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Tertiary Education Expansion and Regional Firm Development

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Tertiary Education Expansion and Regional Firm Development

Tobias Schlegel,[1] Curdin Pfister,[2] and Uschi Backes-Gellner[3]

January 2022

Abstract

This study investigates the impact of a tertiary education expansion on regional firm development, as measured by average profits per firm. We exploit the quasi-random establishment of universities of applied sciences (UASs)—bachelor's degree-granting three-year colleges teaching and conducting applied research—to construct treatment and control groups and to apply both a difference-in-differences model and an event study design. We find that, after the establishment of new UASs in Switzerland, average profits per firm in the treated municipalities increase by 19.6% more than in the control group. This increase corresponds roughly to an additional annual growth in average profits per firm in the treatment group of 0.7%. The effects start shortly after the establishment of UASs but also persist over a period of up to 10 years.

Keywords: Higher Education and Research Institution, Innovation, Regional Firm Development, Administrative Tax Data

JEL Classification: I23, I26, O18, O30

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Introduction

The theoretical literature on education economics focuses on a number of outputs of tertiary education institutions, e.g., knowledge creation through research and development (R&D), human capital creation, transfer of existing know-how or capital investments (Drucker & Goldstein, 2007; Florax, 1992; Goldstein & Renault, 2004; Shaw & Allison, 1999). These different outputs have the potential to impact the regional economy in a variety of ways. Indeed, empirical studies have repeatedly proved the existence of such impacts on the regional economy (for reviews, see Brekke 2020; Caniëls & van den Bosch 2011; Drucker & Goldstein 2007; Peer & Penker 2016). These impacts include—but are not limited to—consumption increases through employees and students of tertiary education institutions (Garrido-Yserte & Gallo-Rivera 2010), more business innovation (Andrews 2019; Cowan & Zinovyeva 2013; Toivanen & Väänänen 2016), new start-ups and spin-offs (Audretsch & Lehmann 2005; Baltzopoulos & Broström 2013; Fritsch & Aamoucke 2017), productivity gains (Andersson et al., 2004; Barra & Zotti, 2017; Hermannsson et al., 2017), and higher regional growth (Manca, 2012).

However, while many of these regional impacts of tertiary education institutions apply to firms, it remains unclear whether they also pay off in terms of regional firm development as measured by average profits per firm in a region. On one hand, the literature shows that innovation, as measured by patents, has a long-run positive effect on firm profits (Czarnitzki & Kraft, 2010; Ernst, 2001; Kaiser, 2009; Geroski et al., 1993) and firm growth (Grabowski & Mueller, 1978). Furthermore, economic impact assessments of tertiary education institutions show local benefits both through the direct and indirect spending of the tertiary education institution and their employees (Schubert & Kroll, 2016) and through local spillover benefits from the human capital created by the tertiary education institution (Bloom et al., 2013; Siegfried et al., 2007).

On the other hand, innovations require expensive investments that could—at least in the short run—lead to a slower development of innovative firms (Beck et al., 2016;

Nunes et al., 2012; Schilling, 2017). Furthermore, as innovations replace old products, negative effects of the innovation of a particular firm on the economic development of its rival firms are possible (Bloom et al., 2013; Chun et al., 2016). More generally, Moretti (2010) argues that general equilibrium effects, such as changes in wages and rents, affect the development of all firms in a region. Higher wages and rents caused by higher average levels of human capital (e.g., Rauch 1993) could thus hamper the development of firms that do not profit from positive impacts of tertiary education institutions. Therefore, whether and, if so, when and to what extent the impacts of a tertiary education institutions have overall positive effects on regional firm development remain open questions.

To answer these questions, we investigate a tertiary education expansion—formerly shown to yield positive impacts on the number of patents (Pfister et al., 2021) and R&D employment (Lehnert et al., 2020)—to analyze whether these impacts have the potential to positively influence regional firm development. The educational expansion we analyze took place in Switzerland in the form of the establishment of universities of applied sciences (UASs)—bachelor's degree-granting three-year colleges teaching and conducting applied research. The establishment of UASs is particularly useful for the purpose of our analysis for two reasons. First, as the establishment of UASs took place in a quasi-random manner (Pfister, 2017; Pfister et al., 2021), the timing and location was unpredictable for local firms and therefore did not influence firms' earlier development (more details—both qualitative and quantitative—on the process of the establishment of UASs and the impact on the regional economy are discussed in Appendix A).

Second, by exploiting the quasi-random establishment, previous studies show positive impacts of the establishment of UASs in the form of increases in innovation, as measured by patents (Pfister et al., 2021) and increases in R&D employment shares (Lehnert et al., 2020). We apply a similar methodology as these studies to identify causal effects of the establishment of UASs. However, we build on—and further develop—their findings by studying a novel outcome variable, i.e., average profits per firm, and by answering the question whether such innovation effects as found in earlier studies translate into increased average profits per firm at the regional level. We, thereby contribute to both the literature on education and regional economics and to the evaluation of a particular policy by analyzing whether the many forms in which a tertiary education expansion can impact firms, pay off in terms of regional firm development as measured by average profits per firm.

To conduct our analysis, we combine two data sources. Our first data set is data on the timing and location of the establishment of UASs in the late 1990s in Switzerland. We, thereby use the dataset on the establishment of UASs from Pfister et al. (2021), augmented by Schlegel et al. (2021), to assign municipalities to treatment and control groups.

For our second data set, we obtained access to Swiss corporate income tax data at the municipality level provided by the Swiss Federal Tax Administration (SFTA) from 1973 through 2016 (latest year available). The data contains information on aggregate firm profits (i.e., the total reported taxable firm profits) and the number of taxable firms at the municipality level, thereby allowing us to calculate the average profits per firm in a municipality, our measure for regional firm development. This data is particularly adequate to analyze effects of UASs on regional firm development because of two reasons. First, the tax data represents an administrative data source, and therefore, is especially reliable because it comprises the whole universe rather than a sub-sample of all taxable firms in Switzerland.

Second, by aggregating data at the municipality level, which constitute the smallest political entities in Switzerland, we can investigate the effects of UASs on regional firm development at a very fine-grained geographical level. This fine-grained geographical level is important, because effects of tertiary education institutions are found to be geographically localized (e.g., Abramovsky & Simpson 2011; Drucker 2016; Rodríguez-Pose & Crescenzi 2008). The tax data allows also a much more fine-grained geographical analysis than alternative outcome measures such as productivity or value added that are only available at the supra-regional level (i.e., the cantonal level) where, on average, around 83 municipalities are aggregated. By combining the tax data set and the dataset on the establishment of UASs, we assess the effects of the establishment of UASs on regional firm development.

In our empirical analysis, we use in a first step a two-way fixed effects difference-in-differences (DiD) approach to exploit the quasi-random variation in the timing and location of the establishment of UASs. To study dynamic treatment effects, i.e., to learn how the impact of the establishment of UASs developed over time, we apply in a second step an event study design. Further analyses shed light on the question whether the detected innovation effects by Pfister et al. (2021) moderate effects on regional firm development. A number of robustness tests, furthermore show that our results are not driven by the characteristics of the municipalities that happen to be in the treatment and control groups.

The results of our DiD regression show that average profits per firm in treated municipalities increase on average by 19.6% more than in non-treated municipalities. Analyzing the dynamic treatment effects, we find that average profits per firm increase immediately after the establishment of UASs and grow further before they stabilize in the long-run. Our further analyses reveal that municipalities that profit from UASs in terms of higher innovation also show the biggest increases in average profits per firm, providing empirical evidence that this might be one channel through which the establishment of UASs impacts regional firm development.

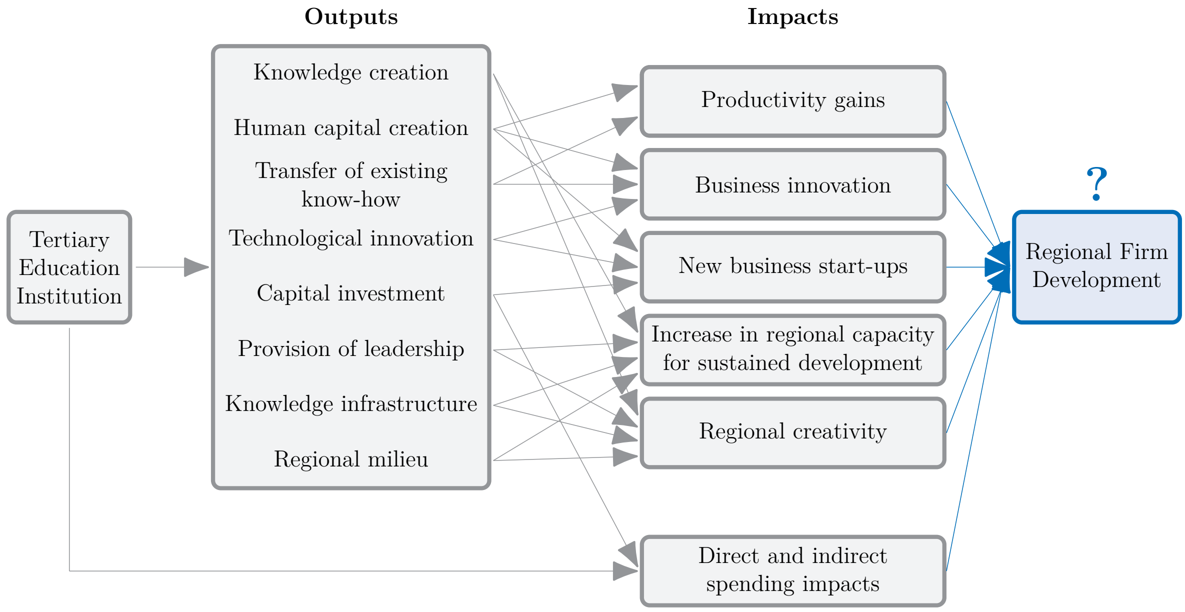
The remainder of the study is organized as follows. In the next section, we survey the literature. The third section presents the data. The fourth section discusses the empirical strategy. The fifth section presents the results, further analyses, and robustness checks. The sixth section summarizes and concludes.

Literature

The outputs of tertiary education institutions and, thereby their impacts on regional economies go beyond the outputs of the core functions of tertiary education institutions, i.e., to create human capital and knowledge by teaching, conducting and sharing R&D (Drucker & Goldstein 2007; Florax 1992; Goldstein & Renault 2004). The left-hand side of Figure 1 summarizes the different outputs frequently discussed in the theoretical literature on education and regional economics. These literature emphasize that beyond their core functions, tertiary education institutions transfer existing knowledge, e.g., by co-operating with firms (Arvanitis et al., 2006; Audretsch & Feldman, 1996) and contribute to a regional knowledge infrastructure, e.g., by providing laboratories Abel & Deitz 2012; Hussler & Ronde 2005; Varga 2000. In addition, tertiary education institutions create employment and make investments (Siegfried et al., 2007). All these outputs have the potential to impact the individuals and the economy of the region with a tertiary education institution in many different ways, as shown on the right-hand side of Figure 1 (Drucker & Goldstein, 2007; Florax, 1992; Schubert & Kroll, 2016).

Many impacts of tertiary education institutions are extensively studied in the literature on education and regional economics. Improved human capital accelerates productivity in the local labor market (Andersson et al., 2004; Barra & Zotti, 2017; Manca, 2012; Glaeser et al., 2014), and existing knowledge spills over to local firms (Valero & Van Reenen, 2019). Other impacts, detected by the empirical literature to be linked to the proximity to tertiary education institutions, are increases in patents (Jaffe, 1989; Toivanen & Väänänen, 2016; Valero & Van Reenen, 2019)—with effect sizes between 7%

(Cowan & Zinovyeva, 2013) and 32% (Andrews, 2019) in higher patenting activities—increased R&D (Abramovsky & Simpson, 2011), and more start-ups (Audretsch & Lehmann, 2005; Bathelt et al., 2011; Fritsch & Aamoucke, 2017). More generally, a tertiary education institution can help to develop a regional knowledge infrastructure that eventually leads to agglomeration economies (Abel & Deitz 2012; Hussler & Ronde 2005; Varga 2000).



Source: Authors' Figure, based on Goldstein et al. (1995) and Goldstein & Renault (2004)

Figure 1. Outputs and expected regional economic impacts of tertiary education institutions

The occurrence of these impacts of tertiary education institutions have the potential to create further effects on the entire regional economy (Drucker & Goldstein, 2007; Florax & Folmer, 1992). The classical examples of such effects are an acceleration of GDP growth or a decrease in unemployment due to changes in the regional economy induced by a tertiary education institution (Barra & Zotti, 2017; Hermannsson et al., 2017; Schubert & Kroll, 2016). A number of empirical studies, therefore, find a positive impact on regional GDP per capita growth rates of increased human capital (measured by the regional share of individuals with tertiary education) in general (Abel & Gabe, 2011; Barra & Zotti, 2017; Chatterji, 1998; Cuaresma et al., 2014) and of tertiary education institutions in particular (Chatterji, 1998; Pastor et al., 2018).

However, less is known on how the impacts of tertiary education institutions influence the economic development of firms in a region (hereinafter, we refer to these effects as "regional firm development"). Therefore, in this study we ask and answer the question of whether the impacts of a tertiary education expansion have an overall effect on regional firm development as measured by the average profits per firm in a region nearby a newly established tertiary education institution in comparison to regional firm development in regions without a newly established tertiary education institution. As tertiary education institutions can affect average profits per firm both positively or negatively, it is empirically not per se clear whether the impacts of a tertiary education expansion translate into a positive or negative effect on regional firm development and how large these total effects might be. The question regarding the direction of the overall effects of tertiary education institutions on regional average profits per firm thus remains an empirical question that we tackle in this paper.

On one hand, three empirical arguments for different *positive* effects on regional average profits per firm exist. First, empirical analyses on the link between patenting and firm performance show that innovations increase firm profits and positively impact firms' performance (as measured by sales), respectively (Czarnitzki & Kraft, 2010; Ernst, 2001; Geroski et al., 1993). Analyzing Danish firms, Kaiser (2009) finds that a one unit increase in the number of patents increases profit rates (i.e., profits/sales) two years later by 6.9%. Kaiser (2009) furthermore points to the facts that firms need time for the transfer of patents to profitable products. Thus also profits from innovation effects of a tertiary education institution need time to evolve.

Second, positive innovation effects of a tertiary education institution on one firm can lead to technology spillovers to other firms both within and across industries (Hall et al. 2010; Keller 2004; Jones 2005). Analyzing such spillovers, Bloom et al. (2013) find that the social returns to R&D (i.e., the aggregate output increase after one firm marginally increased its R&D) are twice as high as private returns to R&D (i.e., the increase in the output of the firm that increased its R&D).

Third, and more generally, Moretti (2010) argues that increases in the demand for local goods and services—an effect that is also observed in the context of tertiary education institutions (Siegfried et al., 2007)—can positively influence regional economic development through regional multiplier effects. Such multiplier effects, however, not only apply to demand-side effects but also indirectly to supply-side effects (when, e.g., leading to more innovation).

