years, and the policy pays for itself in about twelve years.

These results complement a literature that evaluates higher education policies, such as admission cutoffs (Hoekstra, 2009; Zimmerman, 2014; Mountjoy, 2026) or tuition subsidies (Fack and Grenet, 2015; Denning et al., 2019), through their effects on marginal students. Such an accounting exercise of the costs and benefits of a large-scale capacity expansion, including its infrastructure component, has to my knowledge not been conducted before. The distinction matters: beyond the private returns to marginal students, the expansion generated business dynamism and productivity gains that accrue broadly and flow back to the government budget through taxes. Public investments in higher education of this kind can thus pay for themselves within a decade, a relevant benchmark for governments considering similar large-scale and decentralized investments.

# 8 Conclusion

Human capital is central to theories of growth and regional development, yet its causal effect on local economies is difficult to isolate: economic prosperity is at once cause and consequence of the concentration of skills. This paper studies the largest higher-education expansion in modern French history to trace how a shock to the supply of educated workers reshapes a local economy.

The expansion led to a lasting rise in the education of the local workforce: enrollment jumped, and many graduates took their first jobs where they had studied, leaving treated areas with a 17% higher share of college graduates on average. This new supply of skills triggered creative destruction. Firm creation surged by 16%, driven by incorporations rather than subsistence

self-employment. Entry was strongest in skill-intensive industries and tradable sectors, where local demand has little impact, and new firms employed more skilled workers. These firms were founded not by the graduates themselves but by experienced entrepreneurs. Together, these patterns point to the supply of skilled labor as the main force behind the entry boom. Incumbent firms were also affected: exit rates rose and their growth slowed. On net, the local economy expanded, with gains of about 5% in value added and 6% in productivity.

Benefits and burdens fell unevenly across generations. Skilled employment rose by 20% for the young, while older, less-educated workers lost jobs and faced persistently higher unemployment, a cost that complementary policies could mitigate. Cohorts of college-going age when the colleges opened, by contrast, benefited in the long run: a quarter-century later, they were more likely to hold a college degree (by 10%), to be employed (by 3%), and to work in a skilled occupation (by 11%).

Comparing fiscal costs and benefits, I find that the tax revenue generated by the additional growth exceeded the full cost of the expansion, construction included, so that the policy paid for itself in about ten years. Effects were largest in initially less-developed areas, contributing to a decade in which French provinces converged in education and income faster than in any other period the data cover. These results suggest that investing in human capital can succeed as a policy for places: a large-scale expansion of higher education produced lasting gains in the skills, employment, and opportunities of local populations.

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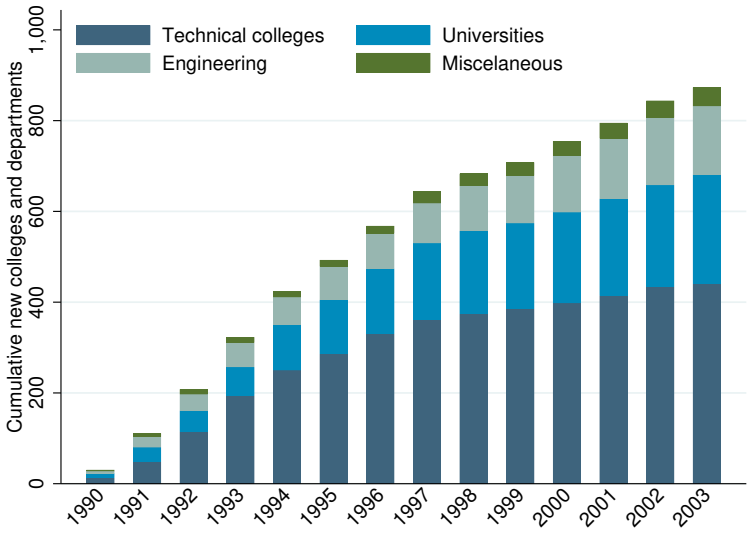
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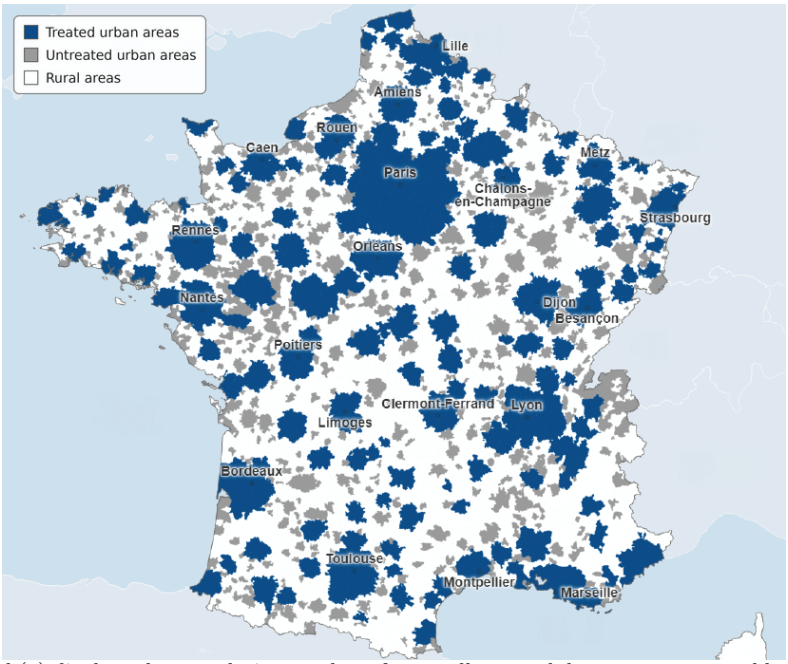
# FIGURES

**Figure 1:** New colleges and departments opened during the Universities of the $21^{st}$ Century policy

**(a)** Cumulative number of new colleges




**(b)** Urban areas receiving new capacity



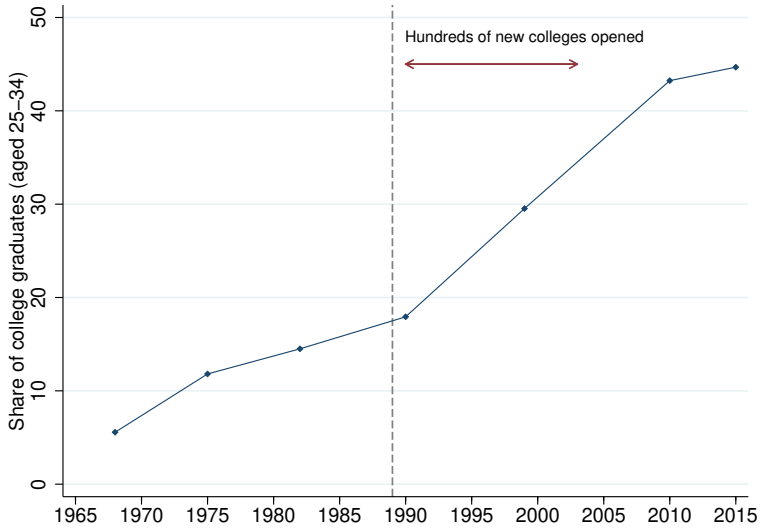

Notes: Figure 1, Panel (a) displays the cumulative number of new colleges and departments opened between 1990 and 2003, by type of institution. Panel (b) displays in navy the urban areas that received at least one new college or department over the same period. Grey areas are urban areas that did not receive any new higher education capacity, and white areas lie outside any urban area. Data: Registry of higher education establishments (SISE). Go back to main text

**Figure 2:** College enrollment and educational attainment

**(a)** College enrollment



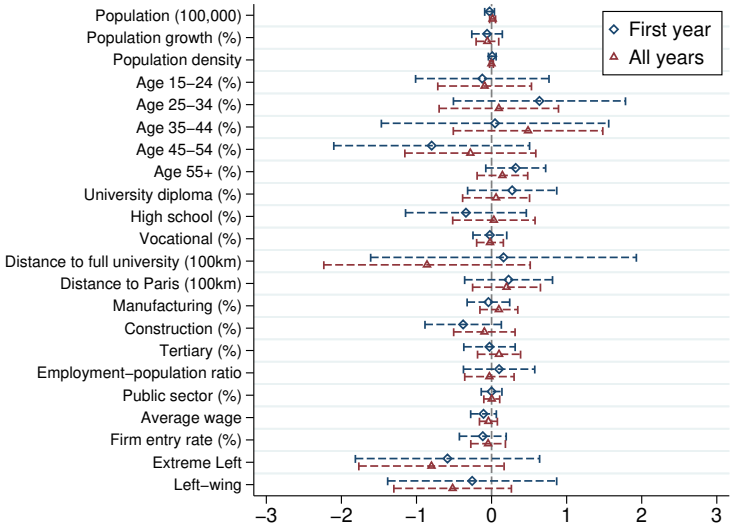

**(b)** Share of college graduates (aged 25–34)



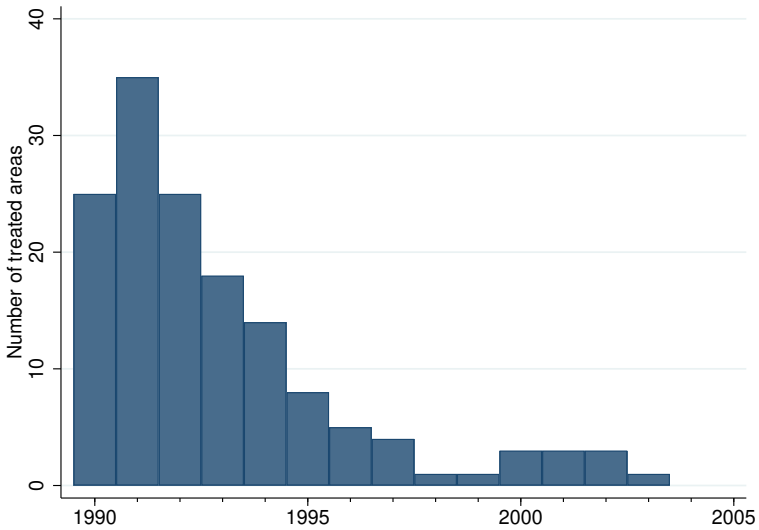

Notes: Panel (a) of Figure 2 plots enrollment in higher education, in millions (left axis), and enrollment as a fraction of the population aged 18–24 (right axis). The dotted line extrapolates a linear trend fitted over 1976–1989, and the vertical line marks 1989, the last year before the policy. Panel (b) plots the share of the population aged 25–34 holding at least a two-year higher education diploma. Data: Ministry of Higher Education and Census data (1968–2015). Go back to main text

**Figure 3:** Timing of new openings

**(a)** Timing of the policy and initial characteristics



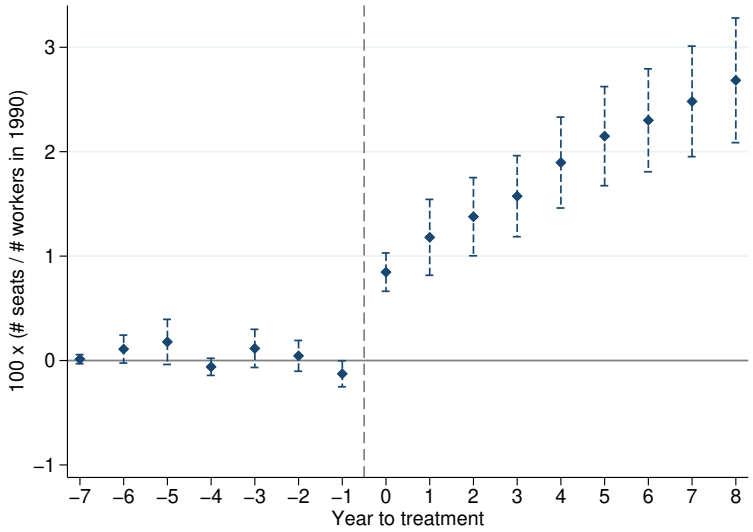

**(b)** Number of treated areas per year



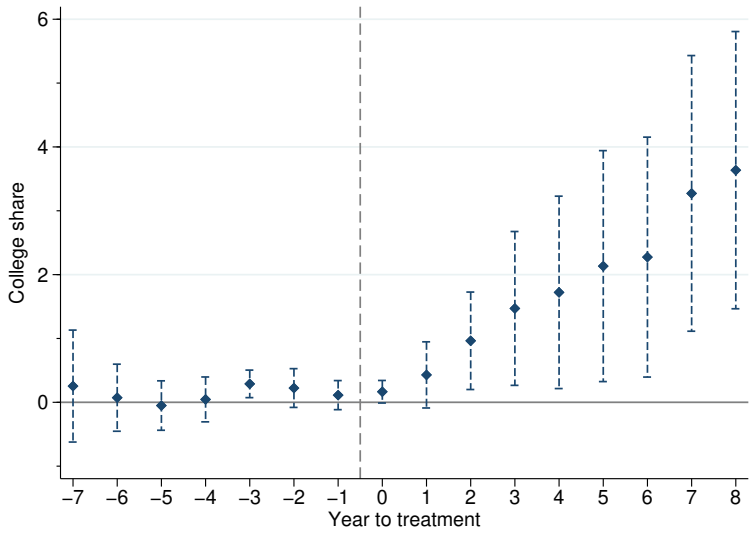

Notes: Figure 3 investigates the timing of new college openings. Panel (a) reports coefficients from regressions of treatment timing on initial area characteristics. The outcome is (i) the first year an area received new capacity (blue diamonds), or (ii) all years in which an area received new capacity (red triangles). Panel (b) plots, by year, the number of urban areas receiving higher education capacity for the first time during the policy period. Data: Registry of higher education establishments (SISE), linked employer-employee data (DADS), and Census data (1990). Go back to main text

**Figure 4:** New capacity and the share of college graduates

**(a)** Higher education capacity



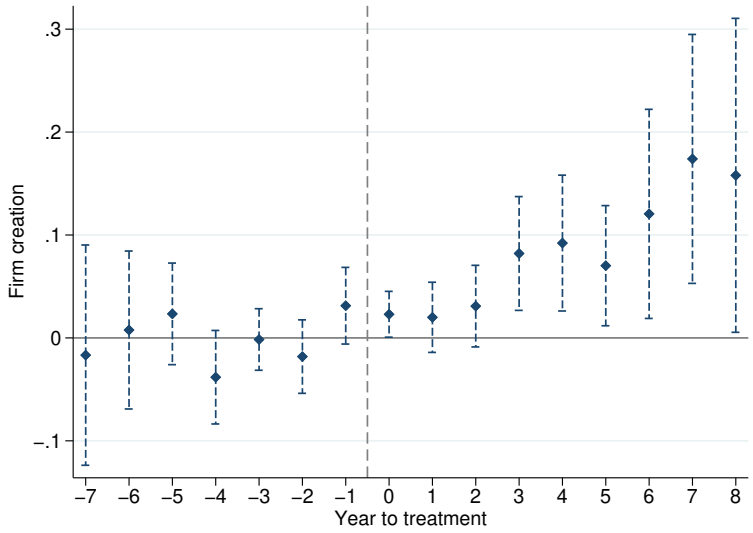

**(b)** Share of college graduates



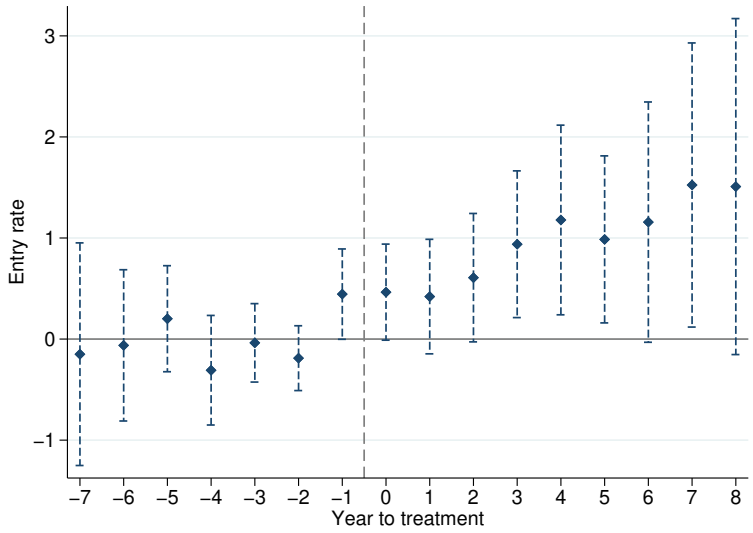

Notes: Figure 4 reports event-study estimates of equation (1). The unit of observation is the urban area × year. Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). In Panel (a), the outcome is the cumulative capacity added since treatment, measured as seats per 100 workers employed in 1990. In Panel (b), the outcome is the share of workers with a university degree. Estimates are weighted by the area's 1990 workforce. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Higher education registry and linked employer-employee data merged with census (DADS and EDP). Go back to main text

**Figure 5:** Business formation

**(a)** New businesses



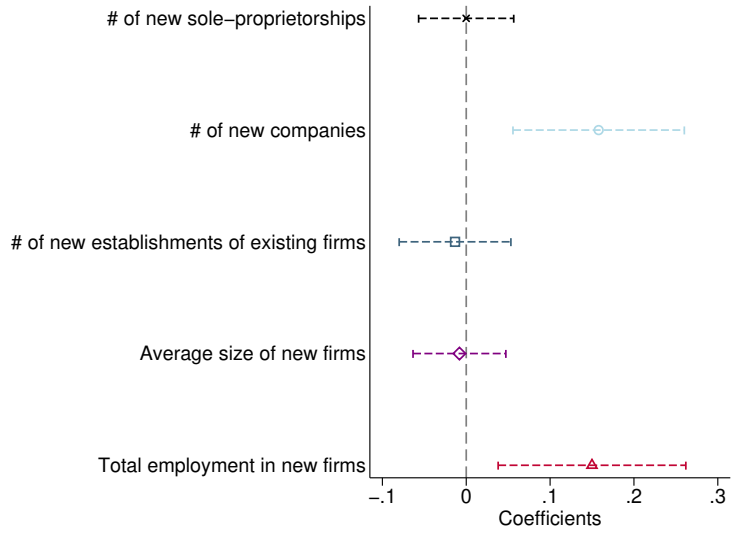

**(b)** Entry rate of new businesses (%)



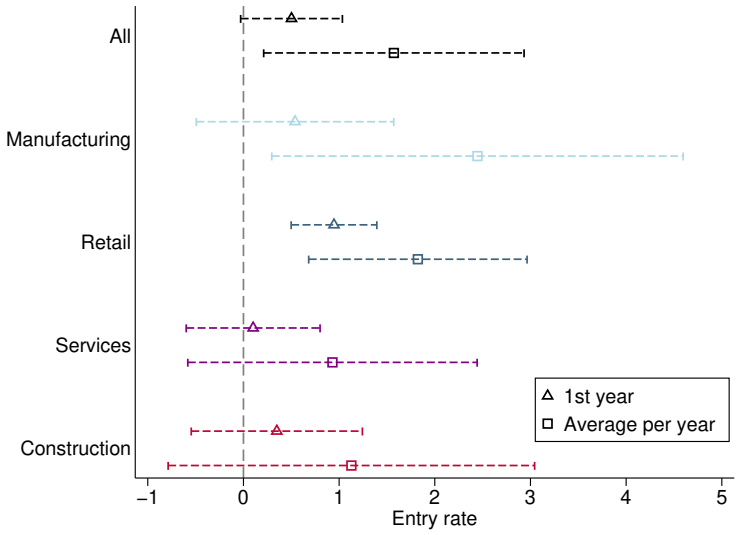

Notes: Figure 5 reports event-study estimates of the effect of new colleges on business formation. Panel (a) shows results for the number of new businesses (firms and establishments), and Panel (b) the corresponding entry rate (i.e., new firms and establishments divided by the stock of businesses). The sample includes incorporated firms and sole proprietorships. Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). The unit of observation is the urban area × year. Estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

