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Micro-level Effects on Publications and Scientific Organization

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When colleges graduate: Micro-level effects on publications and scientific organization☆

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ABSTRACT

We examine the change in status of three Swedish colleges to universities in 1999. This change greatly expanded the inflow of resources in the form of basic funding to the new universities. Using detailed individual data, we follow the careers of staff employed before 1999 at the treated institutions, examining their scientific performance, promotion, affiliation, and coauthorship behavior after the transition to university and comparing them to that of matched sample researchers at control colleges in a difference-in-differences analysis. We find an 89 percent increase in publication by publishing academics, an effect driven by increased funding. But we do not find an increased likelihood of publishing. Publication activity is concentrated among men, those working in technical sciences, and those holding research positions. The change to a university also led to a shift toward research-enhancing practices and organizations, manifested in coauthorship patterns, affiliations, and workforce composition. These changes altogether, enabled the new universities to begin converging in terms of research productivity to the level of established universities. Our results indicate that additional resources to institutions that historically received insufficient investment unlocked the research potential of aspiring researchers.

1. Introduction

In recent decades, governments have changed the landscape of national higher education systems through the upgrading of colleges[1] to universities in many countries. If accompanied by the infusion of resources, such policies can radically change the scientific environment at the upgraded academic institutions. Previous work investigating the relationship between R&D funding and productivity has generated knowledge about the performance of elite scientists who win grants or are based at elite institutions (e.g., Arora and Gambardella, 2005; Azoulay et al., 2010, 2019; Babina et al., 2023). Our research aims to complement prior literature with insights about the results of funding an understudied population of scientists: those who are based at colleges. To achieve this goal, we conduct an in-depth quantitative investigation of the implications for science, in which we exploit the 1999 transition to universities of three Swedish colleges (Karlstad, Örebro, and Växjö), a transition coupled with a substantial increase in basic funding.

The paper uses these transitions to advance our understanding of the role of (basic) science funding on three levels. First, the setting gives us an opportunity to study the effects of raising funding for less endowed academic institutions; second, the experimental setting enables a plausibly causal understanding of the effects of science funding; and, third, our results might offer important lessons for research policy.

The distinct way in which a transition from a college to a university boosts research funding to academics is at the center of our first contribution. Normally, funding of scientific activity takes one of two main routes, either through basic funding (or bloc funding/grants; government appropriations) or competitive grants, each of which has

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[1] Also known as university colleges, community colleges, and polytechnics.

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distinct characteristics. Both reinforce stability and path dependence. Basic funding decisions are often rooted in historical decisions, linked to number of students, and, with few exceptions, subject to relatively small changes over time as they become institutionalized (Geuna, 2001; Auranen and Nieminen, 2010). Patterns of stability emerge in competitive grants at the individual level, as having a successful publication record raises the odds of obtaining research grants. These Matthew effects typically reinforce initial differences over time, giving rise to "winner-take-all" situations (Merton, 1968, 1973). These effects are reinforced by reputation: basic funding provides a floor, which can be viewed as reducing the risk of project failure for reviewers, foundations, and grant-awarding agencies that consider grant proposals. Conversely, because of weak funding in the past, colleges tend to perform little research, and their poor research track record signals "weakness" individually and collectively, that is, leading funders view their projects as risky bets. Therefore, the creation of new academic institutions or changes in institutional status could create break historical path dependence, especially if it is coupled with long-term basic funding.

Second, the path-dependent nature of the funding system also creates an endogeneity problem in the evaluation of funding, which can hinder the interpretation of causal effects typically investigated in studies on the effects of university R&D on scientific results (e.g., Adams and Griliches, 1998; Bonaccorsi and Daraio, 2003; Crespi and Geuna, 2008).[2] In order to interpret the causal effects of scientific R&D on publication, previous papers have studied quasi-experimental conditions that enable them to identify the characteristics that give rise to selection from treatment, that is, funding effects. Some of them study individual productivity gains after the receipt of project grants (Jacob and Lefgren, 2011; Benavente et al., 2012), while others examine broader consequences at the university level (Payne and Siow, 2003; Whalley and Hicks, 2014; Rosenbloom et al., 2015; Babina et al., 2023).

Payne and Siow (2003) examine the effects of US federal funding on 68 universities due to political change, finding that USD 1 million in increased funding raised output by 10 articles and 0.2 patents. Whalley and Hicks (2014) examine the effects due to changes in the value of an endowment, estimating an elasticity of 1 from research spending to paper output, although a negative effect on citation quality was revealed. Rosenbloom et al. (2015) examine the effects for chemists, finding positive effects for both publications and citations, due to increases in federal funding. The effect was about 19 articles per USD 1 million, with a decline in the cost per article in the 1990s and early 2000s. In a recent paper, Babina et al. (2023) examines the effects of cuts in federal funding. Their results indicate a decline in academic publication (though not quantified in terms of articles per dollar), and entrepreneurship, but an increase in patent productivity.

Unlike earlier research, which has concentrated on either individuals or universities with existing resources, sometimes at the higher end of the productivity distribution, our paper gives new quantitative and plausibly causal evidence based on a group of individuals with little to no prior research resources. These academics are typically occupied with teaching activities and, therefore, lack the time for research. The change in institutional status, coupled with increased basic research funding, gives at least some of them the opportunity to devote more time and resources to research. Therefore, we expect to observe increases in research output that is "unlocked" by the infusion of resources. Although a few previous studies evaluated the effect of the establishment and promotion of new colleges and universities, they focused not on the effect on scientific knowledge production or organization but, rather, on growth (Bonander et al., 2016) and innovation outcomes (Andersson et al., 2009; Toivanen and Väänänen, 2015; Pfister et al., 2021; Andrews, 2023).

Our third contribution concerns the potential research policy implications. Most often, funding allocation decisions are, as indicated above, based on a record of past performance and "safe bets" (Stephan et al., 2017; Wang et al., 2017), rewarding scholars (Azoulay et al., 2010, 2019) or their environment, for instance, through initiatives such as those for research "excellence" (e.g., Hellström et al., 2017). Thus evidence of publication gains at institutions without a strong historical publication record might enhance the potential attraction of allocating funds to small players, plausibly at a lower cost to public sources of funds.

The upgrading of colleges to university status is neither a rare event internationally nor confined to Sweden. The two-tiered structure[3] of the Swedish higher education system is similar to that of many other OECD (Organization for Economic Cooperation and Development) countries, where universities are typically responsible for research and education, whereas colleges are tasked primarily with extending education. For example, over the past two decades, twelve new universities were recognized in the UK (Office for Students, 2023). Another example is Israel, where, over the same period, two colleges were awarded university status, and one more is being considered. In Sweden, after the 1999 expansion, three more colleges were upgraded.[4] The evidence in this paper could help policy makers understand the effects of these transitions and the returns from them.

The data to investigate the 1999 transition come from a comprehensive individual-level panel dataset. The primary source is a total of 25,000 researchers' publications linked to Swedish register data and university staff registers. Equipped with this longitudinal material, we perform difference-in-differences (DiD) analyses at both the department and individual level for the period 1997–2009. The department level is utilized to assess the impact of resource influx on total publication levels for researchers at new universities (both pre-1999 incumbents and post-1999 hires) compared to departments at colleges without university status. At the individual level, although we cannot discern access to resources by individuals, we can provide causal estimates for the treated group that worked before and during the transition.[5] We compare them to a control group of similar people working at nontreated colleges, providing a counterfactual baseline for comparison using matched sample estimations. Focusing on this group shows what can be expected of similar groups that obtain funding and are at the lower end of the scientific productivity distribution.

We find that the upgrading to a university was coupled with a boost in basic funding and led to substantially higher rates of publication, which is noteworthy considering that it took place at institutions that lacked a strong publication record. Our department-level analysis shows that this effect is explained by increased funding. Individual-level analyses indicate an average increase in the publication rate of 89 percent or about 0.57 additional publications per year for individuals who publish. We present several robustness checks and conclude that this result is potentially a conservative estimate of the real effect. We find no overall effect after adjusting for quality. Furthermore, the headline effect for number of publications masks substantial heterogeneity among groups of staff. Moreover, staff in the treated colleges were not more likely to publish than staff in the control group.

[2] Some of these endogeneity mechanisms include reverse causality from past publication activity to renewed funding and selection of able researchers into treatment through new funding.

[3] Some scholars reason that the de jure two-tiered system (a product of a set of landmark 1977 Swedish higher education reforms) is de facto more of a multi-tiered one. For simplicity, we consider the Swedish higher education system as two-tiered (Hallonsten and Holmberg, 2013; Holmberg and Hallonsten, 2015).

[4] After the 1999 transitions we study, Mid Sweden University was promoted in 2005, Malmö became a university in 2018, and Mälardalen in 2022 (Ejermo and Sofer, 2023)

[5] We deliberately do not include those newly hired after 1999, as no clear before-after comparison can be made and because of the difficulty in separating treatment from selection for this group.

When we explore heterogeneity in the effect, we find interesting patterns in terms of the field, position, and gender.[6] When we investigate the field differences, we only find a positive effect on those who are active in the technical sciences, but no unambiguous effect was found in other disciplines. Moreover, we find strong positive effects on the rate of publishing by researchers and men but no effect on teachers or women. Women often have stagnant academic careers due to motherhood (Kim and Moser, 2021; Cairo et al., 2023), but this does not necessarily seem to be the main reason here. Instead, we relate the lack of statistically significant results for women to their underrepresentation in the technical sciences and research positions.

Our finding of a relationship between basic funding and production of scientific knowledge is then supplemented with an investigation of the changes in the academic environment and organization that support research. We find that new universities and their scholars became more research oriented and redirected their relationships inward and that collaboration with researchers at other colleges declined. Specifically, we document an increase in the research workforce and a relative decrease in the number of teachers i.e., those focused on classroom instruction. Further, the treated scholars reduced the total number of affiliations they had with other academic organizations relative to the control group. This negative effect results from a sharp decline in affiliations with colleges, but not with established universities. Moreover, we see an overall increase in coauthorship, driven mainly by increased coauthorship with colleagues at their home university.

Finally, we provide descriptive evidence of (field-specific) convergence in the productivity of scholars at the new universities with that of peers at universities, and divergence from that of peers at remaining colleges. These results clearly indicate that the upgrading to university status led to lasting changes in performance, which became more obvious over time.

2. Institutional background

We start by giving the contextual background for our paper, in which we outline the historical setting and origin of the three Swedish universities studied and then describe the evaluation and the political process in which Karlstad, Örebro, and Växjö were awarded university status.

2.1. Historical context: The Swedish system of higher education

After World War II, as occurred in other Western countries, Sweden's system of higher education expanded, especially in the 1960s and 1970s. The existing elitist university system had struggled to meet the increased demand for education, creating pressure on policy makers to address the problem (Askling, 1989). After a lengthy process of designing the reforms, they were finally enacted in 1977.

The 1977 reforms led to the establishment of twelve new colleges, including Karlstad, Örebro, and Växjö. The reforms also reorganized the system, creating two tiers: colleges and universities (Elzinga et al., 1993; Andersson et al., 2009; Holmberg and Hallonsten, 2015). Whereas universities were tasked with conducting scientific research as well as providing education, colleges were only given the task of providing education. This difference in their missions resulted in a division of funding into research and teaching, respectively, in which full universities could draw basic funding from both categories, but the new colleges could only obtain basic funding for teaching (Holmberg and Hallonsten, 2015).

In the 1980s, scholars at the new colleges sought to engage in research. Yet this ambition was hindered by the lack of university basic funding for research and limited access to external funding. The first twenty years after the 1977 reforms were nevertheless characterized by "academic drift", the growing tendency of scholars at these colleges to behave like full universities through "emulation of practices of established academic organizations" (Holmberg and Hallonsten, 2015, p. 181). However, this ambition ran up against institutional barriers rooted in the 1977 reforms.

To address these obstacles, colleges, together with municipal and regional political leaders, pressured policy makers to lift some of the restrictions blocking their development. This effort achieved some success in the 1990s in the form of small-scale changes, limited to specific aspects of colleges' needs. These reforms increased the autonomy of higher education institutions, for example, by allowing them to award professorships and start PhD programs, albeit only through cooperation with established universities (Sjölund, 2002; Holmberg and Hallonsten, 2015). Moreover, external, public, and competitive research funds began to be available in the 1980s and that process accelerated in the 1990s (Benner and Sörlin, 2007). This availability enabled scholars at some colleges to compete for individual research funding (whether due to pressure from management or their own motivation), although with limited success (Bauer et al., 1999; Stensaker and Benner, 2013; Silander and Haake, 2017).

After two decades of mounting political pressure, the Swedish government openly indicated its willingness to upgrade several colleges to universities. In 1997 it designed an "institutional career path" (Holmberg and Hallonsten, 2015, p. 189), a framework and procedure for colleges that aimed to become universities. Sjölund (2002) records the process beginning with the 1997 application for university status by four colleges from an insider's perspective.

2.2. The 1999 university transitions

Historically, transition from a college to a university was not common in Sweden, but it has become more frequent since the turn of the twenty-first century. In 1997, four colleges – Karlstad, Örebro, Växjö, and Mid Sweden – applied for university status, the first step in a process filled with bureaucratic assessments and consideration of those assessments. Fig. 1 details the decision-making process. After receiving the four applications, the Swedish government, which holds the power to grant university status, assigned the task of evaluating these applications and making a recommendation about their potential success to the Swedish National Agency for Higher Education (HSV). After a little more than a year of internal deliberations, the HSV gave the government its official recommendations.

During this year, the process of shaping the agency's recommendations was characterized by several unexpected turns and political infighting. The HSV board, whose members were politically appointed, further delegated the task of evaluation to a committee of professionals. The committee was asked to consider the applicants' educational capacity, supporting infrastructure, and breadth of research in terms of disciplines. The evaluators recommended that the board only award Karlstad university status and reject the applications of the other three institutions. Out of reluctance to accept the recommendations regarding Örebro and Växjö, the board advised them to reapply for "research area status", that is, university rights in one of four broad disciplines – medical sciences, technical sciences, natural sciences, and the humanities and social sciences – but not to expect to receive full university rights. The board also recommended rejecting Mid Sweden's application (Sjölund, 2002; Holmberg and Hallonsten, 2015).