On the other hand, impacts of tertiary education institutions—particularly impacts on innovation—can have negative effects on regional average firm profits, for the following three reasons. First, innovation is costly (Beck et al. 2016; Nunes et al. 2012; Schilling 2017)—in particular when the innovation stems from a co-invention or an acquisition of a patent of a tertiary education institution (Cerulli et al., 2021). Therefore, positive innovation effects linked to tertiary education institutions can (in the short run) lead to a reduction of regional firm development. Kaiser (2009), for instance, finds a short-run effect of a 4.92% decrease of a firm's profitability when the number of patents increased in the previous year.

Second, innovation provokes "product market rivalry effects" (Bloom et al., 2013), potentially also for innovation spillovers from tertiary education institutions. Innovative products by some firms can, thereby slow down the development of "technology losers" in a region (Chun et al., 2016). However, evidence for the existence of product market rivalry effects is mixed. Bloom et al. (2013), for instance find that positive effects from technology spillovers outweigh product market rivalry effects. In turn, Hanel & St-Pierre (2002) estimate negative effects of R&D activities by competitors on the profitability of a firm, even if these activities lead to quasi-public knowledge. Furthermore, while direct market rivalry effects on private firms through R&D conducted by academic universities are limited because they compete only in very few markets (Koch & Simmler, 2020), such negative effects are potentially more pronounced in the case of UASs that (i) conduct more applied R&D (ii) cooperate more often with private firms and (iii) also provide more consulting activities than academic universities (Arvanitis et al., 2008).

Third, tertiary education institutions can also initiate general equilibrium effects such as changes in wages and housing prices (Rauch, 1993) that affect the development of all firms in a region (Glaeser et al., 2001). This applies especially to firms in the tradeable goods sector, where increasing regional prices lower the competitiveness on supra-regional markets.

In sum, whether the impact of a tertiary education expansion on factors like innovation, R&D and regional demand have an overall positive effect on the aggregated regional firm development in regions with newly established tertiary education institutions remains unclear. Answering this question and estimating the size of such an effect thus remains primarily an empirical question. The goal of our paper is to close this research gap by a thorough causal analysis of the effects of newly established UASs in Switzerland.

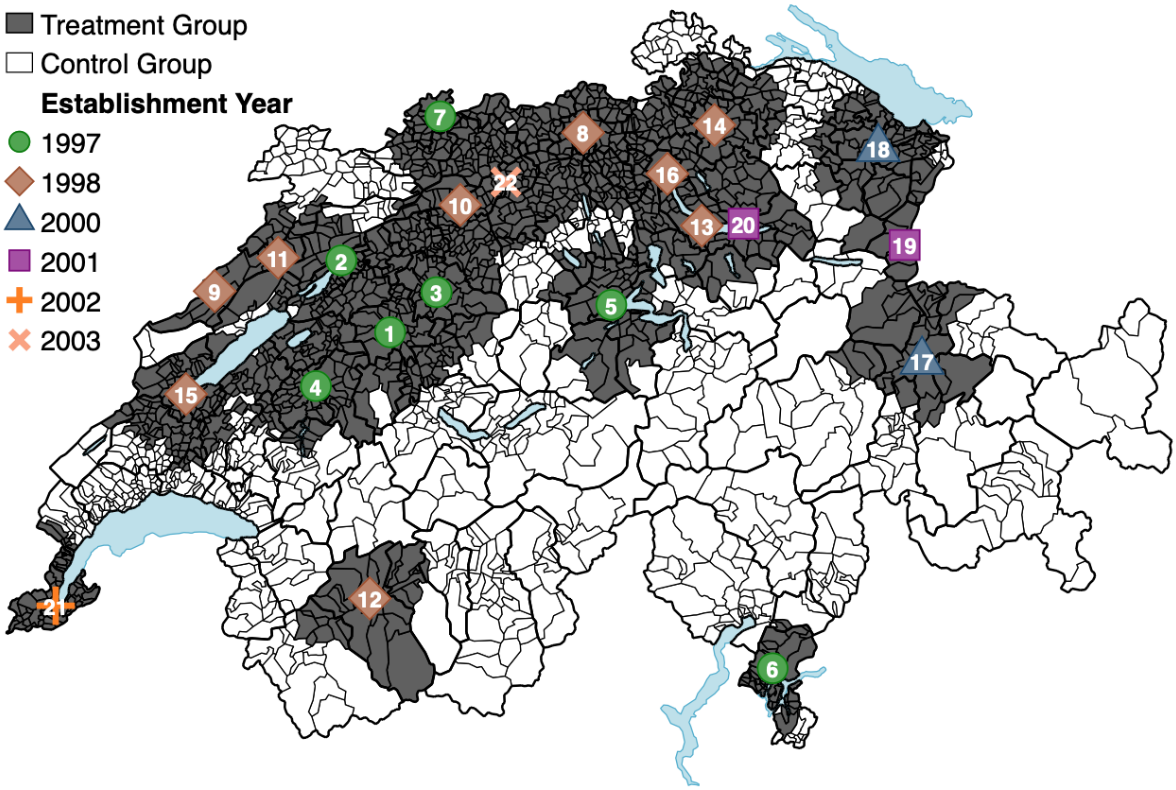
Variables, Data and Descriptive Statistics

To investigate our research question, we merge two datasets: first, data on the timing and location of the establishment of UASs and, second, data on regional firm development as measured by average profits per municipality.

Our first dataset, the data on the establishment of UASs, contains detailed information on the timing and the location of the process of the establishment of UASs from 1997 (the first establishment year) through 2011. This data—collected by Pfister et al. (2021) and Schlegel et al. (2021)—contains information on the establishment of all campuses where STEM fields are taught.[1] The establishment data allows us to form treatment and control groups. We assign municipalities to the treatment group when they are within a 25-km radius from a STEM-teaching UAS campus. In so doing, we follow Pfister et al. (2021), who choose the 25-km radius, because it corresponds to the maximum commuting distance of more than 90% of Swiss workers. Figure 2 depicts the UAS campuses and the resulting treatment and control groups formed from the 2,222 Swiss municipalities.

Our second data set, the data on regional firm development, contains information on aggregate firm profits subject to taxes at the municipality level and is provided by the Swiss Federal Tax Administration (SFTA).[2] We use the data to construct our dependent variable, the average profits per firm, and a number of control variables at the municipality level. The data is available for tax periods 1971 through 2016 (latest year available). For 1971 through 1995, tax data is available for two-year periods, as taxes were assessed only every second year. Therefore, the data represents values averaged over two years. We simply assign the data reported to the first year of this two-year period, e.g., the data for 1971/1972 is assigned to 1971. After 1995, tax data is available yearly. Furthermore, data for some years (1987, 1993 and 1995-1997) is missing for *all* municipalities in the dataset, due to a lack of data quality (SFTA, 2016).

To construct our dependent variable, the average profits per firm, we use the aggregate firm profits subject to taxes at the municipality level and divide it by the number of taxable firms. The calculation of firm profits subject to federal taxes is standardized across Switzerland, making our dependent variable comparable across Swiss municipalities (for a discussion on how the SFTA calculates firm profits subject to taxes, see Appendix B.1). Descriptive statistics for our dependent variable and the data used to construct



Source: Authors' Figure based on Pfister et al.'s (2021) and Schlegel et al.'s (2021) data. *Notes*: The Figure shows all 22 UAS campuses with STEM fields, established between 1997 and 2003. The different establishment years are indicated by differently shaped markers. The 2,222 municipalities are assigned to the treatment group (dark color) if they are within a 25-km radius from a newly established UAS and otherwise to the control group (bright color). The numbers indicate the exact location of each UAS campus in STEM fields: (1) Bern (closed in 2003), (2) Biel/Bienne, (3) Burgdorf, (4) Fribourg, (5) Horw, (6) Manno, (7) Muttenz, (8) Brugg, (9) Le Locle, (10) Oensingen (closed in 2003), (11) Saint-Imier, (12) Sion, (13) Wädenswil, (14) Winterthur, (15) Yverdon-les-Bains, (16) Zürich, (17) Chur, (18) St. Gallen, (19) Buchs (SG), (20) Rapperswil-Jona, (21) Genève, and (22) Olten (closed in 2005).

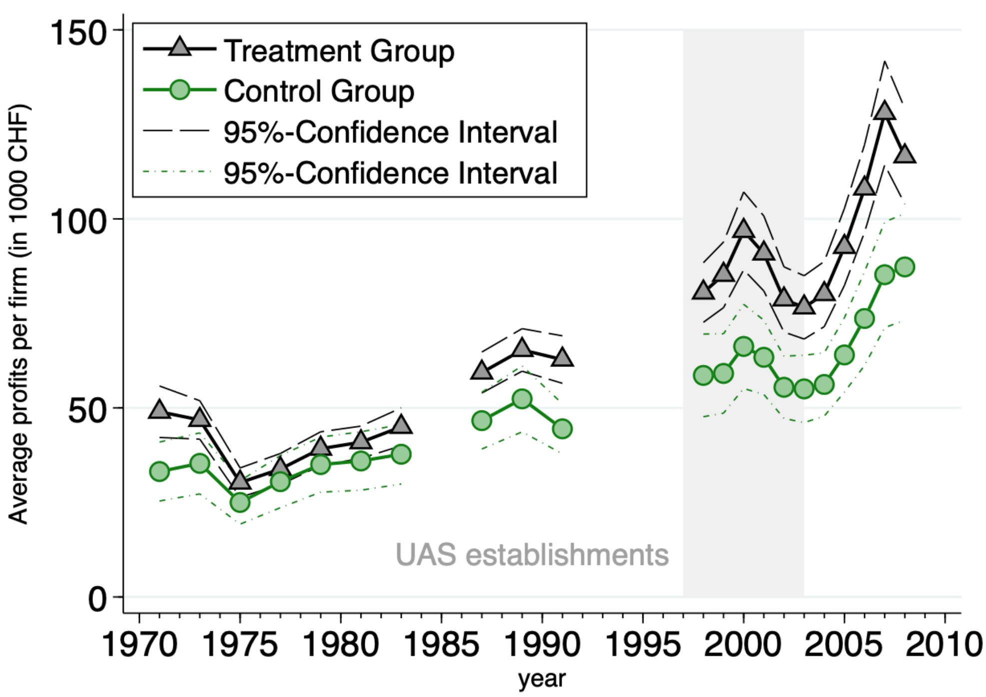
Figure 2. UAS campuses in STEM fields, establishment years and assignment to treatment and control groups

it appear in Table B4 in Appendix B.1. Furthermore, Figure 3 shows the evolution of our dependent variable for treatment (triangle) and control (circle) groups. The Figure gives descriptive evidence that average profits per firm start to increase stronger in the treatment group after the establishment of UASs, as compared to the control group. At the same time, average profits per firm evolve similar in treatment and control groups before the establishment of UASs.

The tax data provides two more municipality-level variables, which allow us to control for potential regional differences in reported average profits per firm (i.e., regional differences linked to the complex Swiss taxation system). Depending on the legal form of the firm (e.g., corporations or limited liability companies), the profits subject to taxation can differ (for details, see Appendix B.1). We therefore control for the "average corporate tax rate" in percentages and the "participation exemption" in CHF. Both variables contain information on the mixture of legal types of firms and thus partly control for different municipality requirements for firm profit taxation.

Furthermore, we control for tax reforms at the cantonal level. We do so, since Galletta & Redonda (2017) show that cantonal flat tax reforms on corporate profit taxes have negative impacts on businesses' investment decisions. This impact, in turn, is likely to also influence average profits per firm. Therefore, it is crucial to control for this potential impact on average profits per firm through cantonal tax reforms. In Appendix B.3, we discuss our strategy to control for cantonal tax reforms in detail.

Combining the data on the establishment of UASs with the data on regional firm development, we study the period 1971 through 2008. While the starting point is determined by data availability, we limit our analysis of this tertiary education expansion to 2008 to avoid biases potentially induced by a later educational expansion, the introduction of master's degree programs (Pfister et al., 2021). In total, we have 21 periods in which data is available between 1971 and 2008.



Notes: The Figure shows the evolution of average profits per firm for treatment (triangle) and control (circle) groups. Where no lines are indicated, data on average profits per firm is missing for *all* municipalities in the corresponding year. Non-solid lines indicate 95% confidence intervals. The shaded area indicates the years during which new UASs were established. The Figure reveals that differences in average profits per firm are not statistically significant between treatment and control groups before the establishment of UASs. However, they do become so after the establishment of UASs.

Figure 3. Development of average profits per firm in treatment and control groups

Empirical Strategy

To examine whether the establishment of UASs has an effect on regional firm development, we apply in a first step a generalized two-way fixed effects DiD approach (i.e., time and unit fixed effects). We assume a treatment lag of three years, because all of the potential impacts through which the UASs could influence regional firm development—positively or negatively—need time to evolve. In so doing, we follow Pfister et al. (2021). They argue that the establishment of UASs affects regional human capital as one potential source of innovations impacts with a lag of three years—the typical time needed for students to finish a bachelor's program at a UAS.

The main specification of our DiD regression Equation has thus the following form with average profits per firm in municipality i, canton k and year t as our dependent variable, i.e., y_{ikt}:

$$y_{ikt} = \delta \text{Treat}_i * \text{Post}_{i(t-3)} + \boldsymbol{x}'_{it}\boldsymbol{\beta} + \alpha_i + \lambda_{kt} + \varepsilon_{it}, \tag{1}$$

where $Treat_i$ is a dummy variable that indicates whether a municipality i is in the treatment group and $Post_{i(t-3)}$ is a dummy variable that indicates whether municipality i is actually treated in year t, i.e., a nearby UAS was established at least three years ago. The main interest of the regression analysis applies to the coefficient δ that represents the lagged effect of the establishment of a UAS on the average profits per firm, i.e., the treatment effect. Furthermore, $\boldsymbol{x}_{it}$ is a vector that contains the two control variables (1) average federal corporate tax rate and (2) the participation exemption. We include municipality fixed effects (α_i) to control for time-invariant municipality-specific characteristics. Furthermore, we also include canton-specific time trends (λ_{kt}) and, thereby follow earlier studies analyzing the Swiss tax system (e.g., Galletta & Redonda 2017). Including canton-specific time trends accounts for policy changes with respect to cantonal tax laws implemented over time that might also affect average firm profits.

In a second step, we extend the two-way fixed effects DiD by exploiting the staggered setting of the establishment of UASs, and adopt an event study design. This exercise provides us, on one hand, with information on the pre-treatment trends that should run parallel for both treatment and control groups. This is also the key identifying assumption for our two-way fixed effects DiD analysis in Equation (1). The event study thus provides us with "a test for causality" (Angrist & Pischke 2009, p. 237). Thereby, the event study is particularly appropriate to analyze pre-treatment trends because it allows for the staggered manner of the establishment of UASs and thus depicts pre-treatment trends for all establishment years from 1997 through 2003.