**Figure 6:** Firm creation, initial size, and industry composition

**(a)** Firm creation and initial size



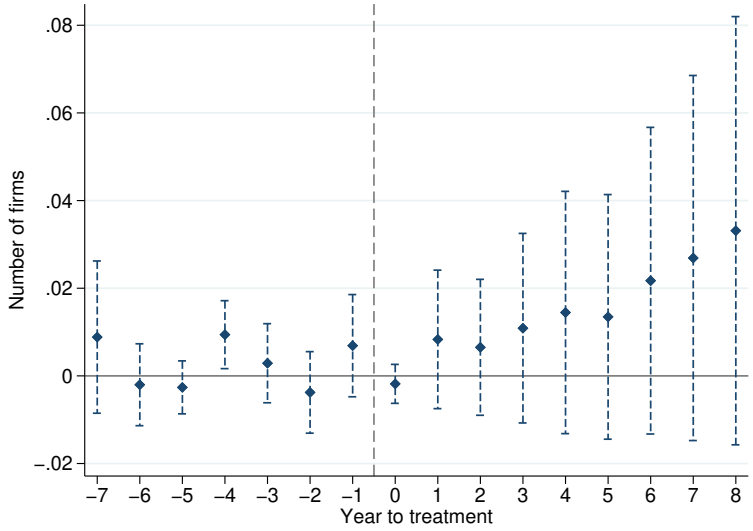

**(b)** New companies by industry



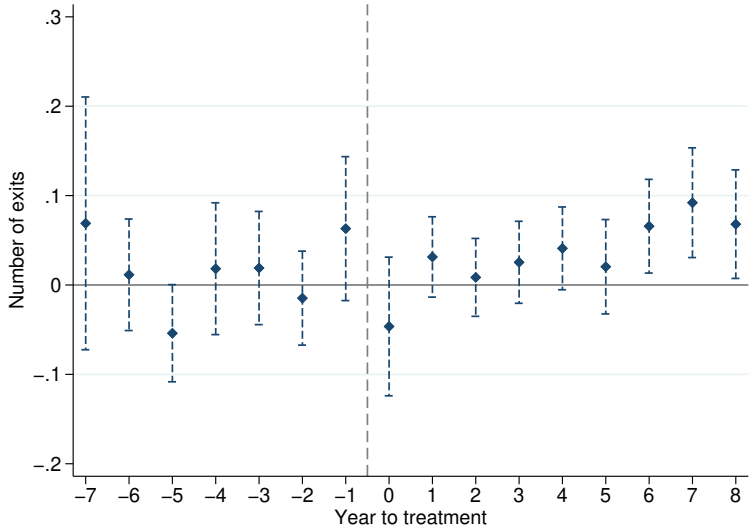

Notes: Panel (a) of Figure 6 plots event-study estimates of the effect of new colleges on firm creation by type of firm and on firm average size at creation. I consider three types of new businesses: (i) sole proprietorships, (ii) incorporated firms, and (iii) new establishments of existing firms. Panel (b) documents the entry of incorporated firms separately by major industry, showing the effect in the first year after opening and the average annual effect over the post-treatment period. Each observation is an urban area × year. Estimates are weighted by the number of firms in the area in 1990. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

**Figure 7:** Active firms and firm exit

**(a)** Stock of firms

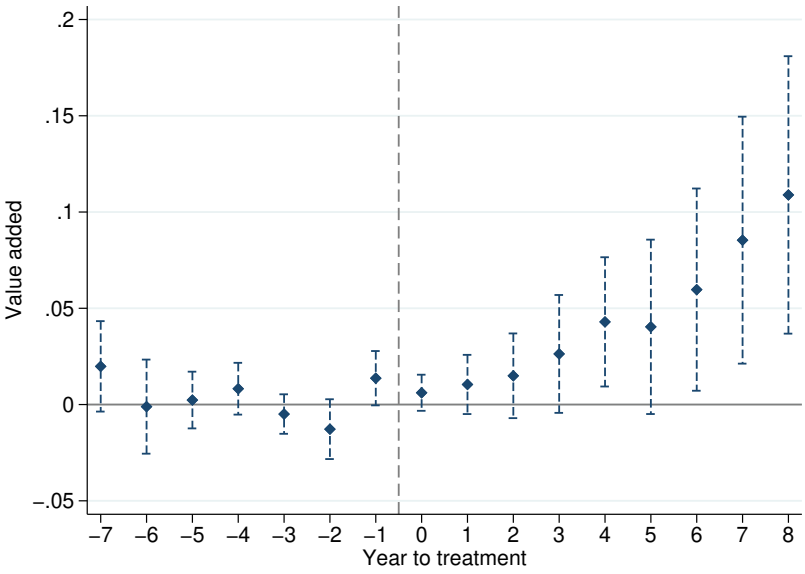



**(b)** Exit of firms



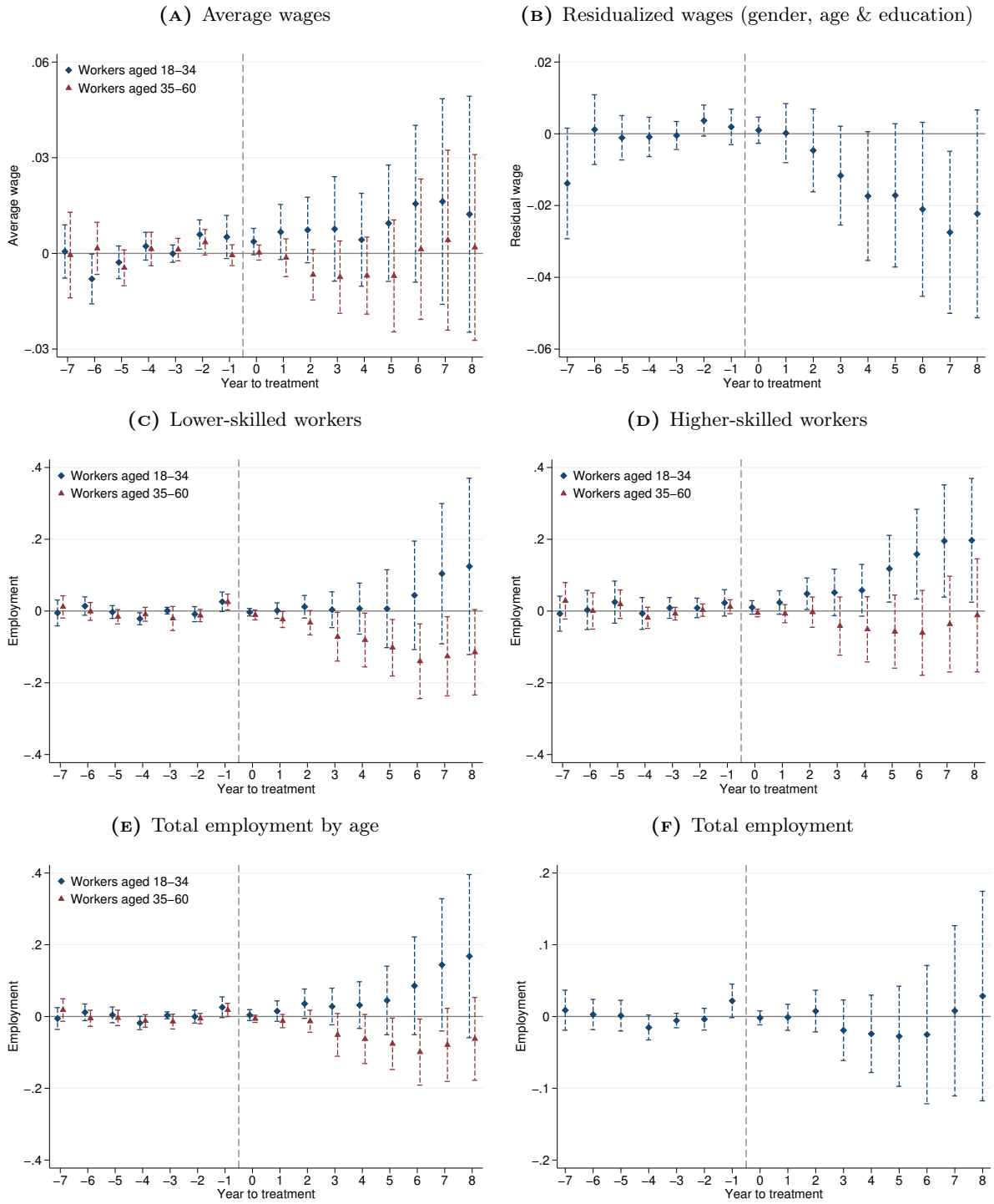

Notes: Figure 7 plots event-study estimates for the number of total active firms (Panel (a)) and the number of firm exits (Panel (b)). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation is an urban area × year. Estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

**Figure 8:** Local growth




Notes: Figure 8 plots event-study estimates for the local value added. Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation is an urban area × year. Estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Balance sheet data (FICAS). Go back to main text

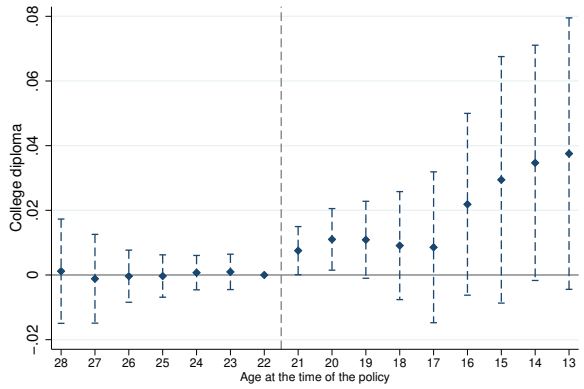
**Figure 9:** Impact on wages and employment



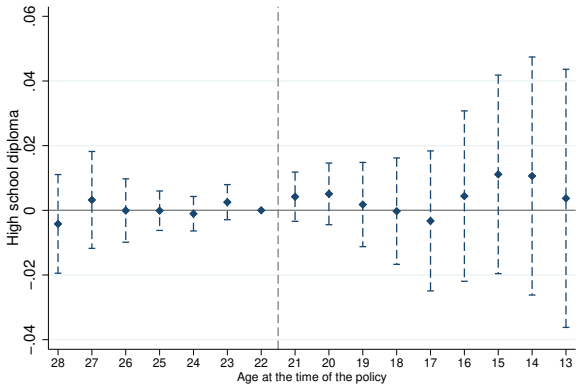

Notes: Figure 9 reports event-study estimates for Panel (a) the daily wage by age group, (b) residualized wages, (c) the number of lower-skilled jobs by age group, (d) the number of higher-skilled jobs by age group, (e) the total number of jobs by age group, and (f) total employment. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Lower-skilled jobs include blue-collar workers and clerks, while higher-skilled jobs include technicians, supervisors, managers, engineers, and executives. Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to an urban area × year. Estimates are weighted by the area's 1990 workforce. Standard errors are clustered at the urban area level. The bars correspond to the 95% confidence intervals. Data: Linked employer-employee data merged with census (DADS and EDP).
Go back to main text

**Figure 10:** Long-run impact on exposed cohorts

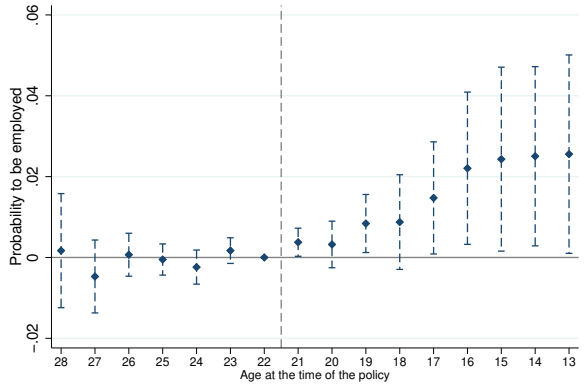
(a) College diploma



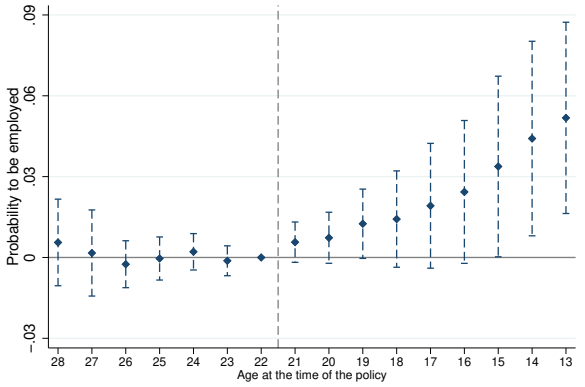

(b) High school diploma



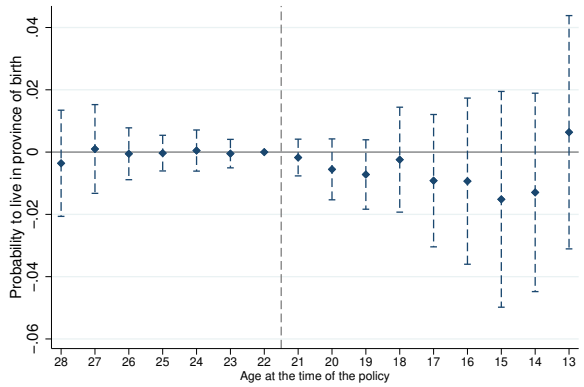

(c) Employment




(d) High-skilled employment



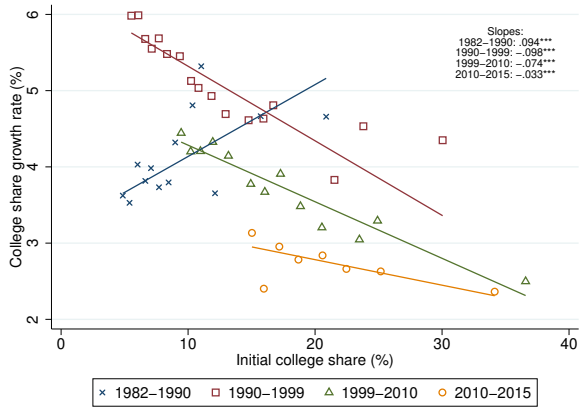

(e) Migration



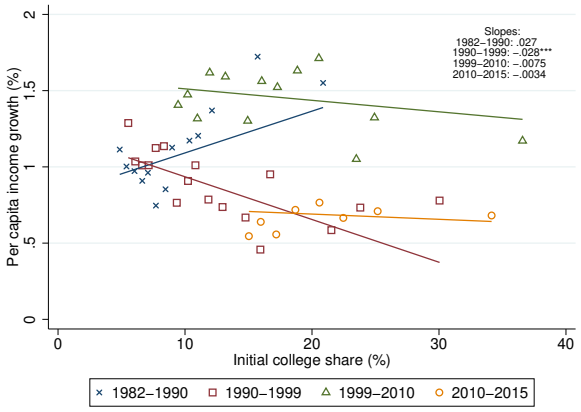

Notes: Figure 10 plots estimates of equation (3) for the probability of graduating from college (Panel (a)), completing high school (Panel (b)), being employed (Panel (c)), holding a high-skilled occupation conditional on working (Panel (d)), and living in the province of birth (Panel (e)). The x-axis corresponds to the age of individuals in the year their area of birth first received new higher education capacity under the policy. Observations are at the individual level. Standard errors are clustered at the area-of-birth level. The bars correspond to the 95% confidence intervals. Data: Census data (2015). Go back to main text

**Figure 11:** Convergence in education and income across provinces

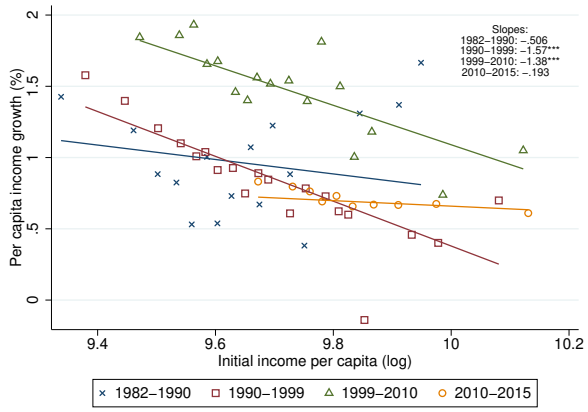
(a) Education




(b) Income per adult and education




(c) Income per adult




Notes: Figure 11 presents binned-scatter evidence on $\beta$-convergence across French provinces over four sub-periods bracketing the policy: 1982–1990, 1990–1999, 1999–2010, and 2010–2015. Panels (a) and (b) plot the annualized growth rate of the share of college graduates and of income per adult, respectively, against the initial share of college graduates. Panel (c) plots the annualized growth rate of income per adult against its initial level. Provinces correspond to the 94 French *départements*. Data: Census data (1982, 1990, 1999, 2010, 2015) and local GDP from Bonnet et al. (2021). Go back to main text

