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New Technologies, Wages, and Worker Selection

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We study the effect of new technologies (NT) on wages and employment using a unique panel that matches data on individuals and on their firms. As in the United States, we show that computer users are better paid than nonusers (15%–20% more). But these workers were already better compensated before the introduction of the NTs. Total returns to computer use amount to 2%. Measurement errors do not affect our estimates. Furthermore, computer users are protected from job losses as long as bad business conditions do not last too long. This result holds even after controlling for possible selection biases.

I. Introduction

During the 1980s, labor market changes in the United States and in Western Europe were equally important. Employment increased, average real wages fell, and wage differentials widened in the United States both

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between and within age and education groups. While, in many European countries, average real wages increased, wage inequality did not change drastically, employment remained stable, but unemployment dramatically increased.

One explanation of these changes insists on the differences in structures and institutional characteristics between the labor markets in the United States and in Western Europe (see, for instance, Katz, Loveman, and Blanchflower 1995). Another explanation, favored by many labor economists, insists on the common shocks, in particular, skill-biased technical change and the growth in international trade. Evidence, summarized in Autor, Katz, and Krueger (1997), point to within-industry and within-firm growth in the demand for skilled workers that accelerated in the recent years both in the United States and in Western Europe. Of particular interest for us are the findings of Krueger (1993) who demonstrated that workers using computers were better paid than nonusers. However, Di Nardo and Pischke (1997) comparing cross sections in the United States and in Germany and Entorf and Kramarz (1997, in press) using longitudinal information for the year 1987 in France showed that these higher returns were in all likelihood explained by unobserved individual heterogeneity. The same conclusion, based on establishment-level data, may be drawn from Doms, Dunne, and Troske (1997) for the United States. In addition, Card, Kramarz, and Lemieux (1996), using simple descriptive evidence, have shown that trends in diffusion of new technologies (NT) as well as growth in trade have been very similar in Canada, France, and the United States. Therefore, despite very different labor market outcomes, the United States and Western Europe appear to function more similarly than previously thought.[1]

In this article, we examine the effect of new technologies on wages and employment in France. Our analysis takes advantage of a unique combination of data sets on workers and on their firms. In particular, individual information on the technologies used at work is available. These data sets being longitudinal, both workers and their firms can be followed from 1991 to 1993. Hence, issues that were difficult to address by Krueger (1993) or by Di Nardo and Pischke (1997) using only cross-sectional information on workers or by Doms, Dunne, and Troske (1997) using longitudinal establishment-level data together with a cross section of individual characteristics of the workers employed at those establish-

[1] See Abowd, Finer, and Kramarz (1996) and Abowd, Kramarz, and Margolis (in press), who show that, both in France and the United States, interindustry wage differentials and size wage effects are mostly explained by person unobserved heterogeneity and only mildly related to firm-fixed effects. Evidence on job and worker flows—cf. Abowd, Corbel, and Kramarz (1997) and Lane, Stevens, and Burgess (1997)—also display such similarities.

ments can be resolved herein. In addition, information on the employment status in June, September, and December 1993 for those workers interviewed in March of the same year for the new technologies survey allows us to provide the first direct evidence on the employment effects of computer use based on individual data.

Using the unique supplement to the 1993 French Labor Force Survey on New Technologies (including computers as well as many other techniques that were not within the scope of the surveys used by Krueger but were, at least for some of them, within the scope of the 1988 Survey of Manufacturing Technology [U.S. Department of Commerce 1993]), we show that NT workers are better paid than nonusers and, hence, we confirm Krueger's findings for the United States as well as Di Nardo and Pischke's for Germany and Entorf and Kramarz's for France during the mid-1980s. Using the longitudinal dimension of the Labor Force Survey, we demonstrate that NT workers were already better paid before working in these NT jobs. For computer users, wages increase with their experience with computers. The wage increases are maximal after 2 or 3 years. But, the total returns never exceed 2%, far from the cross-section estimates (15%–20%). Using the matched worker-firms data, we are able to show the stability of this estimate. After ruling out other potential explanations of these results, we show that these selection effects are strongest for the low-education workers. We evaluate the effect of measurement errors on our results, in particular, by comparing with independent data sources for France. Finally, using the quarterly supplement to our March 1993 Labor Force Survey for June, September, and December 1993, we provide the first direct evidence of the protection effect of computers on workers: computer users are indeed protected from unemployment in the short run, that is, as long as bad business conditions do not last too long.