Örebro and Växjö followed this recommendation, after which the HSV appointed another committee of professionals to assess the narrower applications, again concluding that they failed to satisfy the conditions for even this limited upgrade. The HSV board again rejected the professionals' assessment and advised the government to reject Mid Sweden's application, to award Karlstad full university status, and to grant Örebro and Växjö research area status in the humanities and

[6] Gender here refers to sex assigned at birth and is binary in the administrative records. A very low number of individuals have different values over time. For these individuals we assigned the most common value.

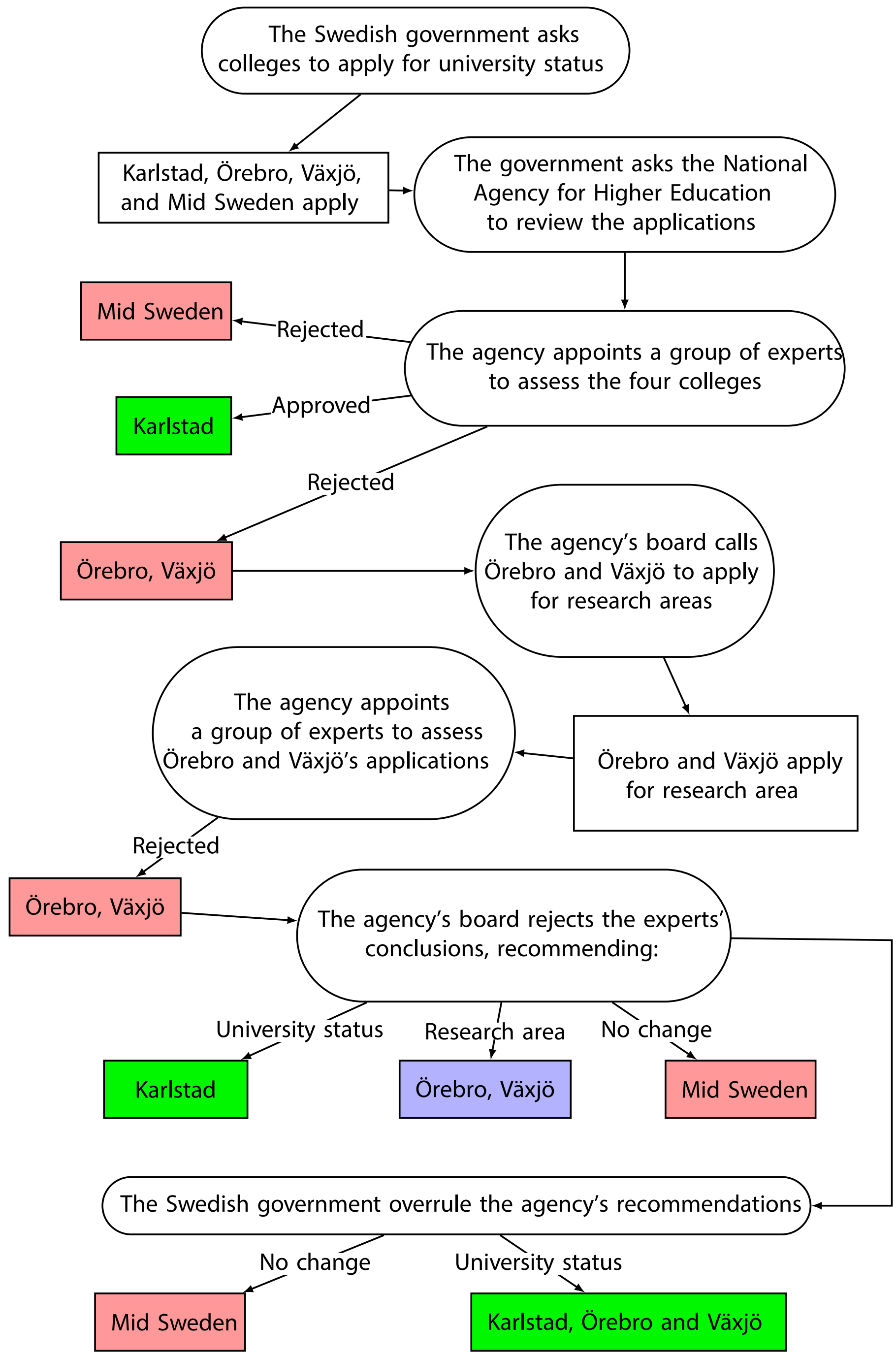

Fig. 1. The process of the 1999 expansion. *Note:* This figure is based on Sjölund's (2002) documentation of the policy-making process and appeared in Sofer (2021).

social sciences. Finally, in early July 1998, the Swedish government, based on personal and political considerations, disregarded the HSV's recommendations, and awarded Karlstad, Örebro, and Växjö full university status and postponed the decision on Mid Sweden (Sjölund, 2002; Andersson et al., 2009).

3. Data and analysis levels

The data we use to study the 1999 university transitions come from a combination of different sources. In particular, bibliometric data in the administrative records of university employees and demographic information on the Swedish population come from Statistics Sweden. This has methodological advantages, as it enables us to apply matching to increase comparability between treated and control samples. Furthermore, our dataset allows us to investigate factors that contribute to scientific productivity at the individual level, such as citations (as a measure of quality), coauthorship, and having multiple affiliations with other Swedish academic institutions.

Our main analyses are conducted at two levels: the department level and the individual level. The department level is used because it is the most granular level for which we have data on R&D funding and, at the same time, can group individuals.[7] Biennial data on R&D funding

[7] The "department" level, strictly speaking, does not mean departments but refers to the intersection between universities and national subjects. We use this term as it tends to correspond to departments, in particular at smaller

by university and discipline comes from Statistics Sweden. We impute data on R&D funding for even years, using the rate of annual growth calculated.

Information on publications comes from a data collection effort called PARIS (Publications by Academic Researchers in Sweden, Ejermo et al., 2016) completed in 2016.[8] The construction of PARIS involved publications and author IDs from the Scopus database, a major source of bibliometric information.[9] Then, staff lists from Swedish universities and colleges were used to match with Scopus author IDs which sometimes needed further disambiguation. The last step in the creation of PARIS entailed linking material that uses the social security number to Statistics Sweden databases. Eighty-five percent of all publications by Swedish academic researchers in Scopus were linked in the database. In addition to publication counts, we also have data on citations and coauthorships.

In our empirical analysis, we define individuals as treated if they worked at one of the three institutions that became universities (Karlstad, Örebro, and Växjö) or as control individuals if they worked at six colleges (Dalarna, Gävle, Jönköping, Kalmar, Kristianstad, and Södertörn) that did not experience this transition. We include in the control group all colleges for which we had complete data beginning in 1997 (Ejermo et al., 2016). We compare individuals at colleges, rather than universities, to ensure that baseline (comparison) conditions were as similar as possible at the outset for the treatment and control groups. We select individuals in both the treatment and control groups who worked at the same institution for two years before the transition and in the first year after it (1997–1999). This ensures that individuals were unequivocally in either the treatment or control group. Individuals are permitted to move between institutions after 1999 in our estimation sample, as we refrain from conditioning on a potential outcome of the university transition, namely, academic mobility. Finally, because staff lists did not stretch back to 1996 for the relevant treated and control academic institutions, we use 1997 as the first year in our analysis. The last year of the analysis is 2009, as in the following year, Växjö (university) merged with Kalmar (college) to form what is now Linneaus university.

Our choice of control colleges is debatable, as some of them obtained research area status during the period under examination. Obtaining research area status implies that an academic institution acquired the right to offer PhD studies and to receive some basic funding. This is relevant to Jönköping, which obtained research area rights in social sciences in 1995, and Kalmar, which received similar rights in the natural sciences in 1999 (The Research Council, 2023).[10] As part of our robustness checks, we rerun the main estimated model without Jönköping and Kalmar.

University registry data at Statistics Sweden include standardized data on positions, fields, and places of employment. These aspects of our data enable us to study promotions, affiliations, mobility, and, together with coauthorship data from PARIS, how collaborations develop as a consequence of the transition. In addition, we observe information on age, gender, and other individual characteristics, which is useful for matching treated and control individuals. The positions that university employees hold are coded consistently by Statistics Sweden, and we use them in the paper where we translate the Swedish original into the term in English with the closest meaning. We also group some positions into categories, namely Research, Teaching, Admin & support, and Other. The names of positions in Swedish and English and group categorization are given in Appendix A, Table A.3.

We render missing data on publications as zero, as this would be the most likely true outcome. Data can be "missing" in two different ways. First, information could be missing when a certain individual is known to have published in other years and have publications in a given year, yet they are not linked to her author ID and, therefore, do not end up in the panel. We consider these missing observations unlikely and rare. Second, data are considered missing when people are missing, that is, they should have published, but the rate of publication is unknown. In both cases, we set publications at 0. Our value for forward citations takes a value of 0 if a scholar has publications in that year that are not cited, but the value is considered missing if the scholar has no publications that year.

Because the main method of analysis (see below) involves a comparison between treated and controls in DiD analysis, biases occur if systemic differences are expected to be found between treated and controls. All estimates which use publications as the dependent variable are based on data at the individual level. We find it more plausible that the second case, that is, people who are completely missing, is a more likely source of error in our data. We also view it as more plausible that we could miss publications by individuals in the treatment group if they are more likely to start publishing as a result of the change in university status. If this characterization is correct, then our estimates will be biased downward.

4. Methodology

4.1. Matched sample method

Our individual-level analysis leads to a different causal interpretation from a department-level analysis. Whereas the department level assesses the results of total publications, whether stemming from incumbent staff or new hires, the individual level focuses on those who worked at the institutions (treated or control) before 1999. Thus we follow individuals who were employed before and after the transition to a university and compare them to a group of individuals who worked at the control colleges before 1999. To minimize the risk that the composition of individuals at treated and control institutions would bias our estimation, we employ matching. We use individuals' 1997 characteristics for matching to make them predetermined in relation to outcome, retaining individuals whose main affiliation is fixed at either a treated or control institution in 1997–1999 but impose no restriction on affiliation or any other characteristic in the post-period.[11]

We use the method of coarsened exact matching (CEM) following the procedure laid out by Iacus et al. (2012). CEM allows us to create our own matching categories, based on both continuous and categorical variables. We can also apply either exact matching or coarsened matching, in the latter case allowing an algorithm, such as Sturge's rule, to determine the bin size. For each treated individual, the algorithm picks one or more control individuals. This procedure generates a number of weights that are used in the regressions and descriptive statistics to correct for imbalances arising from the number of treated and controls in different matching strata.

We base our matching characteristics on factors identified in the literature on the economics of science as important for explaining scientific productivity (e.g., Stephan, 1996, 2012). We match precisely on gender (male/female), position, and research area and use coarsened

academic institutions. See Appendix A, Table A.2 for a list of the national subjects in the study. These subject fields are based on the OECD classification Field of Research and Development (FORD).

[8] This section is based on the description in Ejermo et al. (2020). Further details are provided in Appendix B.

[9] At the time that PARIS was constructed, but Scopus's coverage began in 1996.

[10] We exclude Mid Sweden University from the examinations because of its muddled treatment/control group status: after the decision in 1999 the Swedish government had guaranteed to award Mid Sweden University full university status in the coming years. Moreover, Mid Sweden was given "research area" status in natural sciences, which was coupled with an increase in research resources beginning in 2001.

[11] As detailed above, the first application for university status was submitted in 1997, which marked the beginning of the assessment process.

matching on age. We include all staff at the treated and control institutions. For matching, we divide the staff into four categories based on their 1997 position title: (1) researchers, (2) Ph.D. students, (3) teaching staff, and (4) other (see Appendix A, Table A.3). Although it is unlikely that, for example, cleaning staff would ever become researchers, it is not unthinkable for someone in an administrative position to transition into a Ph.D. student. We thus do not condition the sample as researchers or teachers in 1997.

Table 1 shows descriptive information on the full (unmatched) sample and the matched sample of individuals. For the matched sample, we report weighted averages based on the weights obtained after matching.

Even without matching, treated and control individuals are not very dissimilar. The rate of publication activity is low, with an average of only 0.02 publications/year by the treated and 0.04 by those in the control group.[12] Some characteristics that could affect publication activity are similar, such as average age and the share of women. However, the treatment and controls differ in terms of their share of the staff in different positions as well as disciplines. There are more professors, senior lecturers, lecturers, and temporary staff in the treatment group than in the control group. Further, the share of individuals in the humanities and social sciences, medicine, and other fields is larger in the treated group, whereas the share of scholars in natural as well as technical sciences is higher in the control group.

Matching reduces this heterogeneity. We match 1,353 individuals, or 94% of those in the treatment group, and use 91% of the control individuals. The differences diminish for publications, citations, and the number of coauthors, even though they are not matched upon. The variable "affiliations" indicates the number of affiliations of an individual in addition to the main university. No change is observed in the difference in the number of affiliations after matching.

The last two columns, which divide the matched sample by gender, reveal distinct differences. Female staff have virtually zero publications in 1997, compared to a low number by male staff. Wages are substantially higher for male staff, but age differences are small. Perhaps surprisingly, male staff hold a higher share of teaching positions (37 vs. 29 percent) but also a much higher share are associate professors (27 vs. 7 percent) than among female staff. Male staff are found in the humanities and social sciences somewhat more frequently than female staff, whereas female staff are found more often in medicine. The latter discipline encompasses nursing schools, which were prevalent at the three treated institutions, which probably explains the high share of females. Men are employed much more often in natural and technical sciences, whereas female staff are frequently found in "Other fields", a very heterogeneous group of disciplines.

Fig. 2 shows the development of publications in the matched sample. Treated and controls follow each other well in 1997–1998. After 1999, the number of publications at the new universities rises, but, at the same time, the publication rate in the control group fluctuates. This fluctuation is hard to explain except through random variation. After 1999, the positive trend continues for the treated such that, by 2009, its publication rate is substantially higher than that of the control group.