**Table 1:** Characteristics of 1994 graduates, three years after graduation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Head of company | Manager/ Engineer | Technician/ Supervisor | Clerk | Blue-collar | Wage (% of MW) | Work in region of study | Bac. & higher ed. in same region | Share of new graduates |
| **Universities:** | | | | | | | | | |
| 2-year | 1.22 | 7.45 | 45.62 | 38.41 | 7.30 | 115.75 | 71.77 | 75.23 | 26.37 |
| 3-year | 0.24 | 36.38 | 41.68 | 20.66 | 1.05 | 127.33 | 74.11 | 76.05 | 17.69 |
| 4-year | 0.51 | 50.93 | 33.87 | 14.13 | 0.56 | 145.34 | 67.67 | 64.70 | 14.31 |
| 5-year | 0.79 | 70.28 | 22.23 | 6.39 | 0.31 | 172.48 | 59.98 | 47.88 | 14.09 |
| Ph.D. | 0.59 | 93.07 | 5.22 | 1.03 | 0.09 | 207.65 | 64.39 | 36.07 | 3.12 |
| **Technical colleges** | 0.81 | 5.98 | 61.31 | 21.74 | 10.16 | 121.33 | 64.05 | 76.76 | 9.81 |
| **Engineering:** | | | | | | | | | |
| Engineering schools | 1.63 | 91.75 | 5.22 | 0.70 | 0.70 | 202.46 | 40.30 | 26.24 | 7.25 |
| Instituts Uni. Pro. | 1.03 | 48.28 | 34.65 | 15.27 | 0.77 | 158.91 | 47.08 | 68.05 | 0.76 |
| **Business schools** | 1.24 | 45.80 | 44.94 | 7.65 | 0.36 | 171.19 | 61.14 | 37.90 | 6.59 |

Notes: Table 1 reports descriptive statistics on occupation (columns (1)–(5)), monthly wage (column (6)), the share of graduates working in the region where they studied (column (7)), and the share who obtained both their *baccalauréat* (French high school diploma) and their higher education diploma in the same region (column (8)), by type of diploma. Wages are expressed as a percentage of the French minimum wage (MW). Column (9) reports the share of all 1994 graduates holding each type of diploma. The sample covers individuals who graduated in 1994 and were surveyed in 1997. Statistics are computed using survey weights. "Instituts Uni. Pro." stands for *Instituts Universitaires Professionnalisés*. Data: *Enquête de Cheminement: sortants de l'éducation supérieure* (1994). Go back to main text

**Table 2:** Characteristics of French urban areas in 1990

| | (1) | (2) | (3) |
|---|---|---|---|
| | All Areas | Treated | University Towns |
| **Number of Urban Areas** | 759 | 145 | 94 |
| **Population** | | | |
| Mean | 63,049 | 275,406 | 384,368 |
| Median | 9,541 | 108,738 | 155,988 |
| Standard Deviation | 421,672 | 937,455 | 1,150,348 |
| Density | 153 | 202 | 214 |
| **Age (%)** | | | |
| 0-14 | 20.4 | 20.5 | 20.4 |
| 15-24 | 15.5 | 15.7 | 15.8 |
| 25-34 | 15.5 | 15.7 | 15.8 |
| 35-44 | 15.2 | 15.3 | 15.4 |
| 45-54 | 10.3 | 10.4 | 10.4 |
| 55+ | 23.2 | 22.4 | 22.2 |
| **Education (%)** | | | |
| College | 12.3 | 13.2 | 13.7 |
| High School | 11.1 | 11.5 | 11.6 |
| Vocational Technical School | 21.2 | 21.1 | 21.1 |
| No Diploma | 55.5 | 54.2 | 53.6 |
| **Unemployment Rate (%)** | 9.6 | 9.6 | 9.5 |
| **Industries (%)** | | | |
| Agriculture | 2.9 | 2.4 | 2.2 |
| Manufacturing | 22.9 | 21.9 | 21.6 |
| Construction | 7.1 | 6.9 | 6.9 |
| Services | 67.1 | 68.8 | 69.2 |
| **Occupations (%)** | | | |
| Farmers | 2.1 | 1.7 | 1.6 |
| Blue-Collars | 27.7 | 26.3 | 25.7 |
| Clerks | 26.3 | 26.8 | 26.8 |
| Intermediate White-collars | 22.6 | 23.3 | 23.5 |
| High-skilled White-collars | 13.6 | 14.6 | 15.1 |
| Company Managers | 7.7 | 7.3 | 7.2 |
| **Public Sector (%)** | 24.8 | 24.9 | 24.6 |
| **Average Daily Wage (€)** | 83.1 | 85.5 | 86.6 |
| **Firm Entry Rate (%)** | 10.8 | 11.1 | 11.2 |

Notes: Table 2 reports descriptive statistics for 759 French urban areas in 1990. *Treated areas* correspond to areas receiving new higher education capacity between 1990 and 2003. *University towns* are areas with at least one university before the beginning of the policy. Population density is measured as inhabitants per square kilometer. Educational attainment is divided into four categories: (i) college (at least a two-year diploma), (ii) high school (*Baccalauréat*), (iii) vocational (*CAP-BEP*), and (iv) no diploma. Intermediate white-collar occupations correspond to technicians and supervisors, while high-skilled white-collar occupations include engineers and managers. Data: Registry of higher education establishments (SISE), 1990 census, and registry of firms (Sirene register). Go back to main text

**Table 3:** Expected versus realized capacity (1991–1995)

| | 1991 | 1992 | 1993 | 1994 | 1995 | Total |
|---|---|---|---|---|---|---|
| Expected $m^2$ | 150,000 | 310,000 | 350,000 | 400,000 | 300,000 | 1,500,000 |
| Realized $m^2$ | 73,049 | 198,458 | 389,207 | 348,350 | 504,915 | |
| Cumulative built | 73,049 | 271,507 | 660,714 | 1,009,064 | 1,513,979 | 1,513,979 |

Notes: Table 3 compares the government-planned additional square meters of higher education facilities in 1990 with the actual construction during 1991–1995. Data: DATAR (1998). Go back to main text

**Table 4:** Effects on education, population and local labor markets

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Students 18–24 (%) | University Graduates (%) | Population (log) | Population Aged 18–24 (%) | Unemployment Rate (%) | Unemployment Rate: 18–34 (%) | Unemployment Rate: 35–60 (%) |
| After x Treated | 5.4175*** | 1.7142*** | -0.0018 | 0.4939 | 0.5481* | -0.4117 | 1.4571*** |
| | (0.9614) | (0.3488) | (0.0209) | (0.3543) | (0.3133) | (0.4835) | (0.2500) |
| Controls: | | | | | | | |
| Province & Year FE | X | X | X | X | X | X | X |
| N | 1,365 | 1,365 | 1,365 | 1,365 | 1,365 | 1,365 | 1,365 |

Notes: Table 4 reports difference-in-differences estimates of the effects of new colleges on educational attainment, population and its age structure, and unemployment at the province level. Coefficients are averaged across treatment periods following the procedure in Callaway and Sant'Anna (2021). Columns (1) and (2) present the impact on the share of 18–24-year-olds enrolled as students and on the share of the population holding a university degree. Columns (3) and (4) report the effects on population and on the share of the population aged 18–24. Columns (5)–(7) focus on the local unemployment rate, and on the unemployment rate for individuals aged 18–34 and 35–60, respectively. The unit of observation is province × year. Estimates are weighted by the province's 1990 employment. Standard errors are clustered at the province level. Data: Labor force surveys.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table 5:** Firm entry by industry

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Manufacturing | Agrifood | Retail | Construction | Transport | Hotels & Restaurants | Business Services | Household Services | All Industries |
| After x Treated | 2.431** | 2.147** | 1.893*** | 1.106 | 2.832** | 2.081* | 1.364 | 4.271* | 1.572** |
| | (1.100) | (0.975) | (0.645) | (0.989) | (1.376) | (1.250) | (0.920) | (2.421) | (0.694) |
| Controls: | | | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X | X | X |
| Mean | 7.71 | 6.40 | 9.57 | 9.93 | 10.89 | 7.64 | 16.86 | 12.34 | 10.76 |
| N | 2,175 | 2,175 | 2,175 | 2,175 | 2,175 | 2,175 | 2,175 | 2,175 | 2,175 |

Notes: Table 5 reports difference-in-differences estimates of the effect of new colleges on the entry rate of new incorporated firms, by industry. Coefficients are averaged across all treatment periods following the procedure in Callaway and Sant'Anna (2021). The mean entry rate is computed over the pre-reform period. The unit of observation is the urban area × year, estimated separately by industry. Estimates are weighted by the number of firms in each area × industry cell in 1990. Standard errors are clustered at the area level and are displayed in parentheses. Data: Registry of firms (Sirene register). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table 6:** Effect of new colleges on firm creation, by industry skill intensity

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Exposure: % of college grad. | | Exposure: % of managers | |
| After x Treated$_t$ x Exposure | 0.02255*** | | 0.01716*** | |
| | (0.00304) | | (0.00138) | |
| After x # New Colleges$_t$ x Exposure | | 0.00013*** | | 0.00006*** |
| | | (0.00001) | | (0.00001) |
| Controls: | | | | |
| Area x Industry FE | X | X | X | X |
| Area x Year FE | X | X | X | X |
| Industry x Year FE | X | X | X | X |
| N | 78,936 | 78,936 | 78,936 | 78,936 |

Notes: Table 6 reports triple-difference estimates from equation (2). The model is estimated in long differences between 1988 and 2000, with firm creation averaged over three-year windows centered on those years to mitigate measurement error. Exposure is measured as the share of the industry's workforce holding a college diploma (columns (1) and (2)) or in a managerial position (columns (3) and (4)), computed from employer-employee data over 1985–1989. *Treated*$_t$ is a dummy equal to one if the area received new higher education capacity before year $t$. *New Colleges*$_t$ is the number of higher education establishments and departments opened up to year $t$. The outcome is the inverse hyperbolic sine transformation of the number of newly incorporated firms. Each observation is an urban area × industry × period. Estimates are weighted by the number of active firms in the industry × area cell in 1988. Standard errors are clustered at the urban-area level. Data: Administrative registry of firms (Sirene register), linked employer-employee data merged with census data (DADS and EDP). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table 7:** Effects on incumbent firms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Sales | Value Added | Pre-Tax Profits | Employment | Average Wage | Capital | Labor Productivity | TFP |
| After x Treated | -0.0298*** | -0.0303*** | -0.0561*** | -0.0137** | -0.0090** | -0.0068 | -0.0098** | -0.0248*** |
| | (0.0077) | (0.0072) | (0.0146) | (0.0058) | (0.0037) | (0.0066) | (0.0043) | (0.0071) |
| Controls: | | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X | X |
| N | 609,277 | 596,539 | 443,535 | 556,479 | 556,479 | 601,786 | 548,942 | 532,444 |

Notes: Table 7 reports difference-in-differences estimates of the effect of new colleges on measures of firm growth. The outcome variables are (1) sales, (2) value added, (3) pre-tax profits, (4) employment, (5) average wage, (6) tangible capital, (7) value added per employee, and (8) total factor productivity (TFP). TFP is computed using the method of Ackerberg et al. (2015). Each observation is a firm × year. The sample is a balanced panel of incumbents, i.e., firms active in every year from 1989 onward. Data: Balance sheet data (FICAS). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table 8:** Overall impact on local economic activity

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Sales | Value Added | Pre-Tax Profits | Employment | Wages | Capital | Labor Productivity | TFP |
| After x Treated | 0.0514*** | 0.0478*** | 0.0753** | -0.0219 | 0.0428*** | 0.1011*** | 0.0697*** | 0.0624*** |
| | (0.0175) | (0.0159) | (0.0298) | (0.0248) | (0.0230) | (0.0242) | (0.0227) | (0.0234) |
| Controls: | | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X | X |
| N | 1,885 | 1,885 | 1,885 | 1,885 | 1,885 | 1,885 | 1,885 | 1,885 |

Notes: Table 8 reports difference-in-differences estimates of the effect of new colleges on city-level economic activity. The coefficients are averaged over all treatment periods using the procedure described in Callaway and Sant'Anna (2021). The outcome variables are (1) sales, (2) value added, (3) pre-tax profits, (4) employment, (5) wages, (6) tangible capital, (7) value added per worker, and (8) total factor productivity. TFP is computed using the method of Ackerberg et al. (2015). The results summarize the effect of the policy taking into account firm entry, exit, and growth. Firm-level data are aggregated at the city level. Each observation is an urban area × year. Estimates are weighted by the number of firms in the area in 1990. Standard errors are clustered at the urban area level. Data: Balance sheet data (FICAS). Go back to main text

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

**Table 9:** Heterogeneous effects of new colleges across cities

| | Baseline characteristic $X_{a,1990}$: | | | |
|---|---|---|---|---|
| | (1) Log value added per employee | (2) College share | (3) Population density | (4) Fraction aged 15–24 |
| Panel A – Outcome: College share | | | | |
| After x Intensity$_a$ x $X_{a,1990}$ | 0.076 | 0.003 | -0.144 | 0.114* |
| | (0.199) | (0.099) | (0.178) | (0.065) |
| Panel B – Outcome: Value added | | | | |
| After x Intensity$_a$ x $X_{a,1990}$ | -0.019** | -0.010 | -0.034** | 0.015 |
| | (0.010) | (0.024) | (0.015) | (0.010) |
| Controls: | | | | |
| Area & Year FE | X | X | X | X |
| $X_{a,1990}$ x After | X | X | X | X |
| Intensity$_a$ x After | X | X | X | X |
| N | 290 | 290 | 290 | 290 |

Notes: Table 9 reports heterogeneous effects of new colleges by initial city characteristics. Intensity$_a$ corresponds to the cumulative number of new university seats, averaged over the period of analysis and scaled by the 1990 workforce. Each column interacts treatment intensity with an urban area characteristic $X_{a,1990}$ measured in 1990: (1) log value added per employee, (2) the share of college graduates, (3) population density, and (4) the fraction of the population aged 15–24. Characteristics are standardized to mean zero and unit variance across areas. The outcome is the share of college graduates (Panel A) and log value added (Panel B). Each observation is an urban area × year. The sample includes treated urban areas only. The model is estimated in long differences. Estimates in Panel A are weighted by the 1990 workforce and estimates in Panel B by the number of firms in 1990. Standard errors are clustered at the urban area level. Data: Linked employer-employee data merged with census data (DADS and EDP), Census data (1990), and balance sheet data (FICAS).
Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

# Online Appendix

## Colleges, Business Dynamism, and Local Growth

Elio Nimier-David
(Cornell University)

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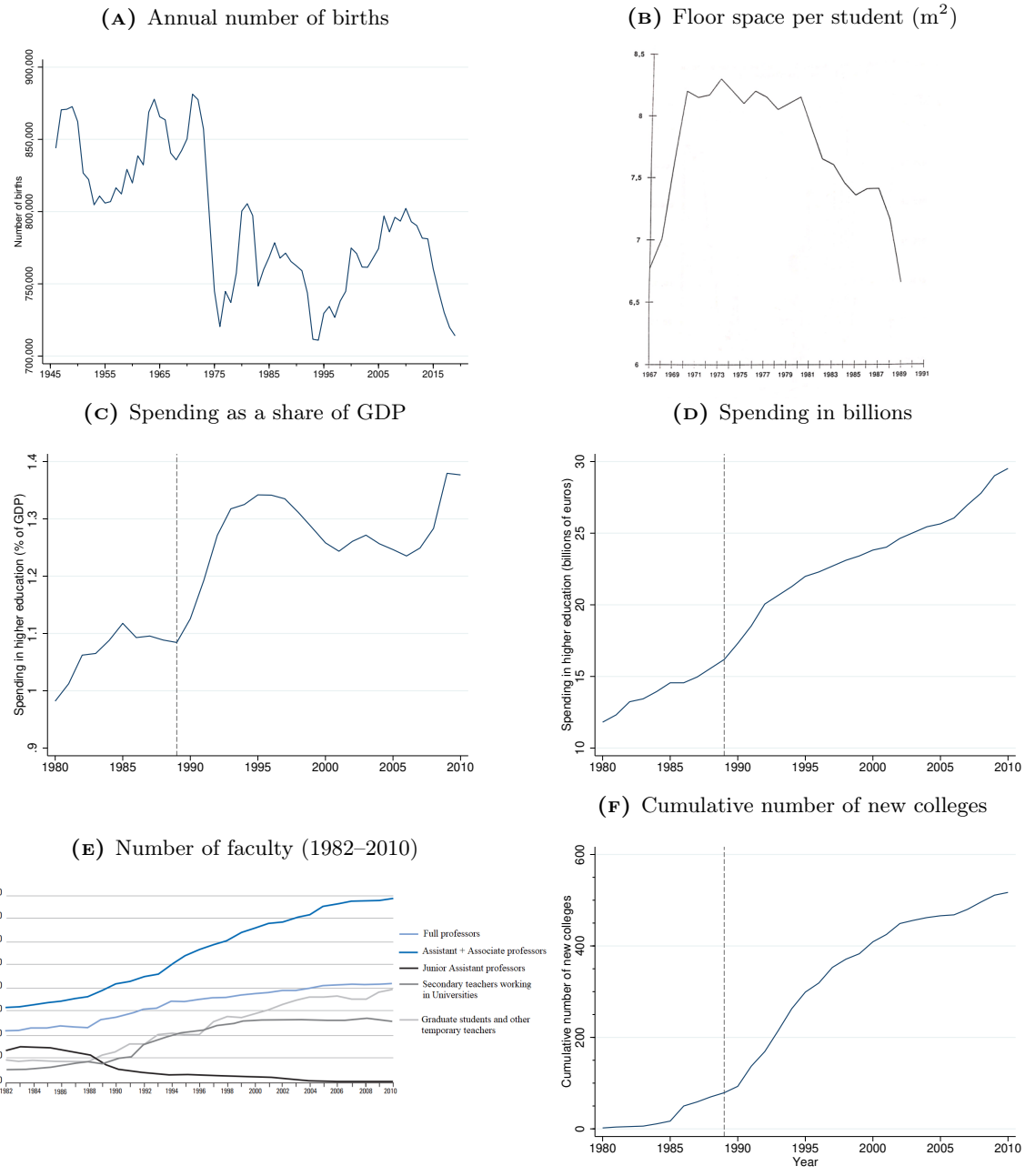
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# A Appendix Figures