On the other hand, the event study allows us to examine year-by-year treatment effects to shed more light on the question of how much time is needed from the establishment of a UAS to the impact on regional firm development. Following the notation used by Schmidheiny & Siegloch (2020), our event study specification is given by the following Equation (2), again with average profits per firm in municipality i, canton k and year t as our dependent variable, i.e., y_{ikt}:

$$y_{ikt} = \sum\nolimits_{j=\underline{j}}^{\bar{j}} \delta_j b_{it}^j + \boldsymbol{x}'_{it}\boldsymbol{\beta} + \tau D_{kt} + \alpha_i + \lambda_t + \varepsilon_{it}, \text{ with} \tag{2}$$

$$b_{it}^j = \begin{cases} \sum\nolimits_{s=\underline{t}-\bar{e}}^{\underline{j}} d_{i,t-s} & \text{if} \quad j = \underline{j} \\ d_{i,t-j} & \text{if} \quad \underline{j} < j < \bar{j} \\ \sum\nolimits_{s=\bar{j}}^{\bar{t}-\underline{e}} d_{i,t-s} & \text{if} \quad j = \bar{j}, \end{cases}$$

where $\underline{j} = -10$ and $\bar{j} = 10$ define the restricted event window, $\underline{t} = 1971$ and $\bar{t} = 2008$ the observation window and $\underline{e} = 1997$ and $\bar{e} = 2003$ the years of the first and last events (i.e., the establishment of UASs), respectively.

The coefficient b_{it}^j in Equation (2) is thus a binned event indicator, since all observations outside the event window are binned in the maximum leads (i.e., $b_{it}^{\bar{j}}$) and lags (i.e., $b_{it}^{\underline{j}}$), respectively. Within the event window, however, $b_{it}^j = d_{i,t-j}$ is the event indicator that switches to 1 for the year of the treatment and is zero otherwise. Therefore, the coefficients of interest are the δ_j that represent the dynamic treatment effects for the j periods before ($j < 0$) or after ($j \geq 0$) the event, relative to our reference period δ_{-1} (Schmidheiny & Siegloch, 2020). The estimation includes municipality fixed effects (α_i), year fixed effects (λ_t), and the same set of control variables x_{it} as in Equation (1). Furthermore, we include a dummy variable indicating all years following a cantonal flat tax reform (see Appendix B.3). We, instead, refrain from controlling for cantonal tax reforms using canton-specific time trends—as we do in our two-way fixed effects DiD—to avoid under-identification when including the additional period specific dummies in the event study.

Regarding the implementation of the event study, our dataset shows two complications as to a standard event study design. First, data for 1971 through 1991 is based on two-year averages, while data after 1997 is based on yearly averages. For the years with two-year averages, we assign the averaged value of the outcome variable and the two control variables to both years. Thereby, we can ensure that the event indicator $d_{i,t-j}$ is identified for each lag.[3] Second, we face missing observations in the dependent variable for 1992 through 1997 for *all* municipalities. Combined with the fact that most UASs were established in 1997 or 1998, however, this means that the event indicators for the last four pre-treatment periods, i.e., $j \in [-4, -1]$, are based on relatively few observations, i.e., municipalities that received the treatment between 2000 and 2003. The post-treatment periods, however, are identified based on the full sample, since after 1997, we have yearly and non-missing data.

To base our event indicators on sufficiently many observations we furthermore restrict the event window.[4] Since treatment is staggered over 1997 through 2003, we have a smaller amount of observations at both ends of the observation window (1971 through 2008). We restrict the event window to ten pre-treatment periods since missing data in 1985 (for *all* municipalities) again lead to a low amount of observations in those pre-treatment periods. Anticipation effects prior to 1985 are also unlikely to affect our estimation. The restriction to ten post-treatment periods is driven by the fact that for the last year of the observation window, i.e., 2008, we would also have only relatively few observations, i.e., only those municipalities that were treated in 1997 (first establishment of UASs). We thus summarize pre-treatment and post-treatment periods outside the event window in bins at the beginning (i.e., $b_{it}^{\underline{j}}$) and end ($b_{it}^{\bar{j}}$) of the event window.

We estimate Equations (1) and (2) using a Poisson pseudo-maximum likelihood (PPML) estimator.[5] Thereby, we tackle two problems raised by Santos Silva & Tenreyro (2006) that standard OLS regression would entail: First, using a log-linearization of the empirical model in the presence of heteroskedasticity leads to inconsistent OLS estimates of the semi-elasticity. Second, the log-linearized OLS model is not defined for observations with zero values in the dependent variable. As our dataset has both of these characteristics, we use a PPML estimator as suggested by Santos Silva & Tenreyro (2006).[6]

Results

Difference-in-Differences and Event Study

To assess the impact of the establishment of UASs on average profits per firm, we report, in a first step, the results from our two-way fixed effects DiD analysis in Table 1 column (5), estimated using PPML. Moving from left to right in Table 1, we append the base model without any controls in column (1) step-wise, with control variables, i.e., the average corporate tax rate and the participation exemption in column (2), with canton-specific time trends in column (3), with labor market region fixed effects in column (4), and, finally, with municipality fixed effects in our preferred DiD specification in column (5). The coefficient for $Treat_i$ shows the level difference between treatment and control groups, which is insignificant throughout all model specifications (and omitted with municipality fixed effects). The coefficient $Treat_i * Post_{i,t-3}$ shows our coefficient of interest, i.e., the treatment effect of a newly established UAS, which is significant throughout all model specifications.

Including control variables and year fixed effects lowers the treatment effect from 0.5420 in column (1) to 0.2337 in column (3) by accounting for, first, the higher average profits per firm in municipalities with higher corporate tax rates and a larger participation exemption (see Appendix B.1) and, second, the strong time trend in average profits per firm (see Figure 3). While the inclusion of regional labor market fixed effects in column (4) leaves the estimated treatment effect unchanged (as compared to column 3), when including municipality fixed effects in column (5), the treatment effect decreases to 0.1790, controlling away time-invariant unobservable municipality characteristics that impact average profits per firm.

Table 1: Regression results with average profits per firm as the dependent variable

Dependent Variable: Average profits per firm	(1)	(2)	(3)	(4)	(5)
$\text{Treat}_i * \text{Post}_{i,t-3}$	0.5420***	0.4059***	0.2337***	0.2366***	0.1790***
	(0.0391)	(0.0498)	(0.0727)	(0.0688)	(0.0622)
Treat_i	0.0765	0.1085	0.0164	−0.0636	
	(0.0734)	(0.0737)	(0.0807)	(0.1114)	
Average corporate tax rate	No	Yes	Yes	Yes	Yes
Participation exemption	No	Yes	Yes	Yes	Yes
Canton-specific time trends	No	No	Yes	Yes	Yes
Regional labor market FE	No	No	No	Yes	No
Municipality FE	No	No	No	No	Yes
Observations	45,066	45,066	45,066	45,066	45,066
(Pseudo) R^2	0.033	0.058	0.157	0.235	0.758

Notes: Results report coefficients from PPML regressions. Dependent variable: average profits per firm. Robust standard errors are clustered at the municipality level. When municipality fixed effects are included (column 5), 68 municipalities are dropped from the estimation, because they are separated by a fixed effect (Correia et al., 2019). Levels of significance: * p<0.10, ** p<0.05, *** p<0.01.

Thus looking at our preferred model in column (5), that contains the most extensive set of control variables, we find that the increase in average profits per firm is, on average 19.6% (0.1790) higher for municipalities in the treatment group after the establishment of a nearby UAS as compared to a municipality in the control group. This positive effect of the establishment of UASs on average profits per firm thus shows that, overall, the impacts on the regional economy due to the establishment of UASs also translate into significant and substantial effects on average firm profits.

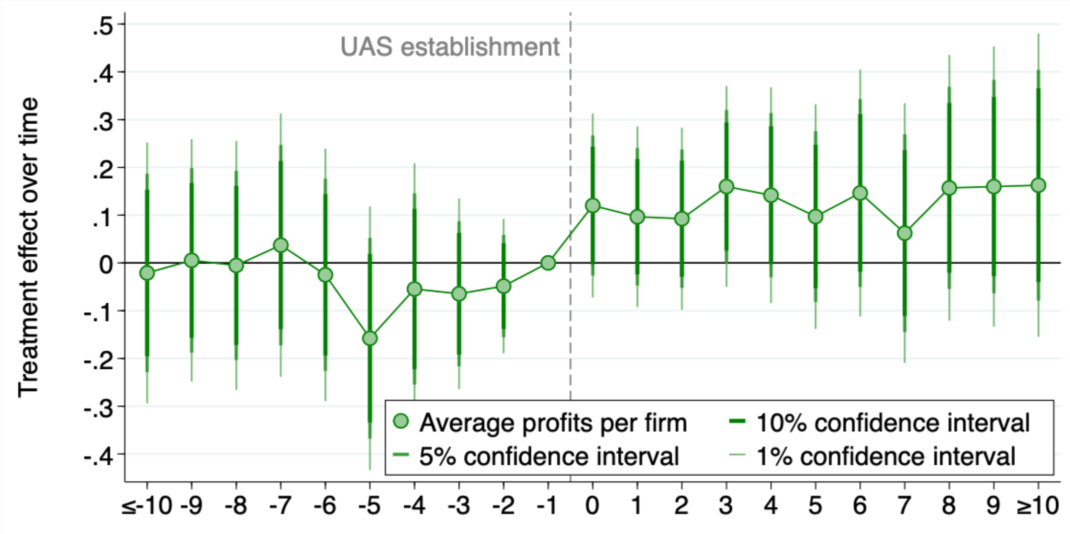
To investigate the robustness of our DiD results with respect to municipality characteristics that could influence the location of newly established UASs, we conduct a number of robustness tests in Appendix D. We (1) investigate whether our results are driven by the composition of our treatment and control groups, (2) use only the variation in the timing of the treatment to identify treatment effects (3) analyze the effect of municipalities with different regional characteristics and (4) run an inverse probability of treatment weighting regression. We find that none of these robustness tests structurally change our main DiD results.

In a second step, we allow for dynamic treatment effects by applying an event study design. The results of the event study appear in Figure 4a, with negative values on the x-axis indicating pre-treatment periods and positive values on the x-axis indicating post-treatment periods. The y-axis shows the estimated treatment effects for average profits per firm in each event period. Figure 4a reveals four findings. First, the coefficients in the event study are less precisely estimated than in the DiD model. Therefore, we are only able to detect few coefficients that are significant at conventional significance levels (e.g. the coefficients for $t = 0$ and $t = 3$). This finding is in line with a recent and ongoing debate on the efficiency of such methods to estimate causal effects of a treatment that has a staggered rollout (e.g. Callaway & Sant'Anna 2020; de Chaisemartin & D'Haultfœuille 2020; Roth & Sant'Anna 2021; Sun & Abraham 2020). In sum, there is a consent that traditional event study designs suffer from too wide confidence intervals.

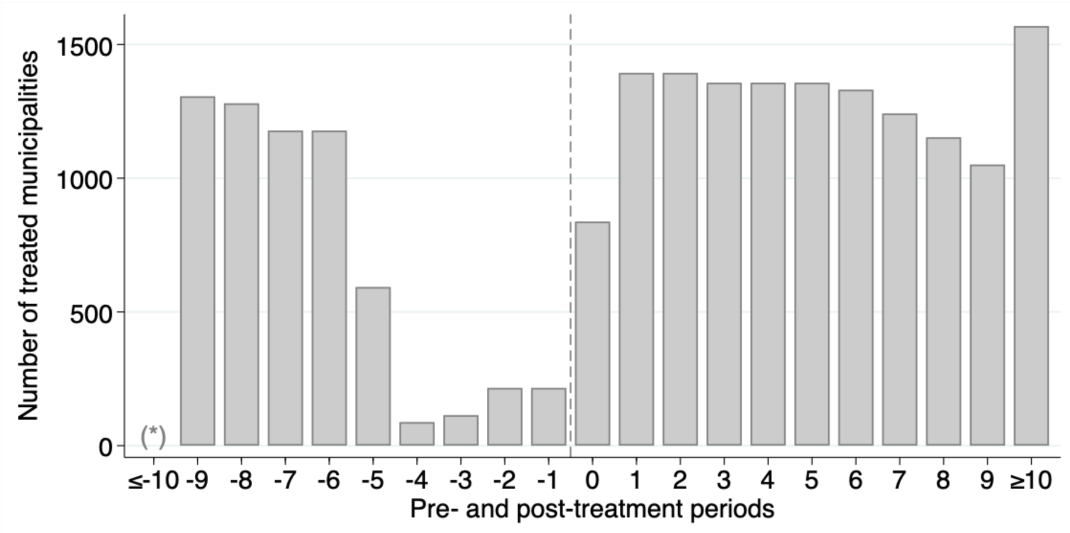
Second, the estimated coefficients for the pretreatment period are mostly close to zero (and insignificant), showing that no relevant anticipatory effects of the establishment of UASs exist in the treatment group (e.g., caused by infrastructure investments for the establishment of UASs). The unlikely existence of anticipatory effects is also supported by joint F-tests not rejecting the null hypotheses that the pretreatment periods are jointly (or even pairwise) different from zero. However, the coefficient for $t = -5$ shows a marked drop, as compared to all other pretreatment periods. The reason for this drop lies in the data structure for our event study design, with missing data between 1991 and 1997 for *all* municipalities. These data gaps lead to the fact, that the coefficient in $t = -5$ is estimated solely based on 1992 and 1998, years with especially low average profits per firm (as compared to the base year of the event study, i.e., $t = -1$) in the corresponding establishment groups (UASs established in 1997 and 2003). We further discuss this issue in Appendix C.2. More generally, Figure 4b shows the observations underlying the estimation of each event period.[7]

Third, Figure 4a shows that in the posttreatment period, treatment effects start to increase immediately after the treatment. This relatively immediate response of average profits per firm to the establishment of UASs points to the existence of positive, though small, demand-side effects of UASs. Thus UASs impact average profits per firm not only through innovation and R&D. The reasoning underlying this interpretation is that tertiary education institutions influence innovation with a lag of between two to five years (e.g., Cowan & Zinovyeva 2013; Fischer & Varga 2006; Pfister et al. 2021), and that innovation, as measured by patents, has a positive impact on firm profits, again with a lag of two to three years (Ernst, 2001; Kaiser, 2009).

Fourth, Figure 4a shows that after the immediate increase, positive effects on average profits per firm grow further up to period $t = 3$ when they nearly stabilize (again the drop in $t = 7$ is linked to our data structure, see Appendix C.2). Thus while showing some fluctuations, the increases over the entire posttreatment period of up to ten years in our data set, give some evidence for an impact of UASs that goes beyond a mere short-term effect. This result of a persistent long-term effect is also supported by means of a joint F-test that rejects the null hypothesis (at the 10% level) that the estimated coefficients for all periods of more than three years after the establishment of UASs are equal to zero.