The article is organized as follows. A simple statistical model is presented in Section II. Then, in Section III, we give a brief presentation of the main features of our data sets (the detailed description is contained in the data appendix). Section IV presents our estimates of the effect of NT on wages. We also discuss at length the potential pitfalls—measurement errors—of our analysis as well as possible interpretations of our results. Then, in Section V, we examine the job loss probability of computer users. A brief conclusion is given.

II. The Theoretical and Statistical Framework

We start by sketching the theoretical framework that can be used to motivate our analysis. Suppose that there are two types of workers, high-ability workers and low-ability workers (H and L, respectively).[2] The employing firm possesses no computer (or any sort of NT) at date $t = 0$. At this date, the firm buys computers, but not every worker will be endowed with a machine. Financial constraints, different expected returns for high- and low-ability types of workers can justify this assumption. The first question therefore must be who will receive these machines at date $t = 1$. The second question is the effect on compensation of using these machines at date $t = 1$ as well as at later dates. A model in which ability is complement to computer use—that is, such that productivity differentials between high-ability and low-ability workers are magnified by computer use—will predict that high-ability workers will receive the machines. In addition, wages for computer users should change across time, the exact pattern of these changes depending on the shape of the productivity increases at each date $t = 1, 2, \ldots$.

In our statistical model, we try to capture these two possible effects. First, if workers who use computers are selected by firms because they are of higher quality (i.e., they have unobservable characteristics that are associated with higher compensation) and if the increase in productivity at the implementation date is small, computer use should generate no or little payoffs in the year that follows computer implementation. Second, productivity increases after some time of computer use should generate increasing returns to experience with computers. The peak in these returns should be compared to the level of wage differentials between computer users and nonusers observed in the cross-sectional dimension. If those levels differ widely, one may conclude that selection effects are more important than productivity effects.

Let us assume that a latent variable, q_{it}, represents the quality of worker i at date t (we allow this quality to time vary through an unspecified learning process). This quality is fully related to the productivity of the workers, hence their wage.

In econometric terms, the allocation process of workers to tasks can be modeled as

$$\begin{aligned} \text{Comp}_{it} &= 1 \quad \text{if } q_{it} > q_0, \\ \text{Comp}_{it} &= 0 \quad \text{otherwise,} \end{aligned} \tag{1}$$

where $q_{it} > q_o$ denotes that the firm evaluates worker i at date t as having sufficient quality to use a computer and where $\text{Comp}_{it} = 1$ denotes that worker i uses a computer at date t.

[2] We define a high-ability worker as a high-wage worker, i.e., a worker who receives a higher compensation as predicted on the basis of his or her observed characteristics.

In this situation, a first wage equation to consider is

$$\ln w_{it} = \alpha \mathrm{Comp}_{it} + X_{it}\beta + \varepsilon_{it}, \tag{2}$$

where X_{it} are time-varying observables for worker i, where ε_{it} is the error term. In the empirical work, we estimate equation (2) both in the cross-section dimension and with individual-fixed effects. To incorporate the potentially increasing productivity of computer users, X_{it} may include the number of years of experience with computers of individual i at date t. To assess firm-specific compensation policies, we add to the previous equations firm-fixed effects. Hence, our final estimated equation is

$$\ln w_{it} = \alpha \mathrm{Comp}_{it} + X_{it}\beta + \sum_k 1_i(k) e_k + \sum_l 1_{J(i,t)}(l) f_l + \varepsilon_{it}, \tag{3}$$

where $1_i(\cdot)$ is an indicator for individual i, and $1_{J(i,\,t)}(\cdot)$ is an indicator for firm $J(i, t) = j$ at which individual i is employed at date t.

Next, to delve further into the selection effects of new technologies, we examine the effect of computer use on job losses. In particular, if computer use implied some training costs that the firm has to recoup or, alternatively, if the mere use of the computer has improved worker i quality, computer users will be protected from unemployment relative to nonusers.

If we denote $\mathrm{unemp}_{it} = 1$ the fact that worker i is unemployed at date t (after t_0), we estimate the following equation:

$$\Pr[\mathrm{unemp}_{it} = 1 | e_{it_0} = 1] = \Phi(\alpha \mathrm{Comp}_{it_0} + X_{it_0}\beta), \tag{4}$$

where X_{it_0} are observables for worker i, where $e_{it_0} = 1$ denotes that worker i was employed at date t_0, and where Φ denotes the probit function (standard normal cumulative density function [c.d.f.]).