4.2. Econometric methodology

We use DiD models to examine whether several outcomes related to scientific performance changed at the treated institutions. The main point of the method is that it nets out trends in the outcome variable that are common to both treated and control groups. We first verify that R&D funding increased at the treated institutions and then turn to our main variable of interest, publications as well to consider other changes at research-supporting scientific organizations. The identifying assumption is that, in the absence of treatment, the publication rate by the treatment group would follow that of the control group. To draw this conclusion, the assumption of a parallel trend in the outcome variable before treatment should hold (Ashenfelter, 1978).

The main estimation model uses a Poisson model, summarized as:

$$E[y_{it}|DiD_{it}, \lambda_t, \delta_i] = exp[\gamma DiD_{it} + \lambda_t + \delta_i], \quad (1)$$

where i is either a department, as defined above, or an individual, and t represents years. The outcome variable, y_{it}, represents, in year t, R&D funding, publications, and the number of employees when we look at departments, and the number of research publications, citation-weighted publications, promotion, coauthors, mobility, and affiliations when examining individuals. λ_t are year fixed effects, and δ_i represent department or individual fixed effects, included in the model to capture time-invariant unobservable factors, such as research culture, motivation, and talent.

DiD_{it} is the treatment dummy variable. It takes a value of 1 for departments/individuals at the treated universities after the treatment was introduced. The coefficient of interest is γ, which represents the treatment effect of upgrading in university status on the outcome. In order to interpret the coefficients as percentage increases, we exponentiate the coefficients and subtract one.

Our use of the Poisson model has three motivations (Bertrand et al., 2004; Silva and Tenreyro, 2006; Azoulay et al., 2010; Cameron et al., 2010; Wooldridge, 2010). First, Poisson models are often used when the dependent variable takes discrete (count) values. Second, panel data entail the risk of serial correlation, to which the Poisson fixed effect model is robust. Third, the model is also robust to distributional misspecification, that is, it can be used even if the underlying distribution is not Poisson.

We cluster standard errors at the level of academic institution (university or college), reasoning that this is the appropriate treatment level (Moulton, 1986; Cameron and Miller, 2015; Abadie et al., 2022). An important aspect of the Poisson individual fixed effect model is that scholars who do not publish[13] are completely eliminated from the estimations.[14] This implies that our Poisson regressions estimate how the transition in 1999 changed the rate of publication among those who published, that is, they represent an intensive margin. But it does not answer the question of whether treated individuals began to publish.

For this purpose, we also estimate linear probability models (LPM), replacing the number of publications with a variable that takes a value of 1 if any publication activity took place. The advantage of this specification is that it does not limit our sample to researchers who published but, rather, includes those who did not. Thus, the LPMs reveal whether the 1999 transitions changed the extensive margins.

We also estimate model lead–lag models, where, instead of assuming a constant treatment effect, we estimate different posttreatment effects by year to determine the pattern of outcomes over time. In addition, estimated lead effects enable us to see whether pretreatment effects exist, which could violate the parallel-trend assumption.

$$E[y_{it}|\{D_{i\tau}\}^{m}_{\tau=-q,\tau\neq-1}, \lambda_t, \delta_i] = Exp[\sum_{\tau=-q,\tau\neq-1}^{m} \gamma_\tau D_{i\tau} + \lambda_t + \delta_i] \quad (2)$$

This model includes q leading years and m lagging years. We estimate one leading year ($q = 1$) and eleven lagging years ($m = 11$), setting 1999 as year zero and use the first leading year (1998) as our reference point.

[12] Removing nonresearcher and nonteaching staff roughly doubles these numbers.

[13] In our data, departments as a whole rarely have zero publications.

[14] Technically, everyone with a constant rate of publication is omitted, which in theory means omission of individuals who publish at a constant rate every year. This does not occur in our sample.

Table 1
Descriptive data for the full sample and the matched sample of individuals at the treated and control colleges in 1997.

	(1) All treated ind.	(2) All control ind.	(3) Matched treated ind.	(4) Matched control ind.	(5) Matched treated female	(6) Matched treated male
Publications	0.02	0.04	0.02	0.03	0.00	0.04
	(0.21)	(0.66)	(0.19)	(0.40)	(0.06)	(0.26)
Citations	3.45	9.68	3.75	4.93	2.50	3.89
	(7.90)	(31.25)	(8.24)	(16.17)	(0.71)	(8.70)
Coauthors	0.01	0.02	0.01	0.01	0.00	0.01
	(0.07)	(0.31)	(0.07)	(0.22)	(0.04)	(0.09)
Affiliations	0.04	0.02	0.04	0.02	0.03	0.06
	(0.20)	(0.15)	(0.20)	(0.14)	(0.17)	(0.23)
Wages, 1000 SEK/year	302.88	315.81	305.36	320.94	260.48	347.48
	(125.75)	(132.41)	(123.50)	(119.78)	(104.47)	(125.20)
Age	45.77	45.31	45.96	46.01	46.17	45.77
	(10.13)	(10.50)	(9.79)	(9.76)	(9.36)	(10.18)
Female	0.49	0.46	0.48	0.48	1.00	0.00
	(0.50)	(0.50)	(0.50)	(0.50)	(0.00)	(0.00)
Position						
Teaching positions	0.33	0.25	0.33	0.33	0.29	0.37
	(0.47)	(0.43)	(0.47)	(0.47)	(0.45)	(0.48)
Other research and	0.03	0.03	0.03	0.04	0.02	0.04
teaching staff	(0.18)	(0.17)	(0.17)	(0.19)	(0.14)	(0.19)
Temporary staff	0.03	0.02	0.02	0.02	0.02	0.02
	(0.16)	(0.14)	(0.14)	(0.14)	(0.13)	(0.15)
PhD students	0.02	0.03	0.02	0.02	0.02	0.02
	(0.15)	(0.16)	(0.13)	(0.13)	(0.12)	(0.13)
Associate Professors	0.17	0.13	0.17	0.17	0.07	0.27
	(0.38)	(0.34)	(0.38)	(0.38)	(0.25)	(0.44)
Postdocs and Assistant	0.00	0.00	0.00	0.00	0.00	0.00
Professors	(0.03)	(0.00)	(0.03)	(0.00)	(0.04)	(0.00)
Professors	0.01	0.00	0.01	0.01	0.00	0.01
	(0.08)	(0.06)	(0.08)	(0.07)	(0.00)	(0.11)
Support staff	0.40	0.54	0.42	0.42	0.59	0.27
	(0.49)	(0.50)	(0.49)	(0.49)	(0.49)	(0.44)
Field						
Agricultural	0.00	0.00	0.00	0.00	0.00	0.00
sciences and veterinary medicine	(0.00)	(0.03)	(0.00)	(0.00)	(0.00)	(0.00)
Humanities and social sciences	0.44	0.38	0.45	0.45	0.38	0.52
	(0.50)	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)
Medicine	0.04	0.01	0.03	0.03	0.07	0.00
	(0.20)	(0.11)	(0.18)	(0.18)	(0.25)	(0.00)
Natural	0.06	0.12	0.06	0.06	0.04	0.08
sciences	(0.25)	(0.33)	(0.24)	(0.24)	(0.19)	(0.28)
Other fields	0.35	0.34	0.35	0.35	0.49	0.23
	(0.48)	(0.47)	(0.48)	(0.48)	(0.50)	(0.42)
Technical	0.10	0.15	0.10	0.10	0.03	0.17
sciences	(0.30)	(0.35)	(0.30)	(0.30)	(0.17)	(0.37)
Observations	1,443	1,404	1,353	1,274	655	698

Note: For variable definitions, see Appendix A, Table A.1. For positions, see Appendix A, Table A.3, and for fields, see Appendix A A.2. In this table, we group librarians, administrative, and technical staff into the group "Support staff." Wages are in 2010 prices.

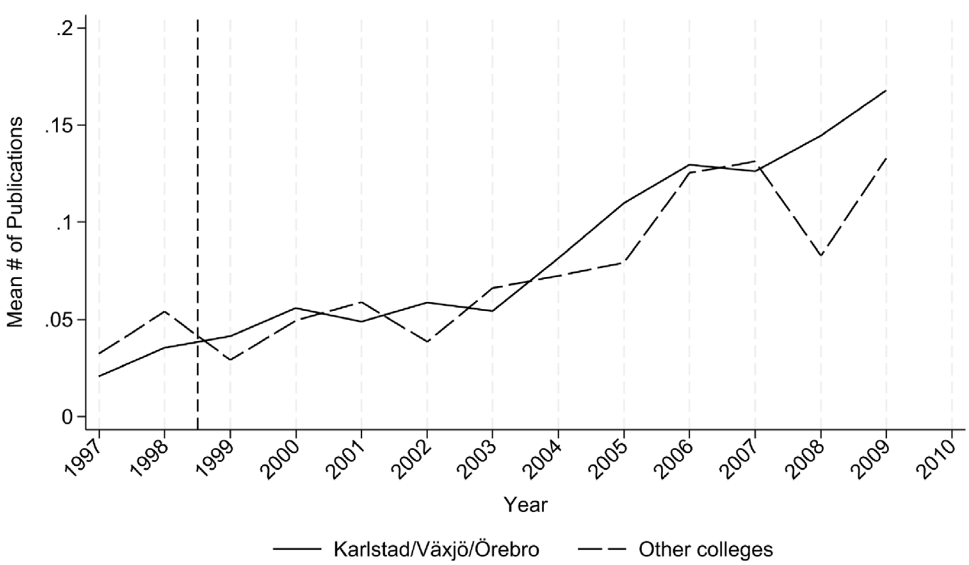

Fig. 2. Trends in Mean Publication Rate, Treatment/Control Matched Sample.
Note: Matching is based on characteristics in 1997. In "Other colleges" we include scholars at the control group institutions.

5. Results

Our empirical investigation is divided into four subsections. We start by providing empirical evidence for our claim, key to our experimental setting, that obtaining university status in Sweden is directly related to an increase in basic funding. Next, we investigate the effect of basic funding on scientific knowledge production. First, we conduct our analysis at the department level, as it is the most granular level of R&D funding data at our disposal (Statistics Sweden does not have individual-level R&D funding data). By aggregating at the department level, we can directly link R&D funding to publications and examine whether increased funding is the driving force behind knowledge production. We also derive estimates of the effects of R&D funding on the number of publications. Notably, the effect estimated at the department level comes from two changes: an increase in publications by incumbent staff as well as new hires.[15] Second, to understand the effects on incumbent researchers, we focus our investigation at the individual level. This enables us not only to achieve higher precision but to explore effect heterogeneity (by gender, position, and scientific field).

We proceed by researching how the transition to a university affected the way in which science is organized and performed. To do so, we consider the effect on the composition of the academic workforce, as well as promotions, mobility, affiliations, and coauthorship patterns. We finalize our empirical results section with several robustness checks, and add descriptive evidence on the convergence of productivity among scholars at the new universities with those at existing Swedish universities (and divergence from colleges).

5.1. University status and R&D funding

As detailed above, in Sweden being a university is about more than just a name. Since 1977, being a university meant access to basic research funding. Fig. 3 compares the trends in total, basic, and other R&D funding (in 2010 prices) and the development of researchers and teachers at the 1999 universities, and six colleges, that were not upgraded.[16] The top-left graph shows that the three treated institutions – Karlstad, Örebro, and Växjö – plus the nontreated Södertörn obtained a substantial increase in total funding after 1999. The top-right graphs shows that, in terms of basic funding, only the three treated institutions obtained substantial increases. The bottom-left figure shows that Södertörn's increased funding came from "Other funding" (see discussion below). Finally, the bottom-right graph shows that the three treated institutions hired many more research and teaching staff than those in the control group.

To substantiate this descriptive evidence, we use the DiD framework to formally test whether the increase in funding can be explained by the upgrading in 1999. The treatment group includes departments at the three 1999 universities, and the control group consists of departments at the six untreated colleges, as described in Section 3. We report the results in Table 2. The table also divides R&D funding into "basic funding" and "Other funding". The latter consists of many sources of funding, reflecting the wide variety in the Swedish funding landscape.[17]

Table 2
Becoming a university: effects on R&D funding at the department level.

	(1) Total R&D	(2) Base R&D	(3) Other R&D
DiD	4.067*	7.844***	−3.776*
	(1.945)	(0.653)	(2.023)
Estimation method	OLS	OLS	OLS
Department FE	YES	YES	YES
Year FE	YES	YES	YES
Mean outcome	8.44	2.76	5.68
Number of departments	134	134	134
Obs.	1008	1008	1008

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 confirms that, at treated departments, total funding substantially increased over that at untreated colleges. On average, the increase was 4.1 million Swedish kronor (SEK) in a department at a treated university over that at a control college. The second model shows that the increase, in line with our prediction, is even greater in basic funding, showing an average increase of SEK 7.8 million over that at control departments. Perhaps the most interesting result is in model (3) for "other funding", which shows that, on average, treated departments receive SEK 3.8 million *less* than control departments. These gains in funding are substantial, as descriptive data show that, before the transition in 1997–1998, an average department at one of the treated colleges had SEK 4.3 million in total funding, of which a minuscule SEK 0.06 million was basic funding and SEK 4.2 million was other R&D funding. Among the control departments, the corresponding figures were SEK 2.6 million, of which (also minuscule) SEK 0.02 million was basic funding and SEK 2.6 million was classified as other R&D funding.

A closer look at the source of increases in "other funding" shows that, among the colleges in the control group, funding from the category "Other grants excl ALF-funds"[18] was particularly sizable.[19] *If* the reason that one or more financiers started to shift funding to the group of control colleges was that Karlstad, Örebro, and Växjö obtained university status, it would be a violation of the SUTVA (stable unit treatment values assumption), which requires that treated and control groups are not affected by one another (Angrist et al., 1996). Clearly, without this funding, the rate of publication might have been lower in the control group. If so, our DiD estimates would be biased downward compared to a situation in which funding had not increased in the control group. A closer inspection of funding data reveals that Södertörn college had a privileged position after 1999, as it was the only recipient of substantial funding from the Foundation for Baltic and East European Studies (The Foundation for Baltic and East European Studies, 2022), see Fig. 3. Omitting Södertörn from Table 2 model (3) makes the coefficient −1.774, which is closer to 0 than when Södertörn was included.