(a) *Dynamic treatment effects*



(b) *Number of observations by period to estimate dynamic treatment effects*

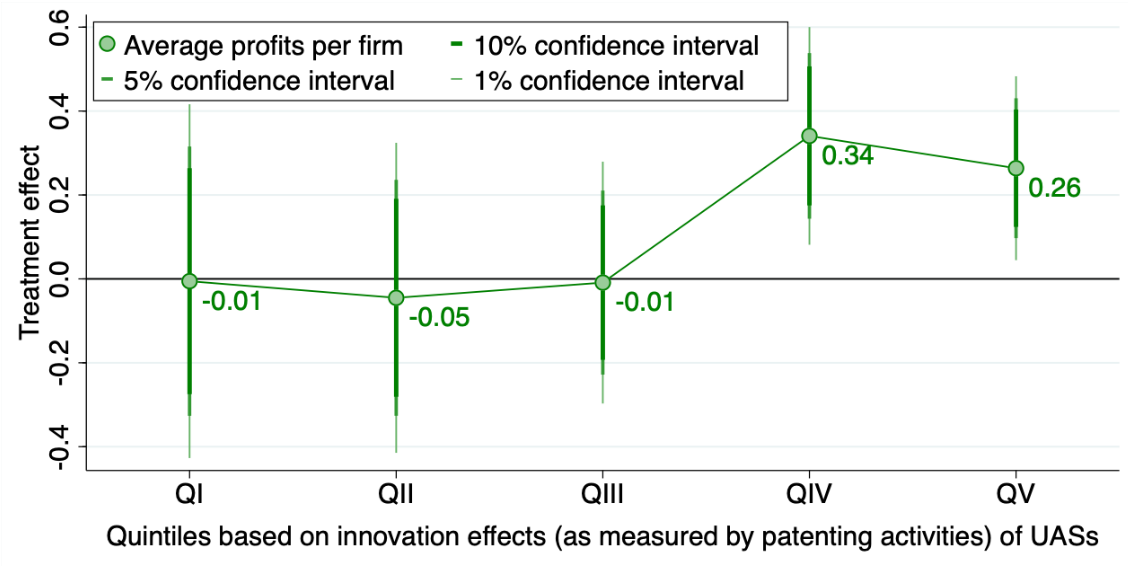
Notes: Figure 4a shows the dynamic treatment effects for average profits per firm. Negative values on the x-axis show the years before the treatment, t=0 corresponds to the year a UAS was established and the positive values on the x-axis belong to the posttreatment period. Depicted results are coefficients from PPML regressions, with $t = -1$ being the base year. Vertical lines mark confidence intervals. Figure 4b shows the number of observations we have for each pretreatment and posttreatment period, for estimating dynamic treatment effects. The few observations immediately before the treatment explain the negative but insignificant pretreatment effects (Figure 4a).
[*] We do not report the much higher number of observations in the bin for $t \leq -10$ (as compared to the other periods) for clarity.

Figure 4. Evolution of treatment effects

Further Analyses

In this section, we shed more light on the potential mechanisms behind the increase of average profits per firm after the establishment of UASs.[8] We suppose that one mechanism for higher average profits is through the positive impact of the establishment of UASs on innovation activity, as shown by Pfister et al. (2021). Drawing on patent data from the PATSTAT Worldwide Patent Statistical Database—April 2013 Version, we analyze whether municipalities that profit strongly in terms of innovation (as measured by patenting activities), after the establishment of UASs also profit more strongly in terms of profits from the establishment of UASs. If so, this provides some evidence that the higher average profits after the establishment of UASs is the result of higher innovation activities (linked to the establishment of UASs) that pay off.

We thus estimate the effect of the establishment of UASs on innovation activities, using a three-year lag, and use the estimated treatment effect to group municipalities into quintiles, according to the size of the innovation effect associated with the establishment of UASs. The resulting categorical variable is then interacted with a treatment dummy, now lagged by six years, to account for the time needed for innovation to affect profits (Ernst, 2001). Figure 5 shows the result of this exercise. The higher and significant coefficients for average firm profits in the two uppermost quintiles with respect to innovation effects of UASs reveal that only municipalities that show positive impacts of UASs in the form of higher innovation, also experience positive effects in the form of higher average profits per firm some three years later. Furthermore, in Appendix E.1, we provide further evidence for the importance of innovation effects linked to UASs as potential channels through which the establishment of UASs affects average profits per firm, by showing that municipalities with a higher share of firms in innovative industries tend to experience a stronger increase in average profits per firm after the establishment of UASs than municipalities with a lower share of firms in these industries.



Notes: The Figure reports coefficients from a PPML regression with control variables as shown in Equation (1). Dependent variable: average profits per firm. The vertical lines indicate confidence intervals. The regression includes an interaction of the treatment dummy with a categorical variable indicating to which quintile a municipality belongs to, with respect to the innovation effect (as measured by patenting activities) after the establishment of UASs. To account for a lag between innovation effects and effects on average profits, we assume a six year, instead of a three-year lag. The regression includes fixed effects at the regional labor market level (as with municipality fixed effects, the quintile dummy would be dropped) and canton-specific time trends.

Figure 5. Effects on average profits per firm by size of innovation effects (as measured by patenting activities) linked to newly established UASs

Conclusion

This study analyzes the effect of the establishment of tertiary education institutions on regional firm development, using the establishment of UASs—bachelor's degree-granting three-year colleges teaching and conducting applied research—in Switzerland as a case study. Our results show a positive and significant increase in regional firm development, as measured by average profits per firm. In treated municipalities, i.e., those near a newly established UAS, these profits increase significantly more than in municipalities in the control group. The magnitude of this effect corresponds to 19.6%, leading roughly to an additional annual growth in average profits per firm in the treatment group of 0.7%. Further analyses provide evidence that the increase in average profits per firm after the establishment of UASs is moderated by innovation impacts of UASs on the regional economy. Our robustness tests show that the effects on average profits are not driven by municipality characteristics that are particular to municipalities in the treatment group.

Furthermore, the analysis of the dynamic treatment effect reveals, first, that average profits per firm react immediately to the establishment of the UASs. Second, the dynamic treatment effects show patterns of increasing average profits up to three years after the establishment and then remain stable in the long run. However, effects are often not statistically significant. The direct increase combined with the long-term persistence reveal that both, short run—rather expenditure based—and long run effects—e.g., through increased innovation—are at play.

However, future research should address some of the limitations this study faces—predominantly imposed by a lack of data. First, the analysis should be expanded to UASs in non-STEM fields, since it is likely that regional firm development, as measured not only by average profits per firm, are also affected by UASs in these fields.[9] Finding out the importance of the impact on regional firm development of UASs in all fields in general, and of each specific field in particular, would be an important future research question. Second, our setting does not allow us to exactly pin down the mechanisms through which UASs influence regional firm development. The establishment of UASs might lead to both supply and demand side effects, e.g., cost-reducing product and process innovations (supply side), new but more expensive products (both, supply side, through more qualified workforce and demand side through more sophisticated consumers) or general equilibrium effects such as changes in wages and rents (demand side). Therefore, providing evidence for positive effects of UASs on regional firm development is only the first step and finding out the underlying mechanisms remains a pathway for future research.

Despite these limitations, our findings are affirmative regarding the achievement of some of the goals of the reform leading to the establishment of UASs (see Appendix A). The goals were not only to create both human capital and knowledge (the core functions of tertiary education institutions), but to foster technology transfers, increase co-operations with local firms and "revitalize the Swiss economy" (Bundesrat, 1994). The positive effects on regional firm development imply that these additional goals are achieved at the aggregated municipality level.

More generally, these positive effects on regional firm development show that, overall, the impact of newly established tertiary education institutions on the regional economy eventually translate into improved regional firm development, as measured by average profits per firm. The repeatedly documented positive impact of tertiary education institutions, e.g., increases in innovation and knowledge spillovers, reflect thus not only quantitative but qualitative increases, meaning that innovations and knowledge spillovers pay off on average. Furthermore, our analysis shows that these increases are not at the expense of regions without newly established tertiary education institution but reflect an additional stimulus for regional firm development.

Notes

1 The reasons to analyze STEM campuses are that we focus particularly on innovation impacts and their influence on average profits per firm. We discuss these reasons in more detail in Appendix A.

2 The data was collected within the research project "The Swiss Confederation: A Natural Laboratory for Research on Fiscal and Political Decentralization" (2010-2013), directed by Marius Brülhart and has already been used in studies in the field of economic geography (e.g., Brülhart et al. 2012; Luthi & Schmidheiny 2014).

3 The leads are identified either way, since after 1997 we have yearly data and the establishment of UASs only starts in 1997.

4 In our particular case, binning is thus not done for identification purposes (Schmidheiny & Siegloch, 2020) for we still have the control group included in our estimation.

5 To attenuate the influence of outliers, we follow Cox (2013) and estimate the PPML estimator excluding the 0.5% biggest municipalities. We discuss this issue in detail in Appendix C.1.

6 (1) The Breusch-Pagan test rejects the null hypothesis of homoskedasticity at all conventional significance levels. (2) As Table B2 shows, our dataset has a considerable number of observations with zero spells, especially in earlier years.

7 As 16 out of the 22 UAS campuses were established between 1997 and 1998, and as data for 1991-1997 is missing for *all* municipalities, the estimation for these pretreatment periods rests on the few observations for municipalities with UASs established after 1998.

8 In Appendix E, we discuss four further analyses. First, we analyze the impact of different pretreatment shares of innovative firms in a municipality. Second, we investigate whether effects are rather driven by increases in profits or by a reallocation of above average profitable firms to the treatment groups. Third, we run separate analysis for establishments of UASs as greenfield (ex nihilo) and brownfield institutions. Fourth, we investigate how the treatment effect changes with growing distance to newly established

UASs and with other levels of geographical analysis.

[9] Apart from STEM, i.e., UASs specializing in "engineering and IT" and "chemistry and the life sciences", UASs also specialize in (1) "agriculture and forestry"; (2) "architecture, construction and planning"; (3) "music, theater and other arts"; (4) "design"; (5) "health"; (6) "applied linguistics"; (7) "business, management and services"; (8) "applied psychology"; (9) "social work" and (10) "sports".

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Online Appendix for “Tertiary Education Expansion and Regional Firm Development”

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A. The Establishment of UASs and their Impact on the Regional Economy

In this Appendix, we discuss the establishment of UASs in Switzerland and its impact on the regional economy around the newly established UASs. In the mid-1990s, the Swiss federal government launched a policy reform targeted at establishing a new type of tertiary education institution, the UAS, bachelor’s degree-granting three-year colleges that teach and conduct applied research. These UASs offer a wide range of study programs, from engineering and information technology (IT) to business and services and to design and architecture (SERI, 2019). We focus our analysis on UAS campuses specializing in STEM fields (i.e., UASs specializing in “engineering and IT” or in “chemistry and the life sciences”) due to two reasons. On one hand, average profits per firm might not be an adequate measure for impacts in many UAS fields, because they rather relate to non-profit, non-governmental or public sector organizations (e.g., UASs specializing in “social work” or in “theater, Music and other arts”). On the other hand, we are particularly interested in analyzing the influence of the positive impacts of UASs on average profits per firm through innovation and R&D personnel (Lehnert et al., 2020; Pfister et al., 2021). These mechanisms, however, are predominantly relevant for UASs specializing in STEM fields.[1]

The goals of the reform leading to the establishment of UASs focused on three of the outputs the literature on education economics associates with tertiary education institutions (see Figure 1 in the paper). First, regarding human capital creation, the UASs increase the prospects for tertiary education among individuals with a vocational education and training (VET) diploma, by allowing them to obtain a bachelor's degree.[2] The implementation of this new degree constituted a post-2000 tertiary education expansion, leading to a Swiss labor force that is more human capital-intensive than before the reform (Wolter, 2017).

Second, knowledge creation is explicitly stated as a goal in the mandate for the newly established UASs, obligating them to teach *and* conduct applied research in collaboration with and on behalf of the regional economy (Bundesrat, 1994). As a consequence, the R&D spending of UASs increased from 100 million Swiss Francs (CHF) (roughly 108 million dollars) in 2000 to 550 million CHF (roughly 593 million dollars) in 2013 (Lepori & Müller, 2016).[3]

Even though some of the newly established UASs rise from existing educational institutions, the reform thus led to an R&D expansion and was by no means a sheer relabeling of predecessor institutions (Backes-Gellner & Pfister, 2019). Focusing particularly on the question of the potential influence of predecessor institutions, Lehnert et al. 2020 find no difference in the effect of newly established UASs on R&D employment and R&D wages across regions with or without a predecessor institution. More generally, the positive effects on innovation quality and quantity (Pfister et al., 2021) imply that the legal mandate—including the necessity to teach and conduct applied R&D—indeed improved the quality and quantity of research, also in regions where predecessor institutions to UASs existed.

Third, the policy reform was also explicitly oriented toward fostering technology transfers, the cooperation of UASs with local firms and the "revitalization of the Swiss economy" (Bundesrat, 1994). This additional goals reflect in the above-average propensity, relative to other tertiary education institutions, to transfer knowledge and technology to firms (Arvanitis et al., 2008). Taken together, the impacts on the regional economy associated with UASs, i.e., increased patenting activity, higher shares of R&D employment and intensified cooperations of UASs with local firms, are likely to further translate into effects on regional firm development as measured by average profits.

Regarding the establishment process between 1997 and 2003 of the in total 22 UAS STEM-campuses in Switzerland (see Figure 2 in the paper),[4] the in-depth analyses of preceding studies show—both qualitatively and quantitatively—that the location decisions for the establishment of new UASs resemble a quasi-random process that can be used to solve endogeneity problems (Lehnert et al., 2020; Pfister et al., 2021). By extensively studying the history of the process of the establishment of UASs, Pfister et al. (2021) show qualitatively that the location decision depended on complex institutional requirements, multi-party negotiations, and a complicated political process. They provide numerous examples that decisions on the actual location of UASs (i.e., the municipality and the region) were changed several times throughout the political discussion. Therefore, the location decisions were both unpredictable until shortly before the UASs were actually established and not linked to the innovative or economic capacity of a region.

Lehnert et al. (2020) and Pfister et al. (2021) both also provide ample quantitative evidence for the quasi-randomness of the establishment of UASs. In a number of robustness tests, the authors show that neither the economic and innovative strength, nor the political power of the largest cities explain the effects they found—providing evidence that the effects are not an artifact of the location decisions of UAS campuses. Therefore, the quasi-random establishment process of UASs in Switzerland is appropriate to address endogeneity issues in our estimation strategy.

B. Variables, Data, Descriptive Statistics

In this Appendix B, we provide information on the legal foundation and the legal definition of the underlying data of our dependent variable, i.e. the average profits per firm. Furthermore, we present the data structure over the years, an important information because the tax data is censored at the lower end of the distribution (Appendix B.1). Appendix B.2 shows descriptive statistics of our dependent variable. Appendix B.3 discusses federal and cantonal tax reforms and how we construct variables to control for them in our empirical analysis.