To control for selection effects in computer use, we also jointly estimate equation (1)—the computer-use equation also modeled with a probit function—with equation (4)—the mobility equation.[3]

Another way of controlling this selection bias would be to use a measure of unobserved personal heterogeneity as an explanatory variable in equation (4). One such measure comes from wage equation (3). Once estimated, this regression provides an estimate of the individual-fixed effect (e_i in eq. [3]), $\hat{e}_i$. This measure can be introduced in equation (4) as a control for the unobserved, but compensated, personal heterogeneity. This gives the following equation:

[3] The exclusion restrictions will be specified in the results section.

$$\Pr[\text{unemp}_{it} = 1 | e_{it_0} = 1] = \Phi(\alpha \text{Comp}_{it_0} + X_{it_0}\beta + \gamma \hat{e}_i). \quad (5)$$

III. The Data: Panels of Workers and Their Firms

The data used in this article come from four sources: the Enquête Emploi, 1991–93, the French household-based Labor Force Survey; the Enquête sur la Technique et l'Organisation du Travail auprès des Travailleurs Occupés (TOTTO), the 1993 supplement to the Labor Force Survey that asked questions about the diffusion of new technologies and the organization of the workplace; the Enquête Emploi Trimestrielle for 1993, the quarterly Labor Force Survey; and, finally, the Déclarations de Mouvements de Main d'Oeuvre (DMMO), an establishment-based survey on hiring and separations. Besides the usual questions in household labor force surveys (salary, tenure, education, age, etc.), the survey appendix contains a rich source of information on the use (e.g., intensity, experience, required training) of well-specified groups of new technologies: microcomputers, computer terminals, robots, numerical command machines, video, laser, computerized measurement instruments, computerized medical instruments, telecommunication NT.

Furthermore, the employing establishments and firms in both individual-based data sets, Enquête emploi and TOTTO, can be identified by using the standardized SIRET (establishment) or SIREN (enterprise) identification numbers. Hence, we can match the DMMO with TOTTO and the Enquête emploi for a subsample of our workers. This feature of the French National Statistical Institute (INSEE) classification system also enables us to follow the workers across firms in the 3 years of our panel. Hence, our data set resembles the Déclarations Annuelles de Salaires (DAS) data set used by Abowd, Kramarz, and Margolis (in press). However, the Labor Force Survey sampling ratio being equal to 1/1,000, the survey contains approximately 18,000 workers (as opposed to 1,000,000 in Abowd, Kramarz, and Margolis [in press]). On the other hand, more individual variables are available in the Labor Force Survey than in the DAS.

The French Labor Force Survey is a rotating panel: every year, a third of the sample exits. In addition to the questions included in the survey, the exiting one-third is given a special supplement. In 1993,[4] this supplement was on NT. Thus, it is possible to build a longitudinal data set providing information about individuals, their jobs, their firms, and the technologies used at work in 1991, 1992, and 1993 using the labor force surveys and some retrospective questions, such as the date of first utilization of computers, in the 1993 TOTTO survey.

[4] In 1987, it was also on NT with a slightly different questionnaire, see Entorf and Kramarz (1997, in press).

The longitudinal sample, where 9,345 individual workers are followed at the most 3 years, has 27,893 observations. People who, both in 1991 and 1992, were not employed, because they were out of the labor force (at home, at school, at the army), unemployed, or self-employed, were excluded from the longitudinal sample. Furthermore, since the sampling framework in the Labor Force Survey is based on domiciles and not on individuals, people who moved between 1991 and 1993 are also excluded, as are the few people who were absent from home during the 1991 and 1992 surveys or who refused to answer. We compared the structure of our longitudinal sample to the complete TOTTO survey. The differences between the two are quite small. Of the 9,345 individuals that were interviewed in March 1993, 8,288 are also followed by the quarterly Labor Force Survey in June, September, and December of the same year. Information is restricted to the employment status at each of these three dates. Finally, 1,299 individuals in 1993 and 3,647 observations in the longitudinal dimension are both in the above panel of workers and in the DMMO data source. Thus, hiring, quits, terminations, end of short-term contracts, and other flow variables in the employing establishment are available for these observations. Because of the structure of the DMMO, which includes establishments with 50 or more employees in the private sector, many Labor Force Survey observations have no DMMO information. However, we will try to use this DMMO information to examine alternative explanations of our results, in particular, to control for economic conditions at the establishment level.