5.2. R&D resources and publication output

In this section, we use the department level to clarify, first, the effects of having university status on publication output and, then, the role of R&D funding. We examine whether the increase in publication is explained by the increase in R&D funding, shown in Fig. 3. As our R&D funding data is biennial and available only for uneven years, we calculate every missing even year (t) using the implied growth rate from the year before ($t-1$) and after it ($t+1$): $R\&D_t = \sqrt{\frac{R\&D_{t+1}}{R\&D_{t-1}}} \cdot R\&D_{t-1}$, in

[15] When aggregating the number of publications to the department level, we include everyone working in these departments, i.e., we include new hires and omit anyone who has left.

[16] Although R&D funding to higher education institutions in Sweden is appropriated on an annual basis, our R&D expenditure data is biennial, and available only for uneven years (1997, 1999, etc.).

[17] The many subcategories under 'Other funding' comprise, among others, grants from the three large funders: The Research Council (VR), FORTE, FORMAS, and a range of agencies and other public organizations, EU funds, including ERC (European Research Council), framework programs for R&D, and enterprises domestically and abroad.

[18] In Swedish: Andra anslag exkl ALF-medel.

[19] ALF-funds is a source of funding for clinical research in medicine.

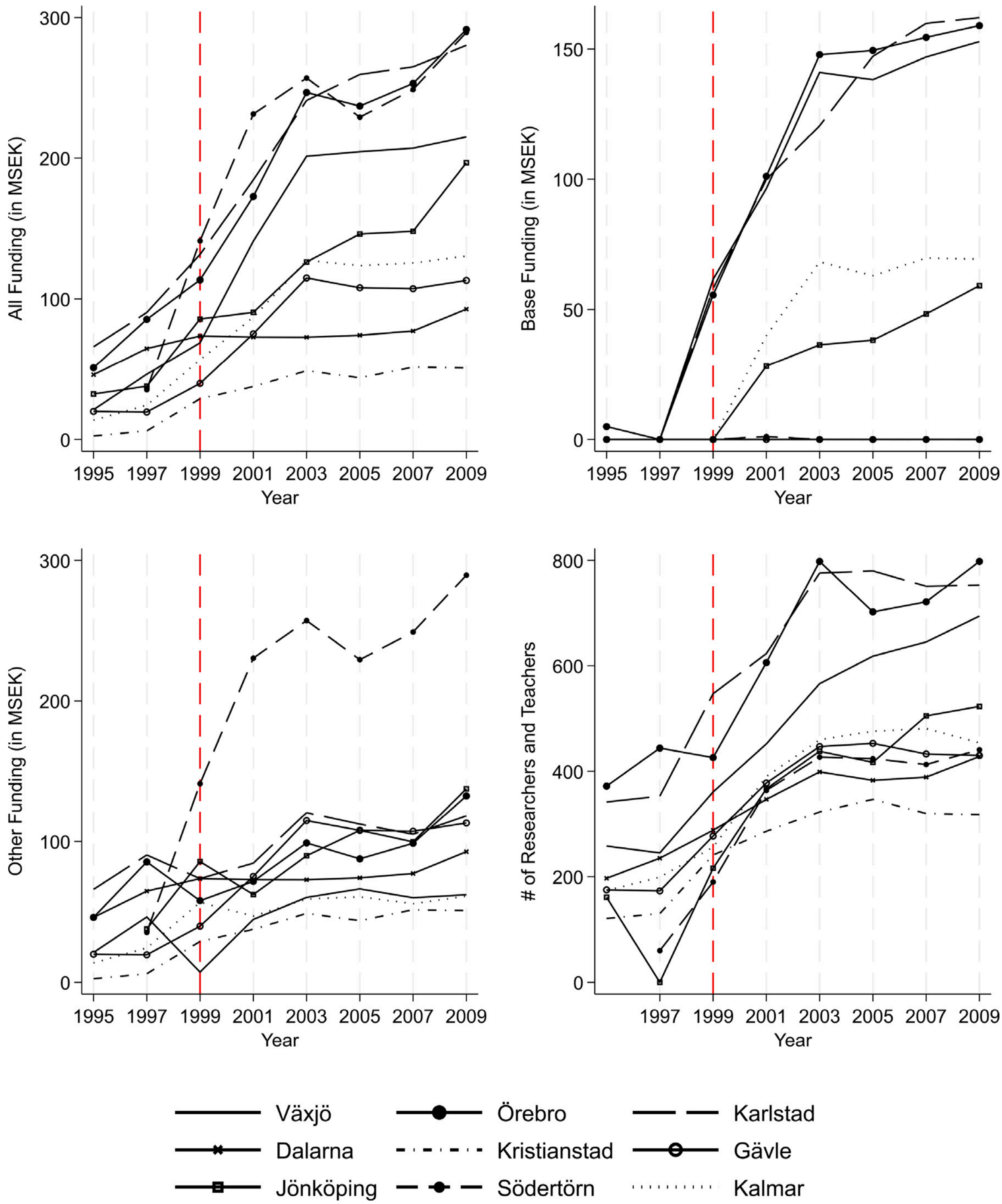

Fig. 3. Funding and Employment Trends for Treatment and Control Institutions.

cases where $R\&D_t$ remains missing,[20] we use the following equation: $\frac{R\&D_{t+1}-R\&D_{t-1}}{2} + R\&D_{t-1}$.

Table 3 reports these results. All the models use department fixed effects. In Table 3 model (1), using data only for 1997–1998, we examine whether the parallel-trend assumption holds through the inclusion of a trend variable for the treated in addition to a general trend variable. The treatment trend is insignificant, indicating that the parallel-trend assumption holds.

Model (2) uses the entire estimation period (1997–2009) with year and department fixed effects. This first estimate of the DiD coefficient indicates a positive and strongly significant effect, $(exp(0.508) - 1) = 66$ percent.[21] Further, using the department R&D funding data, model (3) includes only R&D funding, indicating that SEK 1 million more in funding increases the number of publications fourfold. Model (4) includes both DiD and R&D funding. This model reduces the DiD coefficient, so that, after controlling for R&D funding, the effect is 64 percent. Model (5) includes separate R&D funding coefficients for

[20] For example, when $R\&D_{t-1} = 0$

[21] Note that, as a result of the structure of our dataset, when aggregating publications at the department level, we risk double counting publications in cases of within-department collaboration. We, therefore, prefer to aggregate at the individual level (presented in the next section), to avoid this shortcoming.

Table 3
Becoming a university: effects on publication and the role of R&D funding at the department level.

	(1)	(2)	(3)	(4)	(5)	(6)
Trend (all)	0.546***					
	(0.210)					
Trend for treated	−0.147					
	(0.217)					
DiD		0.508***		0.492***	0.316**	0.077
		(0.181)		(0.184)	(0.156)	(0.142)
R&D-funding			0.004*	0.003*		
			(0.002)	(0.002)		
R&D-funding: control					−0.001	
					(0.004)	
R&D-funding: treated					0.004*	
					(0.002)	
Estimation method	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
Department FE	YES	YES	YES	YES	YES	YES
Trend	YES	NO	NO	NO	NO	NO
Year FE	NO	YES	YES	YES	YES	YES
Department R&D	NO	NO	NO	NO	NO	YES
Mean outcome	7.23	19.36	19.36	19.36	19.36	19.55
Number of departments	33	60	60	60	60	58
Obs.	66	707	707	707	707	689

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Model (1) tests for parallel trends, using only 1997–1998; Models (2)-(6) use 1997–2009. In Model (6) we interact department dummy variables with their respective R&D funding figures. Departments were weighted by the size of their workforce in 1997. See the main text on the imputation of R&D data in even years.

treated and control universities. This leads to a lower DiD effect, indicating a 37 percent increase in publications and an additional average effect from R&D funding to publications of an additional four publications per million SEK by the treated departments, or about 32 per USD 1 million.[22] This compares favorably to other estimates in the literature mentioned earlier, ranging from 10 (Payne and Siow, 2003) to 19 (Rosenbloom et al., 2015), although the sources of publication data and the contexts vary from ours. In the control departments, R&D funding in model (5) has no independent explanatory power. Finally, model (6) includes an R&D funding variable for each department[23] (not shown), estimating the effect on publications separately. In this specification, the DiD coefficient drops substantially, to 0.077, and loses its statistical significance. We regard this as the most relevant test for whether better R&D finances at treated universities drive the publication effect and conclude that this is the case.

Table 4 tests the importance of R&D nonparametrically by dividing treated disciplines by the rate of increase in R&D funding by quartile from the lowest (Q1) increase to the highest (Q4) and reports four estimations. Model (1) thus includes those in Q1 and all control departments, model (2) those of the second quartile (Q2) and all control departments and so on. The regressions control for the initial R&D level in 1997. The table shows a monotonic increase in the DiD coefficient as we move from Q1 to Q4. Q1 has a large negative coefficient, which is significant at the 1 percent level, neither Q2 nor Q3 is statistically significant, and Q4 indicates a very strong and significant positive effect. The results thus confirm the importance of increased R&D funding for explaining the DiD effect using a different approach. Partitioning the division into quartiles reveals that the lower quartiles mainly include humanities and social sciences departments, whereas those in Q4 and Q3 are mainly technical and natural science departments. Table 4 also reports that the range of funding increases for the respective treated departments in each quartile, stressing that the increase in funding varied substantially between the treated departments.

[22] The exchange rate used in this calculation is 8 SEK/USD, approximately the exchange rate around 2000.

[23] We interact the department-level dummy with the department's R&D funding.

5.3. Effect heterogeneity

We now focus our analysis at the individual level, for several reasons. First, it fosters internal validity, as the individual level has the advantage of controlling for individual fixed effects, which could potentially bias the treatment effects, and their inclusion may increase the precision of our estimates. Additionally, by using the matched sample, heterogeneity between the individuals studied is reduced. Moreover, using the individual level allows us to examine the intensive vs. extensive effects of university transitions. Second, we use a split-sample analysis to explore effect heterogeneity, mainly by fields, positions, and gender. Third, we try to understand the timing of the different effects through lead–lag estimations. Fourth, we consider the effect on a quality-adjusted measure of publication.

We start with the results in Table 5, analogous to the department-level analysis (Table 3). Notably, most of our main results are robust to the choice of sample (matched or unmatched). We report cases of deviation in the main text. All estimations using the unmatched sample are in Appendix A.

Models (1)-(2) in Table 5 follow the template set at the department level (Table 3). In model (1) we test for the existence of pretrends. Again, the pretrend variable for the treated is close to zero and statistically insignificant. Model (2) includes year fixed effects and gives rise to an estimated DiD effect, suggesting an 89 percent increase in the publication rate. This translates to 0.57 additional publications per year. We consider this our main specification.

Model (3) tests whether individuals in all three treated colleges separately published more. The tests (at the bottom of the table) clearly indicate that individuals published significantly more in all three than in the control group. Finally, model (4) uses a linear probability model instead of the Poisson, focusing on the extensive margin, which then implies an examination of whether staff began to publish as a consequence of the change in university status. The coefficient is positive, but not significant. Thus we cannot rule out that, on average, individuals at treated colleges are just as likely to publish after 1999 as those in the control group.[24]

[24] In this case, the corresponding unmatched sample regression suggests this coefficient to be of similar magnitude but significant instead (see Appendix C, Table C.1).

Table 4
Becoming a university: effects on publication. Quartile regressions based on the treatment group's department-level R&D increase.

	(1) Q1	(2) Q2	(3) Q3	(4) Q4
DiD	−2.002***	−0.525	0.388	1.370***
	(0.327)	(0.485)	(0.429)	(0.521)
R&D 1997	0.070***	0.076***	0.082***	0.085***
	(0.011)	(0.016)	(0.014)	(0.015)
Estimation method	Poisson	Poisson	Poisson	Poisson
Range of R&D increase (%, treated)	[27,98]	[106,152]	[186,263]	[505,1021]
Mean outcome	20.14	14.83	15.33	15.46
Number of departments	48	48	48	47
Obs.	556	563	563	550

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All control institutions are included in the four regressions. The range of R&D increase is in percentage points. For the control group, the range of the R&D increase is [0,931]. Q1 includes treated departments at the lowest quartile in terms of the R&D funding increase. Q4 includes those with the highest increase, etc. The rate of increase was calculated as $100 \cdot (\frac{\frac{1}{6} \cdot \sum_t R\&D_t}{R\&D_{1997}} - 1)$ with $t = 1999, 2001, 2003, 2005, 2007, 2009$. Departments were weighted by the size of their workforce in 1997.

Table 5
Becoming a university: individual-level effects on publications. Matched samples.

	(1)	(2)	(3)	(4)
Trend (all)	0.517***			
	(0.129)			
Trend for treated	0.022			
	(0.274)			
DiD		0.634***	0.802***	0.013
		(0.172)	(0.181)	(0.009)
Karlstad			0.452**	
			(0.222)	
Karlstad × DiD			−0.335**	
			(0.168)	
Växjö			0.376***	
			(0.134)	
Växjö × DiD			0.184	
			(0.177)	
Örebro			0.413**	
			(0.190)	
Örebro × DiD			−0.109	
			(0.141)	
Estimation method	Poisson	Poisson	Poisson	LPM
Individual FE	NO	YES	YES	YES
Year FE	NO	YES	YES	YES
Mean outcome	1.22	0.64	0.64	0.03
Individuals	74	259	259	2627
Obs.	148	3146	3146	27,930
DiD + Karlstad × DiD			14.02	
p-value			0.00	
DiD + Växjö × DiD			68.58	
p-value			0.00	
DiD + Örebro × DiD			16.64	
p-value			0.00	

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on characteristics in 1997. Model (1) only uses the period 1997–1998 in testing for parallel trends. Other models are estimated for the full study period.

