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Do Online Labor Market Intermediaries Matter? The Impact of *AlmaLaurea* on the University-to-Work Transition

Manuel F. Bagues and Mauro Sylos Labini

4.1 Introduction

The Internet and electronic technologies more generally have a great potential for changing the way employer-employee matches are made (Autor 2001). Since the mid-1990s there has been a well-documented increase in the number of Internet job boards and corporate websites devoted to job applications, and in the shares of job-seekers and recruiters using online resources. For example, according to Taleo Research, the incidence of Fortune 500 companies using their careers website as a corporate job board increased from 29 percent in 1998 to 92 percent in 2002. Moreover, the importance of online technologies may be underestimated, since the possible uses of the Internet in job search are multifaceted and go well beyond viewing advertisements or posting resumes (Kuhn 2000).[1]

However, it has been extremely difficult to assess the impact of online

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1. In a recent report, the U.S. Congressional Budget Office pointed out that "internet job searching may also have played a role in reducing the natural rate (of unemployment)" (CBO 2002).

technologies on labor market outcomes. The Internet is believed to increase the amount of information available to recruiters and job-seekers and at the same time to improve their ability to screen online applications and opportunities. Both aspects are likely to decrease the cost of job search and, therefore, to improve matching productivity (Pissarides 2000).

Nevertheless, it has also been noted that even if searching online has private individual benefits, it does not follow that the equilibrium effects on labor market outcomes are socially beneficial (Autor 2001). In a recent empirical investigation, Kuhn and Skuterud (2004) also find that—once individual observable characteristics are controlled for—Internet seekers do not have shorter unemployment duration than other searchers and, in some specification, it may even be longer. As the authors acknowledge, these results may be contaminated by selection into Internet job search on unobservables that are negatively correlated with employability. However, it is also possible that Internet search is counterproductive at the individual level because of the negative signal it might send to employers. Workers may still use the Internet, the authors argue, because it is very cheap and they are not aware of this drawback.

Therefore, despite their rapid diffusion, whether online electronic technologies are capable of increasing the overall efficiency with which workers and jobs are matched or, conversely, are merely cheaper substitutes for more traditional means (e.g., newspaper ads or face-to-face intermediation) is still an open issue.

This chapter evaluates the impact of the availability of electronic labor markets on the university-to-work transition. In particular, we study the effects of a specific electronic intermediary, the interuniversity consortium *AlmaLaurea*, on graduates' unemployment, mobility, and matching quality. In a nutshell, *AlmaLaurea* collects and organizes online information concerning college graduates' curricula and, conditional on their permission, sells it to firms in electronic format. Hence, similar to other commercial job boards, it makes information about searching candidates available online. However, it also contains information on almost the entire universe of graduates from the institutions that it serves.

The present case study provides exceptional evidence on the effect of online labor market intermediaries for two main reasons: first, the impact of *AlmaLaurea* is observed during a time period when e-recruitment was almost nonexistent in Italy. *AlmaLaurea* was founded in 1994 and, to the best of our knowledge, until 1999 there were no major Internet job boards operating in Italy. Second, different timing of universities' enrollment in *AlmaLaurea* produces counterfactuals that allow us to tackle the problems faced by previous empirical investigations. Although today most Italian universities are members of *AlmaLaurea*, a smaller subset was in the consortium at the time our data were collected. We identify the average effect of *AlmaLaurea* on graduates from this initial subset—that is, the ones that

might have used its services—comparing the dynamics of their employment outcomes with those of graduates from universities that were not members. Hence, we aim at estimating the effect of the *availability* of electronic intermediaries, not the private benefits of using them.

Formally, we measure the effect of *AlmaLaurea* using the difference-in-differences (DID) approach applied to a repeated cross-sectional data set. The data set is built by merging two distinct (but almost identical) surveys run by the Italian Statistical Office (ISTAT) on representative samples of two cohorts of university graduates interviewed three years after graduation. Given that *AlmaLaurea* intermediation activity only started in a subset of universities in the period between the graduation of the two cohorts, we split the sample into two distinct groups: graduates who completed their degree in a university that joined *AlmaLaurea* in 1996 and 1997 (the treatment group) and those who graduated from universities not members of *AlmaLaurea* during that period (the control group). The subtleties of envisaging participation of academic institutions in *AlmaLaurea* as a quasi-natural experiment are discussed in more depth in the following. Here, it suffices to say that, first, individual decisions concerning college enrollment were made before *AlmaLaurea* came into being; second, graduates and universities in the two groups are not statistically different in terms of observable characteristics; third, according to personal conversations with the consortium's director, initially membership in *AlmaLaurea* was fairly accidental and mostly based on informal relationships among a few faculties.

AlmaLaurea, as we discuss more thoroughly in the following, has a number of features that make it likely to be effective. First, it collects official information, which is partially disclosed to firms, concerning also those individuals who decide not to post their resumes online. Second, it achieves very high enrollment rates from graduates. We conjecture that these features are likely to reduce adverse selection.

According to our most conservative estimate, *AlmaLaurea* decreases the probability of unemployment by about 1.6 percentage points and has a positive effect on wages and two self-reported measures of job satisfaction. We also find that it fosters graduates' geographical mobility.

To check the robustness of these findings, we test for pretreatment parallel outcomes and find that graduates from the two groups of universities had similar employment dynamics prior to *AlmaLaurea*'s operation. Our results might also be affected by the adverse consequence of *AlmaLaurea* for graduates from universities, not members of the consortium. To control for this possibility, we build alternative treatment and control groups based on geographical proximity. We find no evidence of such a negative effect.

Our work is related to the growing number of studies that investigate the effect of the Internet and electronic technologies on the labor market (Autor 2001; Freeman 2002). Kuhn and Skuterud (2004) study the impact of Internet job search on the probability of the unemployed finding a job.

According to their analysis, there are no discernible differences between transition to employment of online and traditional searchers. They conclude that either online search is ineffective or that Internet job searchers are negatively selected. In a recent paper—methodologically similar to ours—Kroft and Pope (2008) exploit the uneven geographical expansion of the website Craigslist to assess the impact of online search on labor and housing markets efficiency. Although, consistent with Kuhn and Skuterud (2004), they find that online search had no effect on the unemployment rate, they did find that it lowered more traditional classified job advertisements in newspapers. Stevenson (2008) investigates the importance of online technologies on employed online job search and finds that in the United States, state-level rise in Internet penetration is associated with state-level rise in employer-to-employer worker flows. In this chapter, we focus on the impact of online search on a specific segment of the labor market—that is, transition of university to work.

Our study is also useful for policy evaluation and formulation: the consortium *AlmaLaurea* is cofinanced by the Italian Ministry of Education; therefore, clear evidence on its effectiveness is useful for evaluating how public money is spent.[2] Moreover, should *AlmaLaurea* prove to be an effective institutional arrangement, other European countries might learn from its example, improving public policy aimed at facilitating the university-to-work transition.

The rest of the chapter is organized as follows. Section 4.2 presents an overview of the university-to-work transition in Italy, provides an in-depth description of the *AlmaLaurea* consortium, and briefly discusses the economics of online labor market intermediaries. Section 4.3 outlines the identification assumptions needed to make our empirical strategy valid. Section 4.4 describes the data used in the analysis. Section 4.5 presents the main results. Sections 4.6 and 4.7 justify the validity of the results of our empirical approach, and section 4.8 concludes.

