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Tax credit policy and firms' behaviour: the case of subsidies to open-end labour contracts in Italy

by Piero Cipollone and Anita Guelfi

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TAX CREDIT POLICY AND FIRMS' BEHAVIOUR: THE CASE OF SUBSIDIES TO OPEN-END LABOUR CONTRACTS IN ITALY

by Piero Cipollone* and Anita Guelfi**

Abstract

In this paper we look at tax credit policy as an instrument to foster hiring with open-end rather than with fixed-term contracts. In particular, we examine a specific regulation adopted in Italy in the year 2000 (credito d'imposta). This policy offers a generous and automatic tax credit to all firms hiring workers with open-end contracts. The eligibility criteria are very mild for both firms and workers. Our results seem to indicate, both formally and empirically, that firms used this subsidy primarily to hire under open-end contracts workers who would have been hired under such contracts regardless of the subsidy, albeit after a short transition into temporary employment. Our estimates suggest that, compared with 2000, in 2001 the subsidy did not increase the overall probability of being hired, but did change the composition of new employees. It increased the chances of finding an open-end contract but in a rather uneven way across workers. Conditional on being hired, the probability rose by about 10 per cent for workers holding a college degree, by about 4 per cent for people with a high-school diploma, while it did not change or might even have declined slightly for less educated workers.

JEL classification: D78, H25, J23, J38.

Keywords: tax credit, open-end contracts, fixed-term contracts, evaluation.

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1. Introduction[1]

In the last few years, fixed-term contracts have taken center stage in the economic debate on labour market reforms in Europe. The debate has mostly focused on two main features. The first is the magnitude of the phenomenon: very rapidly, temporary jobs have become a major novelty in the European labour markets. Table 1 shows that in many OECD countries a sizeable share of employees worked under fixed-term contracts in year 2000. In some countries (such as Ireland, UK, Luxembourg and Hungary) they still represent a small share, but in most cases they appear to account for at least 10 per cent of total employees; in a few countries the shares are even higher: 32.1, 20.4, 20.4 per cent in Spain, Portugal and Turkey respectively. Only ten years earlier temporary work represented a much smaller fraction of employment.

The second feature originates from the observation that in several countries – such as Austria, the Czech Republic, Finland, France, Germany and Italy - a sizeable portion of newly created jobs in the 1990s took the form of fixed-term contracts (Table 1)

This rapid expansion has fuelled researchers' effort to understand the effects of fixed-term contracts on labour market outcomes. At this stage there is an unsettled dispute in the literature concerning their effectiveness. On the one hand it is suggested that, by introducing some form of flexibility into an otherwise highly regulated labour market, they tend to provide young workers with a stepping-stone towards permanent employment (Booth, Francesconi, and Frank 2000 for the UK; Contini, Pacelli and Villosio 2000 for the UK, Germany and Italy). On the other hand, there is increasing evidence that they might represent a "dead-end", in that they further segment the labour market between insiders holding open-end contracts and outsiders who find themselves confined at the margins, trapped between repeated spells of unemployment and fixed-term contracts (Blanchard and Landier, 2001 for

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France; Güell, 2002a and Güell and Petrongolo, 2002; Amuedo-Dorantes, 2000 and 2001 for Spain; Istat, 2000 for Italy).

In its 2002 Employment Outlook, the OECD attempts to strike a fair balance suggesting that "Depending on the country considered, between one-third and two-thirds of temporary workers [including temporary work agency workers] move into a permanent job within a two-year time interval, suggesting considerable upward mobility. The other side of the coin is that up to one-fourth of temporary workers are unemployed when interviewed one and two years later, and employers provide significantly less training to temporary than to permanent workers" (OECD, 2002).

This kind of concern has led policy-makers to intervene on fixed-term contracts in the recent years in an attempt to reduce their negative effects while retaining the positive aspects. According to the OECD (2002), governments have intervened both by setting restrictions on the adoption of temporary contracts (and the degree of employment protection accorded to "permanent" employees) and by establishing equal-treatment standards requiring employers to harmonize pay or fringe benefits between temporary and permanent employees, as well as by providing employers with incentives to either hire certain disadvantaged job-seekers on temporary jobs or move them into permanent positions.

Notwithstanding these legislative activities, best practices are yet to be found and there seems to exist a substantial uncertainty about the best way to proceed, perhaps because of the lack of clear-cut evidence from empirical research.

As in others countries, in Italy fixed-term contracts have received a great deal of attention from policy-makers, business associations and unions. In the 1990s, the adoption of fixed-term contracts was encouraged by widening their scope and easing the regulatory burden; at the same time firms received incentives to transform temporary into permanent jobs[2]. However, policy design was not always fully consistent and might have induced unwanted and non-trivial negative implications.

[2] In Spain this same strategy has been adopted since the second half of the 1990s with two important laws (Royal Decree 8/1997 and Royal Decree 9/1997), which reduced social security contributions and dismissal costs for employers who transformed temporary into permanent contracts (Amuedo-Dorantes 2000, 2001).

In this paper we examine the effects of the most important and recent financial incentive of this kind, i.e. a generous tax credit granted to firms choosing to hire workers under open-end contracts. In particular, two questions are addressed. On the one hand, we examine whether this new incentive actually did increase an average worker's likelihood of being hired with an open-end contract. On the other hand, we investigate whether the increase in probability was homogenous across workers, i.e. whether it provided everybody with an additional opportunity to enter permanent employment or instead favoured only specific workforce groups.

Our results seem to suggest that firms mainly used this tax credit provision to hire under open-end contracts workers who, on average, turn out to have the highest probability of being permanently hired even without the subsidy, perhaps after a short transition into temporary employment. Our estimates suggest that, compared with 2000, in 2001 the subsidy did not increase the overall probability of being hired, but it did change the composition of newly hired employees. In fact it increased the chances of finding an open-end contract but in a rather uneven way across workers. Conditional on being hired, the probability rose by about 10 per cent for workers holding a college degree, by about 4 per cent for people with a high-school diploma, while did not change or might even have declined slightly for less educated workers.

The rest of this paper is organized as follows. Section 2 defines the context by highlighting the basic facts about fixed-term contracts in Italy with special regard to regulations, figures and the reasons for concern. Section 3 explains in some detail the nature of the tax credit provision introduced in Italy at the end of 2000. We focus attention on the regulatory aspects, the size of the incentive and its actual usage. Section 4 presents a simple conceptual framework that helps to predict what type of workers turn out to benefit most from the tax credit provision. Section 5 describes the information we use to take these predictions to the data. Evaluation of the prediction is carried out in section 6 in a preliminary, descriptive way. Section 7 extends the analysis of section 6 through a simple econometric framework to estimate, on the one hand, the effects of the new regulation on the probability of being hired with an open-end contract, conditional on having been employed in the subsidized period; on the other hand, its impact on the overall probability of being hired. Section 8 finally concludes.

2. Context. Basic facts about fixed-term contracts in Italy: regulation, figures, concerns

2.1 Regulation

As a general rule Italian law prohibits any time limitation on a labour contract except for some specific circumstances clearly stated by the law itself[3]. These exceptions are: 1) seasonal activities; 2) temporary replacement of an employee on leave; 3) occasional activities which are time predetermined and not usually carried out by the firm; 4) special contracts requiring different skills that are not usually provided by the firm; 5) special skills in the movie and airline industries; 6) technical and administrative top management[4].

A fixed-term contract can be renewed only once under special circumstances for at most the same original duration and in any case with the worker's agreement. If the contract extends beyond the original duration, the corresponding wage rate has to be increased by 20 per cent for each day following the deadline up to the 10th day, and by 40 per cent thereafter. Moreover, if the contract goes beyond the 30th day after the deadline it is automatically considered an open-end contract.

Alongside with this type, there are special fixed-term contracts that are designed to provide young workers with work experience along with formal training (apprenticeship and training-employment contracts, "apprendistato e contratti di formazione lavoro"). The duration of these contracts stretches from 18 months to 4 years; they can be signed by workers aged 16 to 32 (with different duration and regulations depending on age and level of education). They are different in nature from the normal fixed-term contracts as they are thought of as stepping-stones into permanent employment for younger workers. Because of this special status they are rewarded with lower than standard social security contributions for amounts that differ according to contract type, firm size, economic sector and

[3] This general rule was true until a new law was passed in the summer of 2001. Since then, fixed-term contracts are not regarded any longer as an exception to the general rule but are awarded equal dignity to permanent contracts as long as there exist valid technical-organizational reasons for their adoption. A complete history of the regulation of fixed-term contracts is presented in Appendix 1.

[4] For a full description of the regulation for these and other types of contracts, see Ministero del Lavoro (2001).

geographical area and range from a minimum of 25 per cent to a virtual maximum of 100 per cent.

An important feature of the Italian institutional setting is that a great deal of labour market regulation is left to negotiation between business organizations and unions. The law sets the general framework, while the actual details are decided in the national sector contracts. Thus, even in the absence of legislation there might be important changes in the actual regulation of specific issues. A pivotal example of this pattern is the regulation of fixed-term contracts. In the early 1990s, national sector contracts set ceilings on the adoption of fixed-term contracts at firm level of around 5 to 7 per cent of employment in the manufacturing sector and 10 per cent in the construction and retail industries. However, actual usage was below these ceilings because unions managed to narrow the set of specific situations in which fixed-term contracts were allowed. As the climate of industrial relation became less tense in the 1990s, the unions agreed to widen the scope for temporary jobs and relaxed their maximum usage constraint. For example, in 1998 ceilings were substantially increased, up to 20 per cent in construction and 25 per cent in the chemical sector (Bank of Italy, 2000).

2.2 Figures

Fixed-term contracts became a major feature of the Italian labour market in the 1990s. From the second half of the 1970s until the early 1990s, they represented a non-trivial but constant share of total employment, concentrated in the agricultural sector. In the second half of the 1980s this share grew because of the introduction of training-employment contracts in 1984, but the actual take-off occurred between 1993 and 1999 when they soared from 6.1 to 9.8 per cent of total employees (Figure 1). In this same period they were the only expanding form of employment and accounted for virtually the whole growth in payroll employment (Figure 2). This increase was encouraged by the easing of the constraints established by national labour contracts.

In 2000 the rise in fixed-term contracts was accompanied by an upturn in permanent employment following the pronounced growth in labour demand, especially in the northern regions of the country[5].

Fixed-term contracts tend to be evenly distributed among men and women, mostly young (in 2001, 60 per cent of them were less than 35 years old; Table 2), with less than average years of schooling (in 2001, half of them held at most a lower–secondary-school diploma[6]), working in the services (63 per cent in 2001) and agriculture (12 per cent). The great majority of people (44.4 per cent in 2001) holding a fixed-term contract do so because they cannot find a job with an open-end contract (item "No better opportunities" in Table 2). About one-third of them is in an apprenticeship and training-employment type of contract.

2.3 Concerns

In many OECD countries fixed-term contracts have been welcomed as a means of offering younger workers' opportunities to gain access to a first job. However, they are also a source of concern as they might lead to an increase in worker insecurity and precariousness (OECD, 2002). The same reasons for concern seem to hold true in Italy. The growth of precarious jobs has been paralleled by an expansion in the share of low-paid workers[7] among all employees. As illustrated in Figure 3, this share went up from 8 per cent in 1989 to more than 18 per cent in 1998, reversing the trend of the previous 15 years (Brandolini, Cipollone and Sestito, 2001).

We cannot directly and immediately attribute this reversal to the spread of fixed-term contracts, although we do have evidence that people in temporary jobs earn less and work fewer hours than people in permanent jobs. In Table 3 we computed the differential in log hourly wages and worked hours between workers with open-end contracts and workers with

[5] In 2000, as many as 6.6 per cent of firms in the north-eastern regions and 3.4 in the north-western ones claimed they could not find enough workers (Bank of Italy, 2001). For a discussion of the mismatch measure in Italy, see Brandolini and Cipollone (2001).

[6] The Italian schooling system is organized into eight years of compulsory education (five years of primary education and three additional years of lower secondary school leading to a junior high-school diploma), which can be followed by 4-5 years of upper secondary education (terminating with a high-school diploma) and by further years of tertiary education leading at least to a college degree (a college degree is obtained on average after 4 years of tertiary education).

[7] Low-paid workers are defined here as those earning less than two-thirds of the median income.

temporary jobs (distinguishing between fixed-term contracts and workers hired by temporary work agencies), using the Bank of Italy Survey of Household Income and Wealth for the year 2000. The raw differential suggests that men in fixed-term jobs (temporary work agency jobs) earn 32 (43) per cent less than those with open-end contracts. The adjusted differential shrinks considerably but remains sizeable: 12 per cent for fixed-term and 21 per cent for temporary work agency workers. About the same holds true for women in temporary jobs; the wage differential for those in fixed-term occupations is considerably smaller and not significantly different from zero once adjusted for observable characteristics.

The wage differential combines with the differences in hours of work to further widen the overall annual earnings gap between workers in permanent and temporary jobs. On average, a male worker with a fixed-term contract works 530 hours less than a corresponding open-end contract worker. This gap narrows to 390 hours when worker characteristics are taken into account, but it still remains quite sizeable. A similar pattern can be observed for women.

Mobility out of fixed-term employment seems to be rather low. In October 1999 out of 100 workers whose first job was a temporary job, 38 were still in a temporary position after three years (20 in the same initial job and 18 in a different fixed-term job; Table 4), 38 exited employment status either into unemployment or out of the labour force, 4 have changed status into self-employment and 21 gained access to permanent positions.

Workers whose first job was a permanent position faced much brighter prospects; after three years 90 per cent of them were still in permanent employment (81.3 per cent in the same job and 8.8 in a different one), 1.3 per cent had moved into self-employment, 1.8 into a temporary occupation and 6.8 per cent had exited employment status. Self-employed workers showed similar tendencies.

Five years after their first job the chances for fixed-term workers look slightly better: 36 per cent of them have gained access to an open-end contract, 27 remain in a temporary occupation (10 per cent in the same job and 17.2 in a new fixed-term contract) and 30 per cent have exited into either unemployment or inactivity. These figures imply an annual transitional probability into permanent occupation of about 11.5 per cent that translates into an average waiting time of about 8.7 years. This is considerably longer than the OECD suggests, and is similar to the time reported by Blanchard and Landier for France (8.2 years

for young workers in 1996) and by Amuedo-Dorantes for Spain (8.3 for all workers in 1996).

Low mobility does not only affect marginal workers and in fact appears fairly widespread. Table 5 shows transitions from fixed-term contracts (towards all status in the labour market) according to some basic worker characteristics. Compared to women, men show slightly better chances to enter open-end jobs but, at the same time, they tend to have a higher risk of exiting employment. Holding a college degree, as opposed to a high-school diploma, does not improve the probability of gaining access to permanent employment in the first three years after entering a first job. However, more years of schooling seem to reduce the risk of exiting employment while workers with a shorter education appear doomed to the margins of the market, wandering between unemployment and temporary occupations. Indeed, even 5 years after their first job, 40 per cent are either unemployed or inactive while 25 per cent find themselves in transient jobs.

3. Subsidy to open-end contracts: regulation, magnitude, usage

3.1 Regulation

Like many other OECD countries, Italy has attempted to reduce the negative effects of fixed-term contracts. The strategy adopted sought to increase the mobility out of fixed-term contracts by providing fiscal incentives to firms that either transform temporary into permanent positions or directly hire workers under open-end contracts. There are several examples of this strategy[8]. However, until the year 2000 these incentives were small and often targeted to particular areas, firm types or worker categories.

The Italian Finance Law for the year 2001 (issued at the end of 2000) instead provided a new incentive in the form of a general, automatic and quite generous tax credit to all firms hiring workers with open-end contracts. In particular, the provision stated that every firm hiring a new worker on a permanent basis would be rewarded with a tax credit of about € 413 (€ 620 for workers in the South) per month and per worker from the moment of hiring until the end of December 2003. This new tax credit applies to all new hires taking place from October 2000. Thus, for a southern worker hired in October 2000 and retained until December 2003 each firm will receive about € 24,200. The tax credit is awarded only if both

[8] For example, the incentives to transform training-employment contracts into permanent ones or the tax credit for small firms hiring permanent workers in economically depressed areas.

worker and firm are eligible. A worker is eligible if he/she is at least 25 years old and not working with an open-end contract in the 24 months before hiring. A firm is eligible if the newly hired worker raises the overall level of permanent employment - at the firm level - above the average recorded in the period between October 1999 and September 2000. The tax credit can be claimed against any kind of taxes, such as income tax, social security contributions, value-added tax. Furthermore, it can be passed on to different fiscal years.