Fig. 4 examines the temporal pattern in the leading and lagged treatment effects for publications in the data by estimating the specification shown in Eq. (2) using the matched sample, first for all publications and then divided by research area. These effects confirm the previous finding of no significant pretrend in the treatment group based on the coefficient for the leading effect in 1997, which is nearly zero for all publications.

The lagged effects have wide confidence bands, which is not surprising given the low number of individuals who changed their publication rate, as indicated in Table 5. Nevertheless, all coefficients are positive, and 8 out of 11 are statistically significant at the 5 percent level. It is hard to discern any particular differences over time—rather the post-1999 effect seems to be stable.

In Table 6, we begin to explore heterogeneity by dividing the sample by field.[25] We can only estimate these models for the humanities and social sciences, natural sciences, and technical sciences because of the small samples in other research areas (medicine, agriculture, and

[25] The term "fields" roughly corresponds to "faculty", but the term reflects that sometimes subject fields vary in terms of faculty organization, e.g., chemistry could be both in a technical or natural science faculty and are not infrequently in both.

Table 6
Becoming a university: effects on publications by field. Matched samples.

	Humanities and social sciences		Natural sciences		Technical sciences	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD	0.637	0.020	0.491	−0.018	0.827***	0.036
	(0.499)	(0.014)	(0.304)	(0.030)	(0.189)	(0.022)
Estimation method	Poisson	LPM	Poisson	LPM	Poisson	LPM
Individual FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Mean outcome	0.43	0.03	1.17	0.11	0.81	0.10
Individuals	136	1031	52	201	56	262
Obs.	1688	11,802	632	2311	652	2913

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on characteristics in 1997. An individual's field is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of the fields in our dataset, see Appendix A Table A.2. Because of an insufficient number of observations, the estimation can only be done for the fields in the table.

"Other").[26] In all research areas, the estimate is positive, however, it is statistically significant (1 percent level) only for the technical sciences, which increases 128 percent, corresponding to an increase of 0.72 publications per year. These results suggest a high degree of variation in the effect, but the result for technical sciences is consistent with the earlier interpretation that a high rate of increased funding drives the effects. We find no significant LPM coefficients, however.[27]

Similarly, the lead–lag results in Fig. 4 differ between the individual fields and the total. The technical sciences demonstrate a rather strong immediate effect, which becomes somewhat weaker over time but remains positive. This result should be considered with caution, as the confidence interval is rather wide in 1999. Nevertheless, this immediate effect probably stems from the fact that publications in many technical science disciplines can be produced with a short time lag (Björk and Solomon, 2013). Publications in the humanities and social sciences have positive lagged effects, which are significant or nearly significant. However, even before treatment, the effect is almost significant, suggesting that these results should be viewed with some caution. In natural sciences, the publication effect is mostly positive, with a slightly upward trend, but it is inconsistent and only rarely statistically significant.

We now inspect the effect on quality-adjusted publications. Here we acknowledge the criticism of the use of citations as a proxy for quality (e.g. Teplitskiy et al., 2022). Indeed, this measure is rather noisy and subject to high variability, yet it is frequently used in the literature and is available in the PARIS database. Fig. 5 shows the corresponding effect for citation-weighted publications. The overall effect is a resounding zero, in terms of both the coefficient size and the significance level. In technical sciences, the effect is again rather positive, and most lag effects are significant, but in the humanities and social sciences and natural sciences, the coefficients trend negative. Panel A of Table 8 summarizes these results; indeed, the overall effect on citation-weighted publications, presented in Model (1), is statistically insignificant, yet this null result might be driven by the contradictory impact on scholars in different research areas. Models (2) and (3) reveal a negative and statistically significant effect in the humanities and social sciences, as well as natural sciences, respectively, whereas model (4) shows a strong, positive, and statistically significant effect on technical sciences. However, these results may be sensitive to the low number of observations.[28]

Table 7 looks at the differences in publication effects by broad groups based on their position, contrasting researchers with teachers in the treated group to those in the control group. Similarly, the table examines differences by gender. Model (1) shows a strongly significant positive effect on publications by researchers, indicating an increase of 113 percent in the publication rate for those who change their publication rate. For men (model 5), the estimated coefficient is almost as high as for researchers, suggesting that gender is also an important dimension for which effects differ. This coefficient is also strongly significant, indicating an increase of 107 percent. In all groups, the LPM models show no effect on the likelihood of starting to publish. The Poisson models for women and teachers show coefficients of nearly zero and are statistically insignificant. These results indicate very clear differences between men and women in the effects of additional basic funding when it comes to their publication outcomes.[29] There could be several reasons for the differences between men and women. Women could be disadvantaged because they suffer from "child penalties" that typically do not apply to men in academia (Kim and Moser, 2021; Cairo et al., 2023). Our descriptive statistics (Table 1) suggest that the average female in our sample is in her mid-forties, and so is the average male, which indicates that direct penalties for having children may not be severe, though lingering "motherhood effects" could inhibit their research. The descriptive statistics also suggest that more plausible factors include their lower representation in teaching and research positions in technical sciences, the field with the most dominant effect. They are also found less frequently than men in natural sciences, which, together with technical sciences, are the areas with the highest increases in research funding, as seen earlier.

5.4. Organization of science

Science and scientists often follow a pattern of self-organization, rather than top-down management. For example, they often choose with whom they want to collaborate and enjoy flexible working conditions. This allows scientists to attain one or several affiliations and to coauthor with scholars at other academic institutions. At the same time, internal organizational constraints could impact matters related to promotion and recruitment. These elements of scientific organization

[26] This, of course, reflects both the status of the academic institutions before 1999 as they became full universities with all disciplines only after 1999, and the control group, which does not include enough individuals in these fields.

[27] In this table, the results are quite different for the unmatched sample, as reported in Appendix C Table C.2. The main reason for this is likely that the unmatched sample does not take into account field-specific differences in the rate of publication as is done in matching that considers the field.

[28] The results are consistent for natural and technical sciences in the matched and unmatched samples. However, when the unmatched sample is used, the effect for humanities and social science loses statistical significance, and the total effect becomes statistically significant (and remains negative). See Appendix C Table C.4.

[29] When comparing to the results of the unmatched samples, the results for all models, but (6) and (8) are consistent, even if marginally different in magnitude. For both women and men, the unmatched sample LPM model is positive and statistically significant, yet the effect for women shows an economically unimportant coefficient, while the effect for men is economically meaningful. See Appendix C Table C.3.

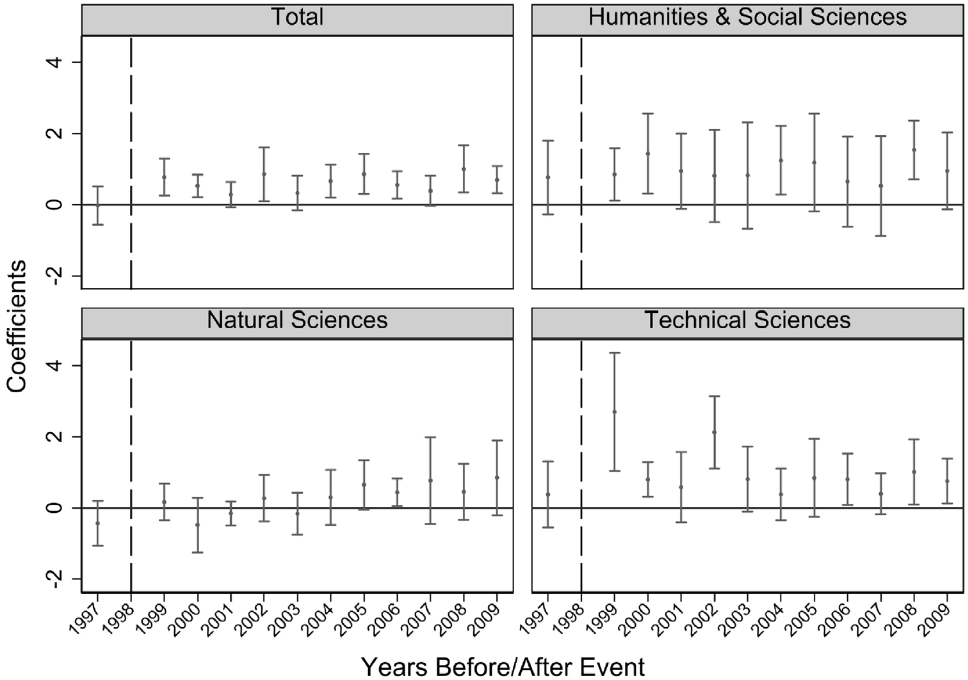

Fig. 4. Publications lead–lag estimates based on the matched sample.

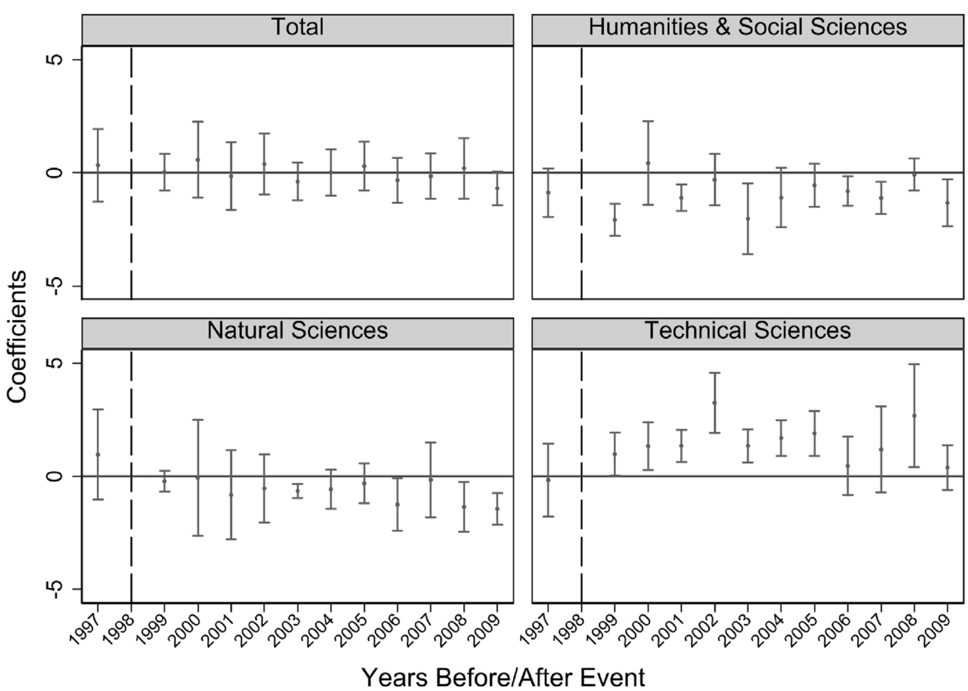

Fig. 5. Citation-weighted publication lead–lag estimates based on a matched sample.

could be research enhancing in many ways and increase collaboration, and affiliations might drive a better division of labor or longer research time through access to external funding. Changes in the composition of the workforce at these institutions could boost knowledge spillovers, efficiency, and idea generation. So, we expand our investigation and study whether and the extent to which upgrading to a university, when coupled with additional basic funding, changes these organizational factors. We do so as these factors, although not directly linked, could constitute mechanisms driving the strong effects on publications that we find.

We first examine whether treated individuals change affiliations. An unusual aspect of academia compared to many other professions is that people are sometimes linked to more than one university (Hottenrott and Lawson, 2017; Hottenrott et al., 2021). This happens for a variety

Table 7
Becoming a university: effects on publications by position and gender. Matched samples.

	Researchers		Teachers		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	0.755***	0.034	0.461	0.010	0.729***	0.024	−0.022	0.001
	(0.205)	(0.020)	(1.110)	(0.016)	(0.178)	(0.016)	(0.289)	(0.003)
Estimation method	Poisson	LPM	Poisson	LPM	Poisson	LPM	Poisson	LPM
Individual FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Mean outcome	0.84	0.10	0.35	0.02	0.73	0.05	0.34	0.01
Individuals	109	417	87	905	200	1380	59	1247
Obs.	1341	4528	1080	9689	2429	14,688	717	13,242

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on the characteristics in 1997. An individual's position is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of positional groupings in our dataset, see Appendix A Table A.3.

Table 8
Becoming a university: effects on citations. Matched samples.

Panel A	All	Science field		
	(1)	(2) Humanities and social sciences	(3) Natural sciences	(4) Technical sciences
DiD	−0.179	−0.348*	−0.949***	1.434***
	(0.271)	(0.211)	(0.079)	(0.103)
Estimation method	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean outcome	7.03	5.84	12.92	4.81
Individuals	150	69	39	31
Obs.	769	316	215	183
Panel B	Position		Gender	
	(1) Researchers	(2) Teachers	(3) Men	(4) Women
DiD	−0.076	−0.625	−0.234	0.337
	(0.199)	(0.612)	(0.266)	(0.211)
Estimation method	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean outcome	7.66	3.96	7.22	6.02
Individuals	67	45	117	33
Obs.	404	182	635	134

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on characteristics in 1997. An individual's field and position is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of the fields and positions in our dataset, see Appendix A Table A.2 and Table A.3, respectively. Because of an insufficient number of observations, the estimation can only be done for the fields in the table.

of reasons, including networking, collaboration, and status, and provide different types of teaching and research job opportunities (Hottenrott and Lawson, 2021).

Table 9 examines the changes in the number of annual affiliations per individual, measured as the number of affiliations that an individual has in addition to her home university, using Poisson regressions. The average number of affiliations is 0.19.