4.2 Background

4.2.1 University-to-Work Transition in Italy

Labor market functioning is deeply affected by different kinds of information imperfections and asymmetries. The education-to-work transition is particularly exposed to these imperfections: first-time job seekers typically lack work experience, and this negatively affects both their outlooks con-

2. Given that we do not know the magnitude of public funding invested, we are not able to measure whether *AlmaLaurea* is a worthwhile social investment; we can only measure whether students from *AlmaLaurea* member universities have benefitted from it.

Table 4.1 **Employment rates of university graduates by age class—2004**

Country	Age class		
	25–29	30–34	35–39
Denmark	79.7	87.7	91.2
Finland	84.4	86.7	87.9
France	80.1	85.0	87.5
Greece	72.2	85.5	87.9
Italy	58.0	81.9	89.4
Spain	76.3	85.9	86.7
Sweden	76.6	88.2	88.3
United Kingdom	90.5	98.1	90.1

Source: Eurostat.

cerning employment opportunities and job characteristics, and employers' screening options.

In most countries, unemployment rates are lower for university graduates than for the rest of labor force, and highly educated people experience a smoother entry into working life (Organization for Economic Cooperation and Development [OECD] 2007). As table 4.1 shows, however, international comparisons depict the university-to-work transition in Italy as one of the most problematic cases among industrialized countries.[3] There are three main possible explanations for this. First, there are frictions on the *supply* side: it might be that education provided by Italian universities is of such a low standard that graduates are obliged to undertake further training, either formal or informal, before getting into work. Second, the slow transition rates may be due to labor *demand* characteristics: the Italian industrial structure, compared to that of other developed countries, is biased in favor of small firms and low-tech industries that typically do not employ highly qualified workers. Third, there may be inefficiencies in the *matching* mechanisms caused by information imperfections and, possibly, by lack of intermediaries.

AlmaLaurea potentially improves labor market functioning for two reasons. First, it reduces search costs for both firms and workers by making accurate qualification, grade, and study data readily available. Second, it may mitigate adverse selection by making it possible to compare searching students with others in their cohorts.

Universities are often active actors in labor market intermediation. For instance, most academic institutions set up and manage placement offices and, in some cases, their faculties establish informal ties with firms.[4] However, when universities receive financial resources on a relatively egalitarian

3. See also the data in Mannheim Centre for European Social Research (2002).
4. See Rebick (2000) for an insightful account of the Japanese case.

Table 4.2 University graduates using university placement offices

Country	Utilization rates (%)	Used to get the first job (%)
Italy	10.3	1.42
Spain	39.3	3.96
France	18.1	3.21
United Kingdom	37.6	6.61
Germany	6.6	0.54

Source: Authors' calculation, based on the data set produced by the Project funded by the European Community under the Targeted Socio-Economic Research (TSER) "Careers after Higher Education: A European Research Study." Details on the project and downloadable material can be found at http://www.uni-kassel.de/wz1/tseregs.htm.2.

Notes: The relevant questions (asked in 1998 to graduates who obtained their degree between autumn 1994 and summer 1995) were: (a) "How did you search for your first job after graduation?"; (b) "Which method was most important for getting your first job after graduation?" Multiple options follow, among which "I enlisted the help of a careers/placement office in my higher education institution." The ratios displayed are computed, respectively, over graduates who have sought a job and over those graduates who have been employed at least once.

basis and their graduates' labor market performance does not affect their financial endowments, they may have little incentive to concern themselves with students' placement. In Italy, before *AlmaLaurea* was established, public universities were involved in minimal formal intermediation activity.[5] Table 4.2 refers to 1995 graduates and, for a selected sample of European countries, displays the share of graduates who used the services of their institutions' placement office (first column) and the share of graduates who got their first job through this channel (second column). It can be seen that Italy ranks low, higher only than Germany, in both respects.[6]

4.2.2 *AlmaLaurea*

AlmaLaurea was founded in 1994 and began online intermediation in 1995 at a time when, to the best of our knowledge, there were no other Internet job boards in Italy. Monster and InfoJob, the current most popular e-recruitment sites (according to Nielsen/NetRatings) started in 2001 and 2004, respectively.[7]

Initially run by the Statistical Observatory of the University of Bologna, *AlmaLaurea* is currently managed by a consortium of fifty private and public

5. There is anecdotal evidence that several departments on an informal basis provided unorganized, paper-based information on their graduates to recruiting companies.

6. Percentages are calculated using the data set built by a European Community-funded project under the Targeted Socio-Economic Research (TSER) "Careers after Higher Education: a European Research Study." See http://www.uni-kassel.de/wz1/tseregs.htm for details.

7. It ultimately proved impossible to establish with any precision the timing of the first Italian Internet job board. Nevertheless, according to personal communications with industry experts in the field the first was JobPilot, which was founded in 1999 and was acquired by Monster in 2005.

universities, with the support of the Italian Ministry of Education. Member universities pay a one-time association fee (ranging from 2,582 to 5,165 euros, according to the size of the university) and an annual subscription fee for the collection and the insertion of new data in the *AlmaLaurea* database (4.96 euros for each student in the database).

AlmaLaurea's institutional objectives are twofold. First, it provides member academic institutions with reliable information on their graduates. Second, it aims at facilitating graduates' labor market transition.

In terms of the first objective, *AlmaLaurea* manages a database that collects information on graduates, drawing it from three distinct sources. First, academic institutions provide official data on grades, course durations, and degrees received for their alumni. Second, undergraduates provide several pieces of information, including military service obligations, periods of study abroad, work experience, and a self-evaluation concerning foreign languages and computer skills. Finally, graduates have the option to upload and update their curricula online for up to three years after graduation.[8] In accordance with Italian privacy law, only a subset of the information in the database can be disclosed to third parties.[9]

With respect to the second objective, *AlmaLaurea* manages a service that gives firm electronic access to graduates' curriculum vitae (CV). The CV is an electronic file containing biographical information, age at graduation, university and high school grades, information on internships, experience abroad, postgraduate education, languages and computer skills, work experience, and work preferences (i.e., type of occupation desired, location, and contract preferred). Graduates may include additional information and a cover letter.[10]

The service is free for graduates. Firms and other institutions can browse individual curricula and observe populational aggregate information for free, but are required to pay if they want to contact a particular graduate. The price ranges between 0.5 and 10 euros per CV, depending on the type of subscription and the number of curricula acquired.[11]

Table 4.3 provides an overview of *AlmaLaurea*'s history and performance. It displays the number of universities enrolled, the share of graduates from *AlmaLaurea* universities, the numbers of resumes available to firms, and number sold by the consortium.