B.1. Data on Average Profits per Firm

Regarding federal corporate taxation, the calculation of firm profits subject to taxes is defined in articles 57-67 of the *Federal Act of 14 December 1990 on Direct Federal Taxation* and in the articles 24-26a of the *Federal Act of 14 December 1990 on the Harmonization of Direct Taxation at Cantonal and Communal Levels*. The calculation is therefore harmonized across Switzerland and thus an ideal measure for our comparison across regions.

The "average profits per firm", our dependent variable, is subject to the legal definition of a firm in Switzerland and to the Swiss taxation law. The Swiss legal system differentiates between natural persons and corporate bodies. Only firms with an own corporate body[5] are subject to corporate taxes (Art. 49 para. 1, Federal Act of 14 December 1990 on Direct Federal Taxation (DBG)). Firms without an own corporate body[6] pay taxes via the taxation of the income of their owners and are thus not in our dataset.

Swiss firms, which are subject to corporate taxation, pay their cantonal and municipal taxes[7] in general on their entire firm profits in the municipality where they have their headquarter (Art. 50 DBG). However, the firm profits of branch establishments[8] are subject to taxation in the municipality where they are located. The firm profits of foreign branch establishments run by Swiss firms are excluded from corporate taxes in Switzerland. Foreign firms also have to pay taxes on the profits of branch establishments located in Switzerland. Again, these profits are subject to taxation in the municipality where the branch establishment is located. We can thus conclude, that the firm profits per municipality in our dataset really reflect the firm profits of local establishments and not of foreign or non-municipality firms.

To calculate our measure for average profits per firm, we use the aggregate firm profits, i.e., the sum at the municipality level of all firm profits subject to taxes, after taxes. It is thus crucial to understand how the Swiss federal tax administration (SFTA) assesses the firm profits subject to taxes, i.e., the taxable unit. The firm profits subject to taxes do not stem directly from the accounting profits, as there are a number of adjustments and possible deductions. Table B1 shows the step-wise transformation from the reported operating profits of a firm to the firm profits subject to taxes. The two most important deductions are, first, the deduction of the tax loss carryforward, i.e., the possibility to subtract the firms' deficits of the past seven years (given that they have not been cleared with earlier tax payments). Second, it is the deduction of the taxes payed during the tax period. Since all, municipality, cantonal and federal taxes are deductible, identical firm profits subject to taxes before taxes, lead to different results for the firm profits subject to taxes after taxes. The possibility to deduct deficits has, for our use of the data, the disadvantage that it masks actual firm profits in a particular year, when a firm incurred a deficit in preceding years. Thus our analyses will provide a somewhat lower bound of potential positive effects of the establishment of UASs on firm profits.[9]

Table B1: Calculation of the federal corporate tax capacity used for taxation

+	Operating income
	+ Revenue from goods sold
	+ Trading Income
	+ Interest income
	+ Securities income
+	Extraordinary income
	+ Capital gains
	+ Revaluation gains
	+ Liquidation gain
=	Operating profit before tax
+	Non-operating expenses
	+ Non-paid, voluntary donations
	+ Open and hidden profit distribution
+	Non-operating deductions
	+ Amortization of undisclosed reserves
+	Non-operating provisions
+	Value increasing expenses
–	Capital invested
=	Firm profits subject to taxes before taxes
–	Tax loss carry forward (up to seven years)
–	Tax expenses
=	Firm profits subject to taxes after taxes

Source: Authors' Figure based on Arnold et al. (2018). *Notes*: The *firm profits subject to taxes after taxes* is the value we have in our dataset and which we use as our measure for average profits. The average profits per firm are then calculated by dividing this value by the number of taxable firms in a municipality.

For some years and municipalities, the data is censored at the lower end of the distribution. For municipalities with less than four taxable firms in a given year, all entries of that year are, for data protection reasons, set to zero. This leaves us with observations showing zero values for average profits per firm even if they are non-zero. Table B2 shows, that the treatment and the control groups are similar with respect to these zero spells[10] Overall, Table B3 shows that 70% of the municipalities report positive aggregate firm profits in all 21 tax periods available, again with similar shares in treatment and control groups.

Table B2: Observations (municipalities) with no reported profits by year

Year	Treatment Group		Control Group		Full sample	
	N	in %	N	in %	N	in %
1971	329	23%	206	26%	535	24%
1973	311	22%	195	25%	506	23%
1975	287	20%	184	24%	471	21%
1977	246	17%	163	21%	409	18%
1979	229	16%	152	20%	381	17%
1981	205	14%	130	17%	335	15%
1983	197	14%	125	16%	322	14%
1987	176	12%	108	14%	284	13%
1989	153	11%	95	12%	248	11%
1991	132	9%	94	12%	226	10%
1998	124	9%	93	12%	217	10%
1999	115	8%	93	12%	208	9%
2000	108	7%	89	11%	197	9%
2001	111	8%	85	11%	196	9%
2002	102	7%	82	11%	184	8%
2003	92	6%	71	9%	163	7%
2004	91	6%	71	9%	162	7%
2005	81	6%	64	8%	145	7%
2006	81	6%	67	9%	148	7%
2007	72	5%	60	8%	132	6%
2008	57	4%	52	7%	109	5%
Total	3,299	11%	2,279	14%	5,578	12%

Table B3: Observations (municipalities) by number of years without reported profits

Years with zero profits	Treatment Group			Control Group			Full sample		
	N	n	in %	N	n	in %	N	n	in %
0	39,482	1,039	72.0%	19,874	523	67.1%	59,356	1,562	70.3%
1	2,432	64	4.4%	1,102	29	3.7%	3,534	93	4.2%
2	1,672	44	3.0%	836	22	2.8%	2,508	66	3.0%
3	1,140	30	2.1%	874	23	3.0%	2,014	53	2.4%
4	950	25	1.7%	608	16	2.1%	1,558	41	1.8%
5	1,026	27	1.9%	760	20	2.6%	1,786	47	2.1%
6	456	12	0.8%	456	12	1.5%	912	24	1.1%
7	950	25	1.7%	494	13	1.7%	1,444	38	1.7%
8	798	21	1.5%	380	10	1.3%	1,178	31	1.4%
9	684	18	1.2%	494	13	1.7%	1,178	31	1.4%
10	646	17	1.2%	342	9	1.2%	988	26	1.2%
11	380	10	0.7%	380	10	1.3%	760	20	0.9%
12	456	12	0.8%	114	3	0.4%	570	15	0.7%
13	190	5	0.3%	304	8	1.0%	494	13	0.6%
14	228	6	0.4%	152	4	0.5%	380	10	0.5%
15	418	11	0.8%	342	9	1.2%	760	20	0.9%
16	190	5	0.3%	114	3	0.4%	304	8	0.4%
17	342	9	0.6%	342	9	1.2%	684	18	0.8%
18	304	8	0.6%	114	3	0.4%	418	11	0.5%
19	266	7	0.5%	152	4	0.5%	418	11	0.5%
20	418	11	0.8%	190	5	0.6%	608	16	0.7%
21	1,406	37	2.6%	1,178	31	4.0%	2,584	68	3.1%
Total	54,834	1,443	100%	29,602	779	100%	84,436	2,222	100%

Notes: With N: year-municipality observations and n: municipalities.

B.2. Descriptive Statistics

Table B4: Descriptive statistics for dependent variable and data used to construct it

Levels

	Treatment Group					Control Group					Sample t-test
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	
*Aggregated firm profits per municipality (in **million** CHF)*											
Pre-treatment	13,940	10.25	162	0	9,026	7,520	3.52	31	0	1,056	***
Post-treatment	15,334	50.74	829	0	43,071	8,272	12.17	119	0	5,237	***
Number of taxable firms											
Pre-treatment	13,940	66.95	529	0	16,185	7,520	42.50	224	0	5,535	***
Post-treatment	15,334	125	754	0	22,882	8,272	68.53	220	0	5,309	***
*Average profits per firm and municipality (in **thousand** CHF)*											
Pre-treatment	13,940	47.25	99	0	1,919	7,520	37.62	105	0	1,921	***
Post-treatment	15,334	94.00	195	0	2,524	8,272	65.84	153	0	2,531	***

Growth Rates

	Treatment Group					Control Group					Sample t-test
	N	Mean	SD	Min	Max	N	Mean	SD	Min	Max	
Aggregated firm profits per municipality (in percentages)											
Pre-treatment	12,546	108	1,914	-100	172,860	6,768	102	1,539	-100	87,700	
Post-treatment	13,940	169	8,075	-100	896,250	7,520	116	1,951	-100	128,936	
Number of taxable firms (in percentages											
Pre-treatment	12,546	9.58	22	-100	320	6,768	7.50	22	-100	225	***
Post-treatment	13,940	5.29	18	-100	1,040	7,520	4.43	17	-100	250	***
Average profits per firm and municipality (in percentages)											
Pre-treatment	12,546	80.06	1,439	-100	119,641	6,768	76.24	1,152	-100	62,614	
Post-treatment	13,940	91.27	2,404	-100	204,267	7,520	99.72	1,636	-100	105,475	

Notes: Table B4 shows descriptive statistics for our outcome variable, i.e., average profits per firm as well as the data we used to construct it. The reported growth rates represent the average of the yearly growth rates at the municipality level over the pretreatment and the posttreatment period, respectively. The levels of significance are denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B.3. Controls for Federal and Cantonal Tax Reforms

Over the years, the Swiss tax system underwent several reforms both at the federal and the cantonal level for which we control in our regression analyses. At the federal level corporate tax rates were, first, changed at times between 1971 and 1997, as shown in Table B5. Only since 1998, the corporate tax rate is fixed at a flat rate. Before, it depended on firm profits. Therefore, the tax rate for this early years contains information on the mix of firms at the municipality level (with high average corporate tax rates in municipalities with very profitable firms, and vice versa). Second, certain legal types of firms, such as associations and foundations profit of lower tax rates. Since 1998, the lower tax rate is fixed at 4.25% (SFTA, 2019). Therefore, controlling for the average corporate tax rate takes the special mix of a municipalities' firm types into account. To do so, we calculate the average corporate tax rate by dividing the gross corporate federal taxes in CHF by the aggregate firm profits in CHF.

Table B5: Development of corporate tax rates over time

Tax rates	*1973/1974*	*1975-1994*	*1995-1997*	*1998-2010*
Tax rate on corporate net profits				8.50%
Base tax rate	3.30%	3.63%	3.63%	
Premium on the corporate net profits exceeding a 4% rate of return	3.30%	3.63%	3.63%	
Premium on the corporate net profits exceeding an 8% rate of return	4.40%	4.84%	4.84%	
Maximum tax rate	8.80%	9.80%	9.80%	
Rate of return where maximum tax rate is reached	22.00%	23.15%	23.15%	
Tax rate on equity	0.825‰	0.825‰	0.800‰	abolished

Notes: Based on SFTA (2019)

We also control for the "participation exemption" in CHF. The participation exemption allows firms holding more than 10% of the equity of other firms, to deduct capital gains and dividend income of their subsidiaries from the firm profits subject to taxes. The participation exemption thus also provides some information on whether a municipality is characterized by for example many holding companies that can deduct their investments from the firm profits to avoid double taxation.

At the cantonal level, a number of flat tax reforms took place (see Table B6). In the DiD estimation, as shown in Equation (1) in the paper, we control for cantonal flat tax reforms by allowing for canton-specific time trends and, thereby follow Galletta & Redonda (2017), who apply the same setting. In our event study design as shown in Equation (2) in the paper, where we include additional period specific dummies, we control for cantonal flat tax reforms by including dummies indicating the years after a flat tax reform to avoid under-identification (see Baker & Fradkin 2017 for a similar approach for DiD and event study). To construct the dummies accounting for cantonal flat tax reforms, we draw on data provided by Galletta & Redonda (2017). We do so, by constructing dummies that switch to one for municipalities after the corresponding canton changed their corporate profit tax regime to a flat tax rate. Table B6 shows the cantons that implemented a flat tax reform as reported by Galletta & Redonda (2017) and also relates the timing of the flat tax reform to the first establishment of UASs in the particular canton.

Table B6: Timing of cantonal flat tax reforms and the establishment of UASs by canton

Canton	Flat tax reform	First UAS establishment	% of municipalities treated
Aargau	-	1997	94%
Appenzell Ausserrhoden	1995	2000	100%
Appenzell Innerrhoden	1993	2000	100%
Bern	-	1997	72%
Basel-Stadt	-	1997	98%
Basel-Landschaft	-	1997	100%
Freiburg	-	1997	75%
Genf	1999	2002	100%
Glarus	2009	-	0 %
Graubünden	2010	2000	26%
Jura	1990	1998	20%
Luzern	1991	1997	55%
Neuenburg	-	1997	77%
Nidwalden	1995	1997	100%
Obwalden	1995	1997	57%
St. Gallen	2007	1998	68%
Schaffhausen	2008	1998	9%
Solothurn	-	1997	97%
Schwyz	2010	1997	55%
Thurgau	2006	1998	46%
Tessin	1995	1997	48%
Uri	2007	-	0%
Waadt	2002	1997	44%
Wallis	-	1998	27%
Zug	-	1997	89%
Zürich	2005	1998	97%

Source: Own Table. Data on cantonal flat tax reforms based on Galletta & Redonda (2017). *Note:* A "-" indicates that there are no cantonal flat tax reform or no municipalities that are treated by a nearby UAS, respectively. The flat tax reforms in the cantons of Glarus, Graubünden and Schwyz take place outside of our observation window and are just reported for completeness.