3.2 Magnitude

The contribution provided by this subsidy looks quite generous. Figure 4 shows the percentage reduction in per-capita labour costs due to the tax credit (using data for the year 2000) by sector and geographical area. This reduction is variable because the tax credit is a fixed amount that only increases for southern workers, while the average labour cost differs across both sectors and geographical areas. The evidence shows a labour cost reduction which ranges from 9.3 per cent in the banking sector in the central and northern regions to almost 60 per cent in the agricultural sector in the South. On average in the private non-farm sector the reduction amounts to about 30 per cent in the South and 16 per cent in the central and northern regions. These estimates understate the effect of the tax credit because labour cost data refer to an average worker, while the correct reference should be the labour cost of a new young worker, which is usually below the average. It should be mentioned, however, that national accounts also include estimates of the labour cost in the underground economy, which is very likely smaller than the legal labour cost for a new entrant; however, this effect only attenuates underestimation.

3.3 Usage

The new tax credit seems to have been very successful in 2001. We have two sources of information about the actual usage of this new instrument. The first source is the Labour Force Survey, which provides data on the number of newly hired employees distinguishing between open-end and fixed-term contracts. Figure 5 extends the numbers of Figure 2 to the year 2001. It reports the quarterly absolute growth in total employees by type of contract with respect to the corresponding period of the previous year. It suggests that in January 2001[9] - i.e. the first survey since the new tax credit came into force - fixed-term contracts

[9] Italian Labour Force Surveys are conducted in the first week of January, April, July, and October, respectively.

stopped increasing, having been the only source of payroll employment growth since 1993; in October 2001 the number of fixed-term contracts was smaller than one year earlier. However a slow-down had already occurred in 2000, although most of it was due to a strong labour demand that turned the labour market into a seller's market, especially in the northern regions , thereby allowing workers to negotiate hires with open-end contracts[10]. In 2001, open-end contracts went up and fully compensated the slow-down in fixed-term contracts. This was the largest increase in permanent employment since 1993 and looks quite remarkable given the sharp slow down in economic activity in 2001 (the growth in value added in the private sector fell from 4.2 to 2.5 per cent between 2000 and 2001).

The second source of information is the figures collected by the Ministry of Finance (and reported in the Ministry of Labour, 2001), to assess the amount of revenues lost owing to the tax credit. Figure 6 shows these forgone revenues as a share of total social security contributions in 2001 and the corresponding number of workers involved. Between January and December 2001, the monthly flow of forgone revenues increased from 0 to more than 0.7 per cent of the monthly flow of social contributions. This involved 188,000 workers in November 2001, that is about 1.2 per cent of total employees. These figures suggest the tax credit has been a great success, far beyond the 83,000 workers initially foreseen for the entire subsidized period, i.e. October 2000-December 2003 (Bank of Italy, 2001).

4. Who is better off? A simple conceptual framework

In this section we set up a simple conceptual framework to answer the following question: will all workers benefit equally from the tax credit? In other words, will firms choose to hire all types of workers, regardless of their observable characteristics? The simple framework we use suggests this is not the case. It shows instead that the best workers (in terms of their observable characteristics) will most probably be hired with open-end contracts. These workers are those the firm would most likely have hired on a permanent basis even without the subsidy, perhaps after a period of temporary employment. At the same time less able workers would not be affected by the new tax credit and could be even harmed by it.

[10] Maya Guell (2002b) explains this effect in an efficiency-wage context in which the type of contract each worker is offered represents a discipline device.

4.1 Setting

Suppose the firm does not know the productivity of new workers. Let y be a worker's productivity when matched with a job and assume this value is drawn from one of the following two alternative productivity distributions G(y): either a uniform [0,yH] or a [0,yL]. To make things simpler, let us assume the second distribution is degenerate to 0.

Given workers' observable characteristics, each firm assigns to each new worker a probability λ to be drawn from [0,yH]. There exist two types of contracts in this economy, namely fixed-term and open-end contracts. They both last two periods. With the first type firms hire a new worker in the first period, observe her/his productivity and then decide whether to hire the worker for the second period or let her/him go. In this last case no firing cost has to be born. We finally assume that in the second period there is no need to fire the worker.

With the second type of contract (open-end contract), firms face the same sequence of decisions: they hire a new worker in the first period, observe her/his productivity and then decide whether to hire the worker or let her/him go. However, in the latter case a firing cost has to be born. As in the fixed-term case, in the second period there is no need to fire the worker. Wages are exogenously given to firms[11].

4.2 The value of contracts

In order to decide which contract to offer to each worker, firms need to compute the value of both contracts.

The value to a firm of a fixed-term contract is given by:

$$V_{FT} = E(y) - w_1 + P(hired) * \frac{1}{(1+r)}\left[E(y \mid hired) - w_2\right] \quad (1)$$

that is the sum of the expected profits from the two periods. Second period expected profits depend on the probability that workers will be retained and on the expected productivity of the retained workers. Since we assumed that a worker's productivity has a uniform [0, yH] distribution with probability λ and 0 with probability (1- λ), then it follows that

[11] We remove this assumption later on.

$$E(y) = \lambda \frac{y_H}{2};$$

$$p(\text{hired}) = 1 - p(\text{fired}) = 1 - [\lambda G(\bar{y}_{FT}) + (1-\lambda) * 1] = \lambda [1 - G(\bar{y}_{FT})] = \lambda \left(\frac{y_H - \bar{y}_{FT}}{y_H} \right)$$

$$E(y \mid \text{hired}) = \left(\frac{y_H + \bar{y}_{FT}}{2} \right)$$

and $\bar{y}_{FT}$ is the cut-off level of productivity below which a firm fires the worker.

Each firm has to choose this cut-off level of productivity ($\bar{y}_{FT}$) and needs to compute the threshold λ below which the contract value turns out to be negative (so that no worker with an attached λ below this threshold is going to be hired). The optimal value of cut-off productivity is $\bar{y}^*_{FT} = w_2$ [12]. This implies that the firm will enjoy extra profits for any worker retained in the second period. The threshold value for λ is

$$\lambda^*_{FT} = \frac{2 w_1 y_H (1+r)}{y_H^2 (1+r) + (y_H - \bar{y}^*_{FT})^2} \tag{2}$$

To make sense this value needs to be less than one[13].

The value to a firm of an open-end contract is instead:

$$V_{OE} = E(y) - w_1 - (1 - P(\text{hired})) * C + P(\text{hired}) * \frac{1}{(1+r)} [E(y \mid \text{hired}) - w_2] \tag{3}$$

where C is the firing cost. The only difference between this value and the one assigned to a fixed-term contract is the expected firing cost $(1 - P(\text{hired})) * C$, which has to be born at the end of the first period.

Given our productivity assumptions, the following holds for an open-end contract

$$E(y) = \lambda \frac{y_H}{2};$$

$$p(\text{hired}) = 1 - p(\text{fired}) = 1 - [\lambda G(\bar{y}_{OE}) + (1-\lambda) * 1] = \lambda [1 - G(\bar{y}_{OE})] = \lambda \left(\frac{y_H - \bar{y}_{OE}}{y_H} \right)$$

$$E(y \mid \text{hired}) = \left(\frac{y_H + \bar{y}_{OE}}{2} \right)$$

The value of this contract is then maximum when

$$\bar{y}^*_{OE} = w_2 - (1+r)C \tag{4}$$

---

[12] This value is derived by maximizing the value function with respect to $\bar{y}_{FT}$.

[13] This constraint poses an upper bound to the first period wage; the bound to the second period wage is the highest productivity value y_H. If the second period wage is higher than this value, no worker will be hired for the second period.

Notice that this value lies below that of fixed-term contracts; thus firms would retain in the second period a worker who would be fired in the case of temporary employment. This happens because when the firm retains a worker it saves on firing costs. With this cut-off productivity value, the threshold for the probability λ turns out to be

(5) $$\lambda^*_{OE} = \frac{2(w_1 + C)y_H(1+r)}{y_H^2(1+r) + (y_H - \bar{y}^*_{OE})^2}$$

which is always greater than the threshold corresponding to fixed-term contracts.
Notice, moreover, that bearable firing costs have an upper bound, that is

(6) $$C^{max} = \frac{w_2}{1+r} = \frac{\bar{y}^*_{FT}}{1+r}$$

which derives from the observation that the lowest value of $\bar{y}^*_{OE}$ is zero.

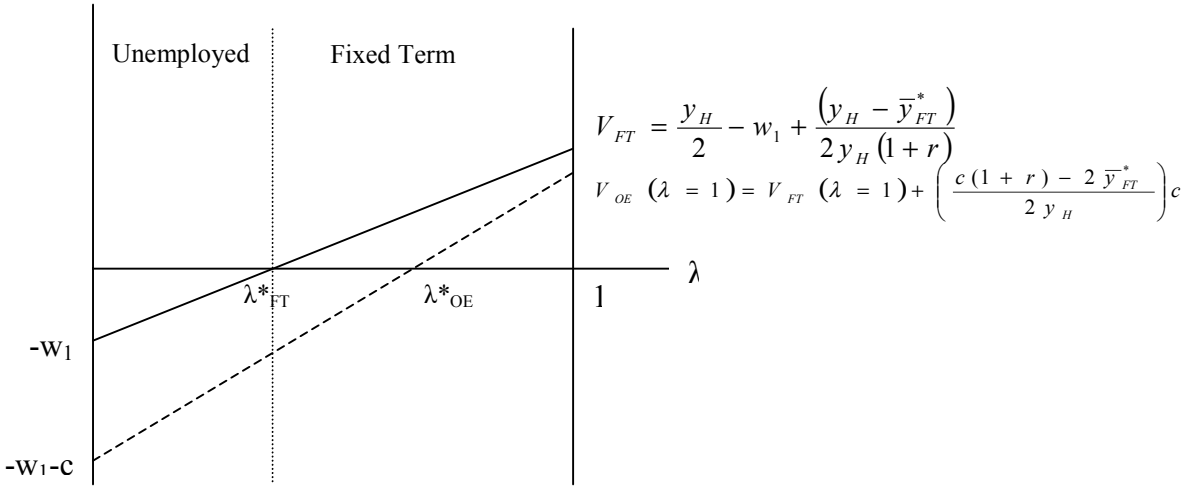
Using the fact that $\bar{y}^*_{OE} = w_2 - (1+r)C = \bar{y}^*_{FT} - (1+r)C$, it is possible to write the value of one contract as a function of the value of the other one, i.e.

(7) $$V_{OE} = V_{FT} + \lambda\left[\frac{C^2(1+r)}{2y_H} + \frac{(y_H - \bar{y}^*_{FT})}{y_H}C\right] - C$$

This relationship is shown in Graph 1.

Graph 1

THE LABOUR MARKET BEFORE THE SUBSIDY



Two facts are worth noting here. The first one is that open-end contracts are dominated by fixed-term contracts for every value of λ; this result captures in a simple way the idea that

all new workers enter employment with a fixed-term job, a feature which does not appear too far from Italian experience in the 1990s[14]. The second noticeable fact is that the slope of the value of open-end contracts with respect to the quality index λ is higher than the corresponding slope for fixed-term contracts. In other words as λ increases, the value of the first contract grows faster than the value of the second one. This happens because of the reduction in expected firing costs. However, this second effect does not overcome the reduction in the overall values due to firing costs.

4.3 The effect of the subsidy

Let us now introduce the subsidy to open-end contracts in the form of a lump sum K given in the first period to each firm hiring workers with such contracts. Thus, the value of an open-end contract will be shifted upwards by an amount K for any given level of λ:

$$V_{OE} = V_{FT} + \lambda\left[\frac{C^2(1+r)}{2y_H} + \frac{\left(y_H - \overline{y}^*_{FT}\right)}{y_H}C\right] - C + k \tag{8}$$

If the subsidy is not too small or too high[15], the value of an open-end contract will be shifted in such a way as to ensure the coexistence of unemployed workers, fixed-term contracts and open-end contracts (Graph 2).

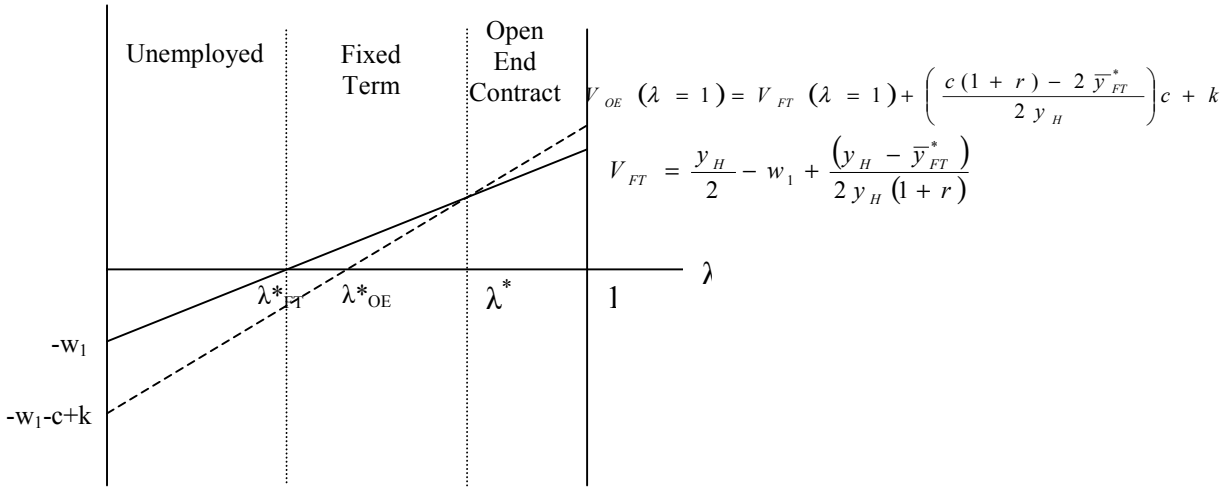
---

[14] Admittedly only about half of newly hired workers (those who are not job switchers) are fixed-term contracts. This is due, at least in part, to the fact that firms are not allowed to hire as many workers as they wish under fixed-term contracts for several reasons, the most important being the existence of contractual upper limits to the adoption of temporary contracts. Another relevant reason is firms' fear of losing their best workers if hired with a fixed-term contract. Furthermore, in the case of firms with high training costs it may be optimal to retain a certain proportion of workers by offering them permanent jobs.

[15] In particular the lowest level of K has to be such that $V_{OE} - V_{FT} > 0$ when evaluated at $\lambda = 1$; this value is $K_{\min} = \left(\frac{2\overline{y}^*_{FT} + (1+r)C}{2y_H}\right)C$. The highest value of K has to be such that $V_{OE} - V_{FT} \leq 0$ when evaluated at $\lambda = \lambda^*_{FT}$. This value is $K_{max} = \left(1 - \lambda_{FT}\left(\frac{y_H - \overline{y}^*_{FT}}{y_H}\right)\right)C - \frac{\lambda_{FT}(1+r)C}{y_H}$. It is true that $K_{max} > K_{min}$ for all C.

Graph 2

THE LABOUR MARKET AFTER THE REFORM



The decision rule implied by this new setting is:

- Do not offer workers any contract if $\lambda \leq \lambda^*FT$;

- Offer a fixed-term contract if $\lambda^*FT < \lambda \leq \lambda^*$;

- Offer an open-end contract if $\lambda^* < \lambda$;

Thus, newly hired open-end workers are those the firm assigns the highest probability λ and are probably those who are most likely to be retained in the second period. This result does not come as a surprise given that firms try to balance off the subsidy (that is identical for all workers) with the additional expected firing costs; since these costs are smaller for expected better workers, these are the ones preferred by firms. Thus, the policy measure we are examining seems to increase the probability of being permanently employed for people who have the highest chance of being employed in permanent jobs regardless of the subsidy.

4.4 Extensions

This paragraph illustrates what happens to the model if we allow for an endogenous wage in the second period. In this section we present the major results[16]. The criterion we

[16] The formal development of this extended model is long and space consuming. We report it in an appendix, available on request.

adopted to let the wage be determined within the model is the fact that in the second period wages are equal to the outside opportunities for workers as measured by the average productivity of non-employed workers. With this rule, before the introduction of the subsidy there exists a unique equilibrium and the wage turns out to be a lower value than the expected productivity of workers and the quality index threshold is different from zero.

Results are less clean when we introduce the subsidy. In this case the model might have either no equilibrium, or one or two equilibria. However the last case can be ruled out on the basis that we are interested in those equilibria in which both contracts are implemented.

The last question we address concerns the comparison between pre- and post-subsidy wages. Results show that pre-reform wages are higher than post-reform ones. This effect depends on the fact that, because of the firing costs, the productivity threshold for an open-end contract turns out to be lower. Thus, the major effect of endogenous wages is given by the fact that, by lowering the overall wage, the subsidy has a small effect on total employment.