8. Recently, the option was extended to five years.

9. More information can be found online at http://www.almalaurea.it/eng/index.shtml.

10. A sample CV (in Italian) is available at: http://www.almalaurea.it/info/aiuto/aziende/esempio_cv.shtml.

11. Firms can choose between self-service or subscription. The so-called self-service involves payment of fifty euros, after which any number of CVs can be acquired at the cost of ten Euros per CV. Subscription allows a firm to prepay for a whole package of downloadable CVs, over a period of one year. The range is between 200 CVs for around 500 euros, and up to 5,000 CVs for 2,600 euros. More detailed information is available (in Italian) at http://www.almalaurea.it/info/condizioni/buono_ordine_abbonamenti.pdf.

Table 4.3 **Evolution of *AlmaLaurea***

	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
Number of universities	15	20	22	25	25	27	37	39	44	50
Share of graduates	.24	.31	.34	.39	.39	.37	.43	.51	.57	.67
Number of CV in *AlmaLaurea*	62,745	105,409	153,843	213,976	286,345	367,497	477,282	624,960	792,575	900,000
Number of CV sold	3,973	15,999	115,603	194,635	164,209	271,364	389,625			
CV sold in the same region (share)	.55	.72	.50	.37	.35	.30	.30			

Source: Authors' calculation based on data provided by *AlmaLaurea*. "Share of graduates" refers to the share of graduates in *AlmaLaurea* universities with respect to the entire population of graduates in Italian universities. Data on curriculum vitae (CV) sold for 2005, 2006, and 2007 are not available.

Note: Data for 2007 are estimates calculated in June 2007.

4.2.3 *AlmaLaurea* and the Economics of Electronic Labor Markets

The *AlmaLaurea* recruitment service turns out to be an insightful example concerning how online communication technologies—coupled with more traditional forms of intermediation—might ameliorate the way in which employers and employees match in the labor market. Online labor market intermediaries are expected to decrease the search costs for both employers and employees. Standard search theory predicts that, everything being equal, this should lead to better matches. Conversely, the effects on unemployment duration are ambiguous. In fact, although Burdett and Ondrich (1985) suggest that it is unlikely, online technologies might induce both job-seekers and employers to be more choosy and to increase their reservation wages and screening standards (Pissarides 2000). Finally, online labor market intermediaries are expected to weaken the constraints posed by geographical distance (Autor 2001). Consistently, in the *AlmaLaurea* case most graduates' curricula are bought by firms located in regions other than the one where the individual graduated (see table 4.3).

On the other hand, a likely consequence of lower costs in distinct job search channels is that job seekers, ceteris paribus, will apply for more jobs. And when employers perceive such *excess application* to be a problem, adverse selection is likely to undermine the effectiveness of cheap search methods (Autor 2001).

With the exception of time required to update personal information, *AlmaLaurea* is completely free for students and therefore is potentially exposed to the adverse selection problem referred to previously: employers might expect that individuals who upload and update their resumes online are somehow negatively selected. However, *AlmaLaurea*'s organizational features are likely to make its intermediation activity less exposed to this risk for two reasons.

First, as explained previously, some pieces of the information contained in the *AlmaLaurea* data set concern the entire graduate population and are provided directly by academic institutions. This information is organized by *AlmaLaurea* and made freely available online at its website.[12] For every member university and degree, the website provides information on average grades, share of students who completed their degree on time, and the share of individuals who studied abroad within an EU subsidized program. Therefore, employers who purchase a CV should be able to identify differences between the selected job-seeker and the entire graduate population, which considerably reduces the adverse selection problem.

Second, academic institutions that joined *AlmaLaurea* are able to enroll the vast majority of their graduates. For instance, more than 92 percent of 1998 graduates updated their CVs online at least once. High participation

12. See (in Italian) http://www.almalaurea.it/cgi-php/aziende/profilo/profilo.php.

rates have been very effective in building a good reputation for the service, and make adverse selection unlikely. To sum up, we expect that *AlmaLaurea*'s particular organizational features protect it from the disadvantages of online labor markets.

4.3 The Empirical Strategy

The basic goal of this chapter is to evaluate the impact of a treatment; that is, the availability of online labor market intermediaries on an array of labor market outcomes—that is, the probability of being unemployed, mobility, and matching quality. This section formalizes and explicitly discusses our empirical approach and outlines the strategies employed to assess its validity.

One of the most serious empirical problems that arises in assessing the impact of online intermediaries is that job-seekers and firms typically self select in the adoption of online technologies. It is therefore difficult to identify to what extent the correlation between their use and labor market outcomes stems from the technology itself and to what extent it stems from some important and difficult-to-measure individual characteristics.

In this chapter we can rely on a transparent exogenous source of variation; that is, the timing of universities' enrollment in *AlmaLaurea.* This heterogeneity allows us to apply the DID method to a repeated cross-sectional data set. This helps to overcome the previously mentioned problem.

The simple DID framework can be described as follows. The causal effect of a treatment on an outcome is defined as the difference between two potential outcomes (Rubin 1974; Heckman 1990). Of course, it is impossible to observe such an effect for a given individual. However, it is possible to identify an average effect if the population of interest is observed in at least two distinct time periods, if only a fraction of the population is exposed to treatment, and if we assume parallel paths over time for treated and controls. The main intuition is that, under this design, an untreated group of the population is used to identify time variation in the outcome that is not due to treatment exposure.

More formally, each individual i belongs to one group, $G_i \in \{0, 1\}$, where for convenience group 1 is the treatment group and 0 the control one. Moreover, individual i is observed only in time period $T_i \in \{0, 1\}$. Let $I_i = G_i \cdot T_i$ denote an indicator for the *actual* subministration of treatment.[13] Then $Y_i^N(t)$ and $Y_i^I(t)$ represent two *potential* outcomes: respectively, that achieved by i at time t if not treated and that achieved if treated before t.

The fundamental problem in identifying the treatment effect on individual i, defined as $Y_i^I(t) - Y_i^N(t)$, is that for any particular individual, it is not pos-

13. Note that in our simple setting I_i assumes the value 1 only for the treatment group ($G_i = 1$) in the post-treatment period ($T_i = 1$).

sible to observe both potential outcomes. What we do observe is the *realized* outcome, which can be written as $Y_i(t) = Y_i^I(t) \cdot I_i + Y_i^N(t) \cdot (1 - I_i)$.

If it is assumed that

$$(1) \qquad E[Y_i^N(1) - Y_i^N(0) \mid G_i = 1] = E[Y_i^N(1) - Y^N(0) \mid G_i = 0],$$

then it easily follows that

$$(2) \qquad E[Y_i^I(1) - Y_i^N(1) \mid G_i = 1] = E[Y_i(1) \mid G_i = 1] - E[Y_i(0) \mid G_i = 1] - \{E[Y_i(1) \mid G_i = 0] - E[Y_i(0) \mid G_i = 0]\}.$$

In other words, if the average outcomes for the treatment and control groups had parallel paths over time in the absence of treatment, then the so-called average treatment effect on the treated (ATT) can be expressed as something whose sample counterpart is observable; that is, as the average variation of the treatment group purged by the average variation of the control group.

Hence in the present study, it is assumed that in absence of *AlmaLaurea* the average occupational outcomes of graduates from early joining universities (hereafter *AlmaLaurea* universities) would have followed the same dynamics as those of graduates from universities that either joined later or did not join (hereafter non-*AlmaLaurea* universities). Thus the average effect of *AlmaLaurea* is obtained simply by subtracting the dynamics of the graduates of the control group from the dynamics of those in the treatment group.