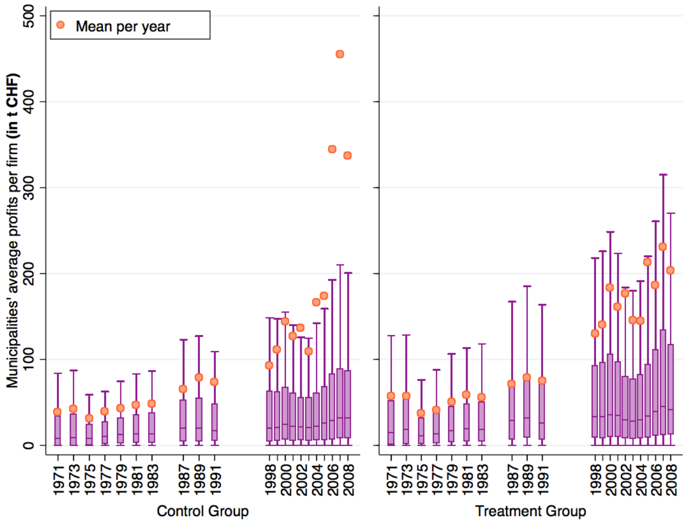
C. Empirical Strategy

C.1. Trimming Outliers for PPML Estimation

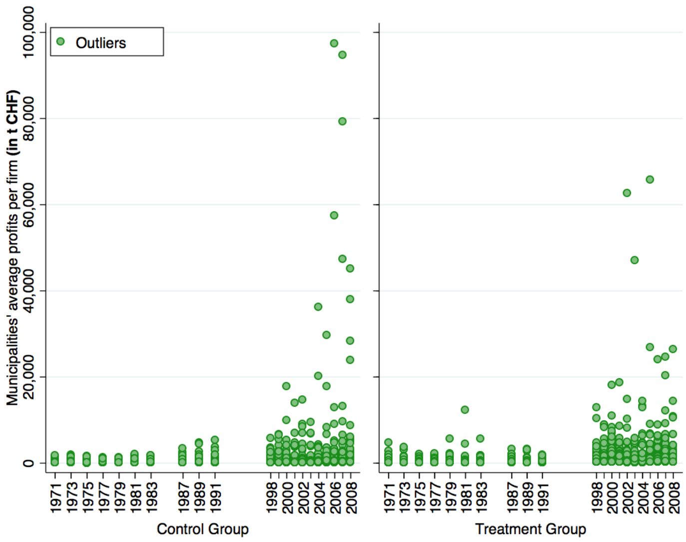
The PPML estimator is based on the distribution of the absolute values of average profits per firm thus giving more weight to extreme outliers. An analysis of the year-by-year distribution of average profits per firm in Figure C1a and of the outliers in Figure C1b, reveal large outliers, especially in the control group after 2004. To attenuate the influence of these outliers, we follow Cox (2013) and estimate the PPML estimator by excluding the 0.5% largest municipalities. The trimming of the data works as follows: First, we calculate for each municipality, using all years and both treatment and control groups, the qth percentile the municipality belongs to regarding average profits per firm. Second, to maintain a balanced panel, we drop all observations of a municipality if it had at least once belonged to the highest percentile. Table C1 shows the number of municipalities deleted.

Table C1: Municipalities dropped in treatment and control groups by trimming

	Municipalities dropped without upper 0.5%	
	Absolute	**In percentage**
Control Group	27	1.2%
Treatment Group	49	2.2%
Total	76	3.4%



(a) *Boxplots for average profits per firm by year and treatment and control groups*



(b) *Outliers in average profits per firm by year and treatment and control groups*

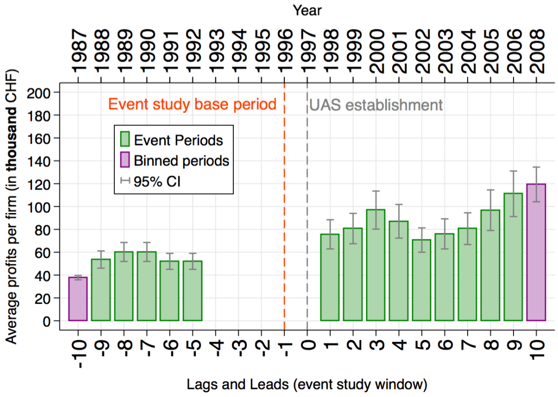
Note: Figures C1a and C1b show boxplots and outliers for treatment and control groups over the years, respectively, giving a sense for the distribution of average profits per firm. Additionally, the orange dots in Figure C1a show means of average profits per firm (in t CHF). In the control group, the means for 2006 through 2008 are very high, due to outliers. In Figure C1b, municipality-year observations are treated as outliers, when their average profits per firm are bigger than 1.5 times the interquartile range of the 25% quartile to the 75% quartile in Fig. C1a. Depending on the year, there are between 70 to 93 municipalities in the control group and 135 to 180 municipalities in the treatment group identified as outliers. The control group shows some heavy outliers, especially between 2006 and 2007. Not dropping these outliers would strongly influence our results when estimating the PPML estimation.

Figure C1. Boxplots and outliers for average profits per firm by the treatment and control groups over the years

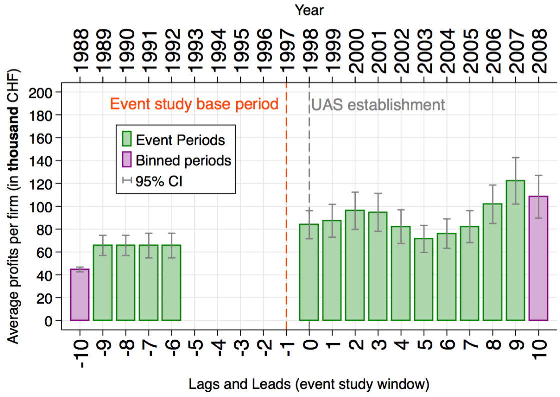
C.2. Data Structure Underlying the Event Study

Figure C2 provides some further insights on the data structure underlying our event study design. Each Sub-figure represents one establishment year and depicts the average profits per firm (on the y-axis) by the leads and lags, of our event study (lower x-axis). Above each graph (upper x-axis), we see the year for which data is used to estimate a particular lag or lead in the event study, e.g., for UASs established in 1997 (Sub-figure C2a), a two-year lead would be estimated based on data from 1999. Regarding the sharp drop in $t = -5$, Figure C2 reveals two things. First, due to the data gaps, estimates of the lag $t = -5$, are based solely on average profits per firm in 1992 (Sub-figure C2a) and 1998 (Sub-figure C2b), corresponding to municipalities that get the treatment in 1997 or 2003. For all other establishment years, data in the period $t = -5$ is not observable. We thus estimate the coefficient for $t = -5$ based on few observations.

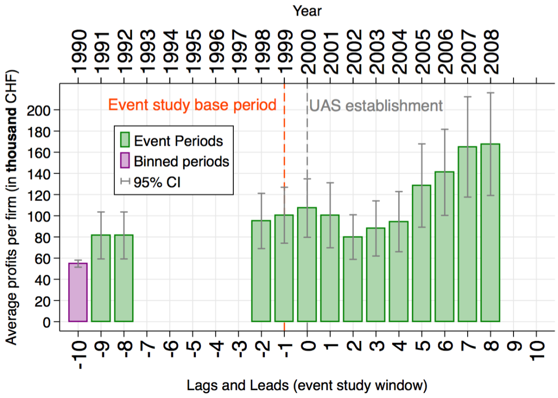
Second, the year-specific values of our dependent variable, i.e., average profits per firm, are especially low for the particular establishment years (as compared to the values of other establishment years). This is especially true for 1998, where average profits per firm are lower for municipalities with a UAS established in 2003 (Sub-figure C2f) as compared to municipalities with a UAS established earlier (e.g., Sub-figure C2d or C2b). This lower level of average profits per firm in municipalities treated in 2003 only, however, does not represent a general pattern. As Figure C2 also shows, in the years around the establishment of UASs, average profits per firm look rather similar for first adopters and these late adopters. We are thus convinced that the sharp drop in $t = -5$ is rather an artifact of our complicated event study structure than a detected negative anticipatory effect, also given that the remaining pre-treatment trends are negligible.



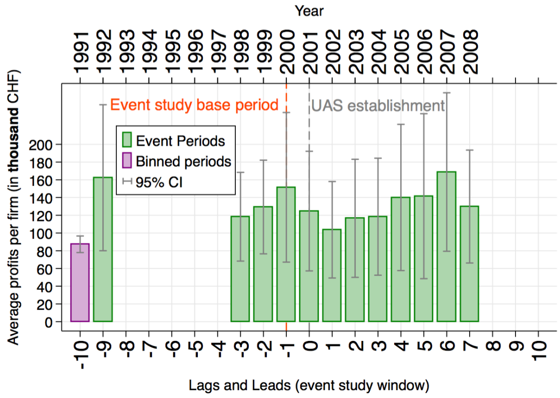
(a) *est. 1997*



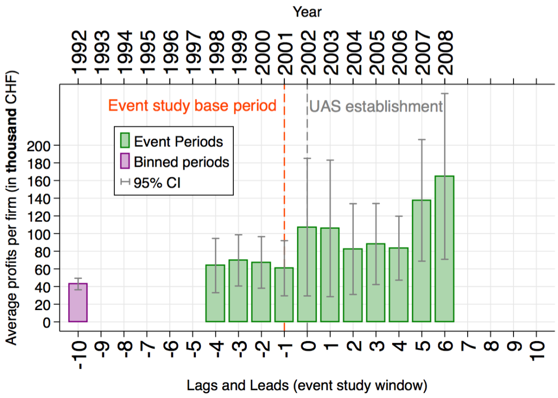
(b) *est. 1998*



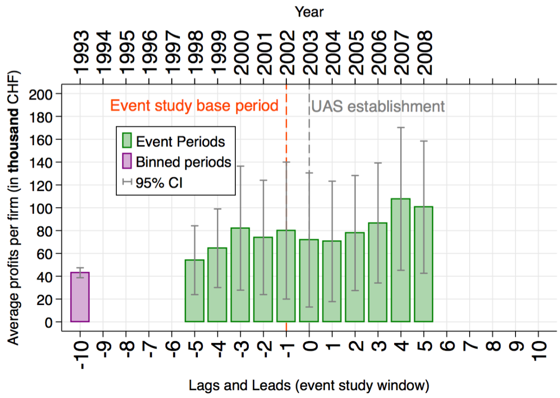
(c) *est. 2000*



(d) *est. 2001*



(e) *est. 2002*



(f) *est. 2003*

Notes: The Figures show the data structure underlying our event study design. Each Sub-figure represents an establishment year and depicts the average profits per firm (on the y-axis) per lead and lag of our event study (lower x-axis). Above each graph (upper x-axis), we show the year for which data is used to estimate a particular lag or lead in the event study, e.g., for UASs established in 1997 (Sub-figure C2a), a two-year lead would be estimated based on data from 1999. The base year of the event study, i.e., $t = -1$, is indicated in orange.

Figure C2. Event study: composition of event periods

D. Robustness Tests

In this Appendix, we provide more evidence for the robustness of our results with regard to the quasi-randomness of our treatment. First, we re-estimate our model by excluding parts of our data to investigate whether our effects are not driven by certain types of municipalities (potentially having a higher propensity to be treated). Second, we apply an inverse probability of treatment weighting, to adjust for potential differences in the propensity score, i.e., the probability of receiving treatment conditional on municipality characteristics, and, thereby generate a balanced sample of treated and non-treated municipalities (Imbens, 2004).

To test the robustness of our results regarding municipality characteristics, we restrict our sample in the following four ways: (1) we drop the labor market regions of the ten biggest municipalities in the treatment group one-by-one and all together to investigate whether estimated treatment effects are driven by the biggest municipalities (and their surrounding labor markets)[11]; (2) we drop one UAS region at a time to analyze whether effects are not driven by a single UAS; (3) we drop the control group to eliminate potential differences between treatment and control groups and to identify the treatment effect only by the variation over time, and (4) we drop municipalities using a categorical variable with nine categories indicating whether a municipality is rather rural or urban.[12] The results of these four exercises are summarized in Figure D1.

Regarding the sequential exclusion of the labor market regions of the ten biggest municipalities, Figure D1 reveals no difference in effect sizes (in green) as compared to the baseline model (in gray). Even excluding the regional labor markets of all ten biggest municipalities together (in dark green), i.e., 211 municipalities corresponding to 10.2% of our sample, only slightly decreases our estimated treatment effect. Also excluding particular UAS regions from the analysis (in orange) leaves our regression results unaltered with respect to effect size and significance. When excluding the control group (in brown) and estimating the treatment effect solely based on the variation in the timing of the treatment, our estimated treatment effect remains positive but becomes insignificant. On one hand, the lower estimated treatment effect can be a consequence of the fact that estimations using only temporal variation to identify treatment effects overweight short-term effects (Borusyak & Jaravel, 2017) and, as Figure 4a in the paper shows, effects on average profits per firm are increasing over time. On the other hand, the insignificance can be the result of the reduction in the number of observations.

Figure D1, furthermore, depicts regressions where we restrict the sample to municipalities in a particular category with respect to the density, size and accessibility of the municipality (in purple). This categorization is done by the SFSO (2012) based on census data from 2000 (all categories indicated in the notes to Figure D1). Again, estimated treatment effects are fairly stable to the exclusion of different municipality types. However, the lower coefficients in Figure D1 when excluding certain categories reveal that the effect is strongest in suburban and center municipalities (categories 8 and 9, respectively), but not limited to them, as also the exclusion of rural commuter municipalities and touristic municipalities (categories 3 and 5, respectively) leads to a decrease of the estimated treatment effect. Overall, these robustness tests show that our results are not solely driven by the biggest municipalities and their respective labor market region, nor by a particular UAS region or by municipalities of a particular economic and regional category.

In our second robustness test, we apply a regression with inverse probability of treatment weighting. Within this setting, we follow Hernán & Robins (2020) and, in a first step, predict the probability of being in the treatment group, i.e., having a UAS established nearby, conditional on municipality characteristics. In so doing, we apply a probit model with the treatment status of a municipality being the dependent variable. To predict the probability, we draw on the control variables from our main specification and further include (1) a categorical variable indicating whether a municipality is rather rural or urban, (2) the population of a municipality, (3) the pre-treatment value of patents per municipality, (4) the mean income of a municipality and (5) the distance to the nearest UAS.[13]

We use the results of the probit regression to predict the propensity score $p(x) = P(Treat = 1|X = x)$, i.e., the probability of being in the treatment group conditional on the municipality characteristics x. Subsequently, we calculate the standardized inverse probability weights, defined as $w(x)=\frac{P(Treat=1)}{P(Treat=1|X=x)}$ for treated and $w(x)=\frac{1-P(Treat=1)}{1-P(Treat=1|X=x)}$ for untreated municipalities, where $P(Treat = 1)$ is the marginal probability of treatment (Hernán & Robins, 2020). In a second step, we include the estimated weights in a regression of average profits per firm on the treatment dummy and our set of fixed effects, i.e., municipality fixed effects and canton-specific time trends. Figure D1 shows the result of this exercise in (in blue). As compared to our baseline specification (in gray), the inverse probability of treatment weighting does not alter our results substantially and the coefficient is still significant at the 5% level. This finding points to the fact that our main results are not an artifact of significant differences between the treatment and the control group.