5. The data

In order to verify whether the new subsidy was having any effect on the level and composition of new workers' flow, we resorted to the Labour Force Survey and defined as new hires those employees who have been holding their current job for less than 13 months[17]. In particular, computations were carried out on data deriving from the October 2001 wave of the Italian LFS. Thus, all new workers selected from this survey are potentially covered by the new subsidy (since it was granted starting from October 2000). To avoid problems related to seasonal patterns, we chose the October survey of every year comprised in our sample (i.e. from 1993 on). Finally, we only included new hires into the private, non-farm sector[18].

[17] To compute this tenure variable we used the question "When did you begin to work with your current employer?"

[18] To be precise we excluded sectors ("branca di attività economica") coded "01" and "10" in the LFS.

These selection rules leave us with a number of observations that range from a low of 3593 in 1993 (representing just over 1 million new hires) to a high 5474 in 2000 (representing 1.6 million new workers; Table 6). In 2001, less than two-thirds of new workers were hired with open-end contracts. This share was 71 per cent in 1993. This decline has been mirrored by a corresponding increase in fixed-term contracts of the apprenticeship and training-employment type, whose share went from 8.2 in 1993 to 14.4 in 2000, before falling to 10.8 per cent in 2001[19]. Fixed-term contracts due to lack of better opportunities represented a steady 15 per cent of all new contracts. In 2001, 53 per cent of new workers were men. This share was about 60 per cent at the beginning of the 1990s and has been constantly declining since. More than 50 per cent of newly hired workers were less than 30 years old (60 per cent in 1993), the decline occurring mainly among workers under 20 and only partially compensated by workers in their twenties. It is interesting to note that a steady 10 per cent of new hires consists of workers aged 45 and over.

From 1993 to 2001, the share of newly hired workers with a low level of education (lower secondary school or less) declined of about 14 percentage points. At the same time the shares of high school and college graduates rose respectively by about 10 and 5 percentage points. Most of the growth was driven by the service sector.

6. A preliminary look at the evidence

This section provides preliminary evidence that firms most probably used the tax credit to selectively hire under open-end contracts only particular types of workers who, according to their own observable characteristics, look the most suitable to be hired in permanent jobs regardless of the subsidy. In this preliminary presentation of the evidence we look at the share of both new open-end contracts by age and level of education and fixed-term contracts by reasons for holding such a contract.

[19] In May 1999 the European Commission established that training-employment contracts for people older than 25 (29 for people with a college degree) could no longer be granted the entire social contribution reduction. There is some evidence that this provision reduced the number of these contracts in the year 2000.

6.1 Question one: did the share of new open-end contracts increase?

Our simple model suggests that, because of the new subsidy, the share of open-end contracts should have gone up. The first question we pose is therefore whether in 2001 there was an increase in the share of open-end contracts and, perhaps more importantly, whether this increase was stronger for eligible workers (i.e. people 25 and over) than for the remaining groups. Table 6 answers the first part of the question as it shows that the share of open-end contracts increased by 2 percentage points (from 62.5 to 64.5) between 2000 and 2001. This synchronism is particularly remarkable since before 2000 the share was declining almost steadily.

To answer the second part of the question Figure 7 tries to evaluate which worker group was responsible for this increase. The figure plots the share of open-end contracts for different age groups between 1993 and 2001. Again, one could read a rise in the share for all workers. However, we found evidence that the share of eligible workers (25 and over) increased slightly more than the control group (workers under 25 years old). Most of the growth in the share of the eligible group is due to the youngest people: the share of workers aged 25 to 40 went from 64.3 to 67.5 per cent, that is 1.2 percentage points above the average increase.

To summarize, two main facts can be observed: i) the share of open-end contracts increased between 2000 to 2001, thereby inverting a previous declining trend; ii) the treated group – especially the youngest component - showed slightly larger growth than the control group. Both results square with our priors.

6.2 Question two: for which group did the share of new open-end contracts increase? Evidence from the years of schooling

What kind of workers did the firm hire with open-end contracts in 2001? Figure 8 tries to answer this question by comparing the average number of schooling years of new permanent and new fixed-term workers respectively between 1993 and 2001. The relative level of education of permanent workers declined until 1995, with a small recovery in the following years, even if the overall changes were quite small until 1999. In 2000, people with more years of schooling entered open-end contracts more frequently than before, bringing about the first sizeable rise in the overall level of education for these contracts. In

2001 an analogous increase took place. Thus, it may appear that the increase in the relative quality of open-end contracts preceded the subsidy. Nonetheless, a closer look at the age composition reveals that in the year 2000 there was an almost equal size increases in both treated (eligible) and control groups (less than 25 years old). By contrast, in 2001 relative education remained constant for the control group while it rose further for the treated group, especially for the older ones (40 years and older).

This evidence therefore seems to support the idea that, compared with the previous years, in 2001 firms chose to hire under open-end contracts people with more years of education. This increase was however limited to people of 25 and over.

6.3 Question two: for which group did the share of new open-end contracts increase? Evidence from the age distribution

Figure 9 shows the age distribution of new open-end contracts as a share of total new hiring. We divided the overall share into 5-year brackets, ranging from 15 to 65 years old. The sum over the 10 age-brackets gives the total share plotted in Figure 7. The overall age structure appears stable over time, except for the decline in the share of the 19-24 age group and the rise in that of the 25-29 year olds in 2001. The latter increase is remarkable both in size and given the relative stability of the previous years.

6.4 Question three: what type of fixed-term contract declined?

Figure 10 illustrates the share of fixed-term contracts by reasons for holding such a contract among workers aged 25 and over (the category "other reasons" includes the answer "don't want an open-end contract"). The crucial fact to note here is that the drop in the total share is not evenly distributed across contract types but is fully concentrated in the apprenticeship and training-employment category. In the previous years (at least since 1993) this type of fixed-term contract never declined.

This pattern can be interpreted as an indication of the fact that while firms hired more people with open-end contracts, they selected those workers they would have alternatively hired with a training-employment contract, which usually represents the main avenue towards permanent employment. Thus, this might imply that firms used the subsidy to anticipate what they would have done later on.

However, it must be stressed that a part of this decline could also be the consequence of the 1999 provision of the European Commission recalled in footnote 18. The major effect of such a provision should have been recorded in the year 2000 rather than in 2001, but we can still allow for this additional explanation without altering the basic message of our conclusion.

7. The effects of the subsidy on the probability of being hired with an open-end contract

7.1 Empirical specification and identification strategy

In this section we use a simple econometric model to evaluate whether the subsidy exerted any causal effect on the probability of being permanently hired. We want to address two specific questions: 1) did the subsidy increase the probability of being hired with an open-end contract? And if so, by how much? 2) was the effect stronger for people with higher probability to access lasting jobs (even without the subsidy)? The econometric specification adopted to answer these two questions is a simple probit model, in which the probability of being hired with an open-end contract depends on age, education and a series of other demographic characteristics, year dummies and a dummy that takes value 1 if the worker is eligible for the subsidy and zero otherwise (labelled "treated"). We also include an interaction between this dummy and the worker's years of schooling:

$$Pr(\textit{hired with a open}-\textit{end contract}) = \Phi\left(x_{it}'\beta\right)$$

$$x_{it}'\beta = \beta_o + \beta_1 treated_{it} + \beta_2 treated_{it} * (educ_{it} - mean(educ_{it})) + g(educ_{it}, age_{it}) + \textit{demographic characteristics}_{it} + \textit{year dummies} \quad (1)$$

The value of the coefficient β_1 provides an answer to question 1, as it measures the average additional effect of being eligible for the subsidy on the probability of being hired with a permanent contract. The value of the coefficient β_2 provides instead an answer to question 2 if we are willing to assume that education is an indicator of the likelihood to be hired with an open-end contract. It measures the additional effect (over β_1) of having a given level of education.

On the basis of both our conceptual framework and a preliminary look at the evidence we expect both coefficients to be positive, implying that the subsidy increased the chances to be hired with an open-end contract and to a greater extent among more educated people.

Before turning to the actual estimation of the empirical model we still need to clarify two issues. The first one deals with the conditioning population we are referring to in estimating equation (1). As we explained in the data description section, we only have access to cross-section data and cannot therefore evaluate transitions from different labour market statuses into employment. We are able to identify new hires because of a specific question in the survey but we do not know where they come from. They could be transiting from any status into employment, including from open-end to open-end contracts. Given this sample limitation we decided to lower our expectations and asked a simpler question, namely we looked at the effect of the tax credit on the probability of being hired with an open-end contract, conditional on having been hired in the past 12 months. This implies that we restricted our sample to the population that has actually been hired in the preceding 12 months.

The second issue refers to the strategy we used to identify β_1 and β_2. Here several approaches can be adopted; they are summarized in Table 7. The first strategy, referred to as W1, exploits only differences over time in the share of open-end contracts for the treated group, which in this case we assume to include all workers aged 25 and over. Thus the effect of the subsidy in 2001 would be the difference in the share of open-end contracts for the treated group between this year and a reference year. The second strategy, referred to as W2, is also a within age group strategy and identifies the effect of the subsidy as the difference between the value of the coefficient in 2001 and the corresponding value in another reference year. It differs from W1 in that it restricts the treated group to workers of 25 to 35 years old who, we believe, should be more sensitive to the new regulation. However, these strategies might deliver very misleading results if the rise in the probability of being hired with open-end contracts were also shared by other age groups that cannot be affected by the subsidy, namely workers under 25. In this case there should be some other reason, common to all workers, which explains the increase.

To control for those possible common effects we include in the sample people who do not belong to the treated group. The next three strategies, referred to as AW1, AW2 and AW3, serve this purpose. Here the identification is both within (different effects for the same group over time) and across age groups (different effects across groups in the same year). In particular strategy AW1 is a “diff in diff” estimator that identifies the effect of the subsidy as

the change, in 2001 with respect to some reference year, of the difference between the effect of cohort "25 and over " and the cohort "under 25". Strategy AW2 is also a "diff in diff" estimator that uses as treated group only workers 25 to 35 years old in 2001 and as control group the remaining workers between 15 and 65 years old. Finally, the strategy AW3 is similar to AW2 except for the control group only including people over 35.

Before turning to the results, we want to stress a last point: while our sample is not well suited to estimate the effect of the subsidy on the unconditional probability of being hired with a permanent contract, we could still provide an approximated evaluation of its impact on the overall level of employment in 2001 for the treated group. This is what we do in paragraph 7.4, where we estimate the effect of the new regulation on the probability of being hired as an employee in the 12 months preceding October 2001. The corresponding sample was extended to include every potentially eligible person, i.e. everybody who, in the previous 12 months, was not working in a permanent position.

7.2 Results.

Before taking model (1) to the data we need to specify the function g() and comment on the demographic characteristics that have been included. The g() function is specified as a cubic in age, a quadratic in schooling and the interaction between the two variables. The demographics include gender, regional dummies and marital status. We estimate such a model under all 5 strategies presented in Table 7. Results are shown in Table 8.

The overall message coming out from the 5 models is that, when the reference year is 2000, the average effect (β_1) is zero for an average worker, while the effect is more relevant for the youngest among the eligible workers.

In detail, results suggest that in 2001 the probability of being hired with an open-end contract went up anything between 0.7 to 4.2 percentage points depending on the model chosen (heading "Treated" in the table). The effect would be smaller (often negative), had we chosen as reference a year before 1998. Moreover, in only two out of five cases is the coefficient statistically different from zero. The average effect is clearly zero in the AW1 model; this means that for the average worker the subsidy did not change the probability of being hired as a permanent worker when considering that people under 25 experienced a similar increase, even without being affected by the subsidy. However, when we restrict the

treated group to people between 25 and 35, we find a bigger effect both in the comparison over time (2 percentage points in model W2) and in the "diff in diff" specification (3.3 in model AW2). This result means that the subsidy did not have any effect on people older than 35 even if they were eligible. It also explains why we did not find any effect of the subsidy in model AW1: the effect for the 25-35 year olds is washed out by the lack of impact on older workers. This explanation is supported by the results of the AW3 model, where we directly compare only subgroups of the eligible population. For an average worker aged 25 to 35 years the probability of being hired in a permanent job went up by 4.2 percentage points between 2000 and 2001 when compared to a worker of 36 or over.

There might be an alternative explanation for these differences in the effect of the subsidy across age groups that is based on the fact that we measure our treated group with errors. We know that some new hires among people of 25 years and over are not eligible for the subsidy because they are just moving between two permanent positions. If the variance of this measurement error turns out to be large, it might seriously attenuate the average effects of the subsidy. If these movers between permanent positions are particularly concentrated among older workers, say those over 35, the effect of the subsidy estimated for younger workers represents more a measure of the attenuation bias due to a measurement error (for older workers) than an indication of firms' age preferences. We will come back to this issue later on[20].

When we address the second question - whether the effect of the subsidy significantly differs across workers (β_2, heading "Treated*educ" in the table) - we get a clearer answer: one year of education above the average increases the probability of being a permanent worker by 1 percentage point. This marginal effect implies that for a college graduate the probability was between 7 and 9 percentage points higher in 2001 than in the 1990s; for a high-school graduate this increase was about 2-3 percentage points. For less educated workers, instead, these estimates suggest the effect might even be negative: for a lower secondary graduate the reduction in probability was about 2 to 3 percentage points compared with average workers, which implies a small negative effect overall (including β_1) with respect to 2000. In sharp contrast with the average effect, the interaction between treatment

[20] See paragraph 7.3.1.

and schooling effect (β_2) is very stable across specifications (it ranges from 0.96 to 1.2 percentage points) and always precisely estimated.

One additional comment on Table 8 is necessary to explain the negative slope of the education variable on the probability of being hired with a permanent contract. The effect is due essentially to the fact that, by controlling for age, workers with more years of schooling, compared with workers with less education, either have been searching for shorter time or are facing their first work experience, which is more likely to occur in a temporary job.

7.3 Robustness checks

7.3.1. Check on measurement errors

Our data set is a collection of surveys that prevents us from measuring people's transitions among different labour market statuses. This feature does not allow us to identify whether or not a newly hired person was eligible for the subsidy because we cannot eliminate those transiting from a previous open-end contract to a new open-end one. We have overlooked this problem by assuming that all people hired between October 2000 and October 2001 were eligible as long as they were at least 25 years old. However, in doing so we are introducing a bias in our estimates: we measure the eligibility status with an error since new hires also include people moving between two permanent jobs, who are not eligible for the subsidy.

Measurement errors for the eligibility status may be responsible for the weak average effects estimated in Table 8 (because of an attenuation bias) and may also underestimate the interaction with schooling. In order to address this problem we resort to an instrumental variable estimation method. The instrument we use is the share of people who, in October 2000, would have been eligible for the subsidy. Practically, we divided the October 2000 sample into cells defined as the intersection of age, gender and region (50 ages*2 genders *19 regions); for every cell we computed the share of people who were not employed in permanent positions with respect to total cell population.

We use this indicator and its interaction with the school variable to estimate with instrumental variables the usual 5 models with the same specification adopted in Table 8. Results are presented in Table 9. To make comparison easier, we also estimated all 5

strategies with a linear probability model in order to highlight the slope variations generated by the changes in the functional form (i.e. from probit to linear model) as well as those due to the instrumental variables.

Using a linear probability model (with robust standard error) hardly changes any of the existing slopes. Instrumental variables, instead, marginally change the coefficient and in all but one case slopes increase. This upward revision is coherent with the idea that the dummy used to indicate the eligibility status is affected by measurement errors, which, however, appear to be small. In most cases, average effects (labelled as "Treated" in Table 9) tend to rise by about a half percentage point, while the interaction coefficient changes at most by one fifth of a decimal point. The largest change occurs in model AW3 (where the treated group is restricted to people 25 to 35 years old). In this case the average effect increases from 4 to 5.2 per cent (and the interaction rises from 1 to 1.2 per cent). On the one hand, this result suggests the attenuation bias mostly affects younger workers; on the other hand it indicates that instrumental variable estimates tend to reinforce our conclusion that firms prefer to hire on a permanent basis young and well educated people.

7.3.2. Check of the functional form

In order to evaluate the robustness of our results we estimated equation (1) with a fully non-parametric specification that allows for one dummy for each age, level of schooling and the interaction of the two sets of dummies; in addition we include gender, regional and marital status as well as year effects. However, we regrouped the schooling variable in three levels: college or more, high school, lower secondary school or less[21].

This fully non-parametric specification seems to confirm most of the results of the more structured model (Table 10). The effect for people with at most a lower secondary education (which in this specification can be directly read looking at the heading "Treated") is zero in all but AW1 specification where it is negative but still poorly estimated. The effects for high-school graduates range from 3 to 4.6 percentage points. Larger values are found when we restrict the treated cohort to younger workers (25-35 years old). However,

[21] College or more also includes degrees granted after 2-3 years of college studies (Diplomi Universitari e Lauree Brevi); high-school also includes technical high-school degrees, which are acquired in 2 or 3 years rather than the usual 5 ones following lower secondary education.

the small t-stats of these estimates point to the fact that, again, the effects are weak. In contrast, the effects are stronger and precisely estimated when we look at college graduates. For this group, estimates range from 10.4 to 11 percentage points.