The previous estimator is easily obtained as

$$(3) \qquad Y_i = \mu + \gamma \cdot G_i + \delta \cdot T_i + \alpha \cdot (G_i \cdot T_i) + u_i,$$

where α is the ATT and the assumption stated in equation (1) is equivalent to mean independence.

The validity of our approach faces a number of threats. As far as the so-called internal validity is concerned—that is, the causal effect within the context of the study—there are two main problems.[14] First, the compositional effect: the use of repeated cross sections is only valid when the composition of the target population does not change between the two periods; that is, $u_i \perp T_i \mid G_i$. Given that individual decisions concerning college enrollment were taken before the existence of *AlmaLaurea*, we can presume that, in our case, this problem is not very severe. Nevertheless, following standard practice, we shall test whether the means of relevant characteristics of the population within each group did change unevenly between the pretreatment and the posttreatment periods.

Second, the assumption of parallel dynamics in the absence of treatment between the two groups (equation [1]) turns out to be strong. It is, in fact, possible that the two groups have different trends for reasons other than

14. See Meyer (1995) for a comprehensive discussion of internal validity in this framework.

treatment. However, if nonparallel dynamics are due to observables, we can overcome the problem by including covariates. This analysis, as we discuss in detail in section 4.4, relies on a large array of individual and university covariates. Nevertheless, if the dynamics of the outcome variables of the two groups are affected by unobservables, identification breaks down.[15] In section 4.6 we try to overcome this important problem using data for an additional pretreatment period in order to test for nonparallel paths between the treatment and control groups before treatment.

Another issue concerns the unit of analysis of our ATT. It could be that *AlmaLaurea* might not be an appropriate individual level treatment, since member institutions are enrolled at once, and there are possibly important interactions among each university's students. If, for instance, the impact of *AlmaLaurea* on a given student depends on the characteristics of students in his or her cohort, we are measuring the effect on university rather than the individual graduate employment performance. Although in the present study we model *AlmaLaurea* as an individual level treatment, in future research we aim to investigate the possibility of within-university spillovers.

Similarly, to be valid, the DID approach assumes no interactions among the agents in the treatment and control groups. If, for example, *AlmaLaurea* graduates improved occupational outcomes harm non-*AlmaLaurea* graduates, our estimates have very different implications, especially in terms of informing policy. In section 4.7, we try to assess this problem by identifying additional control and treatment groups that include only graduates from those universities that are located in the same geographical region.

Finally, in order to generalize the results to different individuals and contexts, external validity is important. It is possible that *AlmaLaurea* would not have had an effect for graduates from those universities that chose not to join. This would also explain why they did not join. However, we do not think this is a major problem since, as mentioned in the introduction, membership tended to be accidental, at least during the first years. Nevertheless, in the following we test whether the observable characteristics of the universities in the two groups differ significantly.

4.4 The Data

Our data on graduates are drawn from two almost identical surveys—*Indagine Inserimento Professionale Laureati* (Survey on University-to-Work Transition) in 1998 and 2001 of individuals who graduated in 1995 and 1998, respectively.[16]

15. Given that decisions to enroll in *AlmaLaurea* are made by universities, we are mostly concerned with university unobservables.

16. The publicly available microdata do not include information concerning from which university the surveyed individual graduated. Therefore, we carried out the analysis at the ADELE ISTAT laboratory in Rome.

Table 4.4 **Universities enrolled in *AlmaLaurea***

1994	University of Bologna starts collecting electronic data concerning its graduates
1995	University of Bologna starts selling data
1996	University of Modena-Reggio Emilia, Ferrara, Parma, and Florence start selling data
1997	University of Catania, Trieste, Udine, Messina, Chieti, Trento, Molise, and Venice School of Architecture start selling data
August 1998	University of Turin and Eastern Piedmont start selling data

Note: The Venice School of Architecture started selling data on January 1. For consistency, it is included in 1997 group. The universities of Siena and Lecce joined in 1997, but did not start to sell CVs until 1999 and 2003, respectively. All the information is available on the *AlmaLaurea* website.

To implement the econometric approach described in section 4.3 we include in our main treatment group those individuals graduating from universities that joined *AlmaLaurea* in 1996 and 1997. As shown in table 4.4, this includes the universities of Modena-Reggio Emilia, Ferrara, Parma, Florence, Catania, Trieste, Udine, Messina, Chieti, Trento, Molise, and Venice School of Architecture. Students in the treatment group account for about 18 percent of the sample (see table 4.5).

In section 4.5.2, we exploit an additional source of variation. As shown in table 4.4, the Universities of Turin and Eastern Piedmont start selling graduates' CVs only after August 1998. Thus, we used graduates from these universities as an additional treatment group in a DID setting in which the "before and after" are the time of graduation before and after August 1998, and only graduates from 1998 are considered.[17]

Unfortunately, ISTAT does not provide information concerning the month of graduation for 1995 graduates. Therefore, graduates from Bologna are not considered in the analysis.[18]

The ISTAT target samples consist of 25,716 individuals in 1998 and 36,373 individuals in 2001. They represent, respectively, 25 percent and 28.1 percent of the total population of Italian university graduates. The responses were 64.7 percent and 53.3 percent, for a total of 17,326 and 20,844 respondents.[19] After eliminating individuals who did not respond to the question concern-

17. In Italy, graduates can complete their degree at different times in the same academic year, depending on when they finish their dissertation.

18. Bologna is also a very special case, the most self-selected one, given that it is the university where *AlmaLaurea* got started. However, results do not change qualitatively whether we include Bologna graduates in the control group or the treatment group.

19. Differences in response rates probably stem from the different interviewing techniques in the surveys: in 1998, ISTAT mailed paper-based questionnaires, while in 2001 the CATI (Computer Assisted Telephone Interview) was used. In principle, this change should affect universities in a homogenous way and therefore it should not represent a major problem for our analysis.

Table 4.5 **Sample design and means of key variables**

	All	*AlmaLaurea*	Non-*AlmaLaurea*
		1998 Survey	
Number of graduates	15,282	3,512	11,770
Weighted share		.188	.812
		2001 Survey:	
Number of graduates	18,181	3,515	14,666
Weighted share		.183	.817
		Means of selected sample characteristics in 1998	
Share of female	.527	.528	.527
	(.004)	(.010)	(.005)
Age	27.45	27.61	27.41
	(.038)	(.086)	(.042)
High school grade	48.38	47.87	48.49
	(.066)	(.151)	(.074)
		Means of selected sample characteristics in 2001	
Share of female	.551	.567	.548
	(.004)	(.009)	(.004)
Age	27.47	27.55	27.45
	(.028)	(.063)	(.031)
High school grade	48.96	48.62	49.04
	(.057)	(.130)	(.064)

Notes: Standard errors in parentheses. Shares, means, and standard errors are computed with stratification weights. High school grades range from 36 to 60. Only individuals that responded to the question about employment status are considered.

ing their employment status, those with missing values for key variables, and graduates from Bologna, Turin, and Eastern Piedmont, we are left with 15,282 and 18,181 observations, respectively. In both years the sample is stratified according to sex, university, and university degree, and in the following analysis all estimations are performed using stratification weights.