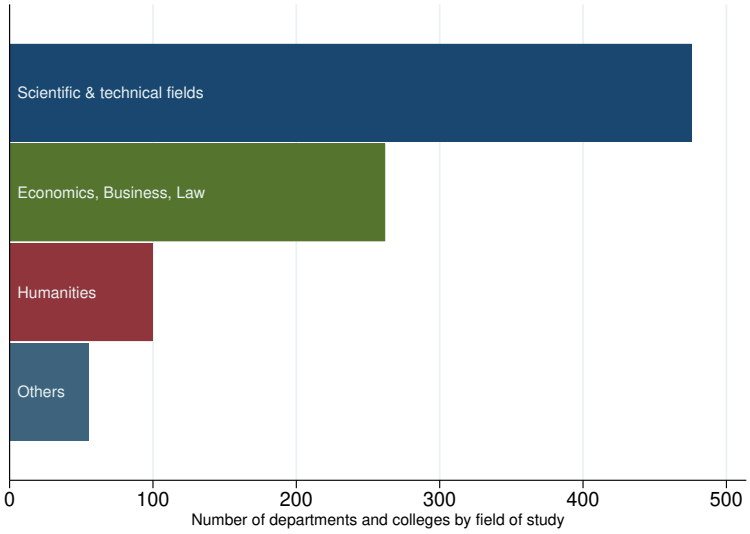
**Figure A.1:** Context of the policy



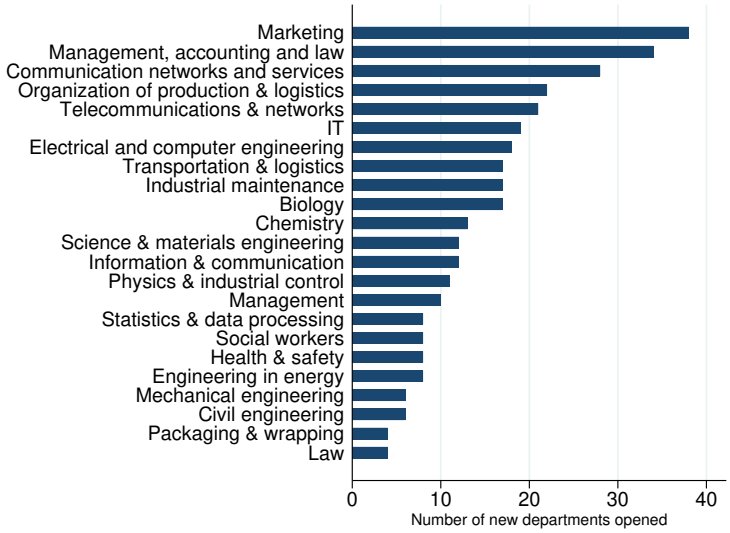

Notes: Figure A.1 reports descriptive statistics on the implementation of the *Universities of the 21st Century* policy. Panel (a) shows the annual number of births in France between 1945 and 2020 using data from INSEE. Panel (b) displays the total surface area (in $m^2$) of public higher education establishments per student for the period 1967–1989. The figure is reproduced from the national archives and was originally produced by the Ministry of Education. Panels (c) and (d) present the evolution of the higher education budget as a share of GDP and in billions of euros, respectively. All values are expressed in 2018 euros. Budget data are provided by the Ministry of Higher Education. Panel (e) shows the number of faculty by status over time, reproduced from DEPP and SIEP (2010). Finally, Panel (f) plots the cumulative number of new higher education establishments (excluding new departments) constructed between 1980 and 2010, normalized to zero in 1980. The dashed vertical line at 1989 marks the beginning of the policy period. Data are drawn from the SISE registry of higher education establishments. Go back to main text

**Figure A.2:** New departments by field of study

**(a)** All colleges and universities

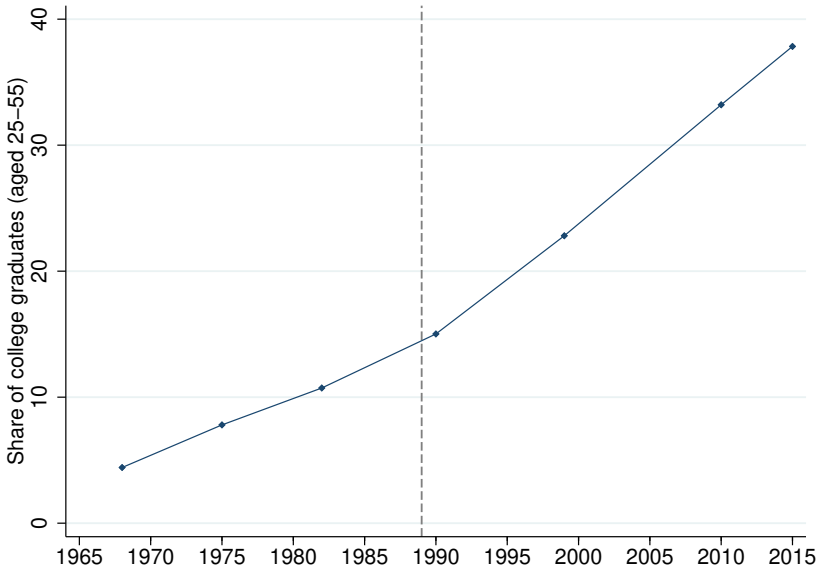



**(b)** Technical colleges



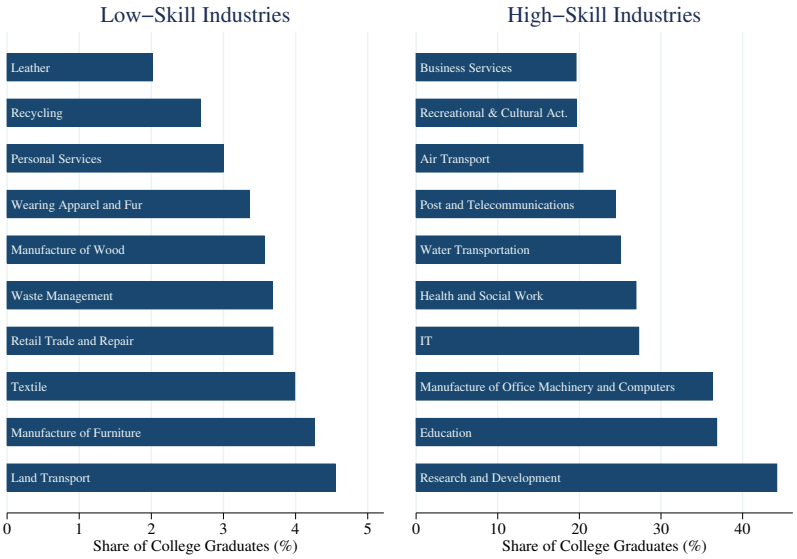

Notes: Figure A.2, Panel (a) shows the number of new departments opened in colleges and universities during the policy period, by major field of study. Panel (b) details the fields of the departments opened in two-year technical colleges (IUT). Data: Registry of higher education establishments (SISE) and official gazette of the French Republic ("Journal Officiel"). Go back to main text

**Figure A.3:** Share of college graduates (aged 25–55)



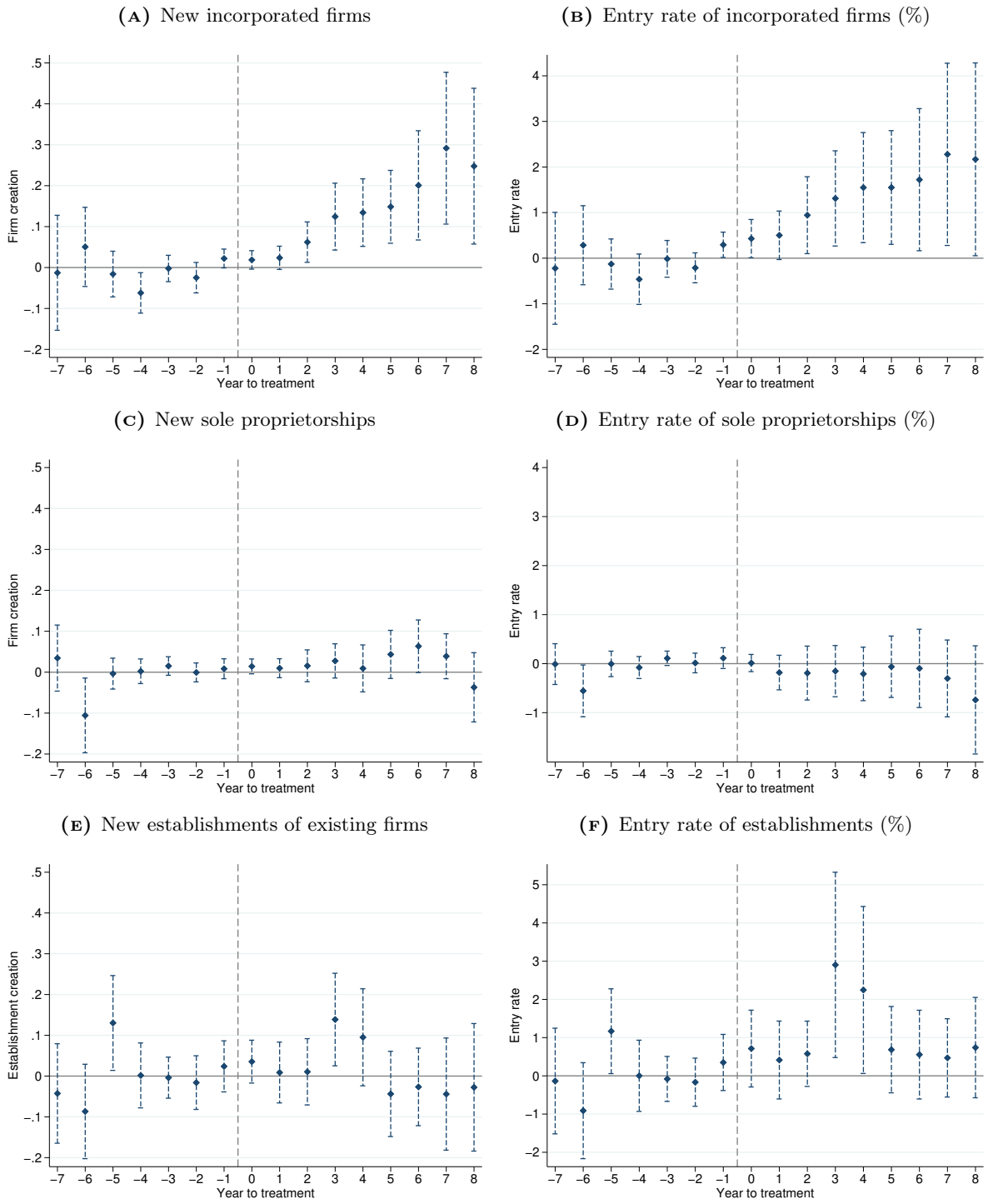

Notes: Figure A.3 plots the share of the population aged 25–55 holding at least a two-year higher education diploma. Data: Census data (1968–2015). Go back to main text

**Figure A.4:** High- and low-skill industries



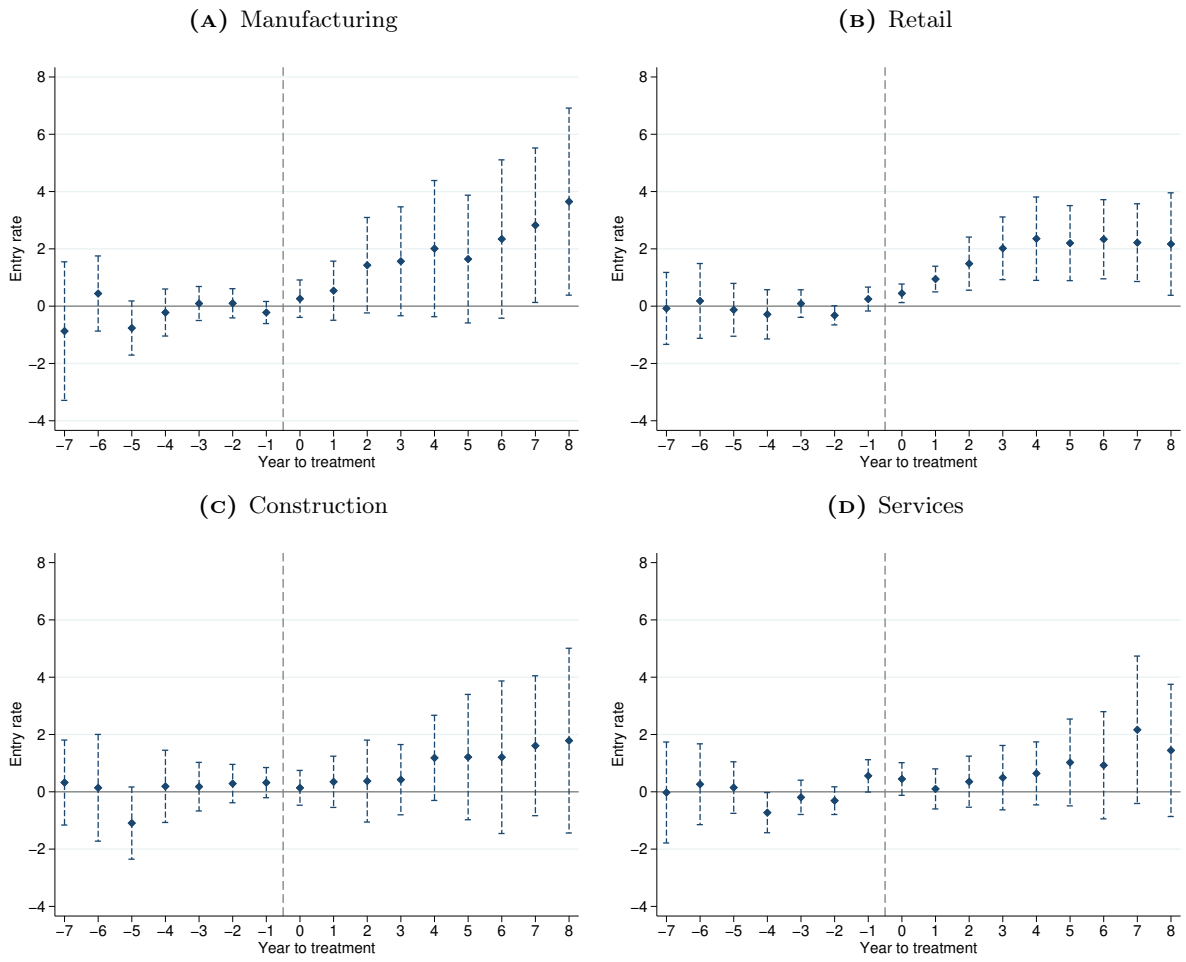

Notes: Figure A.4 reports the 10 most skilled and 10 least skilled two-digit industries based on the share of workers with at least a two-year higher education diploma. The share of high-skill workers is computed over the period 1985–1989, before the beginning of the policy. Data: Linked employer-employee data merged with census data (DADS and EDP). Go back to main text

**Figure A.5:** Firm and establishment creation




Notes: Figure A.5 reports event-study estimates of the effect of new colleges on the creation of new firms and establishments. Panel (a) shows results for the number of newly incorporated firms, and Panel (b) the corresponding entry rate (i.e., new firms divided by the existing stock). Panels (c) and (d) turn to the analysis of sole proprietorships, reporting the number of new firms and their entry rate. Panels (e) and (f) focus on establishments created by existing firms, with results shown for the number of new establishments and the corresponding entry rate. Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity during the policy). The unit of observation is an urban area × year. Estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the area level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

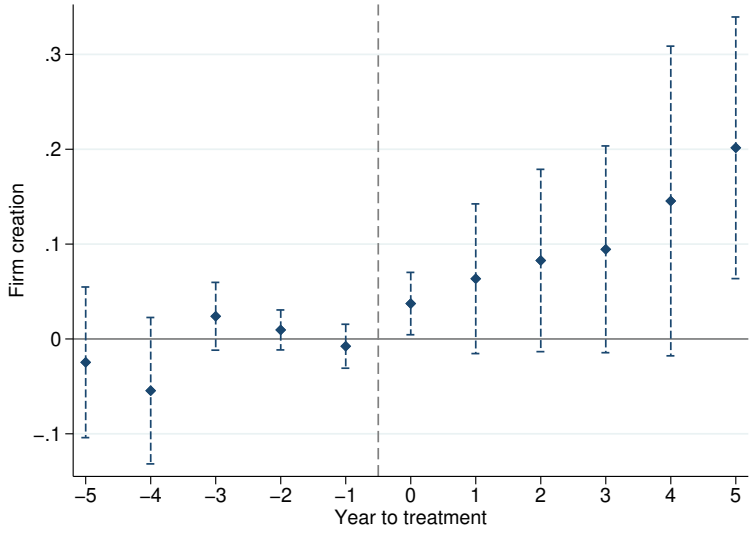
**Figure A.6:** Firm creation by industry



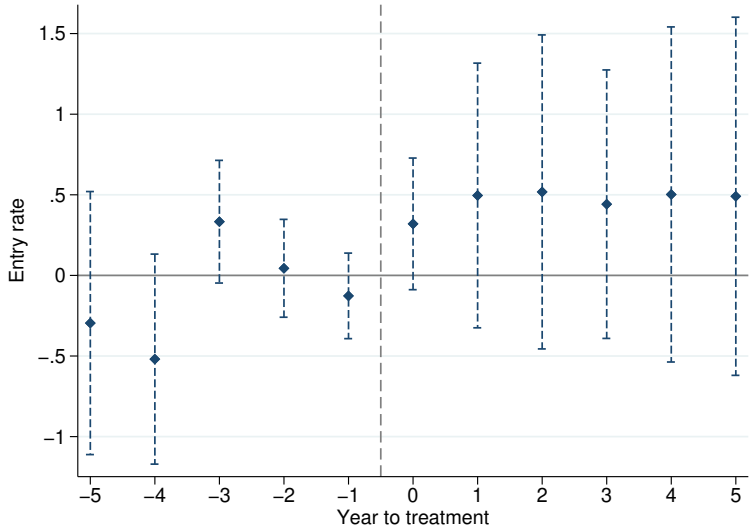

Notes: Figure A.6 reports event-study estimates of the effect of new colleges on the entry rate of incorporated firms, by industry. Panels show entry rates for (a) manufacturing, (b) retail, (c) construction, and (d) services. The services sector includes transportation, hotels and restaurants, business services, and household services. Construction is excluded from the aggregate entry results and reported here for completeness. Event time is measured relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). The unit of observation is the urban area × year. Estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban-area level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

**Figure A.7:** Firm creation at the province level

**(a)** Firm creation




**(b)** Entry rate (in %)




Notes: Figure A.7 reports event-study estimates for the creation of incorporated firms. The outcome variables are: (a) the number of newly incorporated firms and (b) the entry rate of incorporated firms (i.e., the number of new firms divided by the existing number of firms). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity during the policy). Each observation is a province × year. All estimates are weighted by the province's 1990 number of firms. Standard errors are clustered at the province level. The bars correspond to the 95% confidence intervals. Data: Registry of firms (Sirene register). Go back to main text