7.3.3. Check of the identification strategies

We run an additional set of controls that address the following question. We need to be sure that the effects we have identified in 2001 for the treated cohort were absent in the years immediately before, say in 2000 and 1999. Otherwise the additional probability of being hired with an open-end contract cannot be attributed to the subsidy, which has been in force only since October 2000. To carry out this control we estimated equation (1) under the same specification discussed above but adding to every year (1993 to 2000) a dummy for the same group of people who, in 2001, were eligible for the subsidy. We also include the usual interaction with the years of schooling. This specification allows us to estimate one β_1 and one β_2 for every single year comprised in our sample (i.e. from 1993 to 2001); thus a direct comparison of this marginal effect would provide an answer to our concerns. In Table 11 we report the results of this estimation for all our strategies.

For the average effects (β_1), Table 11 tells much the same story as the previous two tables. When we use only within-age-group differences (models W1 and W2) we obtain virtually the same results as the previous model since β_1 is simply the year effect. The estimates of the effect for the year 2001 are small, positive (1.8 and 2.9 per cent increases in the probability) and not very far from zero; however, they compare with a zero effect for the years going from 1998 to 2000. Again the effects are stronger when we exclude older workers from the treated group. The effect in 2001 is not different from the previous years when we include in the control group workers 15-24 years old (model AW1), meaning that the small increases we registered for the treated group in model W1 and W2 were not specific to that group. In contrast, in the remaining two models (AW2, AW3) the small average effect is still there in the new specification. Indeed, we find a positive marginal effect in the years 1998-2000, but the difference between 2001 and the average of these previous years remains in the order of magnitude of 1 to 2.4 percentage points.

Results change instead when we look at the interaction with the years of schooling. Two results need to be highlighted: the marginal effect for the year 2001 - with respect to

2000 - of the interaction term halves with respect to the estimate in Table 8[22] and it seems to be the case that firms began to hire people with higher schooling levels even before the subsidy was in place. These results apparently weaken our story. However, the rises in the marginal effect for the year 2000 can be fully accounted for with the sharp rises in labour demand, mainly in the northern regions of the country: firms facing labour force scarcity competed over workers by offering permanent contracts. Whatever the reasons behind the year 2000 rise might be, it is interesting to note that firms again selected better-educated workers. This suggests that the best educated workers turn out to be those who exploit more promptly the favourable shifts in the probability of being hired in a permanent position. In some sense this observation can represent an implicit confirmation of our story.

Before turning to analyze this labour demand explanation, Table 11 deserves one additional comment. Here the interaction effect is smaller compared with Table 8 when referred to the year 2000, but is equal or even stronger when referred to almost every other year. Thus, if we can provide an explanation for the year 2000 results we have also confirmed the results of Table 8.

To ground our speculation for the year 2000 on statistical evidence we have run two additional tests: the first one entails re-estimating the model (we chose specification AW1) after adding to the specification of Table 11 a set of interaction effects between treated, years of schooling, and northern regions. The idea is to use the northern region dummies to control for labour shortage, which was most severe in these areas (Figure 11). With this new specification the effect for the year 2000 disappears: the marginal effect for that year is identical to that of the previous years (1.04 in 2000, 0.95 in 1999 and 1.12 in 1998, see column three of Table 12). At the same time the dummy for the year 2000 in the northern area is the highest in the whole period (it was zero in that year and about -0.01 per cent in the preceding period). Moreover the marginal effect for the year 2001 rises by about 0.3 percentage points compared with the basic specification; the difference with the year 2000 goes back to about 1 percentage point for every additional year of education above the average, that is the basic value we found in Table 8.

[22] The differences between 2001and 2000 vary from 0.5 to 0.7 in the 5 models.

In the second test we directly use an indicator of labour shortage to control for labour demand. The indicator is the share of firms in the manufacturing sector that in the fourth quarter of each year had difficulty recruiting workers. It is plotted in Figure 11. Using the interaction of this indicator with the years of schooling, we obtain about the same result as the previous control specification. The year 2000 effect disappears and the gap between 2001 and 2000 is again around 1 percentage point for every additional year of schooling.

Overall, the outcomes of our robustness checks appear to support our results, which are summarized in Figure 12. Briefly, we believe that these results suggest firms did use the subsidy to hire new workers with open-end contracts but were very selective in choosing workers. The differences in the average effect (β_1) across the five specifications suggest they mostly hired young workers (25 to 35 years old), and the interaction between treated group and years of schooling indicates that they mostly selected highly educated people. Since this group is the most likely to hold a permanent contract, regardless of the subsidy, our results suggest that the new subsidy increased the probability of being hired with a permanent contract for those people who would have been hired in such a position even without financial support. In other words, the new incentive did not created additional opportunities to enter permanent jobs for everybody but rather for the strongest group in the labour market.

7.3.4. Checks on geographical differences

In this section we address the issue of geographical differences in our results. There are two reasons for running such controls. On the one hand, the effect of the subsidy might be stronger in the southern area where the tax credit was fifty per cent higher than in the central and northern regions. On the other hand, the school effect that we identified could, in reality, be a regional effect. To clarify suppose that the bulk of college graduates who are eligible for the subsidy were located in the southern regions; then the effect we identified for college graduates could be a southern effect (brought about by the larger tax credit) rather than a school effect[23]. In any case we could not disentangle the two.

We tackle this problem by estimating our five models allowing for heterogeneity effects. In the first set of regressions we only focused on the effect of the subsidy and its

[23] We thank Ugo Trivellato for suggesting this point.

interaction with the years of schooling (pooled regression); in the second group of equations we estimated a separate regression for the south, thereby allowing for heterogeneity on all coefficients (split sample regression). Results are presented in Table 13. We found evidence of heterogeneous responses across geographical areas, but only for the interaction between eligibility status and level of education. In contrast, the effect of the subsidy for average workers does not seem to change between north and south. In the pooled sample regression the additional effect for the average workers in the south (labelled as "Treated* South") is always zero. Firms' preferences for better educated workers appear to be more pronounced in southern regions where the interaction effects between eligibility status and schooling years (in addition to the mean) are between 2 and 4 times larger compared to the north (as estimated in the pooling regression).

However, this heterogeneity may be overstated by the pooled regression as it turns out to be smaller, but still very present, when estimated in the sample split regression: in all identification strategies the schooling interaction slope is higher than the corresponding number in Table 8, with non-trivial differences that range from 1 to 5 tenths of a percentage point.

These results seem to be in line with our previous findings. In particular, they point to the fact that in the southern regions the chances for a worker with little education could even have been jeopardized by the introduction of the new tax credit.

7.4 The effects on the probability of being hired

In this section we investigate whether the tax credit, though not improving the relative position of the least educated workers, did improve the overall hiring rate in the year 2001 for the treated group. To this end we estimated the probability of being hired in the preceding 12 months as a function of some workers' demographic characteristics, year dummies and a dummy that indicates whether the worker is eligible for the tax credit:

$$
\begin{aligned}
& Pr(Hired) = \Phi\left(x_{it}'\delta\right) \\
& x_{it}'\delta = \delta_o + \delta_1 treated_{it} + g(schooling_{it}, age_{it}) + \\
& demographic\ characteristics_{it} + year\ dummies
\end{aligned}
\qquad (2)
$$

We estimated this equation on a sample that was extracted from the October releases of the 1993-2001 Labour Force Surveys and includes all people aged 15 to 65 who were

either hired in the last 12 months or are self-employed, unemployed or out of the labour force. Notice that we included among new hires the permanent to permanent switchers and excluded from the sample workers who had held a permanent position for more than 12 months. This sample selection could generate an overestimation of the possible positive employment effect of the subsidy. We adopted an identical specification to that of equation (1), but left out the interaction between the eligibility status and the years of education. We estimated equation (2) under all 5 identification strategies reported in Table 7. Results are presented in Table 14.

Table 14 shows the effect of the tax credit on the probability of being hired in the period ranging from October 2000 to October 2001. The overall message is that it did not change the probability of being hired in 2001 compared to the year 2000. The "diff in diff" estimator using the whole sample (model AW1) shows that the tax credit increased the chances of being hired by 0.3 percentage points, a very small amount if compared with the mean of the dependent variable (10.2 per cent) and statistically not different from zero. The effect is small even if we compare the treated group with itself over time: in model W1 the marginal effect is 0.2 percentage points while the corresponding likelihood of being hired is about 10.6 per cent. The tax credit seems to have had some effect only for the 25-35 year olds and only compared with older workers: in model W2 we estimate an over time effect of 1 per cent (17.7 per cent is the average likelihood of being hired), in model AW3 the "diff in diff" estimate is 0.5 percentage points. But even for this group, the "diff in diff" estimate drops to zero when we include in the control group people aged at most 24 years old.

To sum up, the introduction of the tax credit does not seem to have increased the overall probability of being hired in 2001 with respect to the previous year. This result reinforces our previous conclusions that the major impact of the new provision has been almost exclusively a pure substitution effect between temporary and open-end contracts. However, these higher chances to step into permanent jobs seem to have been limited to better-educated workers.

7.5 Indications of dead-weight losses

Throughout the whole paper we have been claiming that firms chose to hire under open-end contracts those workers they would have hired anyway, regardless of the subsidy. We have provided some pieces of evidence to support this claim. Figures presented in Table 5 suggest that people with more years of schooling have higher chances of eventually moving into an open-end contract. Additional evidence is provided in Table 11, which shows a higher probability of being hired with an open-end contract among people with a higher level of education even before 2001. We also emphasized how a favourable shift in labour demand tends to be better exploited by more educated people.

In this paragraph we reinforce these indications through a complete analysis of the observed transitions between different labour market statuses; the results of this tend to confirm that better educated people have indeed higher chances of ending up working in permanent positions.

To this end we use some additional information provided by a special data set, covering a panel of workers who were observed for two subsequent periods, namely from 1999 to 2000. This special data source was constructed by the Italian statistical office (ISTAT) by matching workers across surveys, thereby exploiting six key variables that either do not change over time or vary in a deterministic and perfectly predictable way[24]. Because of the rotation mechanism characterizing the sample (a "two in, two out, two in" scheme), we can look at transitions on a quarterly and annual basis. Unfortunately, transition data are presently available only for the period 1999 to 2000.

We exploited the annual matches (pooling together the matches for to the four quarters of every year) to construct a matrix of transitions among the existing labour market statuses. This matrix was then used to construct the equilibrium distribution among the labour market statuses of 100 people. Formally, we iterated the following equation 30 times starting from an initial vector V_0[25]:

$$V_{t+1} = T\,V_t$$

[24] This data-set is not available to the general public. We are allowed to use it because it was exploited in a joint research project between ISTAT and the Bank of Italy.

[25] Alternatively we could have solved the linear system of equations $(I - T) = 0$ implied by the equilibrium conditions.

where T is the transition matrix with the generic elements Tij representing the probability of moving from state j (at time t) to state i (at time t+1). We allowed for the maximum detail by considering 11 labour market statuses (open-end contracts, training-employment fixed-term contracts, fixed-term contracts for lack of better opportunities, freely chosen fixed-term contracts, fixed-term contracts for a trial period, other fixed-term contracts, self-employment, unemployment because of firing, unemployment after entering the labour market for the first time, other unemployed people, out of the labour force).

Our exercise entails estimating and comparing several equilibrium distributions, one for every schooling level. In particular, we estimated an equilibrium distribution for people with at most a lower secondary diploma (8th grade), another one for people with a high-school degree and a third one for college graduates. Panel (a) of Figure 13 reports the four equilibrium distributions (we aggregated fixed-term contracts as well as unemployment), the first three referring to every single education level and the last one covering the aggregate category. The evidence seems to support our claim. Compared with people holding at most a lower secondary diploma, high-school graduates do indeed show a 24 per cent higher probability of ending up working in an open-end contract. A college degree further increases this chance by about 4 percentage points. Moreover, higher educated people have a much better chance of being employed in general, no matter if they work as employees or self-employed (i.e. their overall employment rate is higher compared with less educated workers); this happens especially to those holding a college degree.

It might appear strange that, compared with a high-school diploma, a college degree turns out to increase the chance of working with an open-end contract by only 4 percentage points. The reason behind this apparently disappointing result is that many college graduates work as professionals (e.g. lawyers, accountants, physicians as well as engineers). This implies that if working arrangements required these kinds of occupation to be organized within a firm with a traditional payroll labour contract, then the share of college graduates employed with an open-end contract would appear to be much higher. An indirect way of supporting this last claim is to look at the same evidence broken down by gender. Since professionals appear better represented among male college graduates, the college-high school spread in the probability of working with a permanent contract should be lower for men (in Figure 13, panel (b), it is actually negative), than for women (10 percentage points, as shown in Figure 13, panel (c)).

8. Conclusions

In this paper we examine the effects of a new regulatory provision put forward in Italy at the end of 2000 to foster employment with open-end rather than fixed-term contracts. We provide basic information about temporary contracts in Italy; in addition we explain why they might be a source of concern and how policy-makers are trying to reduce their negative effects while retaining their positive sides. One attempt in this direction was the introduction of the tax credit we examined in this paper. The effects of this new provision were examined both formally and empirically. Two basic questions were addressed, namely whether the new incentive created additional opportunities to enter permanent jobs, and whether these chances were available for every worker or limited to specific groups. In other words, we asked whether firms took advantage of the government's financial support by anticipating the employment of people they would have anyway hired regardless of the incentive.

Our analytical and empirical framework is not specific to the case of Italy and might prove useful for analyzing similar programmes that have been adopted in many other European countries to foster hiring into permanent rather than temporary employment.

Results seem to indicate that, while overall employment probability did not change, firms used this subsidy mainly to hire young and well-educated workers on a permanent basis; perhaps those who would have been hired with such contracts regardless of the subsidy, albeit after a short transition through temporary employment. Our estimates suggest that, compared with the previous year, in 2001 the subsidy did indeed increase the probability of being hired with an open-end contract, conditional on being hired, but in a rather uneven way across workers. The probability rose by about 10 per cent for workers holding a college degree, by about 4 per cent for people with a high-school diploma, while did not change or might even have declined slightly for less educated workers. The empirical evidence squares with formal prediction.

Tables and figures

Table 1

FIXED-TERM CONTRACTS IN OECD COUNTRIES;
SHARE AND CONTRIBUTION TO EMPLOYMENT GROWTH 1990-2000
(percentage points)

	Employment growth contributions			Share of temp jobs in payroll employment	
	Temp jobs	Permanent jobs	Total	1990	2000
Austria (4)	2.0	-0.9	1.1	6.0	7.9
Belgium	5.3	12.4	17.7	5.3	9.0
Canada (6)	2.3	7.0	9.3	11.3	12.5
Czech Republic (2)	2.4	-5.4	-2.9	5.5	9.3
Denmark	-0.1	5.0	4.8	10.8	10.2
Finland (1)	4.4	2.7	7.1	13.2	16.5
France	5.9	3.9	9.9	10.4	14.5
Germany (1)	2.4	-4.5	-2.1	10.3	12.6
Greece	-1.0	19.5	18.5	16.6	13.1
Hungary (6)	2.2	5.5	7.7	5.6	7.0
Iceland (1)	38.3	-20.8	17.5	14.7	45.3
Ireland	-1.6	48.9	47.4	8.5	4.4
Italy	4.8	-6.0	-1.2	5.2	10.1
Japan	3.8	7.6	11.4	10.6	12.9
Luxembourg	0.6	16.6	17.2	3.6	3.7
Mexico (4)	3.0	24.2	27.2	23.1	20.5
Netherlands	9.9	15.2	25.1	7.6	13.8
Norway (5)	-2.8	10.8	8.0	12.9	9.3
Portugal	3.9	4.8	8.7	18.3	20.4
Slovak Republic (3)	1.8	-2.2	-0.4	2.9	4.9
Spain	10.2	14.4	24.7	29.8	32.1
Sweden (4)	1.7	5.0	6.6	12.4	14.6
Switzerland (1)	-1.4	0.8	-0.5	13.0	11.7
Turkey	14.1	25.8	39.9	14.4	20.4
United Kingdom	1.9	4.6	6.5	5.2	6.7

Source: OECD Employment Outlook , 2002.
(1) 1991-2000; (2) 1993-2000; (3) 1994-2000; (4) 1995-2000; (5) 1996-2000; (6) 1997-2000.