The surveys collect information on: (a) school and university curricula; (b) labor market experience; and (c) demographic and social backgrounds of graduates. Table 4.5 presents summary statistics for the key variables. In the following analysis, individual level right-hand variables are grouped into two subsets. The first includes characteristics that are predetermined with respect to college efforts and outcomes: sex, age, high school grades, fourteen dummies for high school type, one dummy for having two university degrees, five dummies for each parent's level of education, 104 dummies for province of residence before college enrollment, and 345 dummies for departments (university · field of study). The second contains indicators related to college curricula that could—at least potentially—be influenced by *AlmaLaurea:* university grade and number of years to graduation.

As table 4.5 shows, with the exception only of the share of women, which increased for both groups, the remaining variables show no notable varia-

Table 4.6 **Universities' characteristics**

	All	*AlmaLaurea*	Non-*AlmaLaurea*
Universities in 1995			
Number of universities	59	12	47
Average number of students	23,946	22,033	24,434
	(3,742)	(4,569)	(4,568)
Average number of students per professor	31.09	26.27	32.32
	(2.59)	(2.53)	(3.17)
Average share of delayed students	.288	.278	.291
	(.010)	(.026)	(.011)
Universities in 1998			
Number of universities	61	12	49
Average number of students	25,473	24,134	25,801
	(3,875)	(5,096)	(4,679)
Average number of students per professor	31.82	26.50	33.12
	(2.36)	(3.15)	(2.82)
Average share of delayed students	.362	.396	.354
	(.011)	(.029)	(.012)

Notes: Averages are computed at university level. Standard errors in parentheses.

tions within groups over time. Moreover, control and treatment groups present very similar characteristics for both years, reducing the possibilities of major interactions (beyond the treatment itself) at the individual level, of between being enrolled in a college member of *AlmaLaurea* and graduating in 1998.

In order to control for observable variations in academic institution quality, we use data on university characteristics provided by ISTAT in its annual *Lo Stato dell'Universitá* (University Indicators), for the academic years 1991 to 1998. In particular, we collect information at the level of the individual university, on numbers of students, professors, and delayed students.[20] Table 4.6 shows that universities in the treatment group enroll fewer students per professor than the universities in the control group. The difference, however, is not statistically significant. The two groups have very similar average rates of delayed students. Both indicators are generally considered proxies for university teaching quality.[21] Note, also, that the share of delayed students increased in both groups, but the increase is steeper for the treatment group. In terms of overall number of students, the two groups of universities have very similar averages.

Finally, to control for major economic shocks that may affect graduate labor market performance, we collect province-level information on per capita gross domestic product (GDP) and unemployment rates.[22]

20. In Italy, most students graduate after the official deadline.

21. As discussed in Bagues, Sylos Labini, and Zinovyera (2008), both indicators have drawbacks in a system such as the Italian one, where most universities cannot restrict entry and therefore the number of students per professor depends, among other things, on demand.

22. Italy is composed of 104 provinces, which correspond approximately to U.S. counties.

The present study considers three basic outcome variables measured three years after graduation: occupational status, which takes the value 1 if an individual is unemployed, and zero otherwise;[23] regional mobility, which takes the value 1 if the individual resides in a different region from the one where he or she graduated;[24] and wage, measured as net monthly wage expressed in euros and self reported by the interviewed. We also consider two additional proxies for matching productivity. The first is for the perceived level of adequacy of the knowledge acquired at university with respect to the content of the present job. The second is related to the perceived stability of the job. Both variables are self reported and take values from 1, not at all satisfied, to 4, very satisfied.

4.5 The Impact of *AlmaLaurea*

4.5.1 Universities That Joined in 1996 and 1997

A first picture of the impact of *AlmaLaurea* is obtained by comparing time differences in means of key outcomes within the two groups (treatment and control). Table 4.7 shows that unemployment rates decreased sharply from 1998 to 2001 for the whole target population.[25] Moreover, and most importantly for the present study, occupational status improved the most for those in the treated group: the rate of unemployment decreased about 3.5 points more than in the control group. Note also that the ranking between the two groups reverses. This means that the same qualitative result would be obtained if we used changes in employment logs as outcome variables.

For mobility, rates remained stable for *AlmaLaurea* students, and decreased for non-*AlmaLaurea* ones. Hence, for graduates in the treatment group, regional mobility increased by about 1 point relative to graduates in the control group. However, this difference is not statistically different from zero. Note also that graduates in the treatment group are more mobile than those in the control group. Finally, in terms of matching quality, monthly wages increased by some forty-four euros more for *AlmaLaurea* graduates than for the control group.

To interpret these results as being the sole effect of *AlmaLaurea* involves

23. Following standard definitions, we consider unemployed to be those individuals who declare not to having worked during the week before the interview and to be searching for a job.

24. Italy is composed of twenty regions.

25. Italian labor market conditions improved substantially between 1998 and 2001. According to ISTAT, standardized unemployment rates for the entire population were 11.7 in 1998 and 9.4 in 2001. The change was from 12.8 to 9.8 for university graduates aged between twenty-five and thirty-nine. It could be that our figures display a steeper decrease because individuals in the sample are younger and because of the changes made to the survey technique mentioned earlier.

Table 4.7 **Unemployment, mobility, and wages by year and *AlmaLaurea***

	1998	2001	Diff.
	Unemployment		
AlmaLaurea	.228	.094	–.134
Non-*AlmaLaurea*	.205	.107	–.098
Diff.			**–.036*****
Standard error			(.011)
	Mobility		
AlmaLaurea	.297	.292	–.005
Non-*AlmaLaurea*	.219	.203	–.016
Diff.			**.011**
Standard error			(.014)
	Wage		
AlmaLaurea	899.7	1,118.4	218.7
Non-*AlmaLaurea*	980.9	1,155.1	174.2
Diff.			**44.5*****
Standard error			(16.8)

Notes: Unemployment rates are computed using stratification weights. We consider unemployed to be those individuals who did not work during the week before the interview who were looking for a job. Average gross monthly wages are expressed in euros and are calculated for the 20,838 individuals that provide this information. The bold differences are the results of a DID estimation, where Diff = $(Y^{01}_{Alma} - Y^{98}_{Alma}) - (Y^{01}_{\text{non-}Alma} - Y^{98}_{\text{non-}Alma})$. In parentheses are robust standard errors of regressions of the dependent variables on dummies for year, belonging to *AlmaLaurea,* and their interaction.

assuming that in the absence of the treatment the averages of the two groups would have experienced the same variation (equation [1]). This is a strong restriction when treatment (i.e., graduating from a university enrolled in *AlmaLaurea*) is not randomly assigned across individuals. In the remaining part of the chapter we use the approaches outlined in section 4.3 to assess the extent to which the observed changes may be interpreted as the effect of *AlmaLaurea*.