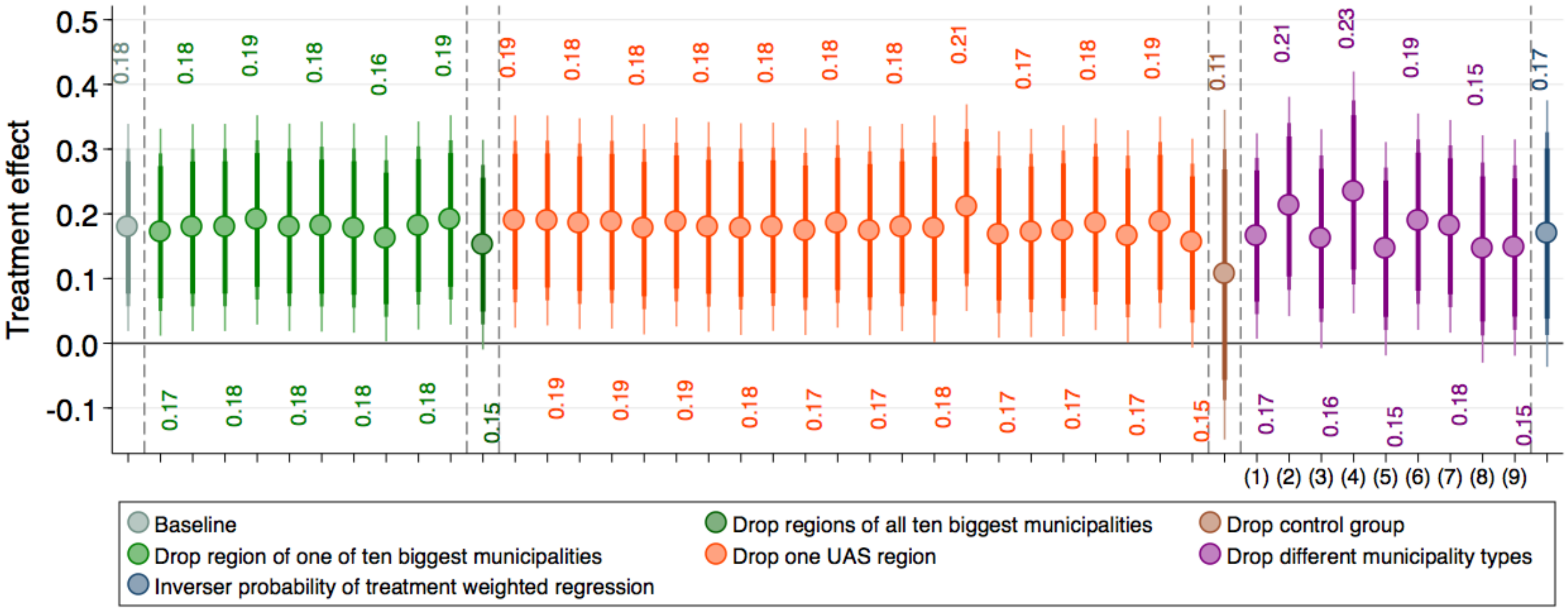


Figure D1. Results of various robustness tests regarding municipality characteristics and probability of treatment

Notes: Figure D1 shows the estimation results of a number of independent regressions, where we restrict the full sample (gray) by (1) dropping the labor market regions of the ten biggest municipalities in the treatment group one-by-one (green) and all together (dark green); (2) dropping one UAS region at a time (orange); (3) dropping the control group (brown) and (4) dropping municipalities depending on how rural or urban they are (purple). For this exercise, the categories omitted are (1) agricultural municipality, (2) mixed-agricultural municipality, (3) rural commuter municipality, (4) industrial and tertiary municipality, (5) touristic municipality, (6) peri-urban municipalities (former rural municipalities that get urbanized), (7) high-income municipalities, (8) suburban municipalities and (9) center municipalities. The last coefficient (blue) shows the estimate where we run a regression weighted by the inverse probability of treatment. All regressions include controls as shown in Equation 1, municipality fixed effects and cantonal time trends. The confidence intervals shown are based on robust standard errors clustered at the municipality level

E. Further Analyses

In this Appendix, we conduct four further analyses. First, we investigate whether municipalities with different shares in innovative firms experience different effects of the establishment of UASs on average profits per firms. This further analysis provides support for our argument (see fifth section of the paper) that one mechanism for higher average profits per firm is through positive impacts of the establishment of UASs on innovation and R&D activity (Lehnert et al., 2020; Pfister et al., 2021). Second, we analyze whether the positive effects on average profits per firm are driven (i) by increases in aggregated profits or rather (ii) by a disproportional increase in the number of above average profitable firms, for example due to a systematic reallocation of profitable firms from the control to the treatment groups. While such a mechanism—if present—would still represent an effect of the establishment of UASs, it would only be favorable for the treatment group at the expense of the control group.

Third, we conduct a further analysis that distinguishes between UASs that were established *ex nihilo* (greenfield institutions) and UASs that were established in municipalities where a predecessor institution already existed (brownfield institution). The effects shown in this analysis provide evidence that the establishment of UASs was more than a sheer relabeling of existing tertiary education institutions. Fourth, we shed more light on the question of the geographical localization of such positive effects. In so doing, we (i) relax our implicit assumption that treatment is homogeneous across the 25-km radius we use to assign municipalities to the treatment group and (ii) we show that treatment effects also exist at higher levels of geographical aggregation.

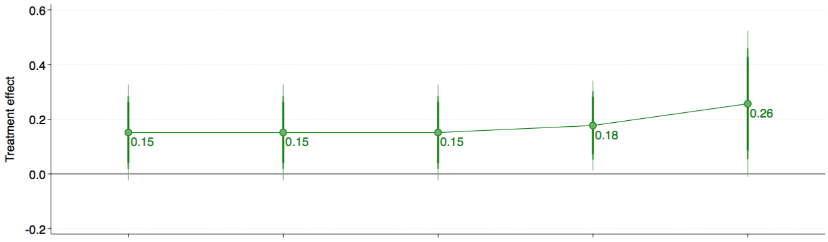
E.1. Impact of differing shares of innovative firms

In this further analysis, we focus on differences in the effects of the establishment of UASs on average profits per firm across municipalities with different shares in innovative industries. Following our argument in the fifth section of the paper, one important channel through which the establishment of UASs influences average profits per firm is the increase in innovation activities—–both higher patenting activities (Pfister et al., 2021) and increases in R&D employment (Lehnert et al., 2020)—linked to the establishment of UASs. Therefore, municipalities with relatively more innovative firms should also experience a stronger increase in average profits per firm.

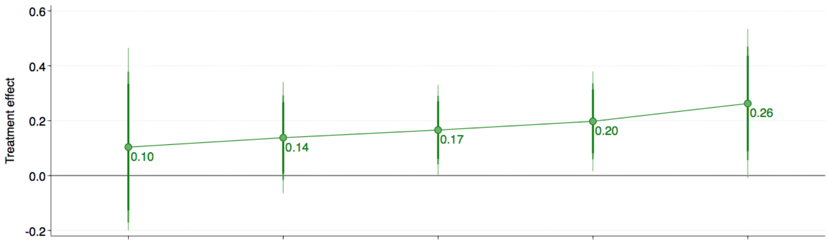
To investigate this hypothesis, we match an additional dataset for industry shares in the pretreatment period (1995) from the Swiss business census at the municipality level. Using this data allows us to take the strength of different industry categories prior to the establishment of UASs into account. We follow an industry taxonomy suggested by Bonaccorsi et al. (2013) and analyze the impact of the share of firms in four different industry categories, i.e., (1) science based manufacturing, (2) knowledge intensive business services, (3) other manufacturing, and (4) other services. Science based manufacturing and knowledge intensive business services are the industry categories that most strongly rely on knowledge spillovers from tertiary education institutions to adopt innovations (Bonaccorsi et al., 2013). In contrast, other manufacturing and other service industries rely either on different knowledge sources to innovate (e.g. through customers or suppliers) or do not engage in innovation that leads to new patents. Accordingly, we would expect municipalities with higher shares in science based manufacturing or knowledge intensive business services to profit most from newly established UASs.

We analyze the importance of a municipalities' firm share in a particular industry by adding two interaction terms to Equation (1) in the fourth section of the paper. First, we interact our posttreatment indicator ($\text{Treat}_i * \text{Post}_{i(t-3)}$) with the share of firms in a particular industry and, second, with the squared share of firms in that industry (to account for non-linear effects). We then run separate regressions for each industry. Figure E1 shows the result of this further analysis, where treatment effects of the establishment of UASs on average profits per firm (y-axis) are evaluated at different percentiles of the distribution of industry shares, i.e., the 5_{th}, 25_{th} 50_{th}, 75_{th} and 95_{th} percentile (x-axis).

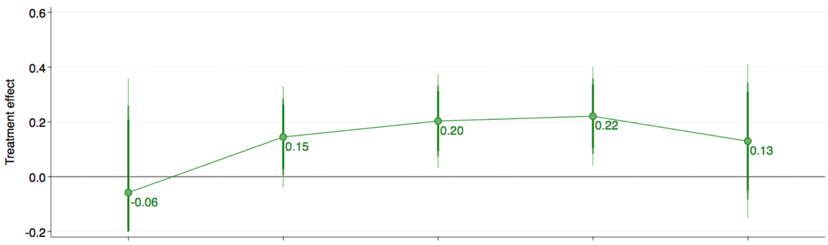
For science based manufacturing and knowledge intensive business services, Figure E1 shows that municipalities with the highest shares of firms in these industries show larger effects of the establishment of UASs on average profits per firm. In contrast, regarding other manufacturing firms, there is a positive effect for a larger share in manufacturing firms, however, a too strong concentration in these other manufacturing firms is detrimental for positive effects of the establishment of UASs on average profits per firm. When it comes to firm shares in other service industries, the effect is stable across municipalities with differing shares in service industries and becomes insignificant if the concentration in other services is too strong. Therefore, this further analysis provides some evidence that overall positive effects of the establishment of UASs on average profits per firm are driven by municipalities with a large share of firms in industries that rely on knowledge from UASs to innovate, i.e., science based manufacturing and knowledge intensive business services.



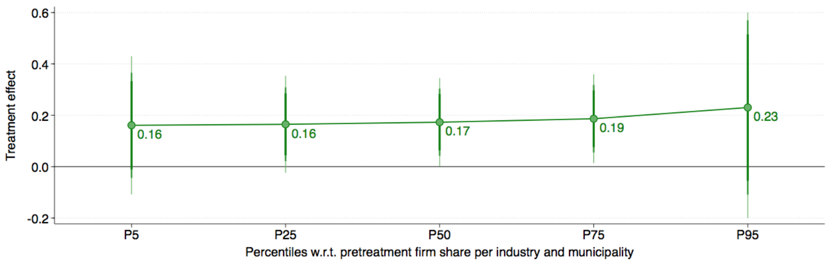
(a) *Science based manufacturing*



(b) *Knowledge intensive business services*



(c) *Other manufacturing*



(d) *Other services*

Notes: Figure E1 shows the coefficients from PPML regressions as shown in Equation (1) with two additional interactions of the $\text{Treat}_i * \text{Post}_{i(t-3)}$ term with (1) the share of firms, and (2) the squared share of firms in a particular industry. The share of firms considered in the regression are with respect to firms in *science based manufacturing* in panel (a), *knowledge intensive business services* in panel (b), *other manufacturing* in panel (c), and *other services* in panel (d). All regressions include municipality fixed effects and canton specific time trends as well as the full set of controls as discussed in the fourth section of the paper. Vertical lines mark confidence intervals.

Figure E1. Treatment effects by a municipality's industry category shares

E.2. Effects on aggregate profits and the number of firms

In this further analysis we focus on two alternative outcomes, i.e., aggregate profits per municipality and the number of firms per municipality. Our main outcome, i.e., average profits per firm is determined by these two quantities. Therefore, this further analysis addresses two important questions for the interpretation of our main results. First, we provide descriptive evidence that the establishment of UASs does not lead to a systematic relocation of firms from the control to the treatment groups. The absence of such a pattern supports the assumption of a stable composition of treatment and control groups, one of the identifying assumptions of a DiD setting (Angrist & Pischke, 2009). Second, we provide empirical evidence that our main finding, i.e., an increase in average profits per firm, is driven by increases in profits and not by an increase in the number of (above average profitable) firms. This result supports the interpretation that our findings represent a supply-side effect.

Regarding a potential reallocation of firms after the establishment of UASs, we would expect that the growth rates of the number of firms in treatment and control groups in the posttreatment years would follow a diverging pattern. However, a year-by-year comparison, shown in Figure E2, reveals that even though, the average number of firms increase slightly stronger in the treatment group (as compared to the control group) after the establishment of UASs, difference are not significant. Moreover, the similar growth rates in the number of firms in the pretreatment periods, and particularly in the last years before the treatment, contradict potential anticipatory firm re-allocations from the control to the treatment groups. Thus the finding of a positive effect on average profits per firm is not merely an empirical artifact of contamination of the treatment group with above-average profitable firms from the control group but represents an overall increase in average profits per firm.

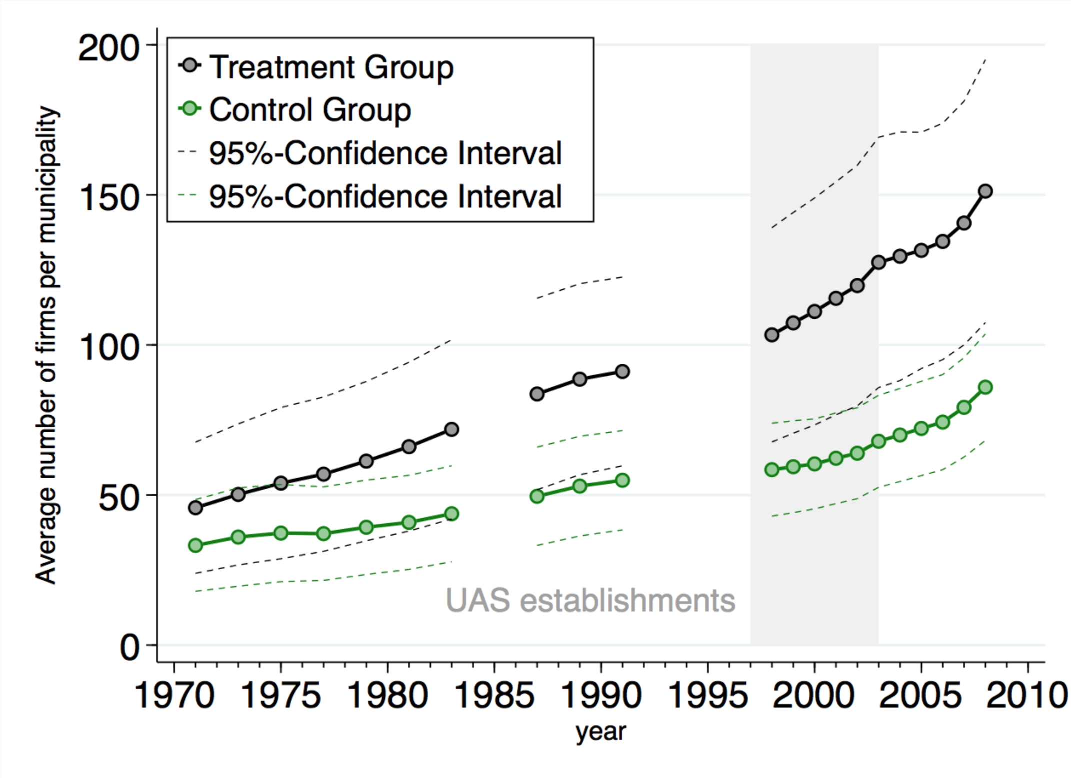


Figure E2. Average number of firms for treatment and control groups over time

Notes: Figure E2 shows the average number of firms by treatment group (black) and control group (green). Where no dots are indicated, data on average profits per firm is missing for *all* municipalities in the corresponding year. Dashed lines indicate 95% confidence intervals. The period of establishment of UASs (between 1997 and 2003) is marked in gray.