# B Appendix Tables

**Table B.1:** Higher education establishments opened during the policy

| | 1990 | 1991 | 1992 | 1993 | 1994 | 1995 | 1996 | 1997 | 1998 | 1999 | 2000 | 2001 | 2002 | 2003 | 1990–2003 | Mean size |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Universities:** | | | | | | | | | | | | | | | | |
| Full Universities | 0 | 4 | 2 | 1 | 0 | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 9 | 2704 |
| Satellite Campuses | 5 | 7 | 1 | 6 | 27 | 8 | 10 | 14 | 5 | 2 | 3 | 4 | 4 | 2 | 98 | 307 |
| Departments | 5 | 11 | 11 | 9 | 12 | 8 | 15 | 10 | 10 | 4 | 8 | 9 | 7 | 14 | 133 | 1362 |
| **Engineering:** | | | | | | | | | | | | | | | | |
| Engineering Schools | 5 | 10 | 2 | 5 | 3 | 3 | 2 | 2 | 2 | 1 | 3 | 3 | 4 | 1 | 46 | 621 |
| Instituts Uni. Pro. | 0 | 9 | 10 | 13 | 3 | 11 | 2 | 8 | 9 | 5 | 17 | 5 | 12 | 1 | 105 | 147 |
| **Technical colleges:** | | | | | | | | | | | | | | | | |
| Two-year Technical Colleges | 2 | 5 | 10 | 11 | 2 | 3 | 2 | 0 | 1 | 0 | 1 | 0 | 0 | 0 | 37 | 671 |
| Satellite Campuses | 1 | 6 | 7 | 11 | 13 | 8 | 3 | 3 | 0 | 4 | 1 | 4 | 2 | 0 | 63 | 287 |
| Departments | 10 | 24 | 50 | 58 | 40 | 25 | 39 | 29 | 11 | 7 | 11 | 13 | 17 | 7 | 341 | 140 |
| **Others** | 2 | 5 | 4 | 1 | 1 | 1 | 2 | 10 | 1 | 2 | 3 | 2 | 3 | 4 | 41 | 432 |

Notes: Table B.1 reports, for each year, the number of colleges and departments opened during the policy period and their average size in terms of seats. Full universities include the eight *Universités nouvelles* and the *École Normale Supérieure de Rennes*. Satellite campuses of universities are typically two- or three-year colleges, comparable to US community colleges. "Instituts Uni. Pro." stands for *Instituts Universitaires Professionnalisés* (IUP). IUP and engineering schools award four- and five-year engineering diplomas, respectively. Technical colleges grant two- and three-year technical diplomas and primarily train skilled technicians. Data: Registry of higher education establishments (SISE) and archival records. Go back to main text

**Table B.2:** Probability of receiving higher education capacity during the policy

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Logit | Marginal effects | Probit | Marginal effects |
| Population (10,000) | 0.6699*** | 0.0574*** | 0.3348*** | 0.0555*** |
| | (0.1448) | (0.0078) | (0.0646) | (0.0065) |
| Population growth (%) | -0.0696 | -0.0060 | -0.0371 | -0.0061 |
| | (0.0511) | (0.0043) | (0.0278) | (0.0046) |
| Population density | 0.0006 | 0.0001 | 0.0003 | 0.0000 |
| | (0.0018) | (0.0002) | (0.0010) | (0.0002) |
| Distance to Paris (km) | 0.0007 | 0.0001 | 0.0003 | 0.0001 |
| | (0.0018) | (0.0002) | (0.0010) | (0.0002) |
| Age 15–24 (%) | 1.0225*** | 0.0875*** | 0.5190*** | 0.0861*** |
| | (0.3781) | (0.0286) | (0.1776) | (0.0280) |
| Age 25–34 (%) | 0.4774 | 0.0409 | 0.1989 | 0.0330 |
| | (0.4112) | (0.0340) | (0.2035) | (0.0328) |
| Age 35–44 (%) | 0.3180 | 0.0272 | 0.0739 | 0.0123 |
| | (0.4229) | (0.0349) | (0.2136) | (0.0349) |
| Age 45–54 (%) | 0.2199 | 0.0188 | 0.0485 | 0.0080 |
| | (0.3651) | (0.0308) | (0.1991) | (0.0327) |
| Age 55+ (%) | 0.3325* | 0.0285* | 0.1543* | 0.0256* |
| | (0.1865) | (0.0148) | (0.0873) | (0.0138) |
| University diploma (%) | -0.0075 | -0.0006 | -0.0421 | -0.0070 |
| | (0.2040) | (0.0175) | (0.1038) | (0.0174) |
| High school (%) | 0.6442** | 0.0551*** | 0.3688*** | 0.0612*** |
| | (0.2552) | (0.0213) | (0.1264) | (0.0212) |
| Vocational (%) | -0.0447 | -0.0038 | -0.0344 | -0.0057 |
| | (0.1040) | (0.0089) | (0.0517) | (0.0086) |
| Distance to full university (km) | 0.0130 | 0.0011 | 0.0058 | 0.0010 |
| | (0.0100) | (0.0008) | (0.0046) | (0.0008) |
| Manufacturing (%) | 0.0316 | 0.0027 | 0.0113 | 0.0019 |
| | (0.1079) | (0.0092) | (0.0498) | (0.0083) |
| Construction (%) | -0.0950 | -0.0081 | -0.0660 | -0.0110 |
| | (0.1590) | (0.0138) | (0.0764) | (0.0127) |
| Tertiary (%) | -0.0348 | -0.0030 | -0.0226 | -0.0037 |
| | (0.1022) | (0.0088) | (0.0493) | (0.0082) |
| Employment-population ratio | 0.0083 | 0.0007 | 0.0082 | 0.0014 |
| | (0.1678) | (0.0144) | (0.0809) | (0.0134) |
| Public sector (%) | 0.0905** | 0.0077** | 0.0550*** | 0.0091*** |
| | (0.0358) | (0.0031) | (0.0195) | (0.0034) |
| Average wage | -0.0218 | -0.0019 | -0.0029 | -0.0005 |
| | (0.0463) | (0.0039) | (0.0248) | (0.0041) |
| Firm entry rate (%) | -0.0182 | -0.0016 | -0.0257 | -0.0043 |
| | (0.0736) | (0.0064) | (0.0384) | (0.0065) |
| Left-wing | 0.3068 | 0.0263 | 0.1909 | 0.0317 |
| | (0.4671) | (0.0398) | (0.2400) | (0.0405) |
| Extreme-left | -0.0464 | -0.0040 | 0.0666 | 0.0110 |
| | (0.6527) | (0.0558) | (0.3523) | (0.0587) |
| Observations | 306 | 306 | 306 | 306 |
| pseudo-$R^2$ | 0.600 | | 0.583 | |

Notes: Table B.2 reports the results of a logit model (column (1)) and a probit model (column (3)) estimating the probability of receiving new higher-education capacity during the policy period as a function of initial area characteristics measured in 1990. The corresponding marginal effects are reported in columns (2) and (4). Distances to Paris and to historical universities are expressed in kilometers; population is expressed in tens of thousands of inhabitants; and population density is measured in inhabitants per square kilometer. Population growth is computed between the 1982 and 1990 census years. The level of education of the population is divided into four categories: (i) university diploma (at least a two-year degree), (ii) high school (*Baccalauréat*), (iii) vocational (*CAP-BEP*), and (iv) no diploma. The firm entry rate is defined as the ratio of the number of firms created in 1990 to the number of active firms. Local political orientation corresponds to the political affiliation of the mayor of the largest municipality in the urban area, elected in 1989. It is classified into three categories: right-wing (*RPR*, *UDF*, and various right), left-wing (socialists and various left), and extreme-left (communists and the Green party). No municipality in the sample was governed by a mayor affiliated with an extreme-right party in 1989. The sample is restricted to the 306 urban areas for which the mayor's political affiliation is available (all 145 treated areas and 161 untreated areas). Data: Registry of higher education establishments (SISE), census data (1990), linked employer-employee data (DADS), administrative registry of firms (Sirene register), and results of local elections (Ministry of the Interior). Go back to main text

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

**Table B.3:** Labor market outcomes and background of new graduates, by diploma

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Industry | | | | | | Labor market | | Parental education | |
| | Agriculture | Manufacturing | Construction | Retail & telecom. | Services | Public sector | Unemployment rate | Permanent contract | Father col. degree | Mother col. degree |
| **Universities:** | | | | | | | | | | |
| 2-year | 0.61 | 21.56 | 4.12 | 27.29 | 29.95 | 16.47 | 10.89 | 65.79 | 14.19 | 10.13 |
| 3-year | 0.00 | 7.96 | 0.00 | 12.99 | 22.72 | 56.33 | 15.35 | 68.29 | 22.12 | 17.48 |
| 4-year | 0.44 | 8.72 | 2.16 | 11.89 | 36.82 | 39.98 | 12.01 | 68.27 | 28.34 | 23.74 |
| 5-year | 0.87 | 15.50 | 1.14 | 8.11 | 39.60 | 34.79 | 11.28 | 75.49 | 37.24 | 31.74 |
| Ph.D. | 0.28 | 13.26 | 0.02 | 3.30 | 18.35 | 64.80 | 10.65 | 75.06 | 43.10 | 30.26 |
| **Engineering:** | | | | | | | | | | |
| Engineering schools | 2.96 | 44.11 | 1.88 | 9.87 | 31.34 | 9.84 | 7.09 | 87.15 | 48.81 | 39.29 |
| Instituts Uni. Pro. | 0.00 | 22.90 | 4.03 | 16.55 | 44.34 | 12.18 | 5.75 | 78.22 | 33.59 | 24.09 |
| **Technical colleges** | 0.88 | 30.91 | 3.92 | 15.68 | 31.44 | 17.17 | 11.08 | 65.32 | 19.96 | 16.49 |
| **Business schools** | 0.54 | 19.52 | 1.26 | 27.97 | 46.71 | 3.99 | 7.51 | 84.53 | 50.36 | 41.13 |

Notes: Table B.3 provides descriptive statistics for individuals who graduated from higher education in 1994, by diploma. Labor market characteristics are observed in 1997. Columns (1)–(6) report the share of employed graduates working in each industry. Column (7) reports the share of new graduates unemployed and column (8) the share holding a permanent contract, conditional on being employed. Columns (9) and (10) display the share of new graduates whose father (respectively mother) holds a higher education diploma. "Instituts Uni. Pro." stands for *Instituts Universitaires Professionnalisés*. Statistics are computed using survey weights. Data: *Enquête de Cheminement: sortants de l'éducation supérieure* (1994).
Go back to main text

**Table B.4:** Firm creation in France

| | (1) | (2) | (3) |
|---|---|---|---|
| | All New Establishments | New Firms | New Establishments of Existing Firms |
| **Mean** | 85,283 | 64,062 | 21,221 |
| **Standard deviation** | 6,730 | 6,815 | 2,729 |
| **Initial # of jobs created** | 249,663 | 152,082 | 97,581 |
| **Average size at creation** | 2.9 | 2.4 | 4.6 |
| **Industries (%)** | | | |
| Agrifood | 1.1 | 1.0 | 1.5 |
| Manufacturing | 9.7 | 9.8 | 9.7 |
| Construction | 10.4 | 12.1 | 5.4 |
| Retail | 33.2 | 30.8 | 40.5 |
| Transport | 4.0 | 3.6 | 5.5 |
| Hotels and restaurants | 5.4 | 5.0 | 6.7 |
| Real estate and firm services | 30.3 | 31.9 | 25.7 |
| Other services | 5.7 | 5.9 | 5.0 |
| **Size (%)** | | | |
| 1 | 48.1 | 53.3 | 39.8 |
| 2–5 | 41.1 | 37.3 | 48.0 |
| 6–10 | 5.6 | 5.1 | 6.0 |
| 11–20 | 3.0 | 2.2 | 3.0 |
| More than 20 | 2.2 | 2.0 | 3.2 |

Notes: Table B.4 provides descriptive statistics on the number of new firms and new establishments created in France between 1987 and 2003. The sample is restricted to incorporated firms. Column (1) reports both categories combined. Columns (2) and (3) correspond, respectively, to new firms created *ex nihilo*, and to new establishments, i.e., locations opened by existing firms. All variables are computed as yearly averages over the period. The initial number of jobs created and the average size at creation are computed using the number of employees at the date of registration (including the entrepreneur). Data: Administrative registry of firms (Sirene register). Go back to main text

**Table B.5:** Characteristics of founders of incorporated firms

| | 1994 | 1998 | 2002 |
|---|---|---|---|
| Mean age | 39.15 | 38.77 | 40.31 |
| | (9.82) | (10.15) | (10.35) |
| Male (%) | 73.7 | 74.9 | 75.8 |
| *Education (%)* | | | |
| Below high school | 49.4 | 42.4 | 37.1 |
| High school | 17.1 | 19.9 | 20.2 |
| College | 33.5 | 37.7 | 41.1 |
| *Situation before creation (%)* | | | |
| High-skilled occupation | 57.0 | 51.5 | 52.8 |
| Middle-skilled occupation | 11.0 | 9.8 | 11.9 |
| Lower-skilled occupation | 23.1 | 27.8 | 21.1 |
| Student | 3.3 | 4.6 | 3.2 |
| Out of the labor force | 5.6 | 6.3 | 11.1 |
| Serial entrepreneur (%) | 38.2 | 39.9 | 44.0 |

Notes: Table B.5 reports descriptive statistics on the founders of new incorporated firms in 1994, 1998, and 2002. Statistics are computed using survey sampling weights; standard deviations are reported in parentheses. "Below high school" includes founders with no diploma or a diploma below the *baccalauréat*; "High school" corresponds to the *baccalauréat*; "College" to a higher-education degree. The situation before creation refers to the founder's occupation or status prior to founding the firm. "High-skilled" includes business owners, executives, managers, and engineers; "Middle-skilled" includes supervisors and technicians; "Lower-skilled" includes blue-collar workers and clerks. Data: SINE survey of entrepreneurs (1994, 1998, 2002). Go back to main text

**Table B.6:** Correlations between treatment year and cities' initial characteristics

| | (1) | (2) |
|---|---|---|
| | First year of treatment | All years of treatment |
| Population (10,000) | -0.0027 | 0.0014 |
| | (0.0033) | (0.0020) |
| Population growth (%) | -0.0601 | -0.0555 |
| | (0.1027) | (0.0766) |
| Population density | 0.0009 | -0.0002 |
| | (0.0026) | (0.0013) |
| Distance to Paris (km) | 0.0023 | 0.0020 |
| | (0.0030) | (0.0023) |
| Age 15–24 (%) | -0.1235 | -0.0926 |
| | (0.4488) | (0.3176) |
| Age 25–34 (%) | 0.6378 | 0.0962 |
| | (0.5788) | (0.4046) |
| Age 35–44 (%) | 0.0462 | 0.4858 |
| | (0.7648) | (0.5067) |
| Age 45–54 (%) | -0.7977 | -0.2828 |
| | (0.6587) | (0.4438) |
| Age 55+ (%) | 0.3216 | 0.1442 |
| | (0.2015) | (0.1716) |
| University diploma (%) | 0.2744 | 0.0601 |
| | (0.2999) | (0.2269) |
| High school (%) | -0.3415 | 0.0306 |
| | (0.4064) | (0.2792) |
| Vocational (%) | -0.0229 | -0.0208 |
| | (0.1136) | (0.0905) |
| Distance to full university (km) | 0.0016 | -0.0086 |
| | (0.0089) | (0.0070) |
| Manufacturing (%) | -0.0421 | 0.0972 |
| | (0.1433) | (0.1283) |
| Construction (%) | -0.3786 | -0.0958 |
| | (0.2568) | (0.2081) |
| Tertiary (%) | -0.0278 | 0.0997 |
| | (0.1726) | (0.1459) |
| Employment-population ratio | 0.1004 | -0.0288 |
| | (0.2399) | (0.1673) |
| Public sector (%) | 0.0003 | 0.0025 |
| | (0.0697) | (0.0538) |
| Average wage | -0.1074 | -0.0420 |
| | (0.0863) | (0.0606) |
| Firm entry rate (%) | -0.1169 | -0.0461 |
| | (0.1574) | (0.1173) |
| Left-wing | -0.2584 | -0.5187 |
| | (0.5685) | (0.3984) |
| Extreme-left | -0.5871 | -0.8005 |
| | (0.6198) | (0.4924) |
| Observations | 145 | 520 |

Notes: Table B.6 reports coefficients from a linear regression of the year of treatment on initial area characteristics measured in 1990. Column (1) uses the first year of treatment (i.e., the year during which additional capacity was built for the first time during the policy). Column (2) uses all years of treatment (i.e., all the years during which the city received additional capacity). The sample comprises the 145 treated areas in column (1) and 520 area × treatment-year observations in column (2). Distances to Paris and to historical universities are expressed in kilometers, population in tens of thousands of inhabitants, and population density in inhabitants per square kilometer. Population growth is computed between two census years (1982–1990). The level of education of the population is divided into four categories: (i) university diploma (at least a two-year diploma), (ii) high school (*Baccalauréat*), (iii) vocational (*CAP-BEP*), and (iv) no diploma. The firm entry rate is defined as the ratio of the number of firms created in 1990 to the number of active firms. Finally, the local political orientation corresponds to the political affiliation of the mayor of the largest municipality in the urban area, elected in 1989. It is divided into three categories: right-wing (*RPR*, *UDF*, and various right), left-wing (socialists and various left), and extreme-left (communists and the Green party). No municipality in the sample was governed by a mayor affiliated with an extreme-right party in 1989. Data: Registry of higher education establishments (SISE), 1990 Census, linked employer-employee data (DADS), administrative registry of firms (Sirene register), and local election results (Ministry of the Interior). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.7:** Continuous treatment specification

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Average effect | Per new college | Per 1,000 seats | $\frac{\text{Seats}}{\text{Population 25–55}}$ |
| *Panel A: Education* | | | | |
| After x Treated | 2.4342*** | 0.0739*** | 0.1228*** | 1.842*** |
| | (0.7198) | (0.0227) | (0.0355) | (0.5660) |
| *Panel B: Firm creation* | | | | |
| After x Treated | 0.1579*** | 0.0037*** | 0.0056** | 0.1287*** |
| | (0.0521) | (0.0014) | (0.0023) | (0.0483) |
| *Panel C: Entry rate (%)* | | | | |
| After x Treated | 1.572** | 0.0369** | 0.056* | 1.2743** |
| | (0.7822) | (0.0184) | (0.0301) | (0.6434) |
| Controls: | | | | |
| Area & Year FE | X | X | X | X |
| N | 2,175 | 2,175 | 2,175 | 2,175 |