The basic identification assumption of the DID method (equation [1]) may be too stringent if treatment and control groups are unbalanced in covariates that are thought to be associated with the dynamics of the outcome variable. To begin with, we follow the traditional way to accommodate this problem and introduce a linear set of controls X_i in equation (3), which then becomes:

$$Y_i = \mu + \beta \cdot X_i + \gamma \cdot G_i + \delta \cdot T_i + \alpha \cdot (G_i \cdot T_i) + u_i. \quad (4)$$

Tables 4.8, 4.9, and 4.10 report ordinary least square (OLS) coefficients of this equation where the outcome is, respectively, unemployment, mobility, and log wages. All standard errors are corrected for the nonindependence of employment outcomes of individuals graduating in the same region, degree,

Table 4.8 **The effect of *AlmaLaurea* on unemployment probability**

	(1)	(2)	(3)	(4)
AlmaLaurea	–.020** (.008)	–.021** (.008)	–.021** (.008)	–.016* (.008)
2001	–.101*** (.013)	–.103*** (.013)	–.099*** (.013)	–.073*** (.013)
Female	.060*** (.006)	.061*** (.005)	.061*** (.005)	–.061*** (.005)
Age	–.002** (.001)	–.004*** (.001)	–.004*** (.001)	–.004*** (.001)
High school grade	–.002*** (.0003)	–.001*** (.0004)	–.001*** (.0004)	–.001*** (.0004)
University grade		–.001** (.005)	–.001** (.001)	–.001** (.0006)
Students per faculty			–.002*** (.001)	–.002*** (.001)
Share of delayed students			–.018 (.073)	–.076 (.077)
GDP				–.001*** (.0003)
Provincial unemployment				.009*** (.003)
Dummies on year delay		YES	YES	YES
R^2	0.147	0.147	0.149	0.150
Observations	33,463	33,463	33,463	33,463

Notes: The results of four different specifications of a linear probability model are displayed. The dependent variable assumes the value 1 if the individual declares not to be working and to be searching, 0 otherwise. All specifications include university · department fixed effects, fourteen dummies for high school type, eleven dummies for having another university degree, five dummies for each parent's level of education, 104 dummies for province of residence before university enrolment. Column (1) includes only predetermined individual control, column (2) considers all individual controls, column (3) incorporates time-variant university characteristics, and column (4) includes Provincial GDP and unemployment rate. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

and year.[26] The analysis is structured along the classification described in section 4.4—hence four specifications are displayed: column (1) includes individual characteristics predetermined before university entry; column (2) presents also potentially endogenous individual controls; column (3) incorporates time-variant university characteristics; and column (4) displays the results of a regression that includes province unemployment and GDP per capita. Note that all specifications include university time department dummies.

Table 4.8 shows that, conditional on individual characteristics, if a university decides to affiliate to *AlmaLaurea* the probability that its graduates are unemployed three years after graduation significantly decreases by about two points. Potentially endogenous individual regressors (column [2]) and university controls (column [3]) do not significantly affect our results. Conversely, controlling for provincial unemployment rates and GDP (column [4]) reduces the magnitude of the coefficient to about 1.6 points, and also

26. If we cluster standard errors at university level, most of the coefficients are not statistically significant at the 10 percent level.

Table 4.9 **The effect of *AlmaLaurea* on mobility**

	(1)	(2)	(3)	(4)
AlmaLaurea	.024** (.011)	.024** (.012)	.027** (.012)	.024** (.012)
2001	–.008 (.007)	–.008 (.007)	.007 (.008)	–.009 (.011)
Female	–.022*** (.004)	–.022*** (.005)	–.022*** (.005)	–.022*** (.005)
Age	–.001** (.001)	.0004 (.0006)	.0003 (.0006)	.0002 (.0006)
High school grade	.001** (.0003)	.0002 (.0003)	.0003 (.0003)	.0004 (.0003)
University grade		.0003 (.0006)	.0003 (.0006)	.0001 (.0006)
Students per faculty			.001 (.001)	–.001 (.001)
Share of delayed students			–.209*** (.001)	–.179** (.077)
GDP				–.0005 (.001)
Provincial unemployment				–.005 (.004)
Dummies on year delay		YES	YES	YES
R^2	0.282	0.283	0.283	0.283
Observations	33,463	33,463	33,463	33,463

Notes: The results of four different specifications of a linear probability model are displayed. The dependent variable assumes the value 1 if an individual resides in a different region from one where he or she attended university, and 0 otherwise. All specifications include university · department fixed effects, fourteen dummies for high school type, eleven dummies for having another university degree, five dummies for each parent's level of education, 104 dummies for province of residence before university enrolment. Column (1) includes only predetermined individual controls, column (2) considers all individual controls, column (3) incorporates time-variant university characteristics, and column (4) includes Provincial GDP and unemployment rate. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

reduces its statistical significance. However, the coefficient is still statistically significant at the 10 percent level. Quantitatively, this implies that, out of the 23,688 individuals that graduated from a member university in 1998, 379 graduates were out of unemployment as a consequence of *AlmaLaurea* adoption. Although we do not have direct evidence on the extent to which *AlmaLaurea* crowded out other search channels, this finding is plausible if one observes the high number of curricula sold by the consortium displayed in table 4.3.

Table 4.9 also shows that regional mobility rates have different dynamics for graduates in *AlmaLaurea* universities: depending on the controls used, *AlmaLaurea* has a positive and statistically significant effect on mobility, ranging from 2.3 to 2.8 points.[27] About 570 individuals, which without the consortium would have been resident in the region where they graduated, moved to a different one.

As mentioned, lower search costs are also expected to improve the quality

27. Similar results are obtained if we consider provincial mobility.

Table 4.10 **The effect of *AlmaLaurea* on wages**

	(1)	(2)	(3)	(4)
AlmaLaurea	.034* (.017)	.036** (.018)	.035* (.018)	.031* (.018)
2001	.222*** (.015)	.227*** (.015)	.227*** (.016)	.201*** (.020)
Female	–.153*** (.008)	–.157*** (.008)	–.158*** (.008)	–.158*** (.008)
Age	.013*** (.002)	.017*** (.001)	.017*** (.002)	.017*** (.002)
High school grade	.005*** (.0005)	.003*** (.0006)	.003*** (.0006)	.003*** (.0006)
University grade		.005*** (.0008)	.005*** (.0008)	.005*** (.0008)
Students per faculty			.003* (.001)	.002 (.001)
Share of delayed students			.020 (.100)	.055 (.102)
GDP				.002*** (.0005)
Provincial unemployment				–.011** (.005)
Dummies on year delay		YES	YES	YES
R^2	0.252	0.259	0.259	0.260
Observations	20,838	20,838	20,838	20,838

Notes: The results of three different specifications of an OLS model are displayed. The dependent variable is the logarithm of monthly net wages. All specifications include university · department fixed effects, fourteen dummies for high school type, eleven dummies for having another university degree, five dummies for each parent's level of education, and 104 dummies for province of residence before university enrollment. Column (1) includes only predetermined individual control, column (2) considers all individual controls, column (3) incorporates time-variant universities characteristics, and column (4) includes provincial GDP and provincial unemployment rates. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

of labor market matches. Table 4.10 shows that, according to our analysis, *AlmaLaurea* significantly increases monthly wages by about 3 percent.[28] Taking as a reference the average wage, this implies that working graduates made about thirty-five more euros per month. We also find that *AlmaLaurea* increases graduates' satisfaction with the adequacy of the knowledge acquired at university, and job stability.[29]

4.5.2 Universities That Joined in 1998

The previous findings may be driven by time-varying omitted university characteristics. To investigate whether this is the case, in this section, we exploit an additional source of exogenous variation. The universities of Turin and Eastern Piedmont joined *AlmaLaurea* in August 1998 and hence sold resumes online only for those 1998 graduates who completed

28. This result needs to be interpreted with caution because of the possible different composition of the two samples. In fact, wage regressions are run only for those individuals who are employed.