To provide evidence that our main finding of increasing average profits per firm is rather driven by supply-side not demand-side effects, we rerun our DiD analysis given in Equation (1) with two additional outcomes: (i) aggregate profits per municipality, (ii) the number of firms per municipality. The idea behind this analysis is that supply-side effects would lead to a pattern where profits of existing firms increase, while demand-side effects would also increase the number of suppliers (i.e., the number of firms) and, thereby attenuate the increase in average profits per firm. Table E1 therefore shows in column (1) the results for aggregate profits per municipality and column (2) the results for the number of firms. Column (3) repeats the result of our main analysis. This further analysis reveals that the effect in average profits per firm is fully driven by positive effects of UASs on aggregate profits, while the number of firms is, on average, unaffected by the establishment of UASs. We thus conclude that our results for average profits per firm reflect rather a supply-side than a demand-side effect, since demand-side effects would also imply an increase in the number of suppliers, i.e., firms.

Table E1: DiD with aggregate profits and the number of firms as outcomes

	Aggregate profits	Firms	Average profits per firm
	(1)	(2)	(3)
Treat_i* $\text{Post}_{i,t-3}$	0.1676**	0.0065	0.1790***
	(0.0807)	(0.0413)	(0.0622)
Average corporate tax rate	Yes	Yes	Yes
Participation exemption	Yes	Yes	Yes
Canton-specific time trend	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Observations	45,066	45,066	45,066
(Pseudo) R^2	0.986	0.977	0.758

Notes: Results report coefficients from PPML regressions. Dependent variable: column (1) aggregate profits per municipality; column (2) firms per municipality; and column (3) average profits per firm (main specification). Robust standard errors are clustered at the municipality level. Due to the inclusion of municipality fixed effects, 68 municipalities are dropped from the estimation, because they are separated by a fixed effect (Correia et al., 2019). Levels of significance: * p<0.10, ** p<0.05, *** p<0.01.

E.3. Effects for greenfield and brownfield institutions

Our third further analysis answers the question of whether the impact of the establishment of UASs on regional firm development differs across regions with predecessor institutions (brown-field institutions) and without predecessor institutions (green-field institutions). For this purpose, we divide our treatment group into two samples (i) one with regions where the newly established UASs are brown-field institutions (1152 municipalities) and (ii) one with regions where the newly established UASs are green-field institutions (142 municipalities). We then compare these two treatment groups to our full control group (752 municipalities). Table E2 shows the regression results using our preferred specification from our main analysis (Table 1 column 5) with average profits per firm as our dependent variable. In column (1), we repeat the results of our preferred specification based on the full sample to compare the effects of restricting the sample.

Column (2) reports the results for brown-field institutions. The result is slightly lower than the one for the full sample, but the effect of the establishment of UASs on average profits per firm remains strongly positive and significant. This result indicates that the establishment of new UASs was more than a sheer relabeling of predecessor institutions. Nevertheless, in regions where no predecessor institutions existed, the establishment of UASs (green-field institutions) leads to an increase in average profits per firm that is almost twice as large as the increase estimated in the full sample or the sample focusing on brown-field institutions. Our result thus shows that regions without predecessor institutions profit even more from the newly established UASs. This larger effect could partly be driven by higher direct and indirect spending linked to the *ex nihilo* establishment of a UAS.

Table E2: Comparison of effects for brown-field and green-field institutions

	Full sample	Brown-field institutions	Green-field institutions
	(1)	(2)	(3)
$\text{Treat}_i{*}\text{Post}_{i,t-3}$	0.1790***	0.1693**	0.3420***
	(0.0622)	(0.0688)	(0.1196)
Average corporate tax rate	Yes	Yes	Yes
Participation exemption	Yes	Yes	Yes
Canton-specific time trend	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes
Observations	45,066	41,349	19,509
(Pseudo) R^2	0.758	0.756	0.758

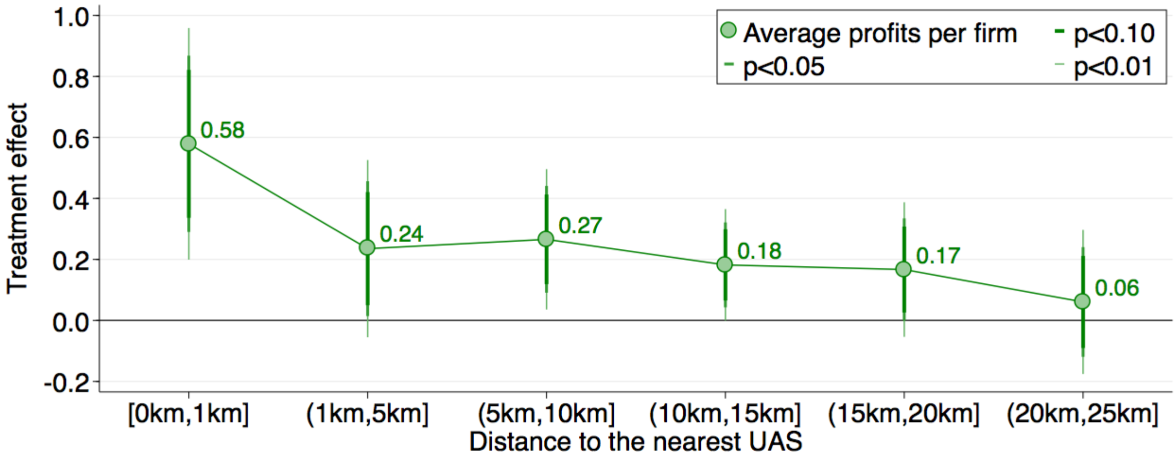
Notes: Results report coefficients from PPML regressions. Dependent variable: average profits per firm. Column (1), full sample (corresponding to our main specification in Table 1, column 5 of the paper); column (2), treatment group constitutes of regions with a brown-field institution only; column (3) treatment group constitutes of regions with a green-field institution only. Robust standard errors are clustered at the municipality level. Due to the inclusion of municipality fixed effects, 68 municipalities are dropped from the estimation, because they are separated by a fixed effect (Correia et al., 2019). Levels of significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

E.4. Geographical localization of effects of UASs

In this further analyses, we investigate the geographical localization of effects of UASs on average profits per firm. In so doing, we first, analyze how the treatment effect develops with increasing distance of a municipality to a UAS. We, thereby provide evidence for our assignment of municipalities to treatment and control groups based on the 25-km radius suggested by Pfister et al. (2021). Second, we show that the treatment effect also persists if we use an alternative geographical level to aggregate our data, i.e., the district instead of the municipality level. We thus provide evidence that our effects are not driven by the geographical aggregation we choose.

Regarding our assumption that the treatment of a newly established UASs occurs within a 25-km radius, we relax the following implicit assumption that the treatment effect is constant within this 25-km by investigating effect sizes within different radii around a UAS. This further analysis is also important, due to two more reasons. First, it provides more detailed information on the localization of effects of newly established UAS and, second, can provide some tentative evidence for the type of spillovers at work. While collaborations (reflecting tacit knowledge) are most sensitive to geographic distances, distance is less important for knowledge spillovers as measures by (patent) citations (reflecting codified knowledge) (Quatraro & Usai, 2017). Thus a constant positive effect across long distances would rather be in favor of knowledge spillovers from codified knowledge translating into higher average profits per firm, while fast disappearing positive effects would be in favor of rather positive effects of tacit knowledge.[14]

We thus rerun our DiD regression including different treatment dummies for municipalities with differing distances to the nearest UAS, i.e. (1) [0km,1km), (2) (1km,5km], (3) (5km,10km] (4) (10km,15km] (5) (15km,20km] and (6) (20,25km]. Figure E3 shows the results of this regression. We find a strong effect in the municipalities nearest to the newly established UASs that decreases with distance and becomes insignificant for municipalities that are farther away than 20 km.



Notes: The Figure reports coefficients from a PPML regression that includes dummies for different distances to the nearest UAS. The control group again constitutes of all municipalities farther away than 25-km from a UAS. The regression includes municipality fixed effects and canton-specific time trends. Dependent variable: average profits per firm. The regression used to estimate these coefficients include control variables, municipality fixed effects and canton-specific time trends as shown in Equation (1). The vertical lines indicate confidence intervals.

Figure E3. Effects on average profits per firm for different treatment radii

This further analysis reveals, first, that effects of UASs on average profits per firm are geographically localized. A finding that is in line with other studies analyzing regional impacts of tertiary education institutions (e.g. Abramovsky & Simpson 2011; Drucker 2016; Rodríguez-Pose & Crescenzi 2008). The strong effect in the municipalities closest to the UASs might thereby be based on effects through tacit knowledge (e.g., co-operations) rather than codified knowledge and might furthermore also be driven by localized expenditure-based effects (e.g., consumption of employees and students). Second, this further analysis reveals that our main specification with a 25-km radius is reasonable but provides a lower bound of the effect by also including municipalities in the treatment group that show no clear positive effect anymore as they are quite far away from a UAS.

For our second analysis regarding geographical localization of the effects of UASs on average profits per firm, we move our unit of analysis to the district level, an administrative unit that lies between municipalities and Swiss cantons and contains on average 25 municipalities.[15] For this analysis, we aggregate all data sets at the district level. While for our outcome and control variables, aggregation is straightforward, the assignment of districts to treatment or control groups is not, since out of the 143 districts, 53 districts contain both treated and untreated municipalities.[16] Therefore, we show two different specifications. The first is following Lehnert et al. (2020) who estimate effects of the establishment of UASs at the regional labor market level (due to data restrictions in their analysis) and only assigns regions to the treatment group if all municipalities within the region are treated. The second specification assumes a treatment intensity, i.e., the treatment variable corresponds to the share of treated municipalities in the district.

Table E3: Treatment at the district level

	(1)	(2)
Treat_i* $\text{Post}_{i,t-3}$	0.2256***	0.3806***
	(0.0674)	(0.1135)
Average corporate tax rate	Yes	Yes
Participation exemption	Yes	Yes
Canton-specific time trend	Yes	Yes
District FE	Yes	Yes
Observations	3,003	3,003
(Pseudo) R^2	0.873	0.874

Notes: Results report coefficients from PPML regressions. Dependent variable: average profits per firm. Column (1) uses a treatment dummy equal to one if all municipalities in the district are treated; column (2) uses a treatment intensity specification, i.e., the treatment variable represents the share of municipalities that are treated in the district. Robust standard errors are clustered at the district level. Due to the inclusion of district fixed effects, 9 districts are dropped from the estimation, because they are separated by a fixed effect (Correia et al., 2019). Levels of significance: * p<0.10, ** p<0.05, *** p<0.01.

Table E3 reports the result of the regressions at the district level. Column (1) shows the results of the regression using a treatment dummy and column (2) the results when including the treatment intensity in the regression. Both coefficients are significant and are higher as compared to our main specification (see column (5) Table 1). This result can imply two things. One important reason for this stronger effect at the district level, in comparison to our main specification at the municipality level, are aggravating issues with omitted variable biases. The analysis at the municipality level allows us to control for time-invariant characteristics that might influence average profits per firm at a much more disaggregated level, thereby preventing omitted variable bias in a stricter way. Remaining with the estimate at the municipality level is thus more conservative and more appropriate.

Notes

[1] Throughout the study we, use the term "UAS" to mean a specific UAS campus specializing in a STEM field and located in a particular municipality, although in practice, (i) there exist UASs specializing in other fields and (ii) one UAS can comprise of several campuses (umbrella organization).

[2] At the secondary and tertiary education levels, Switzerland's education system is divided into an academic education track (baccalaureate and academic university) and a VET track. The VET track combines practical on-the-job training in a host firm (3 days a week) and traditional schooling (2 days a week) and leads to a federal VET diploma. In 2000, around 67% of all students at the secondary education level opted for a VET track (SFSO, 2017). The creation of UASs added a tertiary level for VET graduates and allow those individuals to earn a degree that is different from but equivalent to a bachelor's degree from an academic university.

[3] Conversion by exchange rate as of 2013: 1 USD = 0.927 CHF.

[4] Some of the UASs were later closed as a result of consolidation processes across UAS campuses.

[5] Firms with an own corporate body are limited liability companies (i.e., "Gesellschaften mit beschränkter Haftung"), corporations (i.e., "Aktiengesellschaften"), limited partnerships (i.e., "Kommanditgesellschaften"), cooperative companies (i.e., "Genossenschaften"), associations (i.e., "Vereine"), foundations (i.e., "Stiftungen") and public establishments (i.e., "Anstalten des öffentlichen Rechts").

[6] Firms without a corporate body are sole proprietorships (i.e., "Einzelgesellschaften") and general partnerships (i.e., "Kollektivgesellschaften").

[7] Switzerland is a federal country with three main layers: the federal state, the 26 cantons and the 2,222 municipalities. This federal structure is also reflected in the Swiss tax system (Schweizerische Steuerkonferenz, 2019). Thus firms have to pay taxes at the federal, the cantonal and the municipality level. The cantonal and municipal tax rates

are set by each canton and municipality, respectively. The federal tax rate, however, is standardized at the level of the federal state, where, currently, there exists only one flat tax on firm profits.

[8] Branch establishments are, e.g., production plants, workshops, sales agencies, permanent representations and mines (Art. 51 para. 2 DBG). The definition of branch establishment does not apply to, e.g., storehouses, points of purchases, promotion agencies, and R&D establishments OECD (2017).

[9] A second potential source for lowering our estimated effects is the fact that the federal state has the possibility to grant tax reliefs to startups (for a maximum duration of ten years) in rural areas to foster regional development. However, these tax reliefs appear to be negligible as they add up to on average 37 cases per year between 2002-2011 (Godel & Neuhaus Beaud, 2012).

[10] Around 1% of the zero spells per year are "true zeros", i.e., municipalities with more than three firms (and at most 17 firms) that yet report zero net firm profits.

[11] The ten biggest municipalities in the treatment group—according to the population in 2010—are (1) Zurich, (2) Geneva, (3) Basel (4) Bern, (5) Winterthur (6) Luzern, (7) St. Gallen, (8) Lugano, (9) Biel, and (10) La Chaux-de-Fonds.

[12] The variable contains categories depending on the density, size and accessibility of a municipality and is provided by the SFSO (2012) based on the census in the year 2000. For definitions, see Figure D1.

[13] Although we define our treatment based on the distance, the fact that treated municipalities also show changing distances to the nearest UAS (due to the staggered treatment) helps to identify the propensity score.

[14] We also argued for potential negative impacts of UASs and their effect on average profits through innovation, e.g., *product market rivalry effects*. Bloom et al. (2013) for example show that their impact is less localized than knowledge spillovers. However, as we have found overall positive effects, we abstain from discussing them in detail.

[15] Districts correspond to U.S. counties regarding their position in between municipalities and states, however, Swiss districts are both smaller in surface and population than an average U.S. county.

[16] For the remaining districts, 34 contain only municipalities in the control group and 56 only municipalities in the treatment group.

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