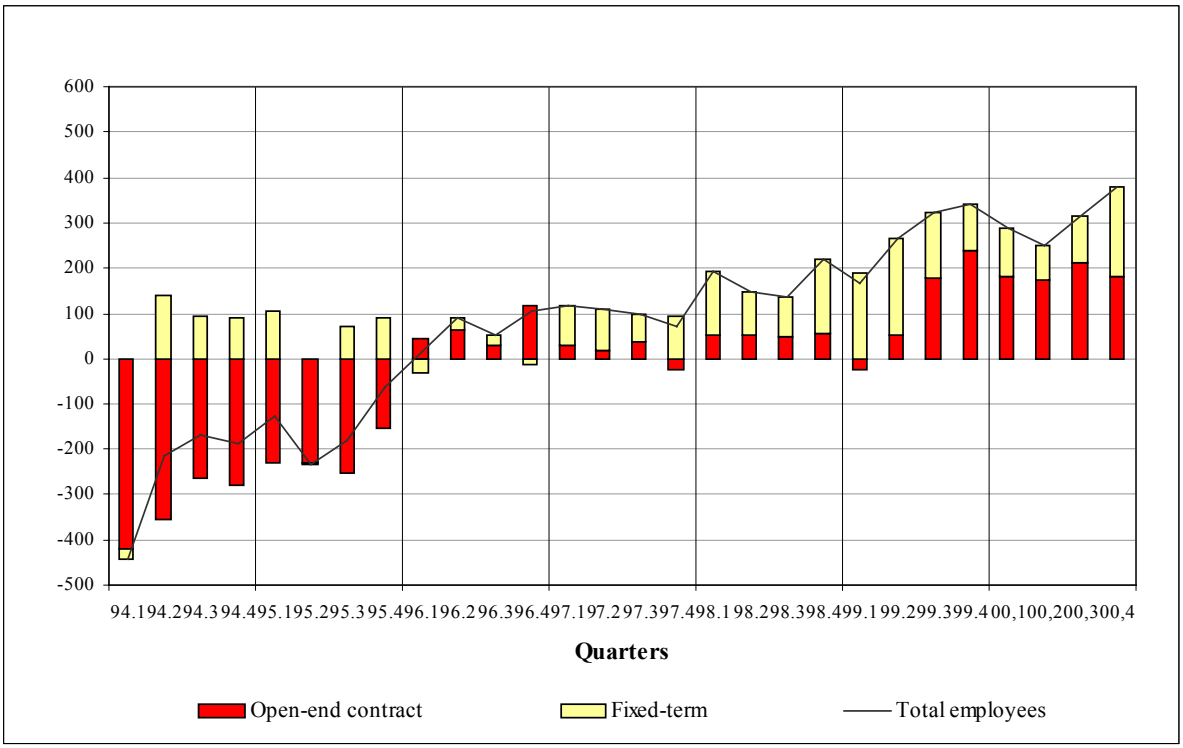
Figure 1

EMPLOYEES WITH FIXED-TERM CONTRACTS AS A SHARE OF ALL EMPLOYEES
(percentage points)

Source: Bank of Italy, Annual Report for the year 2000.

Figure 2

EMPLOYMENT BY TYPE OF LABOUR CONTRACT
(Changes, in 000, on the correspondent quarter)



Sources: Authors' calculation on Labour Force Survey data

Table 2

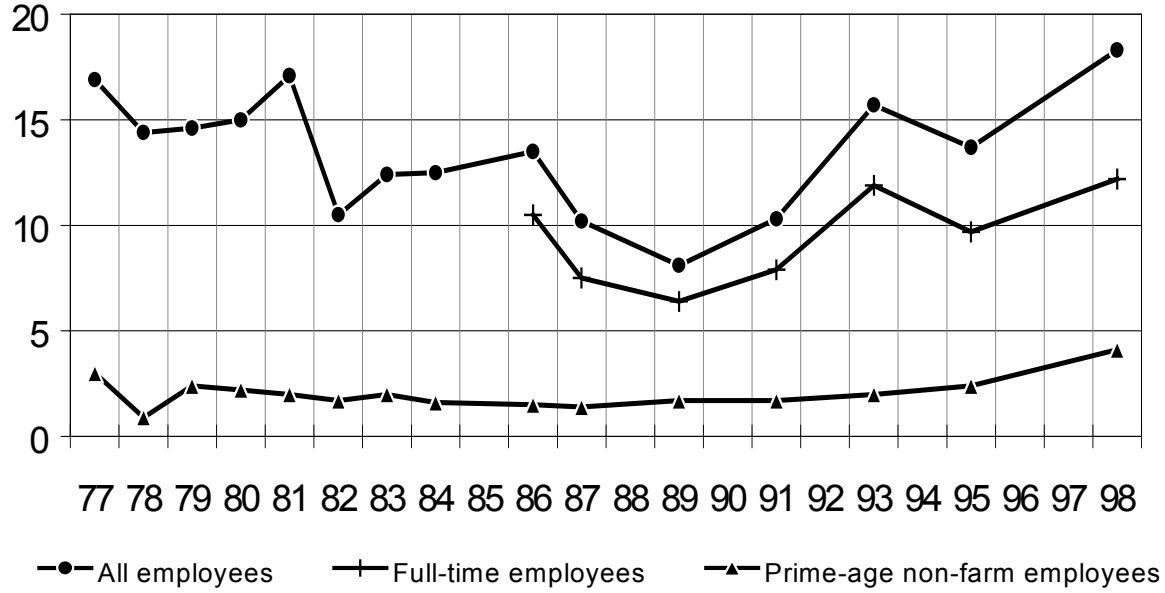
BASIC CHARACTERISTICS OF FIXED-TERM CONTRACTS IN ITALY (1)
(percentage points)

	1993	1994	1995	1996	1997	1998	1999	2000	2001
By gender									
Males	51.2	52.9	52.5	54.3	53.9	53.4	52.3	51.9	50.4
Females	48.8	47.1	47.5	45.7	46.1	46.6	47.7	48.1	49.6
By age									
15-19	10.5	9.0	8.8	8.1	7.2	7.3	6.5	6.2	5.7
20-24	22.9	21.8	22.2	22.1	21.0	21.8	21.6	19.5	18.0
25-29	19.1	20.6	19.9	19.7	20.2	19.5	20.0	19.5	20.3
30-34	14.2	14.2	16.0	16.3	17.3	16.4	16.3	15.9	15.8
35-39	10.0	10.6	10.1	11.1	11.1	11.3	11.7	13.2	13.6
40-44	6.8	7.5	7.3	7.5	7.7	8.3	8.7	9.0	9.5
45 and over	16.5	16.4	15.7	15.2	15.5	15.4	15.2	16.8	17.1
By school level									
Lower secondary school or less	63.9	60.0	58.4	56.4	54.8	52.3	49.8	48.0	48.0
High School	28.4	31.3	32.3	34.2	34.6	36.8	38.2	39.5	39.4
College	7.6	8.7	9.3	9.3	10.6	10.9	12.0	12.4	12.6
By sector									
Agriculture	20.8	18.7	18.0	16.2	13.7	12.9	12.1	11.1	11.7
Manufacturing	14.9	16.6	17.6	17.5	17.9	19.3	18.1	18.4	17.1
Construction	12.4	11.5	10.7	10.9	11.1	9.9	8.8	8.4	8.4
Services	51.9	53.2	53.7	55.4	57.3	57.9	60.9	62.1	62.8
By reasons for fixed-term contract									
Training-employment	23.6	23.3	23.3	24.5	24.8	29.9	32.3	31.4	29.2
No better opportunities	51.7	52.3	51.0	49.9	49.3	45.3	40.7	43.1	44.4
Don't want a open-end contract	7.2	5.4	5.2	4.8	4.7	3.9	4.6	4.3	4.3
Other reasons	17.5	19.0	20.5	20.8	21.2	20.9	22.4	21.2	22.1
As share of employees	6.1	6.8	7.3	7.3	7.9	8.6	9.5	10.1	9.8

Sources: Authors' calculation on Labour Force Survey data.
(1) Workers 15-65 years old.

Figure 3

SHARE OF LOW-PAID WORKERS IN ITALY, 1977-1998
(percentage points)



Sources: Brandolini, Cipollone, and Sestito (2001).

Table 3

LOG-HOURLY WAGE AND HOUR DIFFERENTIALS IN THE YEAR 2000 (1)

	Log of hourly wage		Annual hours of work	
	Males	Females	Males	Females
Unadjusted				
Fixed-term	-.324 (.026)	-.089 (.030)	-533.3 (25.96)	-588.77 (32.69)
Temporary	-.433 (.079)	-.365 (.092)	-258.6 (78.21)	-261.93 (100.50)
Adjusted (2)				
Fixed-term	-.117 (.023)	.019 (.028)	-391.3 (27.39)	-394.89 (29.71)
Temporary	-.211 (.068)	-.213 (.082)	14.60 (78)	-9.17(86.87)

Sources: Authors' calculation on Bank of Italy Survey of Household Income and Wealth.
(1) References are wages and hours of workers in open-end contracts, standard error in parenthesis. (2) Controls include age, age square, a full set of dummies for education (8 categories), marital status (4 categories), geographical area (3 categories), dummy for part-time.

Table 4

TRANSITIONS AMONG LABOUR MARKET STATUS IN ITALY. OCTOBER 1999
(percentage points)

Initial labour market status ↓	Labour market status at October 1999					
	Same job	Different job				
		Self-employed	Open-end contract	Fixed- term contract	Unemployed and inactive	Total
			After three years from the first job			
Self-employed	82.2	3.4	3.5	1.7	9.2	100
Employee with an open-end contract	81.3	1.3	8.8	1.8	6.8	100
Employee with a fixed-term contract	20.3	3.6	20.8	17.6	37.8	100
All	62.4	2.3	11.6	6.7	16.9	100
			After five years from the first job			
Self-employed	79.3	5.1	7.5	2.8	5.3	100
Employee with an open-end contract	71.0	2.8	16.3	2.2	7.8	100
Employee with a fixed-term contract	9.9	6.1	36.4	17.2	30.4	100
All	54.6	4.2	20.5	6.7	13.9	100

Source: Istat, Annual Report for 1999.

Table 5

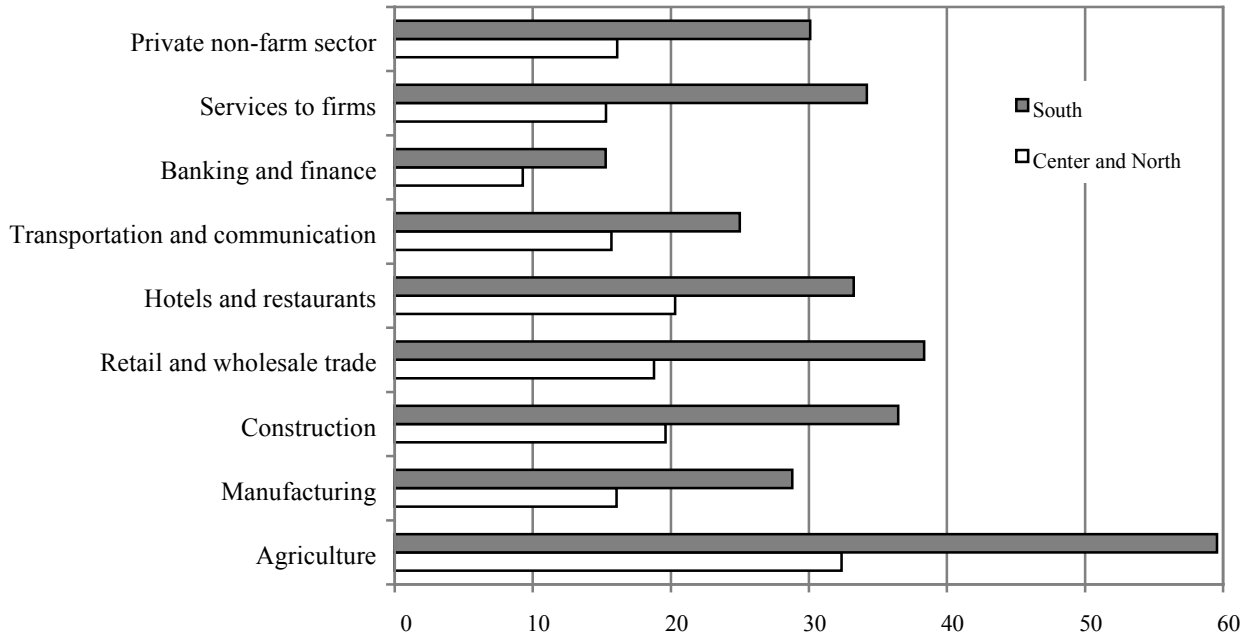
TRANSITIONS OUT OF FIXED-TERM CONTRACTS. OCTOBER 1999
(percentage points)

Persons entering the labour market with fixed-term contract	Labour market status at October 1999					
	Same job	Different job				
		Self-employed	Open-end contract	Fixed- term contract	Unemployed and inactive	Total
			After three years from the first job			
Male	19.2	3.3	23.2	14.6	39.7	100
Female	21.4	3.8	18.3	20.7	35.8	100
College graduate	43.2	3.8	23.0	17.2	12.9	100
High-school graduate	14.9	5.2	23.3	18.0	38.9	100
8th grade graduate	18.5	0.7	15.4	16.9	48.5	100
All	20.3	3.6	20.8	17.6	37.8	100
			After five years from the first job			
Male	8.0	6.2	38.3	19.6	27.9	100
Female	12.1	6.1	34.1	14.2	33.5	100
College graduate	17.1	12.5	38.1	19.6	12.7	100
High-school graduate	8.0	5.1	40.4	17.9	28.5	100
8th grade graduate	10.7	5.8	28.6	15.0	40.0	100
All	9.9	6.1	36.4	17.2	30.4	100

Source: Istat, Annual Report for 1999.

Figure 4

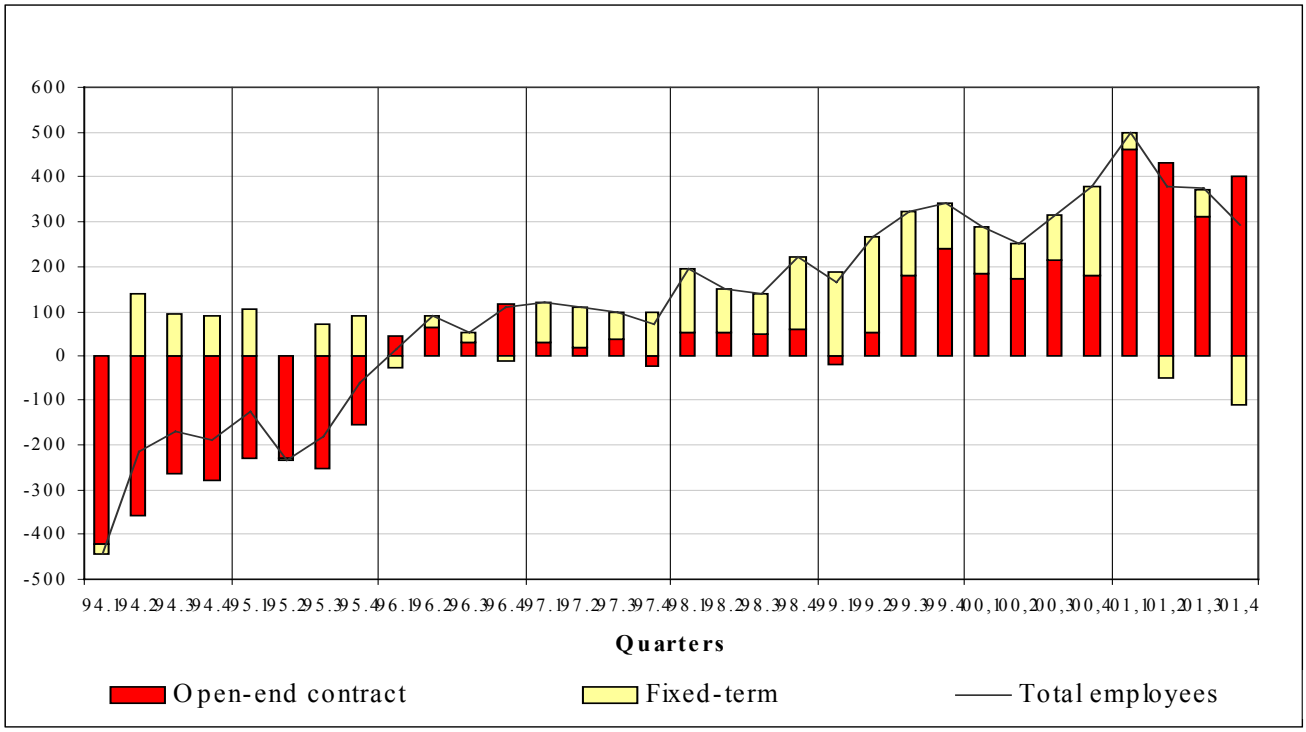
REDUCTION IN LABOUR COSTS DUE TO THE TAX CREDIT BY AREA AND SECTOR
(percentage points)



Sources: Authors' calculation on Istat data.