Notes: Table B.7 reports the effects of new higher education capacity on local educational attainment and firm creation. The unit of observation is an urban area × year. Column (1) displays the average effect estimated using model (1). Columns (2)–(4) present estimates for continuous treatments following de Chaisemartin and D'Haultfœuille (2020), reporting effects per new college, per 1,000 new seats, and per seat per 100 inhabitants aged 25–55, respectively. Panel A focuses on the share of college graduates, Panel B on firm creation, and Panel C on the entry rate of firms. Panels B and C are restricted to incorporated firms. Estimates in Panel A are weighted by the area's 1990 workforce, while estimates in Panels B and C are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban-area level. Data: Registry of higher education establishments (SISE), registry of firms (Sirene register), and linked employer-employee data matched with census data (DADS and EDP). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.8:** Firm growth seven years after creation

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | Survival | Employment | Employment young | Employment older | High-skill workers | High-skill young | High-skill older | Sales | Profits | TFP |
| After x Treated | -0.0042 | 0.0520 | 0.0544* | 0.0490* | 0.0473** | 0.0336* | 0.0261* | 0.0447 | 0.0319** | 0.0370* |
| | (0.0122) | (0.0317) | (0.0325) | (0.0289) | (0.0226) | (0.0197) | (0.0149) | (0.0336) | (0.0146) | (0.0191) |
| Controls: | | | | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X | X | X | X |
| N | 540,211 | 168,887 | 168,887 | 168,887 | 168,887 | 168,887 | 168,887 | 176,350 | 175,496 | 162,906 |

Notes: Table B.8 reports the results of an event study on the growth of new firms. The coefficients are averaged over all treatment periods using the procedure described in de Chaisemartin and D'Haultfœuille (2020). The outcome variables are: (1) survival, (2) total employment, (3) the number of workers aged 18–34, (4) the number of workers aged 35–60, (5) the number of high-skill workers, (6) the number of high-skill workers aged 18–34, (7) the number of high-skill workers aged 35–60, (8) sales, (9) profits, and (10) total factor productivity. Survival is the probability of being observed in the linked employer-employee data seven years after creation. Employment is measured in full-time equivalents based on hours worked. High-skill workers include engineers and managers. The unit of observation is the firm. Standard errors are clustered at the area level. Data: Administrative registry of firms (Sirene register), linked employer-employee data (DADS), and balance sheet data (FICAS). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.9:** Impact of education on firm creation in manufacturing

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | OLS | First Stage | Reduced Form | 2SLS |
| College share (%) | 0.0262*** | | | 0.0463*** |
| | (0.0079) | | | (0.0087) |
| Number of Colleges | | 0.0281*** | 0.0013*** | |
| | | (0.0026) | (0.0002) | |
| Controls: | | | | |
| Area & Year FE | X | X | X | X |
| N | 290 | 290 | 290 | 290 |
| Kleibergen-Paap F Statistic | | | | 114.4 |
| Elasticity | | | | 0.70 |

Notes: Table B.9 reports estimates of the effect of local educational attainment on firm creation in manufacturing, instrumenting the share of college graduates with the number of new colleges (see Appendix Section D). The regressions are estimated in long differences between 1989 and 1999, with firm creation and the college share averaged over three-year windows to avoid zero counts. Each observation corresponds to an urban area × period. The outcome variable in columns (1), (3), and (4) is the log number of incorporated firms created in manufacturing; the outcome in column (2) is the share of college graduates. "Number of Colleges" denotes the cumulative number of colleges and departments opened in the area since 1989. Column (1) reports OLS estimates, column (2) the first stage, column (3) the reduced form, and column (4) the 2SLS estimate. The implied elasticity is computed at the mean graduate share. Estimates are weighted by the number of active manufacturing firms in the area in 1990. Standard errors are clustered at the urban-area level. Data: Registry of higher education establishments (SISE), registry of firms (Sirene register), linked employer-employee data matched with census data (DADS and EDP). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.10:** Characteristics of new entrepreneurs: previous occupation, age, and education

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Executive | Manager | Supervisor | Clerk | Blue-collar | Out of labor force | Serial entrepreneur |
| After x Treated | 0.1424** | −0.0213 | 0.0503 | −0.0371 | −0.0136 | −0.0770** | 0.1036 |
| | (0.0628) | (0.1100) | (0.0659) | (0.1327) | (0.0420) | (0.0326) | (0.0953) |
| Controls: | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X |
| Mean (1994) | 0.295 | 0.209 | 0.131 | 0.231 | 0.059 | 0.053 | 0.408 |
| Observations | 82 | 82 | 82 | 82 | 82 | 82 | 82 |

| | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
|---|---|---|---|---|---|---|---|
| | Former student | Age ≤ 25 | Age ≤ 35 | Age ≤ 45 | Less than high school | High school | College |
| After x Treated | −0.0437** | −0.0472* | −0.0476 | −0.0389 | 0.0130 | −0.0862 | 0.0732 |
| | (0.0212) | (0.0277) | (0.0668) | (0.0652) | (0.1160) | (0.0559) | (0.1284) |
| Controls: | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X |
| Mean (1994) | 0.022 | 0.077 | 0.357 | 0.507 | 0.547 | 0.186 | 0.267 |
| Observations | 82 | 82 | 82 | 82 | 82 | 82 | 82 |

Notes: Table B.10 displays the results of a difference-in-differences analysis on the demographic characteristics of new entrepreneurs. Coefficients compare areas treated between 1994 and 1998 to areas treated after 1998, before and after treatment. Columns (1)–(6) focus on the detailed occupation of the entrepreneur before founding the firm, and column (7) indicates whether they had already founded a company. Columns (8)–(14) document founders' characteristics: whether they had recently graduated, their age, and their education. Each observation is an urban area × year. All estimates are weighted by the area's 1990 number of firms. Standard errors are clustered at the area level and are displayed in parentheses. Data: SINE survey of entrepreneurs (1994, 1998). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table B.11:** Main clients and market scope of new firms

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | Main clients | | | Main market | | | |
| | Individuals | Firms | Administrations | Local | Regional | National | International |
| After x Treated | −0.0907 | 0.0822 | 0.0085 | −0.0737 | −0.0505 | 0.1035 | 0.0207 |
| | (0.0846) | (0.1216) | (0.0584) | (0.0908) | (0.1069) | (0.0783) | (0.0427) |
| Controls: | | | | | | | |
| Area & Year FE | X | X | X | X | X | X | X |
| Mean (1994) | 0.436 | 0.503 | 0.061 | 0.341 | 0.393 | 0.193 | 0.073 |
| N | 82 | 82 | 82 | 82 | 82 | 82 | 82 |

Notes: Table B.11 displays the results of a difference-in-differences analysis on the characteristics of the main clients of newly created firms, averaged at the urban area level. Coefficients compare areas treated between 1994 and 1998 to areas treated after 1998, before and after treatment. The outcomes are (1)–(3) the probability that new firms have as main clients individuals, firms, or public administrations, and (4)–(7) the probability that they serve mainly a local, regional, national, or international market. Each observation is an urban area × year. All estimates are weighted by the area's 1990 number of active firms. "Mean (1994)" reports the outcome mean in the first survey wave. Standard errors are clustered at the area level and are displayed in parentheses. Data: SINE survey of entrepreneurs (1994, 1998). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

# C Data Appendix

## C.1 Datasets

**Urban areas.** An urban area is a group of connected municipalities and represents the broadest statistical definition of a city in France. It consists of an urban center with at least 1,500 jobs and adjacent municipalities where at least 40% of employees work in the urban center. Urban areas are generally smaller than commuting zones (CZs), except in the case of major cities such as Paris, Lyon, and Marseille. They exclude most rural municipalities, while CZs include both rural and urban municipalities. In 1990, 85% of the French population lived in an urban area, and 90% of new firms were created in one. Like municipalities, urban areas are very heterogeneous in size, ranging from about 2,000 inhabitants to over 11 million (Paris). Because of its size and density, the urban area of Paris nearly coincides with the entire Paris region (Île de France). I focus on urban areas as defined by INSEE using 2010 boundaries. Section 3 discusses why this definition best matches the catchment area of new colleges.

**Higher education registry.** The registry covers public higher education establishments and reports each site's establishment type, academic discipline, date of opening, and municipality. Because the goal is to measure local teaching capacity, I exclude sites that do not provide in-person, degree-granting higher education: distance-learning centers, institutions without degree programs (e.g., the Collège de France), and museums and observatories. I also exclude teacher-training colleges, which train future civil servants. I classify the remaining sites into two groups, both of which count as new education capacity in the analysis. The first comprises establishments: universities (the "Universités nouvelles"), university satellite campuses, engineering schools, two-year technical colleges (IUT) and their satellite branches, and university professional institutes (IUP). A final miscellaneous category groups the smaller institution types: business administration institutes (IAE), political studies institutes (IEP), and specialized schools (including art, architecture, agriculture, and sports). Together, these account for a small share of new openings. The second comprises additions within existing institutions: degree-granting university departments and new fields of study in technical colleges, which I hand-collected from the official gazette (*Journal Officiel*). Finally, I assign each site to one of four aggregated fields of study (sciences and technology; economics, law, and business; humanities; and other disciplines such as sports, tourism, urban planning, and architecture), based on the discipline reported in the registry and the establishment type.

"Universités nouvelles": The plan *Universities of the 21$^{st}$ Century* led to the creation of eight new universities and one École Normale Supérieure. Four were created in the Paris region (Cergy, Évry, Marne-la-Vallée, and Versailles-Saint-Quentin-en-Yvelines). Two were opened in the north: the University of Artois in Arras, Béthune, Douai, and Lens; and the University of the Littoral in Boulogne-sur-Mer, Calais, Saint-Omer, and Dunkerque. Two were opened in the west: La Rochelle and southern Brittany (Vannes and Lorient). Finally, a new École Normale Supérieure campus opened in Rennes.

"Instituts Universitaires Professionnalisés": IUPs were created in 1991 to provide advanced technical education to university students. Unlike engineering schools, which operate as autonomous institutions, IUPs were integrated within universities. Their graduates typically entered the labor market with skills between those of skilled technicians and fully trained engineers. These programs were phased out in the mid-2000s and replaced by professional master's degrees.

**Firm registry.** The Sirene registry provides a comprehensive list of existing and newly created firms and establishments, except in the agriculture and finance industries. A firm's creation is

defined as its first appearance in the registry.[39] Appendix Table B.4 reports the average annual number of new firms and new establishments opened in continental France between 1987 and 2003. New firms account for nearly three-quarters of all creations in a given year. They generally start small: half have only one member, the entrepreneur.[40] On average, new firms begin with 2.4 employees, compared to 4.6 for new establishments. New firms therefore represent about 61% of all new jobs created annually by new business units. Most firms are created in real estate and business services (31.9%), retail (30.8%), construction (12.1%), and manufacturing (9.8%).

**Balance sheet data.** The BRN ("Bénéfice Réel Normal") are corporate tax files collected by the Treasury. They contain income statements and balance sheets for the universe of firms that file under the normal tax regime. I use the Fichier des Comptes Antérieurs à SUSE (FICAS) data, a harmonized version of the BRN constructed by the French Statistical Office (INSEE). These data include measures of sales, value added, profits, capital, intermediates, and employment. Consistent with the other datasets, I exclude firms that belong to the agriculture and finance industries, as well as to the non-market sector. I drop firms with gaps in their reporting. Overall, I observe an average of 365,000 firms per year.

**Linked employer-employee data.** There are two versions of the French linked employer-employee data. The first version is a random sample of $1/24^{th}$ of the French labor force available since 1976 (the "Panel DADS").[41] This sample includes individuals born in October of even-numbered years. It contains a worker and a firm identifier, the age and the gender of the worker, the total gross and net wages for each employment spell, the occupation category of the worker, her place of work and residence, the size and the industry code of the establishment she works in. I use these data to study the evolution of local labor markets following the opening of new colleges. Importantly, the Panel DADS can be merged with census data (the "Échantillon Démographique Permanent," hereafter EDP) to recover workers' level of education. The EDP provides information on workers' highest diploma based on census data for a sample of about 13% of the workers in the DADS (i.e., workers born on the first four days of October). See Abowd et al. (1999) for more details. I complement information on education with the occupation code, available for the whole sample. The French occupation code has a hierarchical structure and can be used as a proxy for the level of skill of the job. I divide the workforce into four categories: blue-collar workers, clerks, intermediate white-collar workers, and high-skilled white-collar workers, based on the one-digit occupation code. Intermediate white-collar occupations correspond to technicians and supervisors, while high-skilled occupations include engineers, managers, and executives.

The second version of the DADS is comprehensive. I use these data to study new firms' growth and the composition of their workforce after creation. While the Sirene registry starts in 1987, the comprehensive DADS are only available since 1994. As a result, I study the composition of the workforce in firms created since 1987, at age seven, so that I can observe them in the linked employer-employee data.

I restrict the analysis to mainland France and focus on workers aged 18 to 60. I exclude individuals working in the public sector or in the construction industry from the main analysis, to avoid potential mechanical effects coming from new colleges (e.g., through the recruitment of university staff or the actual construction of the new buildings). This provides a conservative estimate of the effect of colleges on local employment. I keep all individuals who earn at least the equivalent of a month of full-time minimum wage during the year to focus on economically

[39] The Sirene register also records firms that start operating again after a period of inactivity; I do not treat these as creations.

[40] Following Glaeser and Kerr (2009), I compute the initial size of firms as the number of employees plus one, the entrepreneur.

[41] DADS: "Déclarations Annuelles de Données Sociales."

meaningful jobs, and exclude interns and apprentices. I measure employment as the fraction of the year worked (i.e., someone working for six months during the year will count as one half). Worker compensation is measured as the daily wage of full-time workers. I exclude observations below 85% of the daily minimum wage and winsorize top and bottom 1% to exclude extreme values. The Panel DADS includes on average 550,000 workers per year.

**Labor Force Surveys.** The French Labor Force Survey (*Enquête Emploi*, LFS) is INSEE's main household survey on labor market status and the source of France's official unemployment statistics under the International Labour Organization definition. It collects information on employment, job search, education, and demographics. Every year in March, INSEE surveys around 75,000 households based on a stratified sample. The sample includes around 140,000 individuals per year aged 15 and over. Although the LFS underwent changes over time, including a modification of the questionnaire in 1990, the main variables of interest (population counts, level of education, and employment status) remained unaffected. All variables are computed using the sampling weights provided by INSEE.

**Population census.** Census data are available for 1968, 1975, 1982, 1990, 1999, and annually since 2006. Initial city characteristics are drawn from the 1990 census. Demographic data such as population size and age structure come from the full census, while education, occupations, and industries are available for a 40% random subsample. Education is classified into four categories: *no diploma*, *vocational technical school*, *high school* ("Baccalauréat"), and at least a *two-year college diploma*. Long-run outcomes of exposed individuals are based on the 2015 census, which pools the annual survey waves collected between 2013 and 2017. The survey is exhaustive in municipalities with fewer than 10,000 inhabitants and covers 40% of the population in larger municipalities. It records each individual's municipality of birth, which I map to urban-area boundaries.

## C.2 Capacity of Higher Education Establishments

Information on establishment capacity is provided by the Ministry of Higher Education for the year 2016. It is missing for establishments that closed or merged before that date, as well as for departments in two-year technical colleges. I complement the registry with enrollment data from two historical sources: the "Atlas Régional" of higher education and evaluation reports produced by the National Committee of Evaluation. I then predict the size of the remaining establishments, separately by institution type, using city characteristics from the 1990 Census.

The "Atlas Régional" has been published by the Ministry of Higher Education and Research every year since 1991. For each city endowed with a public higher education establishment, it provides the number of students enrolled in universities, university satellite campuses, two-year technical colleges, and engineering schools. I use these Atlases to recover the size of the two-year technical colleges that were built during the policy.

The second source is produced by the National Committee of Evaluation, created in 1984 with the objective of monitoring higher education establishments. I leverage a series of reports produced during the 1990s and 2000s on the "Universités nouvelles" (new universities) to infer their capacity.

Finally, I predict missing values, separately for each type of higher education establishment, using a Poisson model. Predictors include initial city characteristics (log population, log surface area), year-of-opening fixed effects, and region fixed effects.

## C.3 New Graduates and the Labor Market

This section provides further descriptive statistics on new graduates and their labor market outcomes by level of education. The statistics are computed using the "Enquête de Cheminement: sortants de l'éducation supérieure" (1994), a survey conducted by CEREQ in 1997 among a representative sample of more than 7,000 graduates of higher education in 1994.[42] It collects information on their level of education, family background, and labor market outcomes three years after graduation. The statistics are computed by level of education using sampling weights; the level of education is defined as the highest diploma observed in 1997.

Appendix Table B.3 complements Table 1, providing information on the industry in which new graduates work, their unemployment rate, their likelihood of holding a permanent contract, and their parental level of education.

Starting with former students of two-year satellite campuses, they typically become technicians, supervisors, and clerks, working in manufacturing, retail, telecommunications, and services. For example, they may become accountants or maintenance technicians. Students with two years of higher education are also very likely to work in the region where they were educated (72%), indicating a high retention rate. They earn on average 116% of the minimum wage and they account for more than a quarter of all new graduates in a given year. These students are also highly likely to be originally from the region where they studied: more than 75% of students with a two- or three-year diploma obtained their Baccalauréat in the same region, compared with less than 50% for a five-year diploma and 26% for engineering schools. These figures confirm that two-year satellite campuses act mainly as local colleges, educating students from their own region.

Graduates of two-year technical colleges (IUT) tend to hold slightly more skilled occupations: they mainly work as skilled technicians and supervisors (61%), are over-represented in manufacturing, and earn on average 121% of the minimum wage. Typical occupations include lab technician, IT technician, and quality manager. They are slightly less likely than other two-year college graduates to work in the region where they obtained their diploma, though this rate remains high, at 64%. They account for almost 10% of new graduates. Again, more than three-quarters obtained their high school diploma in the same region.

Finally, graduates of engineering schools and *Instituts Universitaires Professionnalisés* (IUP) mostly become engineers and managers working in manufacturing. They have the lowest unemployment rates and earn among the highest wages (respectively 202% and 159% of the minimum wage). They are also the most mobile, with regional retention rates between 40 and 50%. While engineering schools tend to attract many students from other regions, IUP are more "local": almost 70% of their students come from the same region.[43] Graduates of IUP and engineering schools represent about 8% of new graduates.