Model (1) of Table 9 examines whether the number of affiliations changes for those at treated universities. We find a very clear drop, which might be because the Poisson individual fixed effects specification includes only those who change the number of affiliations in the estimation sample.[30] In this group, the estimated effect suggests a drop in affiliations of 52 percent, which is statistically significant at the 10 percent level. Decomposing this estimate by the type of affiliation reveals substantial heterogeneity. Model (2) examines affiliations with colleges and model (3) with universities. For this examination, an affiliation to a college which became a university is considered a college affiliation throughout, in order not to create a mechanical trend in the composition of affiliations. Affiliations with colleges decline significantly by 78 percent, while affiliations with universities has a negative coefficient that is not significant. In other words, a clear compositional change emerges. Finally, models (4) and (5) examine whether patterns of mobility change, whether to any other job or to another academic department, respectively. Both coefficients show a positive sign but are insignificant.[31] Mobility is rare in academia (Ejermo et al., 2020) and does not seem to be affected by the change in university status.

Becoming a university could lead to an increase in the size of the workforce as well as a change in its composition, and both changes in the scientific environment could support research activities. We

[30] We also estimated LPM models, to check whether the likelihood that individuals have more than one affiliation changed as a result of the 1999 university status change. Unlike with the Poisson models, this uses the full sample of staff. We obtained qualitatively similar results.

[31] The results of the unmatched sample are consistent in all models but Model (1), where the unmatched sample is statistically insignificant. See Appendix C Table C.5.

Table 9
Becoming a university: effects on mobility and affiliation. Matched samples.

	(1) Affiliations	(2) Affiliations colleges	(3) Affiliations universities	(4) Mobility	(5) Academic mobility
DiD	−0.737*	−1.498***	−0.352	0.039	0.029
	(0.391)	(0.491)	(0.455)	(0.031)	(0.025)
Estimation method	Poisson	Poisson	Poisson	LPM	LPM
Individual FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Mean outcome	0.19	0.18	0.13	0.04	0.02
Individuals	429	296	183	2627	2627
Obs.	5099	3525	2212	27,930	27,930

Clustered standard errors on the university level are shown in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on the characteristics in 1997. For more details on which institutions are included in the universities/colleges, see Appendix A Table A.4.

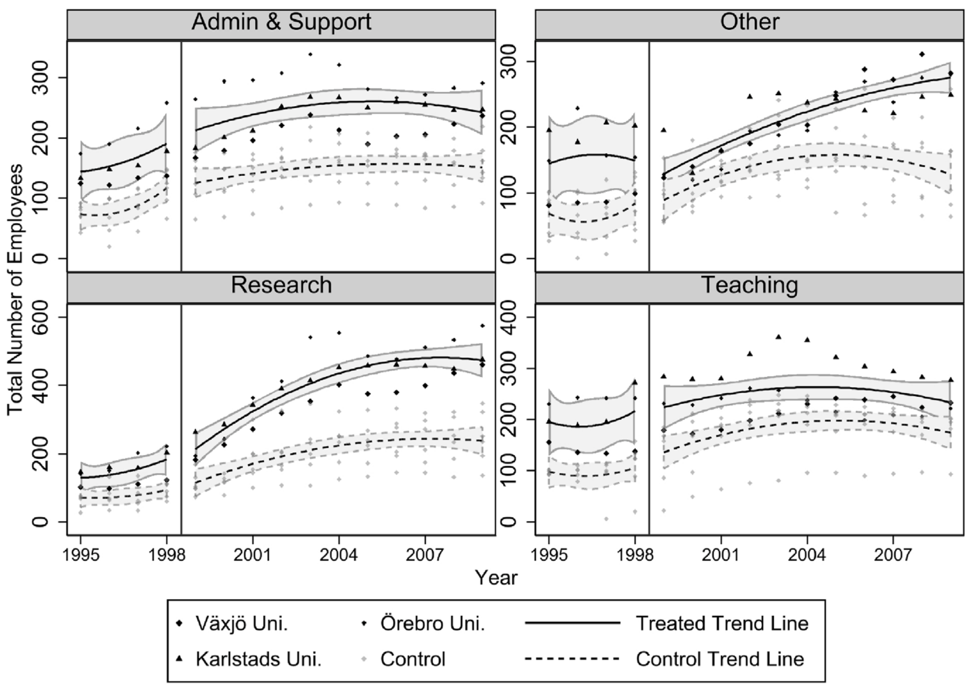

Fig. 6. Workforce dynamics by broad work category. *Note:* For details on the categories of positions, see Appendix A, Table A.3.

begin our examination of changes in the scientific workforce with some descriptive statistics. Fig. 6 decomposes the staff into four main employment categories (based on their position, as documented above). We observe a jump in the number of employees in the administration and support group soon after the upgrade to a university, consistent with the need to establish infrastructure for later expansion. Further, some teaching staff were already at the treated institutions before the upgrade, and, if anything, after 1999 this category has a relative decline. Among all the categories, research and "other" staff grew the most. The relative reduction in the number of teachers, as well as the divergence illustrated in the number of researchers, indicates a change in the orientation of these institutions, reflecting a shift in focus from teaching to research.

Table 10 gives a more detailed investigation of the changes in the composition of the workforce. It tests the growth of specific categories using a formal DiD model at the department level. We divide the staff into nine categories. Many, but not all, employment categories grew more at the treated departments than the controls. Positive estimates are found for professors, Ph.D. students, postdocs and assistant professors, other research and teaching staff, and administrators, whereas negative effects are found for teaching positions (adjuncts), associate professors, technical staff, and temporary employees, who experienced a relative decline. The effects are generally large, with the coefficients indicating increases between 30 percent (postdocs and assistant professors) and 198 percent (administrators) relative to control departments, whereas the negative effects range from −14 percent (associate professors) to −45 percent (temporary employees).

Also, with respect to workforce dynamics, we checked whether the upgrades led to an increase in promotions or career shifts. Table 11 looks at whether our matched sample of individuals had a higher chance of promotion. Promotion is a dummy variable that takes a value of 1 in the year of a person's promotion, and 0 before promotion. All observations after the first year of a promotion are discarded, as researchers are no longer in the "risk set" of being promoted. In each of these regressions, the pool of those who are potentially promoted (at both treated and control institutions) varies as follows: in models (1)

Table 10
Becoming a university - changes to departments' workforce composition.

Panel A	Research				
	(1) Professors	(2) PhD Students	(3) Associate professors	(4) Postdocs and assistant professors	(5) Other Research and teaching staff
DiD	0.680*** (0.027)	0.881*** (0.023)	−0.153*** (0.009)	0.263** (0.130)	0.274*** (0.039)
Estimation method	Poisson	Poisson	Poisson	Poisson	Poisson
Department FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Mean outcome	10.34	22.75	34.55	3.32	11.93
Number of departments	70	67	82	39	68
Panel B	Teaching	Admin, Support & Other			
	(1) Teaching Positions	(2) Administrators	(3) Technical Staff	(4) Temporary Employee	
DiD	−0.410*** (0.009)	1.092*** (0.049)	−0.392*** (0.013)	−0.604*** (0.027)	
Estimation method	Poisson	Poisson	Poisson	Poisson	
Department FE	YES	YES	YES	YES	
Year FE	YES	YES	YES	YES	
Mean outcome	53.15	9.50	6.31	2.55	
Number of departments	81	41	32	27	

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Only departments that were among treated and control institutions were included. Departments are weighted by the staff size in 1997.

and (2) for promotion to professors, the sample comprises nonprofessors, in models (3) and (4) for promotion to associate professors, the sample consists of nonprofessors and non-associate professors, and in models (5) and (6) for promotion to researchers, the sample consists of nonresearchers. We then divide the samples into whether these individuals were promoted anywhere or whether the promotion was at the same institution they were registered in 1997–1999.

The results suggest that promotions were not affected in any of the categories, as all the coefficients are nearly zero and insignificant.[32] However, regardless of which type of promotion we are considering, promotion is a very rare event. The results may seem surprising, given the infusion of research funding and the rise in the rate of publication overall found previously. Promotion could have contradictory effects on publication activity. On the positive side, it can improve the chances of obtaining research funds. Moreover, for some positions, it could increase the time available for research (e.g., full professorships). On the negative side, for other promotions, research time could decrease (e.g., if someone goes from a postdoc to an associate professor). Promotion can also increase administrative burdens (which expand with seniority), which tend to diminish the time available for research (Myers et al., 2023). Combined with the findings regarding workforce recruitment, these results imply that the increase in the number of professors, postdocs, and assistant professors came from recruitment, rather than from the faster promotion of incumbents.

One key characteristic of working in the sciences is collaboration with larger teams, an increasing tendency that results in a larger number of coauthors on scientific papers (Jones, 2009, 2010). As noted above, coauthorships can enhance the productivity of scientists, and, therefore, we study whether upgrading to a university impacted the scope of this phenomenon in the treated universities. Table 12 examines the effects on coauthorship patterns. All models use Poisson regressions. Model (1) looks at the overall number of coauthors. The coefficient implies an increase in coauthors of 166 percent. This increase is much larger than that for publications seen earlier, suggesting that increased collaboration is a mechanism that contributes to the rise in publications. Models (2) and (3) examine coauthorship with colleagues at other colleges and universities.[33] The effects are positive and of similar size, but not statistically significant.[34] The increase in coauthorship with colleagues at one's home university are much larger than coauthorship with externals. Model (4) looks at all collaborations, that is, those who work there beginning in 1997 and those who are newly hired, showing a coefficient that represents an increase of 383 percent. In model (5) we decompose this group and focus only on coauthorship with incumbents, finding an increase of 947 percent. Clearly, researchers at treated institutions mainly increase collaboration with their existing colleagues.

5.5. Robustness

How sensitive are our results to changes in our choice of treated and control institutions? We next investigate the robustness of our results to different combinations of control and treated groups. First, as our review of the policy-making process (see Section 2.2) shows, Karlstad University stood out as having received more positive feedback during the evaluation process as well as more consistent support from the experts, who asserted that this institution was ready to obtain university status. Second, among the control institutions, as previously mentioned, Jönköping and Kalmar received research area status,[35] granting them limited basic funding in specific fields of study. Moreover, Södertörn obtained designated funding from an external funder, which was not available to other colleges (The Foundation for Baltic and East European Studies, 2022).

[32] For the unmatched sample, the coefficients for associate professors are both significant and somewhat larger.

[33] Similar to the grouping used in the affiliation analysis, coauthors from the three treated (new) universities are considered as 'colleges' before and after 1999.

[34] The estimation results of the unmatched sample are consistent in all models. See Appendix C Table C.7.

[35] Kalmar received research area status for natural sciences in 1999, Jönköping received research area status for social sciences in 2004 (The Research Council, 2023).

Table 11
Becoming a university: effects on promotions. Matched sample.

	Professor		Associate professor		Researcher	
	(1) Any	(2) Home	(3) Any	(4) Home	(5) Any	(6) Home
DiD	−0.002	−0.002	0.004	0.006	0.010	0.014
	(0.007)	(0.006)	(0.005)	(0.005)	(0.010)	(0.008)
Estimation method	LPM	LPM	LPM	LPM	LPM	LPM
Individual FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Mean outcome	0.00	0.00	0.02	0.01	0.02	0.02
Individuals	2605	2607	2159	2166	1627	1640
Obs.	26,999	27,109	21,297	21,523	15,831	16,124

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on characteristics in 1997. "Any" denotes promotion at any academic institution, whereas "Home" is only at the individual's academic institution in 1997.

Table 12
Becoming a university: effects on coauthorships. Matched sample.

	All Coauthors	Colleges	Universities	Home institution	
	(1)	(2)	(3)	(4) All	(5) Incumbents
DiD	0.977***	0.413	0.318	1.575***	2.348***
	(0.260)	(0.645)	(0.272)	(0.441)	(0.488)
Estimation method	Poisson	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Mean outcome	0.45	0.15	0.36	0.32	0.18
Individuals	171	37	83	133	88
Obs.	2069	426	1036	1608	1044

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Matching is based on characteristics in 1997. For more details on which institutions are included in the universities/colleges, see Appendix A Table A.4. Model (5) estimates the effect on the number of coauthors from one's home university who worked there in 1997–1999.

Table 13 shows the results of several robustness checks, in which each model omits one or several academic institutions from the sample, as indicated by the column heading. After the institution is dropped, we rerun the matching and estimate the same type of model as model (3) in the individual-level analysis (Table 5). As mentioned earlier, the original coefficient was 0.634.

We expect dropping Karlstad to increase this estimate, as the difference between before and after the transition should be more pronounced at the remaining treated institutions Örebro and Växjö. Moreover, omitting one of them is also expected to raise the coefficient, as each would have better prospects for publishing, given their research area status or exceptional success in attracting external funding. Intuitively, dropping more than one institution should raise the coefficient even further.

The results in Table 13 confirm these expectations. Omitting Karlstad in model (1) raises the effect on Örebro and Växjö, indicating that, after becoming a university, the cash infusion made a larger difference for them than for Karlstad. Dropping Kalmar from the control group in model (2) and Jönköping in model (3) raises the coefficient, as expected. The rise is smaller without Jönköping, plausibly because Jönköping obtained rights associated with fields in social sciences, whereas Kalmar did so in natural sciences. The effect of omitting Jönköping might not differ as much for publications as the humanities and social sciences are driven less by equipment, and so resources might not pose as great a constraint. Dropping both Kalmar and Jönköping in model (4) raises the coefficient more than omitting just one of them.

Model (5) excludes Södertörn, due to its access to exclusive external funding, as mentioned above. As with Jönköping, the coefficient does not change much and ticks upward only marginally. It is unclear whether this is because Södertörn was given no research area rights or because of other differences.[36] Model (6) drops all academic institutions that had either obtained research area status or had access to exclusive funding. The coefficient rises even further, again supporting a cumulative impact. Finally, model (7) excludes Karlstad, showing the highest DiD coefficient, 0.865, far above the original 0.634.

In sum, these robustness checks all support the notion that the control institutions each had special circumstances, hence, there is motivation for dropping them from our analysis, and the direction of the changes in the coefficient all go in the predicted direction. Therefore, our main conclusions are that, first, the coefficient remains firmly positive and strongly significant, regardless of changes in the composition of the sample; and, second, the main estimated effect is, if anything, an underestimate of the true effect.