Figure 5

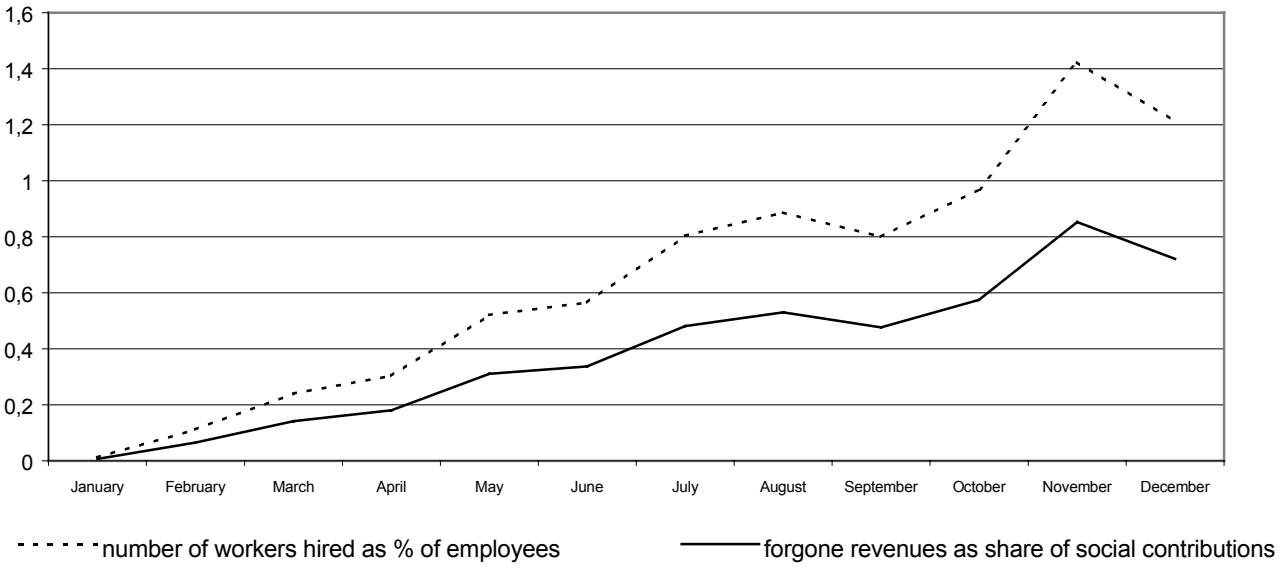
EMPLOYMENT BY TYPE OF LABOUR CONTRACT
(Changes, in 000, on the corresponding quarter)



Sources: Authors' calculation on Labour Force Survey data.

Figure 6

USAGE OF TAX CREDIT: EVIDENCE FROM FISCAL DATA FOR YEAR 2001
(revenues figures are flows, workers figures are stocks)



Sources: Authors' calculations on Ministry of Labour (2002) and Istat.

Table 6

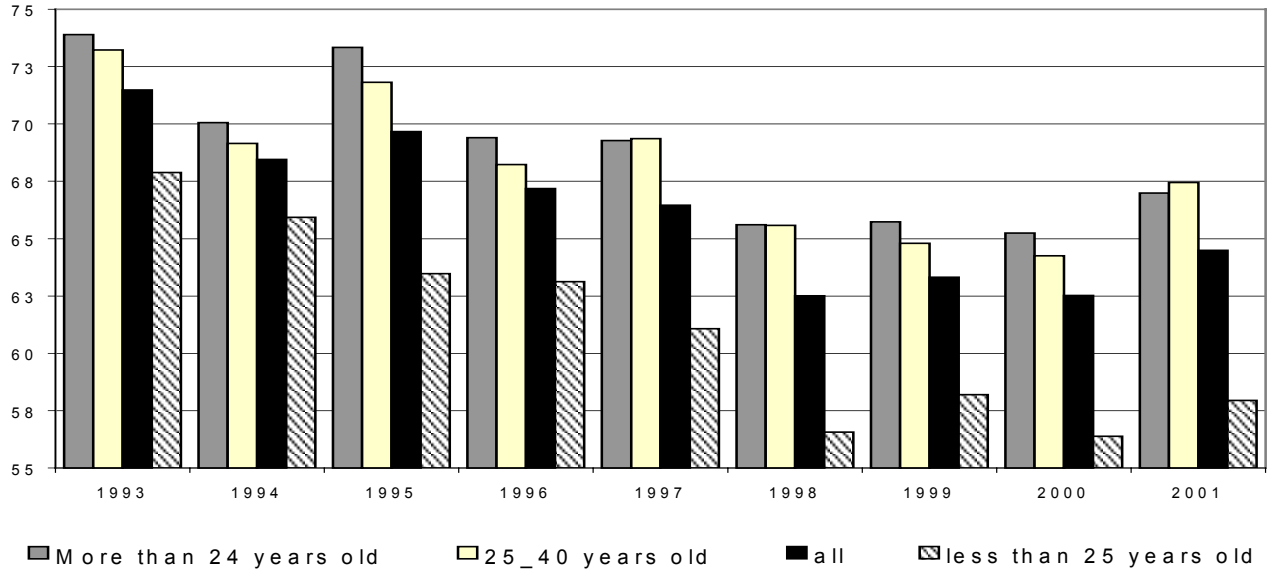
BASIC CHARACTERISTICS OF THE SAMPLE
(percentage points)

	1993	1994	1995	1996	1997	1998	1999	2000	2001
By gender									
Males	59.5	59.0	58.8	58.7	58.6	57.3	56.6	54.0	53.0
Females	40.5	41.0	41.2	41.3	41.4	42.7	43.4	46.0	47.0
Age									
15-19	11.9	11.4	10.4	9.0	8.1	7.7	7.3	7.3	6.2
20-24	28.1	27.3	26.7	26.4	26.1	26.5	24.6	23.4	21.5
25-29	19.4	20.2	19.8	22.6	22.3	22.7	23.1	22.5	24.4
30-34	14.3	14.4	16.4	15.8	16.2	16.9	17.1	16.6	16.8
35-39	9.6	10.4	9.7	9.7	10.5	10.2	10.3	12.2	12.3
40-44	6.2	6.5	7.1	7.2	5.9	6.7	7.9	7.7	8.5
45 and over	10.6	9.8	10.0	9.3	10.8	9.3	9.8	10.3	10.5
School level									
Lower secondary school or less	57.2	55.5	54.9	51.6	50.0	46.9	45.8	43.1	43.2
High- school	34.9	36.3	37.9	39.9	40.3	42.2	42.5	45.5	43.6
College	7.9	8.2	7.2	8.4	9.7	10.9	11.7	11.5	13.2
Sector									
Manufacturing	30.0	33.3	34.9	31.9	32.2	31.7	29.0	28.8	26.9
Construction	16.3	14.1	14.0	14.3	14.2	12.4	12.1	10.7	10.7
Services	53.7	52.6	51.1	53.8	53.6	55.9	58.9	60.5	62.5
Type of contract									
Open-end contracts	71.4	68.5	69.7	67.1	66.4	62.5	63.3	62.5	64.5
Fixed-term contracts	28.6	31.5	30.3	32.9	33.6	37.5	36.7	37.5	35.5
Training-employment	8.2	8.0	9.0	9.9	10.1	14.5	14.1	14.3	10.8
No better opportunities	14.0	15.5	13.4	14.6	15.0	14.9	13.2	14.4	14.9
Don't want an O.E.C.	1.2	1.1	1.2	1.0	1.3	1.3	1.2	1.6	1.4
Other reasons	5.2	6.9	6.7	7.3	7.1	6.8	8.2	7.2	8.3
Number of workers	1,033,806	1,180,133	1,363,629	1,305,653	1,319,812	1,482,466	1,516,225	1,619,386	1,560,900
Number of observations	3,593	4,072	4,779	4,519	4,533	5,084	5,167	5,474	5,146

Sources: Authors' calculation on Labour Force Survey data.

Figure 7

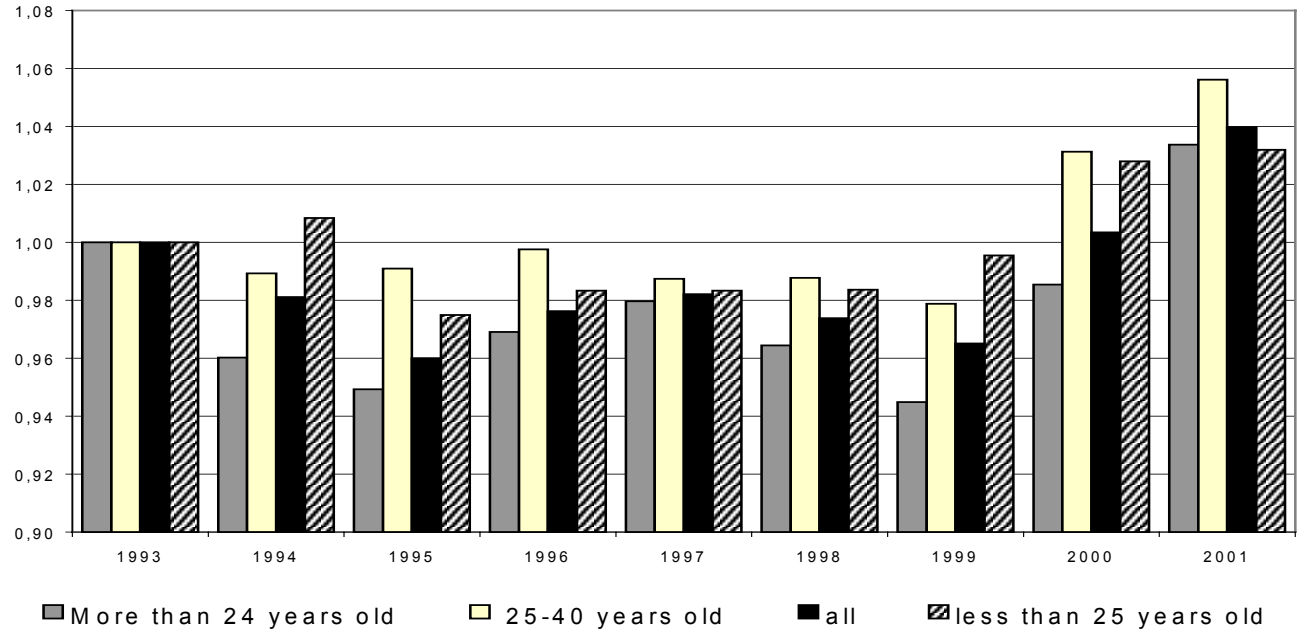
SHARE OF OPEN-END CONTRACTS IN TOTAL NEW CONTRACTS



Sources: Authors' calculation on Labour Force Survey data.

Figure 8

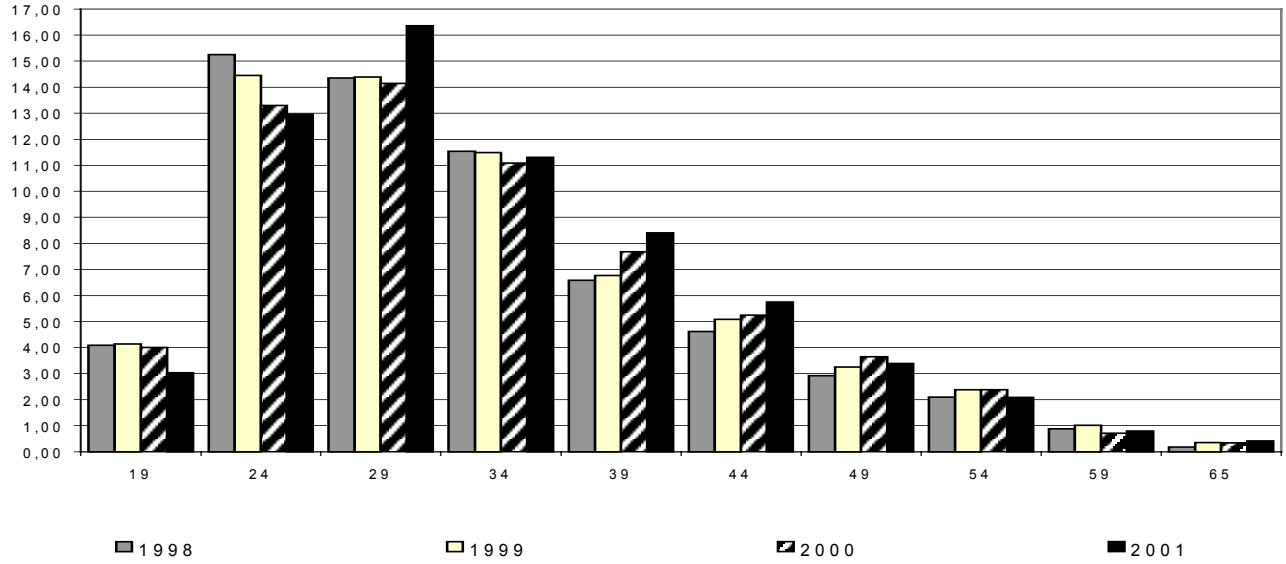
YEARS OF SCHOOLING OF NEW HIRES WITH OPEN-END CONTRACTS COMPARED WITH NEW HIRES WITH FIXED-TERM CONTRACTS
(indexes, 1993=1)



Sources: Authors' calculation on Labour Force Survey data.

Figure 9

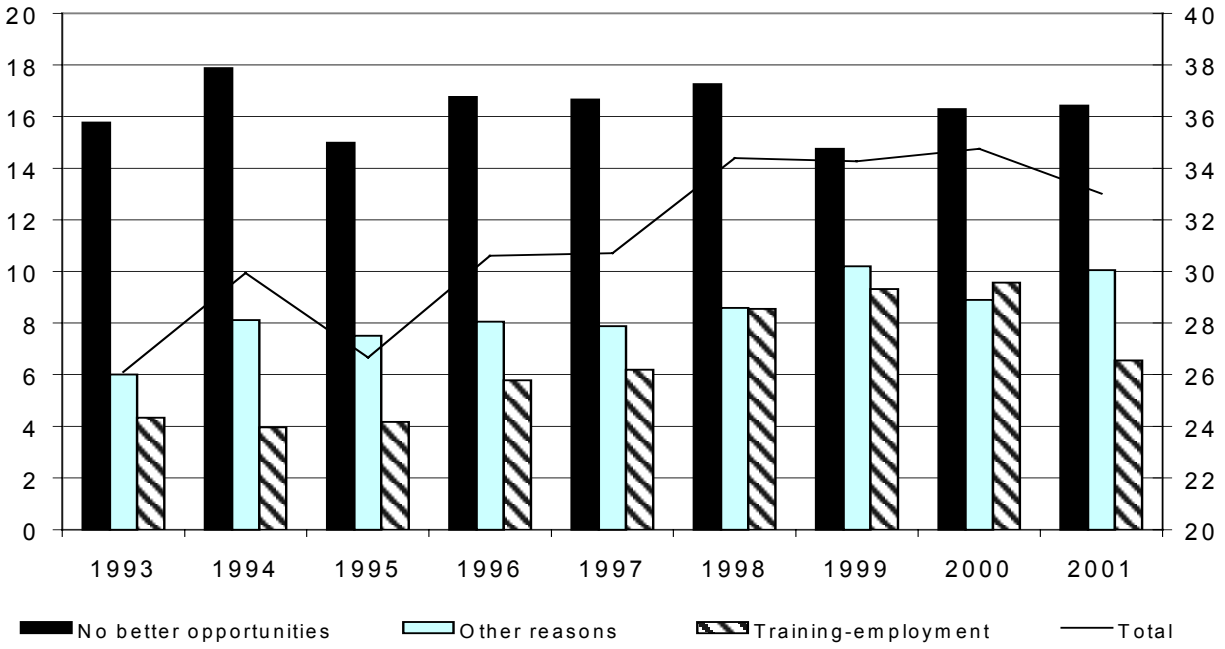
AGE DISTRIBUTION OF SHARE OF OPEN-END CONTRACT IN TOTAL NEW HIRES



Sources: Authors' calculation on Labour Force Survey data.

Figure 10

SHARE OF FIXED-TERM CONTRACTS IN TOTAL NEW HIRES (1)



Sources: Authors' calculation on Labour Force Survey data.
(1) Workers 25 years and older.