Lastly, children from higher socio-economic status (SES) families are over-represented in longer and more prestigious tracks: almost 50% of graduates from engineering schools have a father with a university diploma, compared with only 20% for graduates of two-year technical colleges.[44]

[42] CEREQ is a public institution that studies the interactions between education policies and the labor market.

[43] This difference reflects differences in administrative organization. Engineering schools are independent institutions that recruit students via a national competitive exam, whereas IUP are administratively tied to regional universities and, as a result, must prioritize students from their region.

[44] These figures are 40% and 16%, respectively, when considering the mother's level of education.

# D Firm Creation and Higher Education

The policy offers a quasi-experimental design for studying whether a more educated workforce fosters business dynamism. Section 5.1 shows that the construction of new colleges produced a sizeable increase in the educational attainment of the local workforce, and can therefore be used as a localized shock to the supply of educated workers. To estimate the semi-elasticity of firm creation with respect to education, I instrument the share of the workforce with a college diploma by the number of higher education establishments constructed during the policy, estimating the following two-stage least squares model:

$$\text{College Share}_{a,t} = \alpha_a^1 + \delta_t^1 + \mu \text{ New Colleges}_{a,t} + u_{a,t} \tag{A}$$

$$y_{a,t} = \alpha_a^2 + \delta_t^2 + \gamma \widehat{\text{College Share}}_{a,t} + \epsilon_{a,t} \tag{B}$$

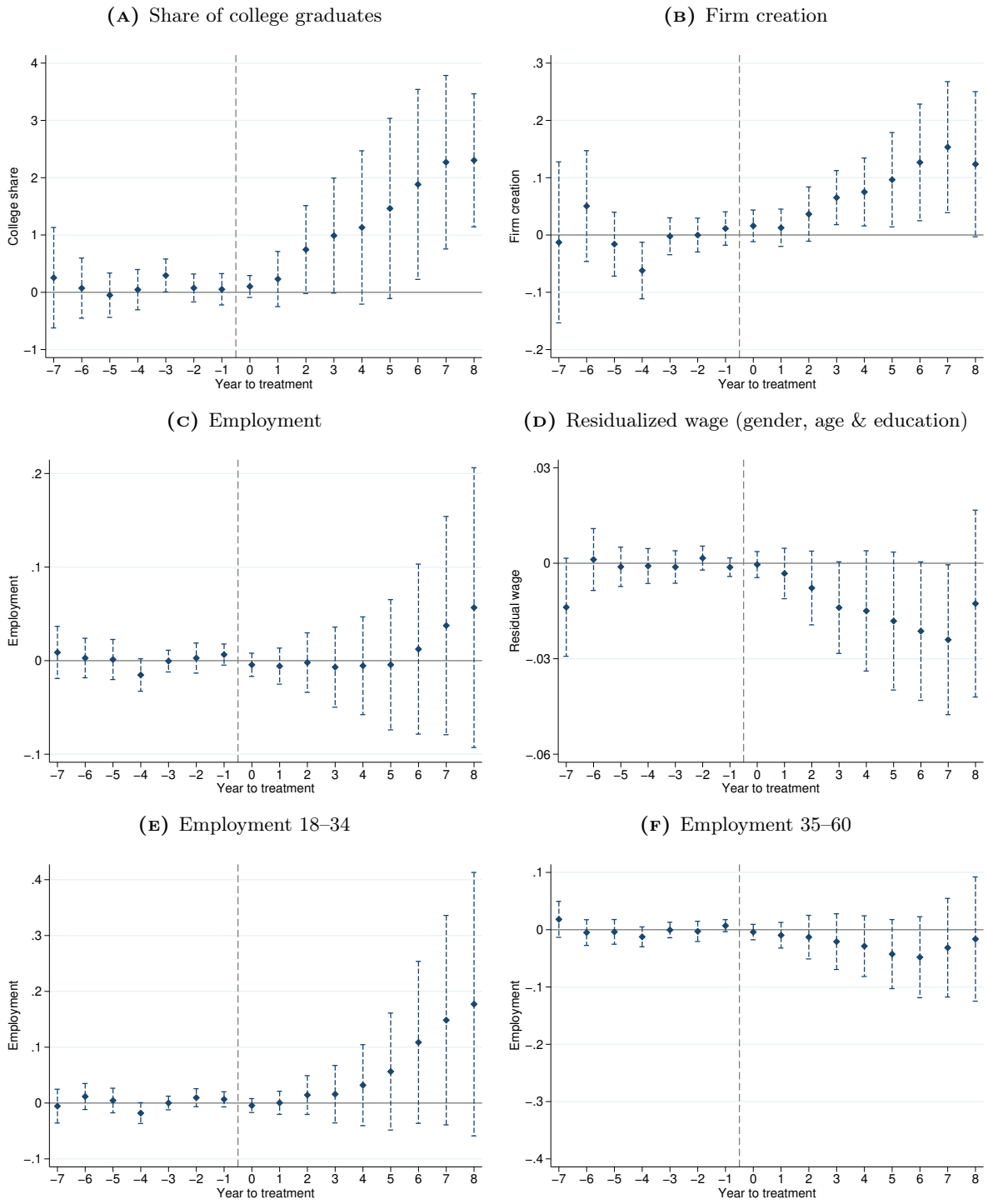
where equation (A) corresponds to the first stage while equation (B) is the second stage of the 2SLS regression. $y_{a,t}$ measures firm creation in area $a$ in period $t$. $\alpha_a^k$, $k \in \{1, 2\}$, controls for time-invariant area characteristics such as initial differences in education and entrepreneurship. $\delta_t^k$ accounts for time-varying shocks common to all areas. $\text{College Share}_{a,t}$ is the share of the workforce with a college diploma. $\text{New Colleges}_{a,t}$ is the cumulative number of colleges opened between 1989 and $t$.[45] Regressions are estimated in long differences between 1989 and 1999, firm creation and the college share are averaged over three-year windows, and results are weighted by the area's 1990 number of manufacturing firms. Standard errors are clustered at the urban-area level.

Two points are worth noting. First, this approach allows me to estimate the impact of education on firm creation even if new graduates are mobile, as long as the level of education increases more in treated areas. The impact of the new colleges on firm creation is now scaled by their effect on the local level of education. Second, identification requires that the effect of new colleges on firm entry operates solely through the increase in educational attainment. This exclusion restriction motivates the focus on manufacturing, where entry is unlikely to respond to local demand from students and university staff (see discussion in Section 5.2). Under this assumption, $\gamma$ measures the impact on firm creation of increasing the share of the workforce with a college diploma by one percentage point. Given how demanding this exclusion restriction is, I interpret $\gamma$ as a benchmark for the education–entry link.

[45] Alternatively, I use a dummy variable equal to one if area $a$ received at least one new college before $t$.

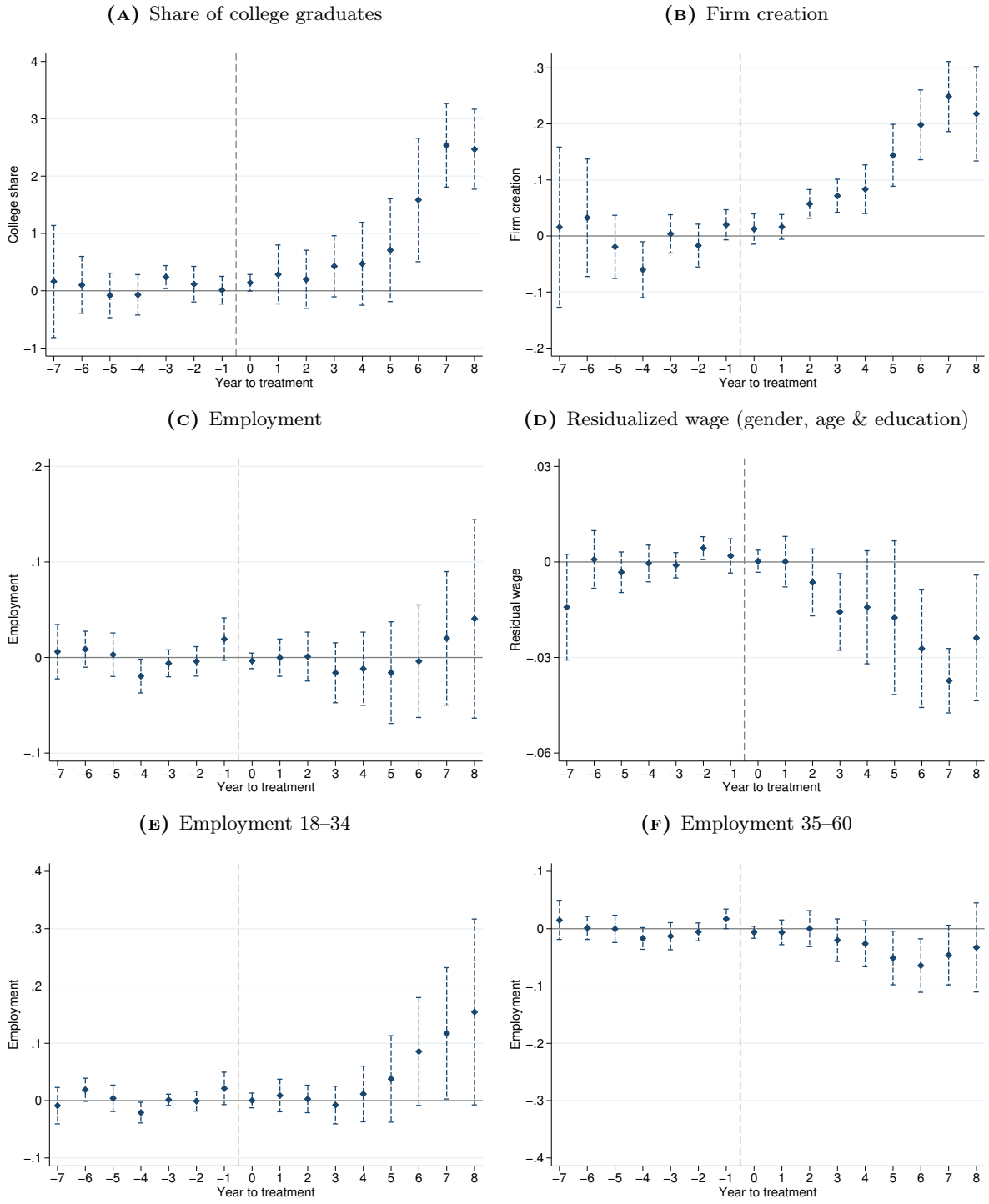
# E Robustness Analysis

**Figure E.1:** Main results excluding the Paris urban area



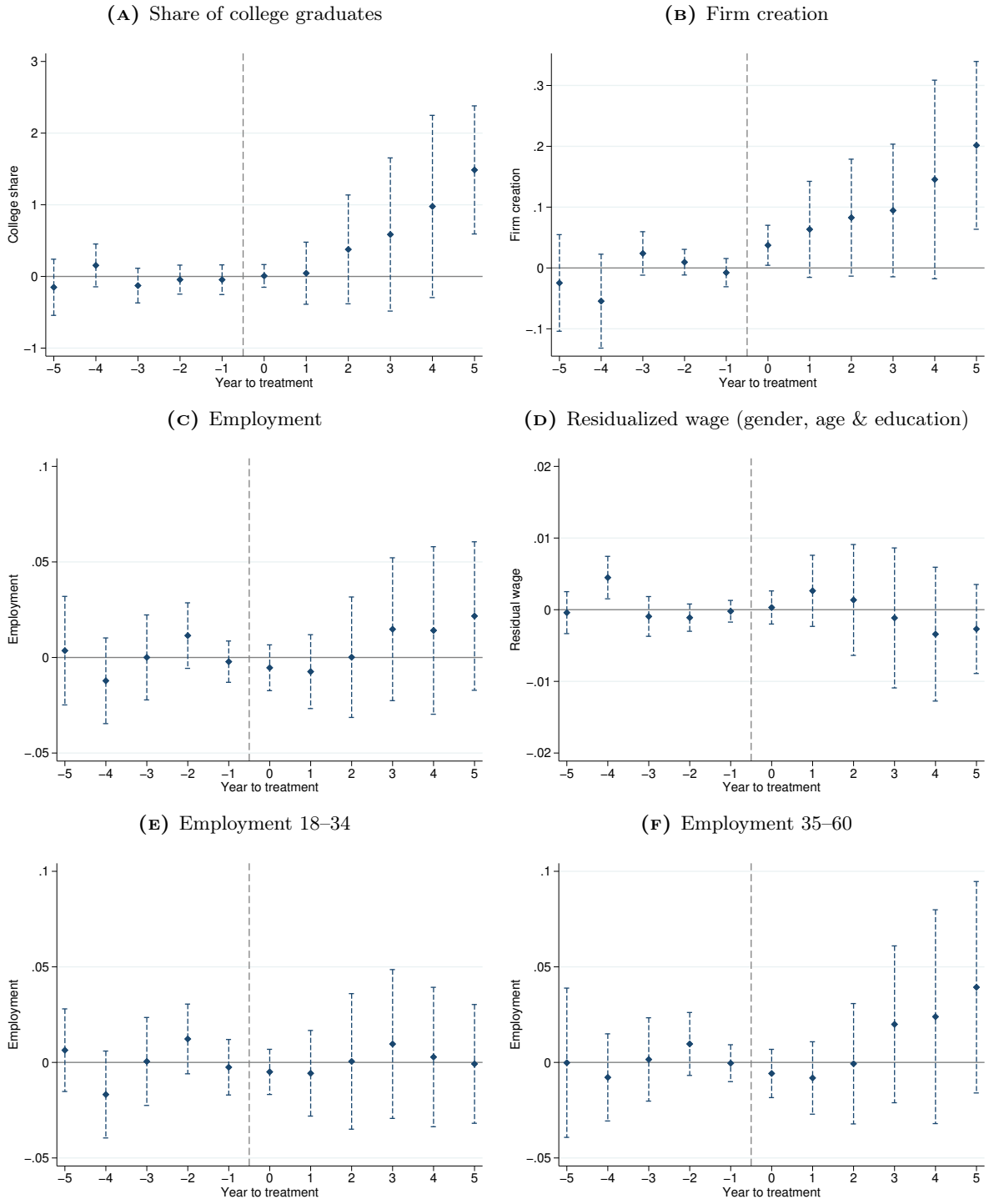

Notes: Figure E.1 reports event-study estimates for (a) the share of college graduates, (b) the number of newly incorporated firms, (c) total employment, (d) residualized wages for all workers, (e) employment of workers aged 18–34, and (f) employment of workers aged 35–60. The Paris urban area is excluded from the sample. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to an urban area × year. Estimates in panels (a) and (c)–(f) are weighted by the area's 1990 employment. Estimates in panel (b) are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban-area level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text

**Figure E.2:** Main results controlling for initial population



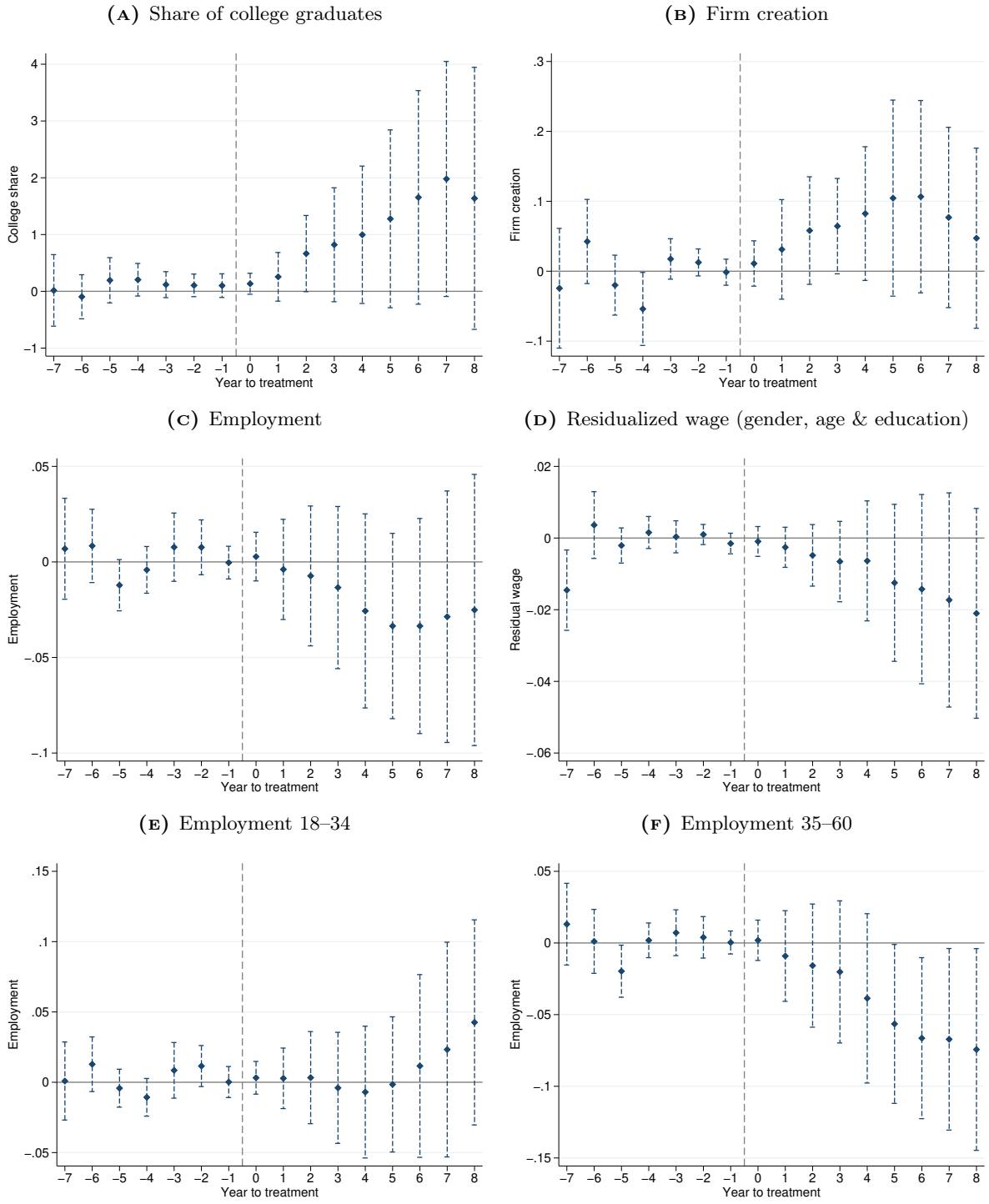

Notes: Figure E.2 reports event-study estimates for (a) the share of college graduates, (b) the number of newly incorporated firms, (c) total employment, (d) residualized wages for all workers, (e) employment of workers aged 18–34, and (f) employment of workers aged 35–60. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to an urban area × year. Estimates in panels (a) and (c)–(f) are weighted by the area's 1990 employment. Estimates in panel (b) are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban-area level. All regressions control for 1990 population using a stabilized IPW method (see Abadie (2005); Callaway and Sant'Anna (2021)). Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text