29. Results are not reported but are available upon request by the authors.

Table 4.11 **Effect of *AlmaLaurea:* The case of Turin and Eastern Piedmont**

	Panel A *Unemployment*		
	Pre-August	Post-August	Diff.
Turin and Eastern Piedmont	.038	.016	–.022
Non-Turin and Eastern Piedmont	.102	.104	.002
Diff.			**–.024****
Standard error			(.011)
	Mobility		
Turin and Eastern Piedmont	.165	.164	–.001
Non-Turin and Eastern Piedmont	.227	.228	.001
Diff.			**.002**
Standard error			(.026)
	Wage		
Turin and Eastern Piedmont	1,151.4	1,103.9	–47.5
Non-Turin and Eastern Piedmont	1,152.3	1,134.1	–18.2
Diff.			**–29.4**
Standard error			(32.1)

	Panel B		
	Unemployment	Mobility	Long wage
AlmaLaurea	–.025*** (.008)	.009 (.022)	–.016 (.018)
Female	.043*** (.005)	–.021*** (.007)	–.149*** (.009)
Age	–.002* (.001)	.0005 (.001)	.017*** (.002)
High school grade	–.001*** (.0002)	.0001 (.0003)	.002*** (.0006)
University grade	–.001** (.0006)	.0004 (.001)	.005*** (.001)
Dummies on year delay	YES	YES	YES
Dummies for month of graduation	YES	YES	YES
R^2	0.122	0.251	0.226
Observation	20,547	20,547	12,975

Notes: The analysis is on 1998 graduates. The treatment group is composed of graduates from the universities of Turin and Eastern Piedmont. Before and after is graduation before and after August. All specifications include university · department fixed effects. Robust standard errors in parentheses. All regressions are clustered at region · degree · year. The bold differences are the result of a DID estimation.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

their degree after that date. In our alternative DID setting the new treatment group is composed of graduates from these two universities, with the before-and-after being graduation after August 1998. In this specification, only 1998 data are considered and dummies for month of graduation are included. As table 4.11 shows, *AlmaLaurea* significantly decreases unemployment probability by about 2.5 points, which is a similar magnitude to

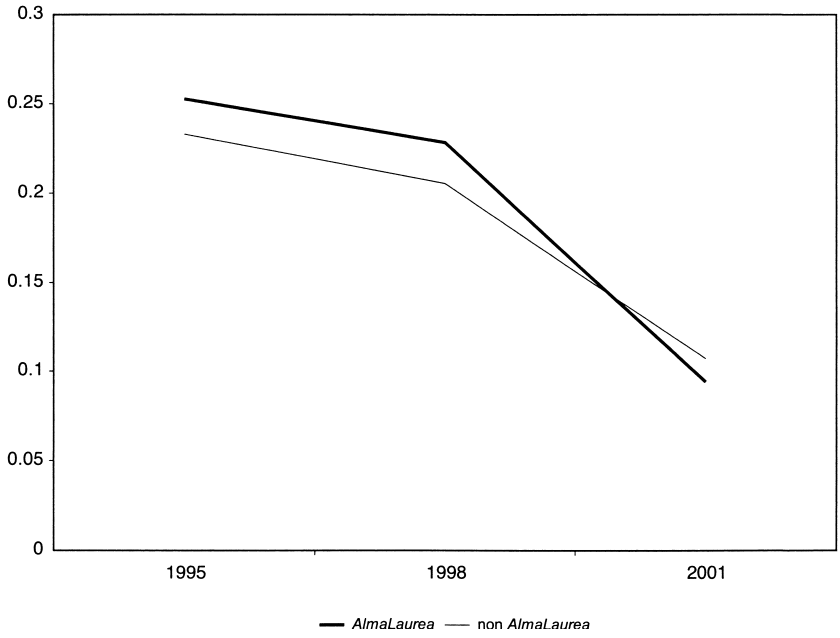

Fig. 4.1 Shares of unemployed graduates
Note: Only graduates from university departments that were in the database in 1995 are considered.

the effect achieved, as shown previously. However, there is no significant effect on either mobility or wages.

4.6 Unparallel Outcomes

Possibly the most important threat to the internal validity of the previously discussed results is the extent to which the "parallel trends" assumption stated in equation (1) is valid. One of the standard ways of assessing its plausibility is to use data from the pretreatment periods to check whether trends were parallel in the past. If this is the case, it is likely that the results achieved here stem from the treatment itself.

The Italian Statistical office conducted an earlier university-to-work survey on 1992 graduates, who were interviewed in 1995.[30] As depicted in figure 4.1, prior to 1998 the employment rate dynamics for the control and the treatment groups were remarkably similar. We apply the DID method with linear controls on data for 1992 and 1995 graduates; that is, before *AlmaLaurea* came into existence. Table 4.12 shows that the DID coefficient

30. Unfortunately, the 1995 survey does not include data on wages.

Table 4.12 **Pre-adoption falsification test of *AlmaLaurea***

	Unemployment	Mobility
AlmaLaurea	.004 (.013)	.011 (.012)
1998	–.027*** (.008)	.005 (.006)
Female	.079*** (.008)	–.026*** (.005)
GDP	–.001** (.0004)	–.003 (.003)
Provincial unemployment	.003 (.002)	.001 (.002)
R^2	0.150	0.322
Observations	27,373	27,565

Notes: In the first column the dependent variable takes the value 1 if a given graduate is unemployed, and 0 otherwise. In the second column the dependent variable takes the value 1 if a given individual resides in a different region from the one where he or she attended universities. Only individuals that graduated in 1992 and 1995 are considered. *AlmaLaurea* takes the value 1 for 1995 graduates from universities that enroll in *AlmaLaurea* between 1995 and 1998. All specifications include university · department fixed effects. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

for unemployment is positive, negligible, and not statistically different from zero. The result is similar for mobility: the *AlmaLaurea* coefficient is not statistically different from zero. This reduces the likelihood that the coefficients in tables 4.8 and 4.9 stem from unparallel trends in the two groups.

Of course, these checks do not control for time-specific unparallel outcomes. In fact, possible interactions between *AlmaLaurea* enrollment and unobserved time-variant characteristics cannot easily be ruled out. One could argue, for example, that those universities that self selected in the treatment group are the ones whose unobservable teaching quality improved most. This might affect the occupational outcomes of their graduates.