Table 7

IDENTIFICATION STRATEGIES

	Model	Treated	Sample	Control
Within-age-group identification	W1	Aged 25 or over in 2001	Aged 25 or over in 1993-2001	Aged 25 and over in 2000-1993
	W2	Aged 25-35 in 2001	Aged 25-35 in 1993-2001	Aged 25-35 in 2000-1993
Across+within-age-group identification	AW1	Aged 25 or over in 2001	Aged 15 or over in 1993-2001	Aged 15-24 in 2001-1993 + 25 and over in 2000-1993
	AW2	Aged 25-35 in 2001	Aged 15 or over in 1993-2001	Aged 15-24 and 36 or over in 2001-1993+25 and over in 2000-1993
	AW3	Aged 25-35 in 2001	Aged 25 or over in 1993-2001	Aged 36 or over in 1993-2001 + 25 and over in 2000-1993

Table 8

PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT CONDITIONAL ON BEING HIRED IN THE PREVIOUS 12 MONTHS (1)

	Model W1		Model W2		Model AW1		Model AW2		Model AW3	
	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat
Treated	.016	1.37	.021	1.27	.007	0.45	.033	2.10	.042	2.36
Treated*educ (2)	.010	4.28	.011	3.57	.011	4.76	.012	3.79	.010	3.16
Age	.016	0.88	.331	.72	.045	6.44	.043	6.01	.013	.74
Age2	-.003	-0.68	-.001	-.68	-.001	-4.97	-.001	-4.59	.000	-.50
Age3	.000	0.53	.000	.64	.000	4.04	.000	3.74	.000	.35
Educ	-.001	-0.15	-.005	-.40	-.018	-3.57	-.019	-3.82	-.003	-.51
Educ2	-.001	-6.08	-.001	-5.70	-.001	-5.42	-.001	-5.55	-.001	-6.06
Age*educ (2)	.001	5.06	.001	2.60	.001	9.99	.001	10.75	.001	5.68
Female	-.133	-20.6	-.110	-13.25	-.096	-18.15	-.096	-18.15	-.133	-20.6
1993	.077	5.80	.057	3.20	.087	7.97	.087	8.00	.078	5.85
1994	.040	3.12	.033	1.97	.055	5.25	.056	5.26	.041	3.15
1995	.069	5.76	.063	3.93	.063	6.23	.063	6.24	.070	5.79
1996	.032	2.61	.027	1.65	.041	4.00	.041	4.02	.032	2.63
1997	.032	2.67	.030	1.82	.034	3.33	.034	3.34	.033	2.69
1998	.003	0.28	.010	.62	.002	.28	.003	.30	.036	.31
1999	.004	0.39	-.001	-.09	.007	.75	.008	.76	.005	.40
2000	Reference		Reference		Reference		Reference		Reference	
2001	--		--		.012	.77	-.000	-.03	-.014	-.87
Number of observations	27847		16490		42367		42367		27847	

(1) Probit model. ME stands for marginal effect, defined as the slope parameter times the density evaluated at the sample mean of each characteristic. Data are from the October survey of each year, include regional and marital status dummies. (2) Scaled by the mean educ=10.24 years.

Table 9

PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT CONDITIONAL ON BEING HIRED IN THE PREVIOUS 12 MONTHS: ALTERNATIVE ESTIMATION METHODS (1)

	Model W1		Model W2		Model AW1		Model AW2		Model AW3	
	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat
Probit model										
Treated	.016	1.37	.021	1.27	.007	.45	.032	2.10	.042	2.36
Interaction with educ (2)	.010	4.28	.011	3.57	.011	4.76	.012	3.79	.010	3.16
Linear probability model										
Treated	.016	1.35	.020	1.27	.010	.60	.033	2.15	.040	2.27
Interaction with educ (2)	.010	4.26	.011	3.58	.011	4.78	.012	3.87	.010	3.18
Instrumental variables										
Treated	.019	1.49	.024	1.41	.004	.20	.034	2.05	.052	2.64
Interaction with educ (2)	.011	4.80	.014	4.08	.012	5.22	.014	4.33	.012	3.71

(1) Data derived from the October survey of each year, include regional and marital status dummies. (2) Scaled by the mean educ=10.24 years.

Table 10

PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT CONDITIONAL ON BEING HIRED IN THE PREVIOUS 12 MONTHS; NON-PARAMETRIC SPECIFICATION (1)

	Model W1		Model W2		Model AW1		Model AW2		Model AW3	
	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat
Treated	-.009	-.57	-.008	-.34	-.024	-1.20	-.006	-0.27	.007	0.29
Treated*high school	.029	1.49	.045	1.71	.031	1.55	.046	1.72	.043	1.69
Treated*college	.105	4.43	.107	3.62	0.11	4.51	.109	3.60	.104	3.58
High-school	-.251	-.73	.035	.96	.214	0.96	.214	0.96	-.251	.345
College	-.486	-1.57	-.069	-1.44	-.457	-1.49	-.396	-1.35	-.416	-1.43
Female	-.133	-20.45	-.109	-13.11	-.096	-18.06	-.096	-18.08	-.133	-20.47
1993	.075	5.58	.056	3.14	.085	7.82	.085	7.82	.075	5.60
1994	.039	3.06	.034	1.96	.055	5.23	.055	5.23	.039	3.08
1995	.069	5.70	.063	3.90	.063	6.20	.062	6.20	.069	5.72
1996	.032	2.64	.027	1.65	.042	4.10	.042	4.11	.033	2.67
1997	.034	2.76	.030	1.87	.035	3.44	.035	3.44	.034	2.78
1998	.003	.29	.011	.68	.003	0.29	.003	.29	.004	.31
1999	.005	.46	-.001	-.04	.007	0.77	.007	.77	.006	.47
2000	Reference		Reference		Reference		Reference		Reference	
2001	--		--		.015	0.94	.004	.33	-.009	-.56
Number of observations	27817		16490		42335		42335		27817	

(1) Probit model. ME stands for marginal effect. Data are from the October survey of each year and include age dummy fully interacted with school dummies: also include regional and marital status dummies.

Table 11

PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT CONDITIONAL ON BEING HIRED IN THE PREVIOUS 12 MONTHS. SPECIFICATION WITH TREATED GROUP FOR EACH YEAR (1)

	W1		W2		AW1		AW2		AW3	
	M.E.	T stat	M.E.	T stat	M.E.	T stat	M.E.	T stat	M.E.	T stat
Average effect										
2001	0.018	1.5	0.029	1.74	0.016	0.84	0.039	2.49	0.057	2.82
2000	Reference		Reference		0.018	1.02	0.020	1.26	0.027	1.3
1999	0.007	0.6	0.011	0.67	0.013	0.74	0.018	1.15	0.024	1.12
1998	0.005	0.4	0.018	1.11	0.024	1.34	0.039	2.48	0.050	2.39
1997	0.034	2.79	0.038	2.23	0.021	1.13	0.021	1.26	0.022	1.01
1996	0.034	2.72	0.036	2.15	0.002	0.11	0.007	0.4	0.019	0.83
1995	0.070	5.75	0.073	4.44	0.046	2.5	0.033	1.97	0.015	0.64
1994	0.041	3.16	0.041	2.36	-0.013	-0.68	-0.006	-0.33	0.014	0.59
1993	0.081	6.01	0.064	3.53	0.009	0.41	-0.021	-1.04	-0.028	-1.05
Interaction with school (2)										
2001	0.006	2.18	0.005	1.26	0.017	5.05	0.013	3.94	0.005	1.16
2000	0.002	0.31	0.001	0.05	0.010	2.95	0.008	2.23	0.000	-0.11
1999	-0.006	-2.03	-0.009	-2.15	0.004	1.09	-0.002	-0.54	-0.009	-2.29
1998	-0.003	-1.08	-0.006	-1.45	0.007	2.02	0.002	0.5	-0.006	-1.52
1997	-0.001	-0.35	-0.006	-1.27	0.009	2.45	0.002	0.5	-0.006	-1.37
1996	-0.003	-0.93	-0.008	-1.69	0.007	1.93	0.000	-0.02	-0.008	-1.84
1995	-0.007	-2.03	-0.010	-2.23	0.003	0.9	-0.002	-0.61	-0.010	-2.3
1994	-0.005	-1.65	-0.005	-1.13	0.005	1.29	0.003	0.65	-0.006	-1.28
1993	0.000	0.1	-0.004	-0.89	0.010	2.71	0.003	0.82	-0.005	-0.95
Year effect										
2001					0.020	1.11	0.009	0.68	-0.003	-0.16
2000					Reference		Reference		Reference	
1999					0.013	0.73	0.013	1.05	0.014	0.76
1998					0.001	0.04	-0.001	-0.1	-0.007	-0.4
1997					0.034	1.92	0.037	2.86	0.040	2.13
1996					0.052	2.99	0.050	3.83	0.043	2.23
1995					0.047	2.74	0.063	4.9	0.082	4.43
1994					0.074	4.22	0.068	5.11	0.052	2.66
1993					0.094	5.3	0.103	7.62	0.110	5.49

(1) Model specified in Table 8. Probit model. ME stands for marginal effect. Data are from the October survey of each year; in addition to the reported variables models include cubic in age, quadratic in year of schooling, interaction between age and year of schooling, female dummy and regional and marital status dummies. (2) Scaled by the mean educ=10.24 years.

Table 12

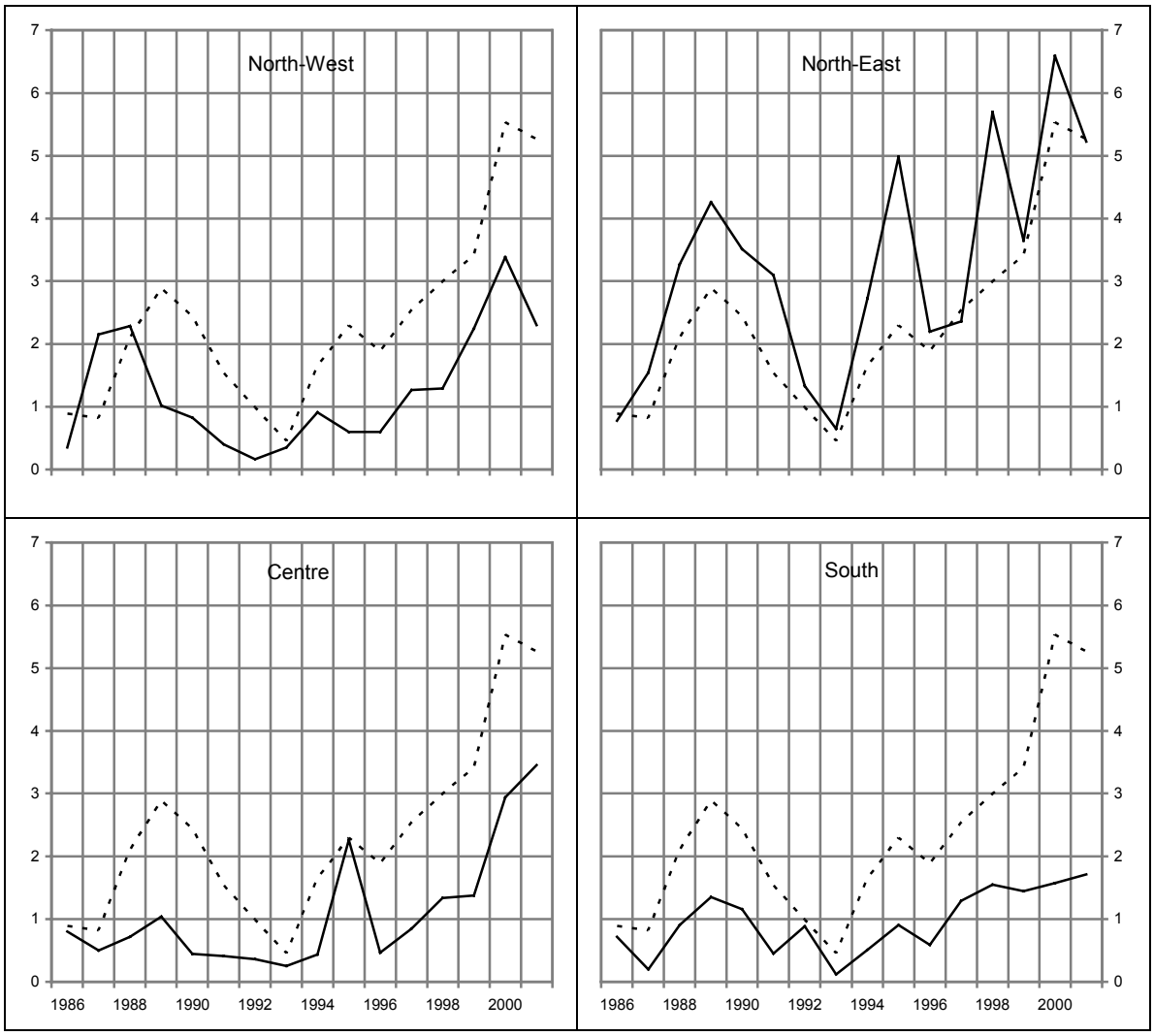
PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT CONDITIONAL ON BEING HIRED IN THE PREVIOUS 12 MONTHS. SPECIFICATION WITH TREATED GROUP FOR EACH YEAR(1); EXPLAINING YEAR 2000 EFFECTS

		Model AW1					
		Basic		Basic and controls for northern regions		Basic and controls labour shortage	
		M.E.	T stat	M.E.	T stat	M.E.	T stat
Average effect	2001	0.016	0.84	0.017	0.9	0.017	0.91
	2000	0.018	1.02	0.018	1.02	0.021	1.17
	1999	0.013	0.74	0.015	0.84	0.015	0.86
	1998	0.024	1.34	0.025	1.43	0.023	1.32
	1997	0.021	1.13	0.023	1.24	0.020	1.09
	1996	0.002	0.11	0.002	0.11	0.002	0.09
	1995	0.046	2.5	0.050	2.74	0.046	2.54
	1994	-0.013	-0.68	-0.013	-0.67	-0.013	-0.67
	1993	0.009	0.41	0.011	0.52	0.011	0.54
Interaction with school(2)	2001	0.017	5.05	0.020	4.95	0.012	3.31
	2000	0.010	2.95	0.010	2.62	0.002	0.46
	1999	0.004	1.09	0.010	2.4	-0.004	-0.87
	1998	0.007	2.02	0.011	2.86	0.007	2.06
	1997	0.009	2.45	0.015	3.51	0.012	3.14
	1996	0.007	1.93	0.008	1.83	0.014	3.29
	1995	0.003	0.9	0.014	3.15	0.008	2.01
	1994	0.005	1.29	0.007	1.51	0.006	1.7
	1993	0.010	2.71	0.018	4.14	0.018	3.94
Labour shortage indicator* schooling(2)						0.004	3.1
Interaction with school (2) in the northern regions	2001			-0.005	-1.21		
	2000			-0.001	-0.16		
	1999			-0.011	-2.53		
	1998			-0.009	-2.02		
	1997			-0.012	-2.46		
	1996			-0.002	-0.34		
	1995			-0.019	-3.82		
	1994			-0.003	-0.62		
	1993			-0.017	-2.95		
Year effects	2001	0.020	1.11	0.020	1.11	0.021	1.18
	2000						
	1999	0.013	0.73	0.013	0.74	0.013	0.76
	1998	0.001	0.04	0.001	0.04	0.004	0.22
	1997	0.034	1.92	0.034	1.92	0.037	2.11
	1996	0.052	2.99	0.052	2.99	0.055	3.16
	1995	0.047	2.74	0.047	2.75	0.049	2.85
	1994	0.074	4.22	0.074	4.22	0.076	4.36
	1993	0.094	5.3	0.094	5.3	0.094	5.29

(1) Model AW1 of Table 11. Probit model. ME stands for marginal effect. Data are from the October survey of each year; in addition to the reported variables models include cubic in age, quadratic in year of schooling, interaction between age and year of schooling, female dummy and regional and marital status dummies. (2) Scaled by the mean educ=10.24 years.

Figure 11

SHARE OF MANUFACTURING FIRMS WITH DIFFICULTIES IN RECRUITING LABOUR FORCE



----- Italy —Specified geographical area

Source: Isae.

Figure 12

ESTIMATED CHANGE IN PROBABILITY OF BEING HIRED WITH AN OPEN-END CONTRACT FOR THE TREATED GROUP
(reference: year 2000).