**Figure E.3:** Main results at the Province level



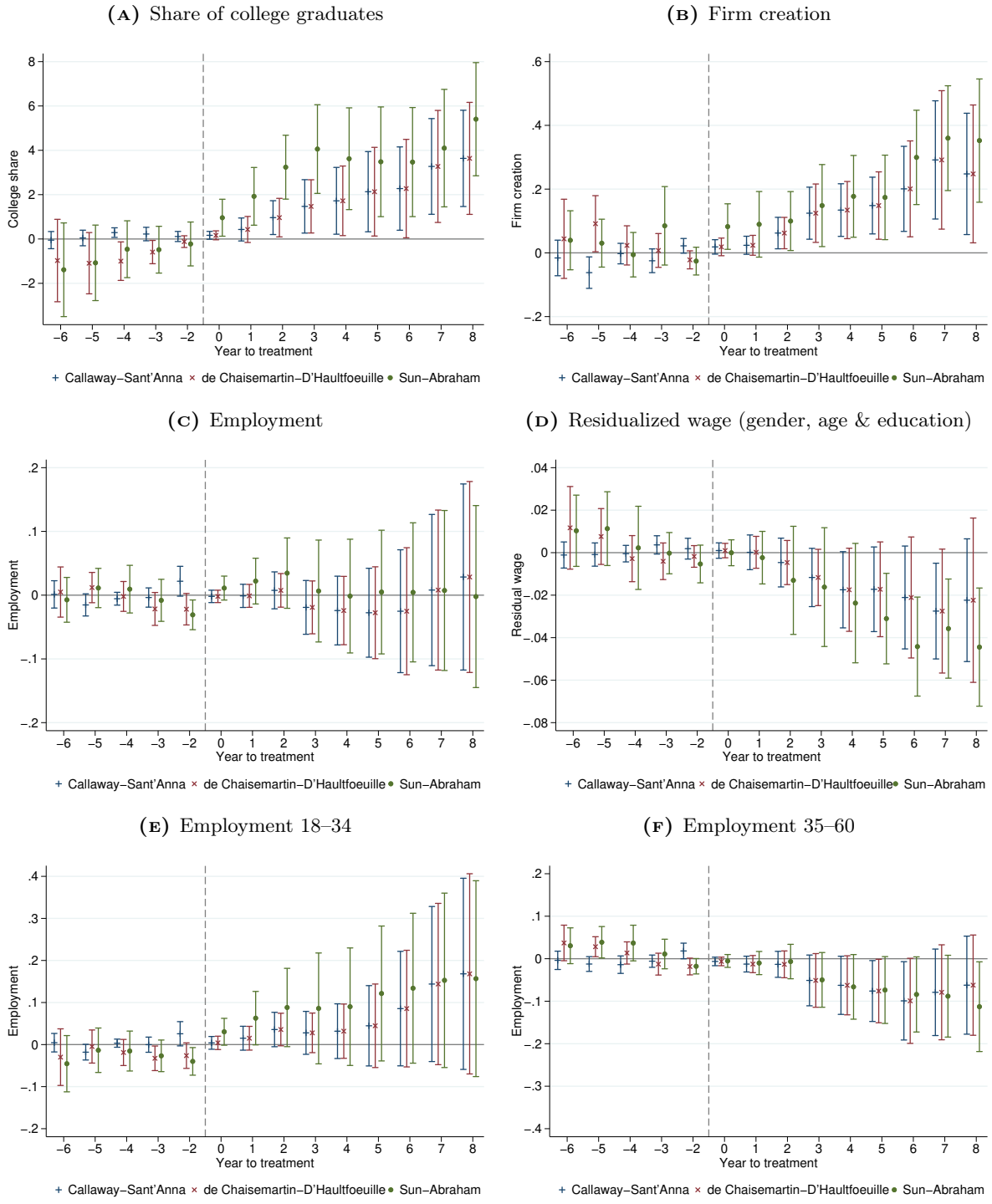

Notes: Figure E.3 reports event-study estimates for (a) the share of college graduates, (b) the number of newly incorporated firms, (c) total employment, (d) residualized wages for all workers, (e) employment of workers aged 18–34, and (f) employment of workers aged 35–60. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to a province × year. Estimates in Panels (a) and (c)–(f) are weighted by the province's 1990 employment. Estimates in Panel (b) are weighted by the province's 1990 number of firms. Standard errors are clustered at the province level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text

**Figure E.4:** Main results at the Commuting Zone level




Notes: Figure E.4 reports event-study estimates for (a) the share of college graduates, (b) the number of newly incorporated firms, (c) total employment, (d) residualized wages for all workers, (e) employment of workers aged 18–34, and (f) employment of workers aged 35–60. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to a commuting zone × year. Estimates in Panels (a) and (c)–(f) are weighted by the CZ's 1990 employment. Estimates in Panel (b) are weighted by the CZ's 1990 number of firms. Standard errors are clustered at the CZ level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text

**Figure E.5:** Main results using alternative estimation methods




Notes: Figure E.5 reports event-study estimates for (a) the share of college graduates, (b) the number of newly incorporated firms, (c) total employment, (d) residualized wages for all workers, (e) employment of workers aged 18–34, and (f) employment of workers aged 35–60. Each panel reports estimates obtained with three heterogeneity-robust estimators: de Chaisemartin and D'Haultfœuille (2020); Callaway and Sant'Anna (2021); Sun and Abraham (2021). Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). Event time is defined relative to the first year of treatment (i.e., the first increase in higher education capacity under the policy). Each observation corresponds to an urban area × year. Estimates in Panels (a) and (c)–(f) are weighted by the urban area's 1990 employment. Estimates in Panel (b) are weighted by the urban area's 1990 number of firms. Standard errors are clustered at the urban-area level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text

**Table E.1:** Alternative estimators

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | College share | Firm creation | Employment | Employment 18–34 | Employment 35–60 | Residualized wage |
| Callaway and Sant'Anna (2021) | 2.4342*** | 0.1579*** | 0.0070 | 0.0929 | -0.0480 | -0.0114 |
| | (0.7198) | (0.0521) | (0.0459) | (0.0718) | (0.0372) | (0.0073) |
| de Chaisemartin and D'Haultfœuille (2020) | 2.1984*** | 0.1492** | 0.0016 | 0.0818 | -0.0506 | -0.0127 |
| | (0.7196) | (0.0566) | (0.0399) | (0.0628) | (0.0352) | (0.0099) |
| Sun and Abraham (2021) | 3.3613*** | 0.1981*** | 0.0097 | 0.1025 | -0.0552* | -0.0234*** |
| | (0.8781) | (0.0506) | (0.0413) | (0.0693) | (0.0306) | (0.0087) |
| Borusyak, Jaravel, and Spiess (2024) | 3.0143*** | 0.1387*** | 0.0149 | 0.0944** | -0.0463** | 0.0110 |
| | (0.5392) | (0.0431) | (0.0277) | (0.0431) | (0.0235) | (0.0155) |

Notes: Table E.1 reproduces the main results using heterogeneity-robust estimators: Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), and Borusyak et al. (2024). Estimates are averaged over the post-treatment period. The outcomes are: (1) the share of college graduates, (2) the number of newly incorporated firms, (3) total employment, (4) employment of workers aged 18–34, (5) employment of workers aged 35–60, and (6) residualized wages for all workers. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). The unit of observation is the urban area × year. Estimates in columns (1) and (3)–(6) are weighted by the area's 1990 employment. Estimates in column (2) are weighted by the area's 1990 number of firms. Standard errors are clustered at the urban-area level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table E.2:** Spillovers on surrounding areas

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | College share | Firm creation | Employment | Employment higher-skill | Employment lower-skill | Residualized wage |
| After x Treated | 2.818*** | 0.0002 | 0.056** | 0.137*** | 0.030 | 0.008 |
| | (0.963) | (0.101) | (0.028) | (0.046) | (0.032) | (0.012) |
| Controls: | | | | | | |
| Area & Year FE | X | X | X | X | X | X |
| N | 8,880 | 8,325 | 8,880 | 8,880 | 8,880 | 8,880 |

Notes: Table E.2 reports the effects of college openings on outcomes in neighboring urban areas. The sample consists of areas whose centroid lies within 50 km of the centroid of an urban area receiving a new college, excluding areas that directly received a college. Treatment is defined as the first year a college opens within the 50 km radius. Coefficients are averaged across treatment periods following Callaway and Sant'Anna (2021), using not-yet-exposed areas as the control group. The outcomes are: (1) the share of college graduates, (2) the number of newly incorporated firms, (3) total employment, (4) higher-skilled employment, (5) lower-skilled employment, and (6) residualized wages for all workers. Daily wages are residualized on age and education dummies, separately by gender. Education is grouped into four categories: no diploma, vocational diploma, high school diploma, and university diploma (two years or more). The unit of observation is the urban area × year. Estimates in columns (1) and (3)–(6) are weighted by the urban area's 1990 employment. Estimates in column (2) are weighted by the urban area's 1990 number of firms. Standard errors are clustered at the urban-area level. Data: Linked employer-employee data matched with census data (DADS and EDP) and the administrative registry of firms (Sirene register). Go back to main text
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

# F Marginal Value of Public Funds

This appendix details the assumptions, data sources, and computations behind the Marginal Value of Public Funds (MVPF) analysis of Section 7.2. Following Hendren and Sprung-Keyser (2020), the MVPF is defined as the ratio of the beneficiaries' willingness to pay for the policy, $\mathcal{W}$, to its net cost for the government, $\mathcal{C}$. Denoting by $S_t$ the number of additional students enrolled in year $t$, $B_t$ the construction outlays, $c$ the annual cost of educating a student, $f$ the annual tuition fee, $w$ the gross annual earnings of a full-time minimum-wage worker, $\Delta Y_t$ the additional GDP generated by the policy, $\tau_w$ the tax rate on earnings, $\tau_Y$ the government's revenue share of GDP, and $r$ the discount rate, the net government cost is

$$\mathcal{C} \;=\; \sum_{t=1990}^{T} \frac{B_t \;+\; c\,S_t \;-\; f\,S_t \;+\; \tau_w\,w\,S_t \;-\; \tau_Y\,\Delta Y_t}{(1+r)^{\,t-1990}} \tag{C}$$

and the willingness to pay is

$$\mathcal{W} \;=\; \sum_{t=1990}^{T} \frac{(1-\tau_Y)\,\Delta Y_t \;-\; (1-\tau_w)\,w\,S_t \;-\; f\,S_t}{(1+r)^{\,t-1990}} \tag{W}$$

The MVPF is $\mathcal{W}/\mathcal{C}$. All flows are expressed in constant 2018 U.S. dollars and discounted back to 1990, the first year of the policy, at a baseline rate of 5%. Appendix Table F.1 summarizes the parameters and their sources.

**Government cost.** The construction program amounts to about \$10 billion, which I spread evenly over 1990–1999 ($B_t$ = \$1 billion per year for ten years); this budget includes buildings financed by the plan but delivered in 2000–2002 (Dupont, 2003). For the number of additional students $S_t$, I attribute to the policy the *entire* increase in national higher-education enrollment relative to 1989 (Figure 2, Panel (a)): 109,500 students in 1990, rising to 655,000 in 1995 and stabilizing around 675,000 from 2002 onward. This attribution is deliberately an upper bound on the cost side. Each additional student costs $c$ = \$10,837 per year, the average public expenditure per university student reported by the Ministry of Higher Education (Dalous and Jeljoul, 2014).[46] Students pay an annual registration fee of $f \approx$ \$175, which enters the government budget as a receipt. While studying, the additional students forgo taxable earnings: I conservatively assume that each of them would otherwise have worked full time at the gross minimum wage of $w =$ \$17,597 per year, and that the government loses $\tau_w = 8\%$ of these earnings in taxes.[47] Finally, the government recoups $\tau_Y = 25\%$ of the additional GDP in taxes, the ratio of taxes (net of all social insurance contributions) to GDP in France over the period.[48]

**Willingness to pay.** Beneficiaries' willingness to pay comprises three terms. First, they receive the additional GDP net of taxes, $(1-\tau_Y)\,\Delta Y_t$. Because value added comprises labor income and profits, this single statistic captures the earnings gains of workers and the profit gains of firm owners, and nets out the losses of incumbent firms documented in Table 7. Second, the additional students forgo net-of-tax earnings, $(1-\tau_w)\,w\,S_t$, which enter negatively. Third, they pay tuition fees, $f\,S_t$.

[46] This figure is consistent with observed aggregates: 655,000 additional students at \$10,837 each imply about \$7 billion per year, in line with the increase in the national higher-education budget between 1990 and 1995 documented in Appendix Figure A.1, Panels (c) and (d).

[47] At the minimum wage, the income tax is essentially zero; the 8% wedge corresponds to the CSG–CRDS and the non-benefit-linked share of employee health contributions over the period (Institute of Public Policies).

[48] Over 1990–2015, total compulsory levies averaged about 44% of GDP, of which social insurance contributions represented close to 19 points (INSEE, comptes nationaux; OECD, Revenue Statistics).

**Additional GDP.** The series $\Delta Y_t$ is recovered from the event-study estimates on the value added of local firms (Figure 8). The estimates are not statistically significant over the first three years of treatment, so I assume no effect on GDP, followed by positive growth averaging +4.8% over a nine-year period (see Table 8). I apply this path to the market-sector GDP of treated urban areas, obtained by allocating French 1989 GDP from the national accounts across areas in proportion to their share of national payroll, and aggregate across areas by calendar year, so that the staggered timing of the policy is reflected in the aggregate series.[49]

Three points are worth noting. First, the event-study estimates on log value added are weighted by initial urban-area size, so that the estimated path is representative of the average treated area. Second, the analysis of surrounding areas points to zero or positive spillovers (Appendix Table E.2), and the main effects survive aggregation to the province level (Appendix Figure E.3), indicating that local gains are not the result of displacement: the estimates thus capture genuinely additional growth and, if anything, understate it. Finally, to the extent that some students withdrew from local employment while studying, their forgone output is already netted out of the value-added estimates; separately subtracting forgone earnings in equation (W) therefore double-counts part of this cost, biasing the willingness to pay downward.[50]

**Table F.1:** Parameters and sources for MVPF computation

| Parameter | | Value | Source |
|---|---|---|---|
| Construction cost | $B_t$ | \$1 billion per year, 1990–1999 | Sénat report (Dupont, 2003) |
| Additional students | $S_t$ | 109.5k (1990) → ≈675k (2002) | MESR; Figure 2 |
| Cost per student | $c$ | \$10,837 per year | MESR–EESR (Dalous and Jeljoul, 2014) |
| Tuition fee | $f$ | \$175 per year | Légifrance (JORF 302, 12/29/1995) |
| Minimum wage (gross, full time) | $w$ | \$17,597 per year | INSEE |
| Tax rate on earnings | $\tau_w$ | 8% | Institute of Public Policies |
| Tax rate on GDP | $\tau_Y$ | 25% | INSEE; OECD |
| Value added effect | | 4.8% | Table 8 and Figure 8 |
| Discount rate | $r$ | 5% | Hendren and Sprung-Keyser (2020) |

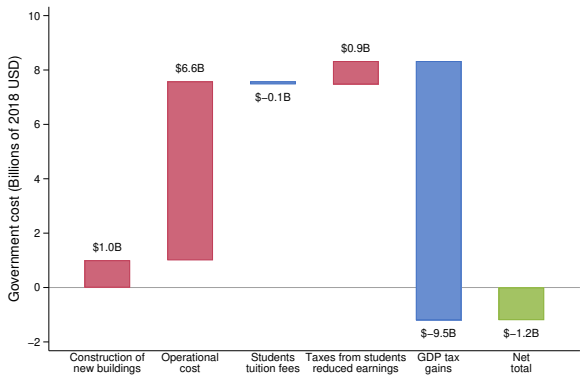
Notes: Table F.1 describes the parameters used in the MVPF computation and their sources. All monetary values are expressed in 2018 U.S. dollars.

[49] INSEE does not produce official GDP estimates at the urban-area level. I therefore start from 1989 GDP in the national accounts, subtract the 19.2% produced by non-market services (*services non marchands*: public administration, education, and health), and allocate the remainder across urban areas in proportion to their private-sector payroll shares. Payroll is observed at the establishment level, locating activity where it takes place even for multi-establishment firms. Excluding the non-market sector aligns the base more closely with the firm-level sample used to estimate the value-added effects: the growth path estimated on private firms is applied only to the part of GDP those firms represent.

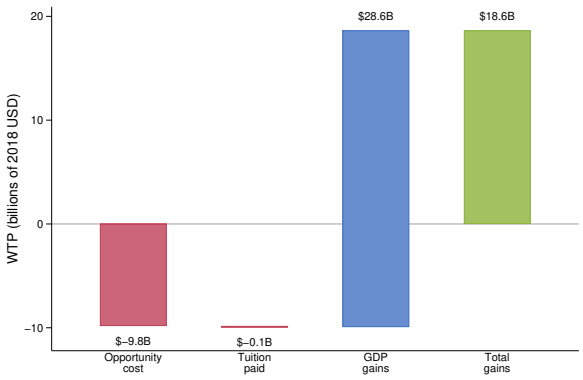
[50] Symmetrically, the forgone-tax term in equation (C) overlaps with $\tau_Y \Delta Y_t$, biasing the net cost upward. Both forces make the estimated MVPF conservative.

**Figure F.1:** Government cost and willingness to pay

**(a)** Government cost




**(b)** Willingness to pay




Notes: Figure F.1 decomposes the annual cost and willingness-to-pay components five years after the start of the policy. Panel (a) breaks the cost into five components: (i) the direct cost of building the new colleges; (ii) the operating cost associated with the additional students; (iii) the additional tuition fees paid by students; (iv) the forgone taxes on students' earnings; and (v) the taxes collected on the additional GDP generated. Panel (b) reports the components of the willingness-to-pay calculation: (i) the opportunity cost of studying (forgone earnings, net of taxes); (ii) the tuition fees paid; and (iii) the additional GDP, net of taxes. Go back to main text