5.6. Convergence

Finally, we zoom out and document trends in scientific productivity. We are especially interested in understanding the extent to which, if at all, treated institutions can now be considered members of the "university club". We ask: Do the new universities converge to attain the same level of publication as the existing universities, and do they "take off" more than they would have if they had remained colleges? Fig. 7 shows the development of publications per researcher and teaching staff among the three groups, divided by broad research area. As in the approach used in the department-level analysis, when aggregating the number of publications per institution, we do not limit the sample to publications only by incumbents. Although our results

[36] One major difference that could have an impact on performance is that Jönköping is a private foundation, which sets it apart from the other colleges in our sample that are run by the government.

Table 13
Robustness checks. Matched samples.

	(1) Karlstad	(2) Kalmar	(3) Jönköping	(4) Kalmar & Jönköping
DiD	0.865***	1.012***	0.685***	1.193***
	(0.223)	(0.347)	(0.163)	(0.231)
Estimation method	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean outcome	0.58	0.58	0.60	0.56
Individuals	176	225	239	204
Obs.	2177	2737	2890	2468
	(5) Södertörn	(6) Kalmar, Jönköping & Södertörn	(7) Karlstad, Kalmar, Jönköping & Södertörn	
DiD	0.641***	1.305***	1.423***	
	(0.188)	(0.196)	(0.285)	
Estimation method	Poisson	Poisson	Poisson	
Individual FE	YES	YES	YES	
Year FE	YES	YES	YES	
Mean outcome	0.63	0.57	0.57	
Individuals	249	186	123	
Obs.	3024	2249	1534	

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Each estimated model drops the institution(s) indicated in the column heading, reruns the matching, and runs model (3) in Table 5. Matching is based on characteristics in 1997.

are not definitive, as these graphs are just descriptive and not based on matching or controlling for other factors, we find some expected as well as unexpected patterns.

The graphs reveal that a gradual pattern of convergence seems to take place in the technical sciences, in which the new universities increase their rate of publication, approaching the rate at the existing universities and diverging from that by the colleges. This tendency is seen in medicine, as the treated institutions started somewhat below the rate at colleges but surpass them over time, although they remain far behind the existing universities. In natural sciences, convergence with the existing universities is found, but it also occurs at the colleges in the last two years that we observe. This could be the result of either an outlier that mechanically increases the mean for colleges or suggests that university status may not necessarily be the main factor. Another potential source of the difference in the timing of "taking off" might be the time required to accumulate physical capital that supports research,[37] for example, in medicine, building a university hospital.

Finally, no convergence is seen in the humanities and social sciences. In fact, all three groups perform similarly, and their development over time is strikingly similar. Perhaps this tendency reflects a more fundamental characteristic of these sciences, in that the results might simply be due to increasing the staff, and no clear scale effects emerge from, for instance, sharing equipment.

6. Concluding discussion

Funding for scientific research tends to be distributed through grant competitions (for individuals, teams, or institutions) or through path-dependent basic funding. These allocation mechanisms shape the research questions that economists can address about science. Specifically, this characteristic of research funding distribution leads to empirical challenges (e.g., endogeneity) and limits our understanding of the impacts of funding on non-elite scholars or those at historically teaching-oriented institutions. This paper adds new evidence on the causal effects of basic research funding on scientific knowledge production. To do so, we use the 1999 upgrading of three Swedish colleges (Karlstad, Växjö, and Örebro) to universities. We exploit the fact that becoming a university is coupled with allocation of a university's basic funding from the government, earmarked for research, funding that is generally not available to non-universities. We employ this characteristic of the Swedish academic system and estimate that scholars who experienced the transition to a university increased their publication activity by 89 percent. This result is confined to those who were already publishing, as we did not find a significantly changed likelihood of initiating publication activity. An investigation at the department level shows that the increases in funding explain this change in publication activity. To put these results into perspective: compared to previous estimates of the effect of university R&D funding on publication, our estimates are high but not implausible. Our examination of citation-weighted publications reveals no statistically significant effect.

To deepen and unpack the overall effect that we find, using the granularity of our database, we explore the effects of heterogeneity. We find that the rise in R&D funding was the highest in technical and natural sciences, corresponding to a clear and steep rise in publication activity in technical sciences but not in natural sciences. Furthermore, we see positive effects among those who held research positions before 1999, but no statistically significant effects for those in teaching. With respect to gender differences, we find that the effect is concentrated among male staff, whereas for female staff, the absence of positive effects could be explained mostly by the fact that women held fewer research positions and rarely worked in technical sciences. With regard to heterogeneity in the effect on citation-weighted publications, we document a positive effect in technical sciences and a negative one in the humanities and social sciences as well as natural sciences. However, due to the lower statistical power of these tests, we refrain from drawing strong conclusions about the effect on the quality of scientific output.

Although the main relationship studied is between basic funding and scientific knowledge production, the context of university transition enables us, at the same time, to study some changes in what we call "the organization of science". We focus on aspects that we see as supportive of individuals' research productivity. We find that the transition to a university was accompanied by a strong rise in the number of coauthors, mostly among colleagues at the same institution (incumbent scholars). We also find that the number of affiliations with other institutions generally fell, driven mainly by a drop in affiliations

[37] Unfortunately, we do not have data related to universities' physical capital.

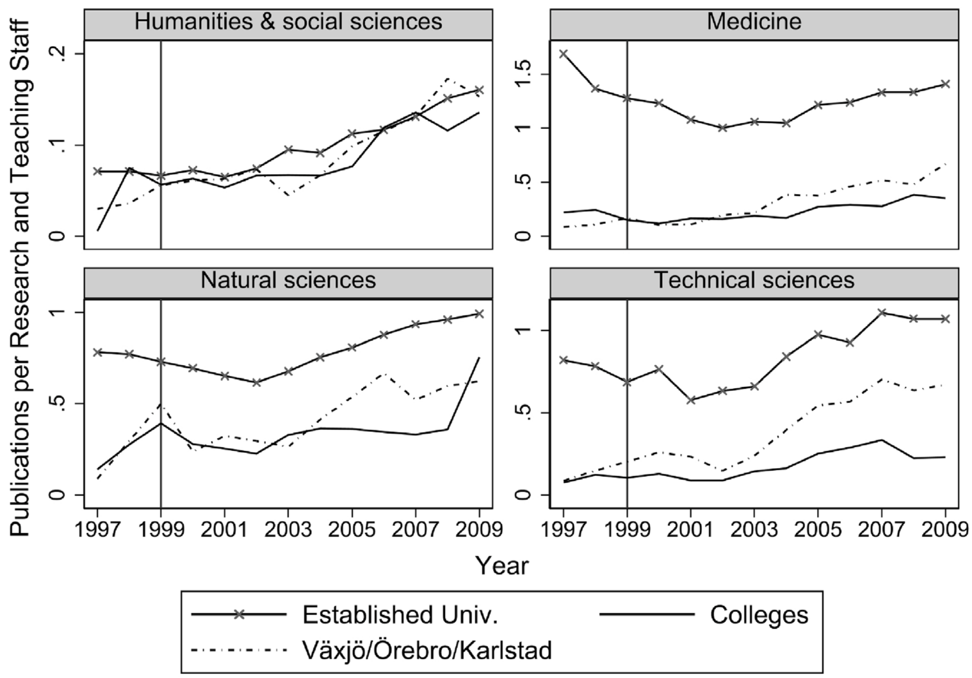

Fig. 7. Convergence in publication per individual by science field (research and teachers).
Note: Institutions in each group, determined by data availability, are indicated in italics, as detailed in Appendix A, Table A.4.

with colleges. These results indicate that scholars at the treated colleges reduced the need to draw resources from outside their institution. They also suggest that they realigned the focus of their attention, away from colleges.

When we examine changes in workforce dynamics, we find that treated institutions changed the overall composition of their workforce, demonstrating the increased centrality of research activities in their mission. We report an increase in almost all research positions, indicating a shift to a research-supporting environment. The employment groups with the largest numerical increase were Ph.D. students and administrators. Although we have no data about the level of teaching activity, it seems logical that the results are due to the increase in time in which researchers could engage in research, perhaps on projects that they, together with colleagues, could now realize. One could speculate about whether the larger size of these two groups led to a better division of labor, boosting the efficacy and productivity of senior researchers.[38] Although the effect on the number of teachers was negative, we do not find evidence that those in teaching positions shifted to research positions more frequently. We also see no evidence of greater mobility or opportunities for promotion relative to the control group. Finally, we provide descriptive evidence showing that the productivity level of scholars at the new universities began to converge with the level of those at full universities. This is particularly true in technical and natural sciences; in medicine, this process seems to be slower, and in social sciences and humanities, no initial differences emerged. One potential explanation for these trends is the time needed to accumulate physical infrastructure that is crucial for research activity in some fields (e.g., in medicine, a university hospital).

The results of this paper indicate that increasing basic funding for individuals who previously had a low publication rate could lead to substantial increases in the rate of publication. In fact, several robustness checks suggest that, if anything, our results may be conservative, mainly because researchers at our control institutions often received partial treatment in the form of research area rights or because of an infusion of alternative funding. Beyond our contribution to the literature on university funding and knowledge production, this paper also informs policymakers. For example, in Sweden and other developed countries, discussions are ongoing about whether funding should be channeled to colleges or to established centers—that is, to the center or to the periphery. This paper suggests that channeling resources to small institutions might benefit research, because the funding would have a proportionately large impact. Although these results do not tell us how resources should be prioritized, the increase in their basic funding is not very large and, therefore, should not be seen as crowding out resources for pre-existing institutions.

Our study suffers from limitations, mainly as a result of data shortcomings. First, the lack of individual-level funding data prevents us from controlling for the effect of external funding on publication activity. In addition, it limits our ability to assess potential trade-offs between basic funding and competitive grants. Second, our registry-linked publication data only includes aggregate publication activity per individual scholar, hindering us from using common quality measures, such as the journal impact factor. This, in turn, prevents us from drawing deeper insights into the impacts on the quality and orientation of scientific publications. Lastly, the coverage of our dataset is limited in terms of the years, enabling us to use only two years before the 1999 university transitions, and the number of control colleges (6 out of 15). Nevertheless, the data used in this study, which leverage bibliometric indicators merged with registry data, permitted us to address many aspects related to the threats of identification.

Indeed, we welcome further research on experimental situations, which can also gauge the effects of funding on research output in other situations, such as for colleges and established universities as a whole. Greater availability of individual-level research funding data might

[38] A recent study discusses the importance of research time as a key, overlooked, driver of professors' productivity (Myers et al., 2023).

Table A.1
Definitions and explanations of variables used in the estimations.

Variable	Definition
Publications	No. of publications in year *t* for individual *i*
Citation-weighted publications	No. of forward citations related to all publications in year *t* for individual *i* within a three-year window.
DiD	Difference-in-difference variable. Takes a value of 1 for treated individuals after 1999; otherwise, 0.
Affiliations	Number of additional affiliations, other than home institutions
Affiliations: Colleges	Number of additional college affiliations, other than home institutions. See Table A.4.
Affiliations: Universities	Number of additional university affiliations, other than home institutions. See Table A.4.
Promotions: Professor	Promotion to associate professor, excluding professors are excluded from the pool.
Promotions: Associate professor	Promotion to associate professor, excluding professors and associate professors.
Promotions: Researchers	A change in employment category, from teaching to a research position. See Table A.3.
Academic mobility	Change in employer in which the new employer is a higher education institution.
Mobility	Change in employer to any new employer.
Coauthors: All	Number of coauthors.
Coauthors: Colleges	Number of coauthors from colleges. See Table A.4.
Coauthors: Universities	Number of coauthors from universities. See Table A.4.
Coauthors: Home institutions	Number of coauthors from one's home university.
Coauthors: Home institutions, incumbents	Number of coauthors from one's home university, who worked there in 1997–1999.

enable us to tease out the marginal effect of basic funding relative to competitive grants, an insight of great value to policy makers. Although we already know quite a bit about how academic scientists contribute innovations that can add to local economic development (Bonander et al., 2016), we also welcome research that delves into the effects of research on teaching quality. If research has positive effects on the knowledge and capability of teachers, this could well spill over to students and the economy at large. As our study focuses on the impact of university upgrading over a ten-year period, future research could also examine the longer-term impacts on scholars as well as the universities' organization.

An important point for policy makers is that positive effects on scientific output can be achieved even when funding is not subject to competition. In other words: should policy makers allocate more money for grant-financed research or basic funding? The results in this paper suggest that the effects for basic funding can be large, even if those funds are not competitively awarded and are given to institutions that lack a long research tradition.

CRediT authorship contribution statement

Olof Ejermo: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Resources, Project administration, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Yotam Sofer:** Writing – review & editing, Writing – original draft, Visualization, Validation, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Olof Ejermo reports financial support was provided by Swedish research council for health, working life and welfare. Yotam Sofer reports financial support was provided by Carlsberg Foundation. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The authors do not have permission to share data.

Appendix A. National subjects and positions

See Tables A.1–A.4.

Appendix B. Collection of publication data

In the first step, colleges and universities were asked to provide lists of all individuals engaged in research or teaching at their universities. The lists were requested based on the premise that staff lists can be considered public documents according to Swedish law. Moreover, researchers have extended rights to use and create databases, enabling them to obtain individuals' personal identifiers, such as social security numbers (SSNs).

Most academic institutions complied with the request, although some academic institutions did not respond to it, despite repeated reminders. Eventually, 22 out of the 25 institutions supplied the requested information. They typically supplied information on the first name, last name, and e-mail address of relevant individuals. However, the supplied information varied in terms of both content (e.g., multiple rows per person, email or not, nonstandardized positions or not, temporary or permanent positions, affiliation information at the university, etc.) and coverage in terms of the years covered. After careful harmonization and anonymization of the data, it was sent to Fraunhofer ISI, Germany, for linking names and individuals with author IDs in the Scopus database. With respect to the scope of the database, the Swedish university system had from 50,000 employees in the mid-1990s to close to 80,000 in 2021. About 60 percent of them are researchers or teachers. The staff lists submitted had 70,202 unique individuals, of whom about 35%, or 25,020, were linked to an author ID in Scopus. This might seem low, but many individuals never publish. To some extent, this low rate may have resulted from Scopus's focus on journal articles, a focus that has since broadened.