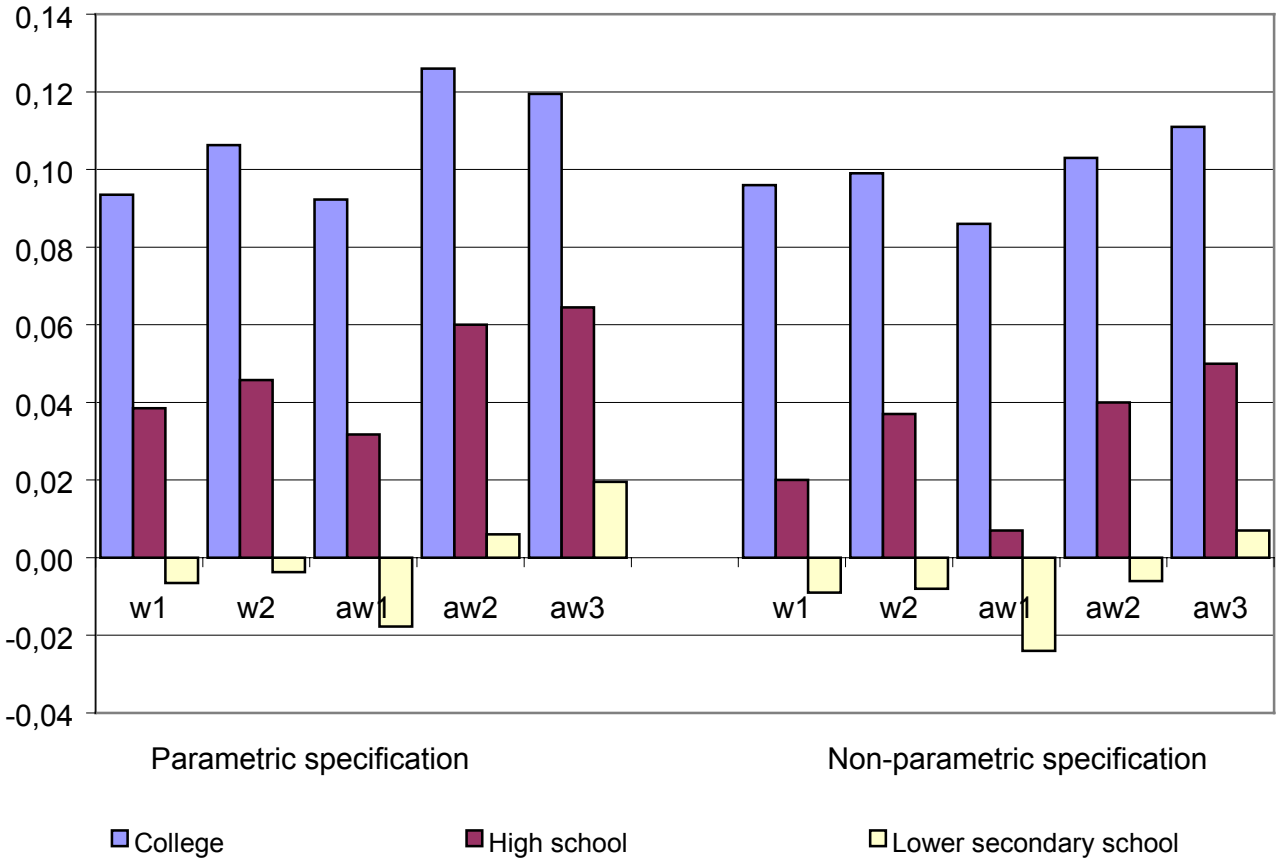


Table 13

GEOGRAPHICAL DIFFERENCES (1)

	Model W1		Model W2		Model AW1		Model AW2		Model AW3	
	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat
Pooled regression										
Treated	.011	0.86	.021	1.14	.009	0.49	.035	2.00	.039	2.02
Interaction with education (2)	.006	2.31	.007	1.87	.007	2.72	.007	1.99	.005	1.49
Treated* South	.017	0.88	-.005	-0.18	-.004	-0.22	-.02	-0.53	.005	0.18
Interaction with education and South (2)	.014	3.18	.020	3.06	.013	2.88	.020	3.01	.021	3.19
Sample split: only South										
Treated	-.011	-0.50	-.004	-0.13	.013	0.41	.022	0.72	.024	0.67
Interaction with education (2)	.011	2.71	.016	2.45	.012	2.86	.016	2.61	.015	2.38

(1) Data derived from the October survey of each year 1993-2001; regression includes cubic in age, quadratic in education, interaction between age and education, female, year, regional and marital status dummies. (2) Scaled by the mean educ.

Table 14

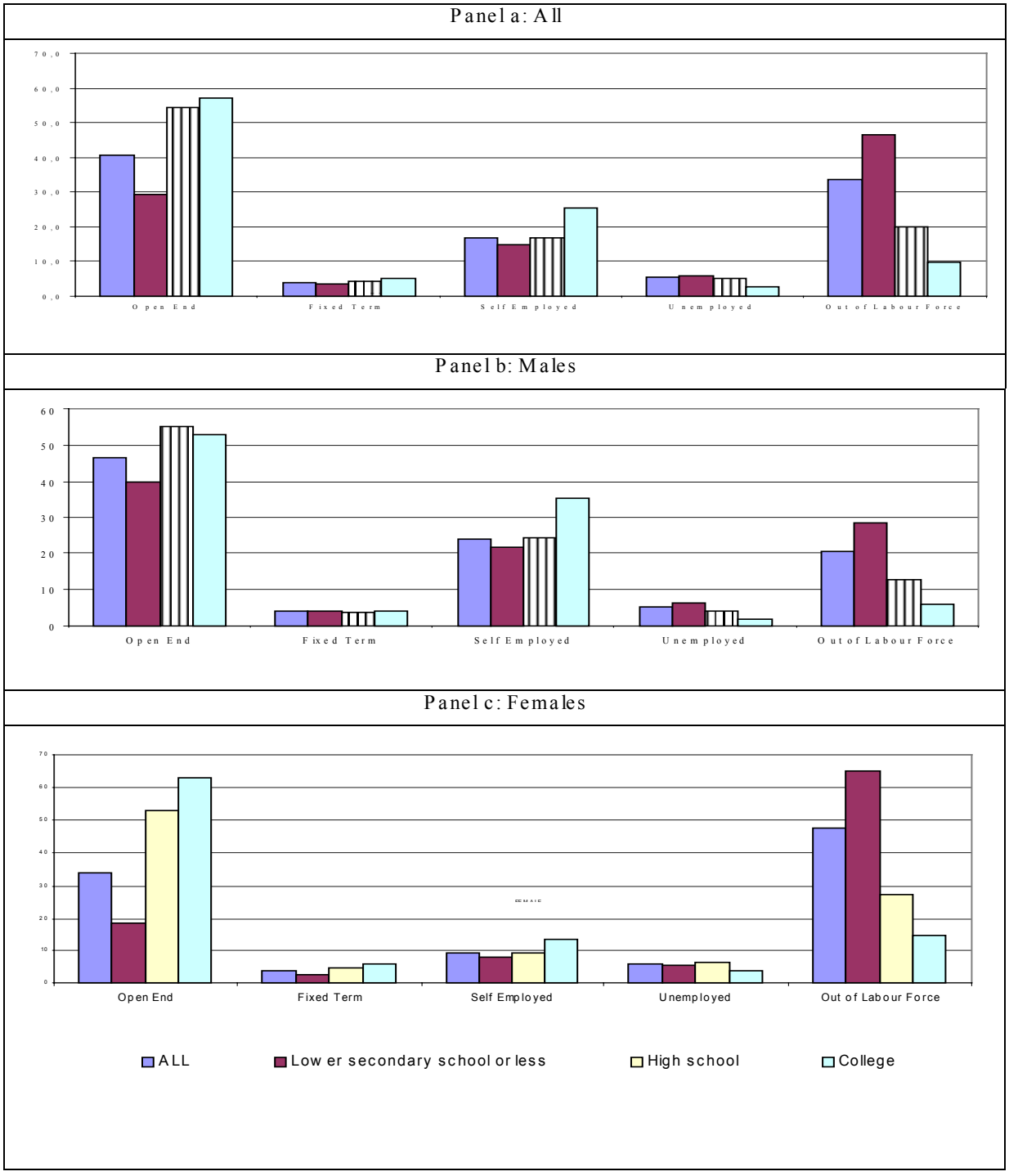
PROBABILITY OF BEING HIRED IN THE PREVIOUS 12 MONTHS (1)

	Model W1		Model W2		Model AW1		Model AW2		Model AW3	
	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat	M.E.	t-stat
Treated	0.002	1.68	0.01	2.23	0.003	1.34	0.001	0.74	0.005	2.48
Age*100	0.007	0.04	7.58	0.63	2.88	45.39	2.88	44.91	0.002	0.01
Age2*100	0.018	4.26	-0.19	-0.48	-0.05	-29.30	-0.05	-28.92	0.018	4.32
Age3*100	-0.03	-10.11	0.02	0.36	0.02	13.06	0.02	12.88	-0.03	-10.2
Educ*100	-0.25	-4.87	-3.01	-9.61	-0.67	-15.49	-0.67	-15.50	-0.25	-4.89
Educ2*100	0.02	9.13	-0.01	-1.72	0.02	10.60	0.020	10.58	0.02	9.08
Age*Educ(2)*100	0.01	14.09	0.13	13.31	0.02	10.60	0.020	28.36	0.01	14.15
Female	-0.11	-120.5	-0.17	-63.6	-0.09	28.30	-0.085	-111.8	-0.11	-120.5
1993	-0.012	-9.23	-0.035	-8.28	-0.013	-11.15	-0.013	-11.13	-0.012	-9.22
1994	-0.015	-12.03	-0.039	-9.46	-0.016	-13.20	-0.016	-13.19	-0.015	-12.0
1995	-0.012	-9.28	-0.028	-6.61	-0.012	-9.97	-0.012	-9.96	-0.012	-9.28
1996	-0.012	-9.77	-0.028	-6.55	-0.014	-11.50	-0.014	-11.49	-0.012	-9.77
1997	-0.012	-9.17	-0.029	-6.88	-0.012	-10.46	-0.012	-10.45	-0.012	-9.17
1998	-0.011	-8.93	-0.022	-5.26	-0.010	-8.77	-0.010	-8.77	-0.011	-8.93
1999	-0.007	-5.15	-0.014	-3.22	-0.007	-5.64	-0.007	-5.64	-0.007	-5.15
2000	Reference		Reference		Reference		Reference		Reference	
2001	--		--		-0.002	-0.89	-.000	-0.11	0.000	0.03
Number of observations	488374		140178		684061		684061		488374	
Mean dep.var.	0.106		0.177		0.102		0.102		0.106	

(1) Probit model. ME stands for marginal effect. Data are from the October survey of each year and include regional and marital status dummies. (2) Scaled by the mean educ =10.3 years.

Figure 13

EQUILIBRIUM WORKERS' DISTRIBUTION AMONG LABOUR MARKET STATUSES ESTIMATED ON THE BASIS OF 1999 TO 2000 TRANSITION MATRIX
(percentage points)



Source: Authors' calculations on Istat data.

Appendix 1
Fixed-term contract regulation, historical overview

The origins of fixed-term contracts

According to Italian labour law, open-end contracts have always been the rule. However, in the 1920s Italian legislation already provided for the possibility of setting a time limitation to labour contracts, the only condition being the existence of a "special relationship" between employer and employee26. This provision was then included in the 1942 Civil Code (art.2097). Originally, the adoption of contracts of limited duration implied strong differences in worker treatment: according to the private employment act of 1924 and, later on, to the first version of the 1942 Civil Code, fixed-term workers were not entitled to most of the rights an open-end worker usually had (e.g. holidays, seniority pay, Christmas bonus). In order to avoid the fraudulent use of these flexible contracts27, in 1962 specific legislation on fixed-term contracts (Law 230/1962) was introduced, which established a general ban on the adoption of fixed-term contracts except for a very specific list of circumstances, namely28: i) seasonal activities29; ii) temporary replacement of an employee on leave; iii) occasional activities which are time predetermined and not usually carried out by the firm; iv) special contracts, requiring skills that are not usually provided by the firm; v) top management. Therefore, instead of representing a valid alternative, the adoption of temporary contracts was only recognized as an eventual exception. This law was also very restrictive as far as the possibility of renewal was concerned: in particular, it established that fixed-term contracts could only be renewed once and for a

[26] See art.1, co.2, R.D.L. 1825/24 ("Il contratto di impiego privato può anche essere fatto con prefissione di termine; tuttavia saranno applicabili in tal caso le disposizioni del presente decreto che presuppongono il contratto a tempo indeterminato, quando l'aggiunzione del termine non risulti giustificata dalla specialità del rapporto ed apparisca invece fatta per eludere le disposizioni del decreto").

[27] In the 1950s there was a huge increase in temporary work, fostered by the increasing weakness of the trade unions. The phenomenon appeared increasingly unacceptable, given the strong economic growth Italy was experiencing.

[28] See Law 230/1962, art.1, co.1-2 ("Il contratto di lavoro si reputa a tempo indeterminato, salvo le eccezioni appresso indicate. E' consentita l'apposizione di un termine alla durata del contratto: a) quando ciò sia richiesto dalla speciale natura dell'attività lavorativa derivante dal carattere stagionale della medesima; b) quando l'assunzione abbia luogo per sostituire lavoratori assenti e per i quali sussiste il diritto alla conservazione del posto, semprechè nel contratto di lavoro a termine sia indicato il nome del lavoratore sostituito e la causa della sua sostituzione; c) quando l'assunzione abbia luogo per l'esecuzione di un'opera o di un servizio definiti e predeterminati nel tempo aventi carattere straordinario od occasionale; d) per le lavorazioni a fasi successive che richiedono maestranze diverse, per specializzazioni, da quelle normalmente impiegate e limitatamente alle fasi complementari od integrative per le quali non vi sia continuità di impiego nell'ambito dell'azienda; [….]") and art. 4 ("E' consentita la stipulazione di contratti di lavoro a tempo determinato purchè di durata non superiore a cinque anni, con i dirigenti amministrativi e tecnici, i quali possono, comunque, recedere da essi trascorso un triennio e osservata la disposizione dell'art. 2118 c.c.").

[29] In order to limit the area of application of this hypothesis, a decree was issued in 1963 providing a rigid list of activities which could be thought of as "seasonal" (e.g. agricultural activities, but also summer movie workers).

duration not exceeding the original one. In case of renewals exceeding by 30 days the original deadline, contracts were automatically converted into open-end ones. Unlike the previous regulations, it also extended to fixed-term workers some of the guarantees previously accorded only to permanent workers.

The regulatory evolution of fixed-term contracts during the 1970s and 1980s

The 1962 law has been the basic reference for temporary work regulation over the last forty years. Since from 1977, however, its original rigidity has been gradually smoothed through a series of regulatory measures, designed to progressively expanding the application area of fixed-term contract. The worsening condition of the Italian labour market led policy makers to partially abandon the traditional negative view on temporary work, which could represent a useful flexible tool for combatting rising unemployment. For example, compared to the original list of "exceptions" to open-end contracts provided by the first paragraph of the 1962 law, the possibility of hiring under fixed-term contracts in case of particular activity hikes was progressively extended to the tourist and commerce sectors (Law 876/1977) and later on to the remaining part of the economy (Law 79/1983). Besides, the increasing need for labour market flexibility led in 1984 to the introduction of special types of temporary contracts specifically designed to facilitate initial entry into the labour market (particularly for young people) and hence partially departing from the general rule in order to escape its rigidity. In particular, two different types of contract were introduced – the apprenticeship contract and the training-employment contract – both aimed at providing work experience together with professional training to young workers entering the labour market (16 to 24 years old in the case of apprenticeship contracts, 16 to 32 years old in the case of training-employment contracts). According to the law, the duration of these contracts can range from 18 months to 4 years, with different length and rules according to the workers' age and education. Unlike the original idea of fixed-term contracts, these contract models have been thought of as "stepping stones" towards permanent employment and thus enjoy a favourable tax treatment. In particular, firms hiring under these special contractual forms are rewarded with lower social security contributions, the magnitude of this reduction ranging from 25 per cent to 100 per cent according to the specific contract type, firm size, economic sector and geographical area.

The EU Directive on temporary contracts and the latest Italian reform

In 1999 the European Union issued a specific Directive on temporary work, aimed at facilitating the adoption of this contractual form across the Member States. After two years, in August 2001 Italy implemented the Directive through a legislative act, which represents the first actual reform of the existing regulation in 40 years. Indeed, for the first time the new regulation explicitly rejects the negative prejudice towards fixed-term contracts. In particular, it succeeds in overcoming the original principle according to which "if none of the listed exceptions apply", then "the contract has to be considered an open-end one". Through this reform the Italian system changes from one in which employers could hire under fixed-term contracts only if some very precise and limited circumstances applied to one in which the possibility of putting a limit on the duration of a contract is merely conditional upon the existence of "technical, productive, organizational and replacement reasons". At the same time, workers are guaranteed by the provision that these reasons must be explicitly stated by the employer (in writing). Therefore, the new regulation inverts the logic of the previous one in that the new decree specifically lists the situations in which a fixed-term contract cannot be adopted. Moreover, it delegates the task of establishing the quantitative limits to the collective bargaining process at sector level, even though it explicitly lists a number of cases which must be excluded from any limitation (e.g. fixed-term contracts signed during start-ups).

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