To investigate this possibility we build a placebo treatment group composed of graduates from the universities of Siena and Lecce. According to *AlmaLaurea* official sources, these universities decided to join *AlmaLaurea* in 1997, but did not start selling their students' resumes online until 1999 and 2003, respectively. If these graduates also experienced an improvement vis-â-vis the others, the likelihood that *AlmaLaurea* enrollment proxies for something else is higher. We ran a regression similar to the one in equation (4), but with graduates from Siena and Lecce as the treatment group and non-*AlmaLaurea* universities as the control. Table 4.13 shows that this group experienced a slight increase in unemployment and wages and a decrease in mobility. None of these changes is statistically significantly different from zero. These findings provide evidence against the possibility that enrollment in the treatment group is correlated with unobservables that independently cause employment improvements.

Table 4.13 **Effect of *AlmaLaurea* using a placebo treatment group**

	Unemployment	Mobility	Log wage
Placebo *AlmaLaurea*	.024	–.017	.011
	(.025)	(.026)	(.036)
R^2	0.152	0.389	0.260
Observations	26,278	26,278	16,464

Notes: Placebo *AlmaLaurea* takes the value 1 for graduates for 1998 from the universities of Siena and Lecce, 0 otherwise. All specifications include the full set of controls used in the 4th columns of tables 4.8, 4.9, and 4.10. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

4.7 Alternative Treatment and Control Groups and Displacement Effect

The DID design can be further strengthened using alternative treatment and comparison groups. In fact, this is likely to reduce the importance of biases or random variation occurring in a single setting (Meyer 1995). In the ideal specification, treatment and control groups should face the same time-specific shocks: the more similar the two groups are, the better. Given that our dependent variables concern labor market outcomes and that (according to our data when the survey took place) more than 75 percent of Italian graduates reside in the region where they attended university (see table 4.7), a new sample is created including only graduates from regions that include both *AlmaLaurea* and non-*AlmaLaurea* universities.

Three Italian regions fit this criterion: Tuscany, Abruzzo, and Sicily. Graduates in these regions represent about 17 percent of the entire population and, within this group, about 57 percent of graduates are in the treatment group universities. As can be seen from figure 4.2, *AlmaLaurea* universities are Florence, Chieti, Catania, and Messina. Non-*AlmaLaurea* universities are Pisa, Siena, L'Aquila, Teramo, and Palermo. Table 4.14 shows that with respect to the general case, in this setting *AlmaLaurea* has a stronger effect on employment probability (3.5 points) and wages (5 percent) and about the same impact on mobility. The result for wages is not statistically significant. Overall, however, the general results are confirmed and even strengthened.

This control exercise is also helpful for checking for an additional potential problem in our analysis. As mentioned in section 4.3, graduates from nearby universities might be used to assess whether there is a displacement effect on non-*AlmaLaurea* students due to a reallocation of hiring. Interactions are in fact more likely for graduates' occupational outcomes from nearby universities. Hence, for example, the impact of *AlmaLaurea* might be exaggerated if individuals in the control group were negatively affected by *AlmaLaurea* itself. For instance, Pisa, in principle, is a better control group for Florence than Bari; nevertheless, the risk that its graduates' labor market performance is negatively affected by the presence of *AlmaLaurea* in Florence is higher. To control for this possibility, we perform a DID analysis

Fig. 4.2 Regions with both *AlmaLaurea* and non-*AlmaLaurea* universities
Note: Map displays only those cities that have a university.

with non-*AlmaLaurea* universities in regions where there are *AlmaLaurea* universities constituting the treatment group, with the control group being the remaining non-*AlmaLaurea* universities. From table 4.15, it can be seen that there are no significant differences in the trajectories of the two groups. This suggests that there are no major interactions among the graduates in the two groups and that *AlmaLaurea* does not have negative spillovers on universities located close by.

4.8 Conclusions

Since the late 1990s we have seen a large increase in the importance of online labor market intermediaries. While their diffusion may potentially

Table 4.14 **Alternative treatment and control groups, based on geographic proximity**

	Unemployment	Mobility	Wage
AlmaLaurea	–.035**	.024*	.053
	(.017)	(.026)	(.039)
R^2	0.149	0.492	0.263
Observations	6,225	6,225	3,521

Notes: Only graduates from regions that have both *AlmaLaurea* and non-*AlmaLaurea* universities are included. All specifications include the full set of controls used in column (4) of tables 4.8, 4.9, and 4.10. Robust standard errors in parenthesis. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 4.15 **The effect of *AlmaLaurea* on nearby universities**

	Unemployment	Mobility	Wage
AlmaLaurea	–.008	.006	.010
	(.012)	(.015)	(.023)
R^2	0.152	0.295	0.260
Observations	26,436	26,436	16,464

Notes: Only individuals who graduated from non-*AlmaLaurea* universities are included. The variable *AlmaLaurea* takes the value 1 if a 1998 graduate is awarded a degree from a non-*AlmaLaurea* university that is located in a region where there are also *AlmaLaurea* universities, and 0 otherwise. All specifications include the full set of controls in the 4th columns of tables 4.8, 4.9, and 4.10. Robust standard errors in parentheses. All regressions are clustered at region · degree · year.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

improve labor market functioning, increasing the total quantity and quality of matches, solid evidence of their benefits is still missing. In addition, recent works have underlined the possibility of adverse selection in the use of electronic intermediaries among the unemployed (Kuhn and Skuterud 2004).

In this article we exploited the exceptional case study provided by the early adoption of the online intermediary *AlmaLaurea* by several Italian universities. The absence of other electronic intermediaries for those universities that had not adopted *AlmaLaurea* at the time of our study provides us with an adequate control group to estimate the effect of the treatment.

We employed the difference-in-differences method on a repeated cross-sectional data set. Given that enrollment in *AlmaLaurea* is not random, evaluating its impact is not trivial. However, assuming parallel outcomes

between treatment and control group makes our estimation valid. The inclusion of time-variant indicators concerning individual and university characteristics and standard tests aimed at ruling out alternative explanations do not raise major concerns in relation to this important assumption.

The evidence shows that the adoption of the online labor market intermediary under study improved graduates' labor market outcomes three years after graduation. In particular, according to our most conservative estimate, *AlmaLaurea* decreased graduates' unemployment probability by about 1.6 percentage points.

Our study also suggests that online labor market intermediaries may have a positive effect on matching quality. In fact, in our case study, the wages of graduates from member universities increased by about 3 percent. Finally, we also observe an increase in mobility by about 2.4 percentage points.

The findings of this chapter are specific to a given segment of the labor market (i.e., university-to-work transition) and to a peculiar electronic intermediary. Thus, their external validity has to be carefully assessed. In particular, the single characteristic of *AlmaLaurea* that possibly made it a successful intermediary is also the most unusual: member universities certify the information contained in electronic curricula and also provide some information on the entire population of graduates. This important caveat helps to integrate our findings within the existing literature that does not find any effect of online search on the overall unemployment rates and duration (Kuhn and Skuterud 2004; Kroft and Pope 2008).

The results presented in this chapter also contribute to the policy discussion on the university-to-work transition. The poor labor performance of Italian graduates has been traditionally ascribed to demand-and-supply factors. We show that graduate labor market functioning can also be improved by the introduction of online intermediaries.

In future research we aim at exploring whether the positive impact of electronic labor market intermediaries affects the whole graduate population evenly. Also, while in this chapter we focus on average outcomes, the effect on outcome distribution remains an issue for further research.

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