Ejermo et al. (2016) found that around 85% of publications were linked. Fraunhofer ISI created a panel that included the number of publications per individual and per year, a collaboration matrix detailing who collaborated with whom in a given year, the number of publications on which they collaborated, and the number of citations to an individual's publications in a given year. These citation counts were created by adding up the number of citations to a publication using a three-year window after the publication year.

Appendix C. Unmatched sample results

See Figs. C.1 and C.2.

See Tables C.1–C.7

Table A.2
List of national subject fields (Swedish: nationell förteckning över forskningsämnen) at the two-digit level used in the study.
Source: Statistics Sweden and the Swedish Higher Education Authority (2016).

Two-digit code	Name in English (Name in Swedish)
11	Humanities (Humaniora)
12	Law (Rättsvetenskap/Juridik)
13	Social sciences (Samhällsvetenskap)
14	Mathematics (Matematik)
15	Natural sciences (Naturvetenskap)
16	Technical sciences (Teknikvetenskap)
17	Agricultural sciences (Skogs- och jordbruksvetenskap samt landskapsplanering)
18	Medicine (Medicin)
19	Odontology (Odontologi)
21	Pharmaceutics (Farmaci)
(22)	Veterinary medicine (Veterinärmedicin)
23	Other/cross-disciplinary research topics (Övriga forskningsområden)
24	Cultural heritage and cultural production 241; Sport science 242; Gerontology 243 (Kulturarv och kulturproduktion 241; Idrott 242; Äldre och åldrande 243)

Note: (22) is not represented as a field among the treated and control individuals in the study. The Swedish standard is based on the OECD-classification Field of Research and Development (FORD). The names in Swedish are translated into English by the authors.

Table A.3
List of positions and group division.
Source: Author's classification based on position information in the university registry.

Swedish name	English translation	Group
Professorer	Professor	Research
Lektorer	Associate professor	Research
Adjunkter	Teaching positions	Teaching
Annan forskande och undervisande personal	Other research and teaching staff	Research
Doktorander	PhD students	Research
Administrativ personal	Administrative staff	Admin & support
Bibliotekspersonal	Librarians	Admin & support
Teknisk personal	Other research and teaching staff/technical staff	Other
Arvodister	Temporary staff	Other
Meriteringsanställningar	Postdocs and Assistant professors	Research

Note: The Swedish word *lektor* is a permanent position mainly devoted to teaching, hence, the term *Senior lecturer* is appropriate. Many of them were promoted with the title "Docent" (not recorded in the data), for which a better translation is associate professor, which we use throughout.

Table A.4
List of higher education institutions by group.

Universities	Colleges	1999 Universities
Lund University	**Jönköping University**	**Karlstad University**
Gothenburg University	**Kristianstad University**	**Örebro University**
Uppsala University	**Dalarna University**	**Växjö University**
Umeå University	**Södertörn University**	
Chalmers University of Technology	**Gävle College**	
Stockholm University	**Kalmar College**	
Karolinska Institute	Blekinge Institute of Technology	
Royal Institute of Technology	University of Borås	
Luleå Technical University	Halmstad University	
Linköping University	University of Skövde	
Swedish University of Agricultural Sciences	University West	
Stockholm School of Economics	Malmö University	
	Mälardalen University	
	Mid Sweden University	

Note: We group higher education institutions according to their status in 1999. Boldface indicates institutions included in our (DiD) estimations of the effect of becoming a university, as publication coverage dates back to 1997. Italics indicate universities with coverage dating back to 1997, used in the convergence analysis (Section 5.6). Nine additional art, sports, and theological higher education institutions are not included in our dataset.

Table C.1
Development of publications after university status in 1999: Karlstad, Växjö, and Örebro. Full sample.

	(1)	(2)	(3)	(4)
Trend (all)	0.444***			
	(0.159)			
Trend for treated	−0.049			
	(0.300)			
DiD		0.531***	0.518**	0.013**
		(0.135)	(0.258)	(0.005)
Karlstad			0.082	
			(0.282)	
Karlstad × DiD			−0.122	
			(0.248)	
Växjö			0.072	
			(0.274)	
Växjö × DiD			0.337	
			(0.264)	
Örebro			−0.002	
			(0.125)	
Örebro × DiD			−0.035	
			(0.209)	
Estimation method	Poisson	Poisson	Poisson	LPM
Individual FE	YES	YES	YES	YES
Year FE	NO	YES	YES	YES
Mean outcome	1.37	0.78	0.78	0.04
Individuals	85	294	294	2847
Obs.	170	3545	3545	29,790
DiD + Karlstad × DiD			18.35	
p-value			0.00	
DiD + Växjö × DiD			79.84	
p-value			0.00	
DiD + Örebro × DiD			26.85	
p-value			0.00	

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Model (1) only uses 1997–1998 in testing for parallel trends. Other models are estimated beginning in 1997.

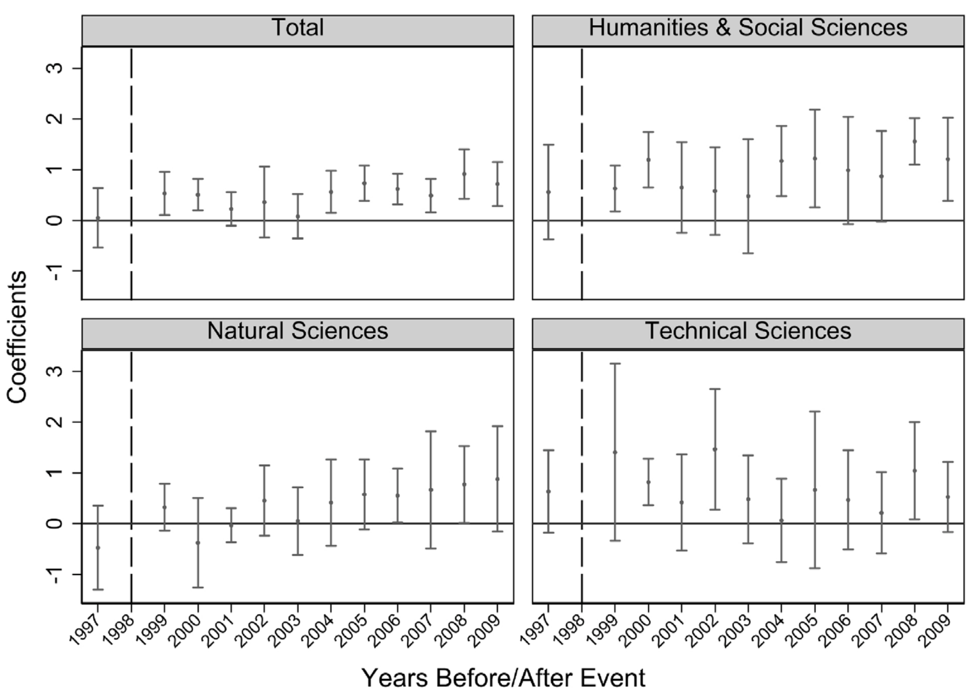

Fig. C.1. Publications lead–lag estimates based on the full sample.

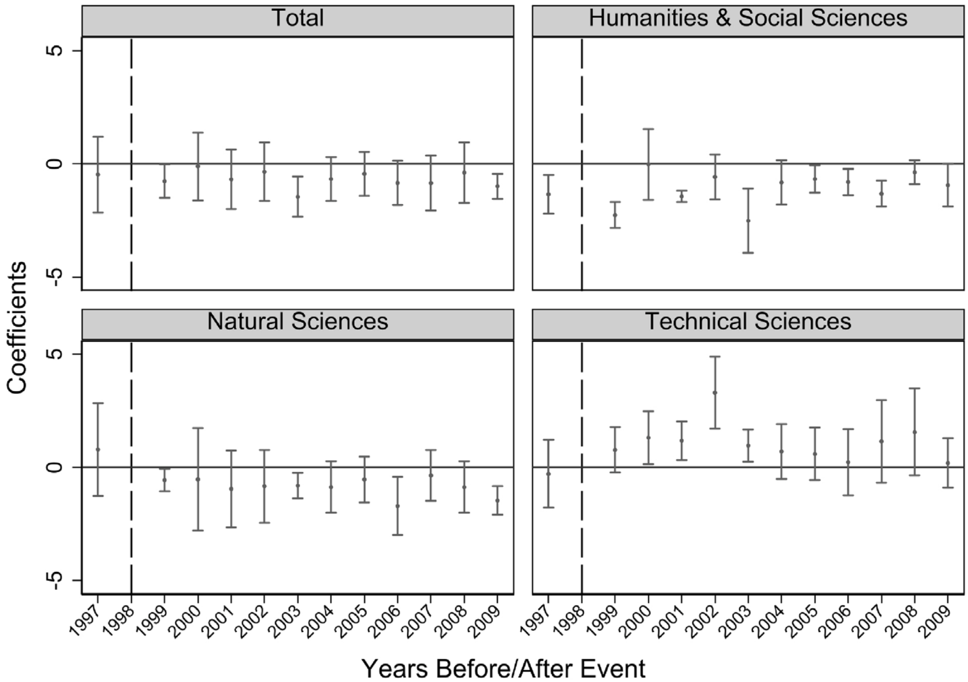

Fig. C.2. Citations lead–lag estimates based on the full sample.

Table C.2
Becoming a university: effects on publications, by field. Full sample.

	Humanities and social sciences		Natural sciences		Technical sciences	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD	0.760**	0.019**	0.605*	0.008	0.428	0.023
	(0.310)	(0.007)	(0.322)	(0.032)	(0.317)	(0.018)
Estimation method	Poisson	LPM	Poisson	LPM	Poisson	LPM
Individual FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Mean outcome	0.71	0.03	0.96	0.09	0.76	0.08
Individuals	142	1049	63	239	63	314
Obs.	1759	12,000	749	2700	743	3472

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An individual's field is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of the fields in our dataset, see Appendix A Table A.2. For consistency, the table includes the same fields shown in Table 6 (matched sample).

Table C.3
Becoming a university: effects on publications, by position and gender. Full sample.

	Researchers		Teachers		Men		Women	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DiD	0.626***	0.024	0.601	0.014	0.590***	0.022**	0.156	0.003**
	(0.146)	(0.017)	(0.841)	(0.010)	(0.174)	(0.009)	(0.265)	(0.001)
Estimation method	Poisson	LPM	Poisson	LPM	Poisson	LPM	Poisson	LPM
Individual FE	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Mean outcome	0.91	0.11	0.35	0.03	0.86	0.05	0.49	0.02
Individuals	120	451	100	986	223	1503	71	1344
Obs.	1477	4833	1229	10,338	2700	15,821	845	13,969

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An individual's position is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of groups of positions in our dataset, see Appendix A, Table A.3.

Table C.4
Becoming a university: effects on citations. Full sample.

Panel A	All	Science field		
	(1)	(2) Humanities and social sciences	(3) Natural sciences	(4) Technical sciences
DiD	−0.524** (0.237)	−0.056 (0.220)	−1.081*** (0.159)	1.291*** (0.230)
Estimation method	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean outcome	9.43	11.52	12.70	4.25
Individuals	171	74	43	35
Obs.	875	328	230	213
Panel B	Position		Gender	
	(1) Researchers	(2) Teachers	(3) Men	(4) Women
DiD	−0.447** (0.214)	−1.365*** (0.515)	−0.690*** (0.191)	0.433** (0.172)
Estimation method	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Mean outcome	8.46	4.05	10.14	6.64
Individuals	75	51	130	41
Obs.	468	199	698	177

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An individual's field and position is determined by her classification in 1997 or imputed from later years; see the main text for details. For a more detailed overview of the fields and positions in our dataset, see Appendix A Table A.2 and Table A.3, respectively. Because of the low number of observations, the estimation can only be done for the fields in the table.

Table C.5
Becoming a university: effects on mobility and affiliations. Full sample.

	(1) Affiliations	(2) Affiliations colleges	(3) Affiliations universities	(4) Mobility	(5) Academic mobility
DiD	−0.509 (0.334)	−1.473*** (0.421)	−0.126 (0.384)	0.043 (0.030)	0.029 (0.024)
Estimation method	Poisson	Poisson	Poisson	LPM	LPM
Individual FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Mean outcome	0.19	0.18	0.15	0.04	0.01
Individuals	459	313	196	2847	2847
Obs.	5427	3720	2351	29,790	29,790

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For more details on which institutions are included as universities/colleges, see Table A.4.

Table C.6
Becoming a university: effects on promotions. Full sample.

	Professor		Associate professor		Researcher	
	(1) Any	(2) Home	(3) Any	(4) Home	(5) Any	(6) Home
DiD	−0.001 (0.005)	−0.002 (0.004)	0.007* (0.004)	0.007* (0.003)	0.008 (0.009)	0.009 (0.008)
Estimation method	LPM	LPM	LPM	LPM	LPM	LPM
Individual FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES
Mean outcome	0.00	0.00	0.02	0.01	0.02	0.02
Individuals	2821	2823	2329	2336	1735	1749
Obs.	28,777	28,888	22,569	22,803	16,592	16,886

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. "Any" denotes promotion at any academic institution, whereas "Home" refers to promotion at the home academic institution only.

Table C.7
Becoming a university: effects on coauthorships. Full sample.

	All coauthors	Colleges	Universities	Home institution	
	(1)	(2)	(3)	(4) All	(5) Incumbents
DiD	0.904***	0.807	0.285	1.535***	2.237***
	(0.257)	(0.613)	(0.299)	(0.405)	(0.466)
Estimation method	Poisson	Poisson	Poisson	Poisson	Poisson
Individual FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
Mean outcome	0.52	0.17	0.41	0.36	0.20
Individuals	199	44	100	152	98
Obs.	2382	494	1220	1838	1169

Note: Clustered standard errors at the university level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. For more details on which institutions are included as universities/colleges, see Appendix A Table A.4. Model (5) estimates the effect on the number of coauthors from one's home university, who worked there in 1997–1999.

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