The list of NTs included in the questionnaire is rather long (see table A1). To keep the analysis tractable and to have a sufficient number of workers per NT, we had to group the NTs into categories. In these categories, information technologies are separated from production technologies (especially manufacturing technologies). Among information technologies, we distinguish computers from communication NT. Among production technologies, we distinguish manufacturing technologies from a more heterogeneous group of NTs including video, laser, and some computerized instruments.

Thus, the first group includes microcomputers and terminals linked to a mainframe. At least in principle, these machines are not dedicated to a particular task but are multipurpose instruments. Even if computers are primarily used for administrative work, they are now also widely used by technicians or blue-collar workers in manufacturing (see table 1). The second group contains two communications NT: fax machines and the "minitel," the French videotext, used to access various databases. The third group of manufacturing technologies includes robots and numerical command (NC) machines. Robots automatically perform some tasks such as industrial handling or painting. The NC machines are the automatic, flexible, and complex version of mechanical machine tools. These two

Table 1
Proportion of NT Users in 1993 among Workers in the Longitudinal Sample (N = 9,345)

	Percent of Workers Using:			
Categories of Workers	Computers	Robots, NC	Communication NT	Video, Laser, Measurement Instruments
Female	45.6	1.7	45.9	16.3
Male	35.0	7.5	36.9	21.6
Education degree:				
No diploma	13.6	6.9	14.7	8.0
Elementary school	22.9	5.0	24.5	9.7
Junior high school	61.9	2.6	61.9	17.0
Vocational junior high school	37.5	6.1	39.6	16.6
Vocational high school	65.5	3.2	66.4	26.2
High school	68.8	1.7	67.5	23.7
Technical college or undergraduate college	57.4	2.5	57.4	39.6
Graduate College	62.9	1.1	63.5	43.2
Married	40.7	4.7	40.3	19.4
Single, divorced, widowed	38.4	4.7	41.5	18.2
Lives in Ile de France	49.8	2.8	53.3	17.9
Lives in another region	37.5	5.2	38.0	19.4
Experience:				
Less than 5 years	55.4	6.5	56.1	20.9
5–20 years	45.1	5.0	46.0	20.9
More than 20 years	36.3	4.5	37.6	17.8
Tenure:				
Less than 3 years	32.6	4.7	36.2	15.1
3–6 years	37.0	5.0	39.6	17.4
7–14 years	39.0	4.3	40.1	18.3
More than 14 years	44.5	4.9	44.2	21.6
Occupation:				
Manager	67.4	1.1	72.7	37.2
Middle-level white collar	56.7	3.6	59.2	31.6
Lower-level white collar	47.7	0.8	49.5	10.8
Skilled blue collar	15.0	11.1	13.2	14.1
Unskilled blue collar	6.4	8.2	4.5	5.6
Sector, status, and size of the firm:				
Food industries	52.5	6.0	42.1	20.8
Equipment goods industries	39.9	15.0	34.6	19.1
Telecom and transport industries	53.2	1.2	57.6	14.3
National or local government	40.9	1.5	41.7	25.3
Public firm	61.5	2.4	60.8	19.1
Private firm, over 500 employees*	46.1	6.6	42.1	19.8
Private firm, 50–499 employees	34.1	7.3	36.3	16.0
Private firm, less than 50 employees	30.2	5.4	38.0	12.0
Part-time worker	34.2	1.0	32.6	11.4
Full-time worker	40.8	5.2	42.3	20.1

Source.—Enquête Emploi, 1993.
Note.—NT = new technologies; NC = numerical command.
* Include firms with unknown size.

types of machines may include computers, but they are dedicated to very specific industrial tasks. They are used mostly by blue-collar workers (see table 1). The fourth, and last, group of NT includes video-based techniques, laser-based instruments and machines, computerized measurement instruments, and computerized medical instruments.

Table 2 shows that communication NTs are the most popular: 41.2% of the individuals in our sample declare that they use at least one of them. Computers are almost as popular—they are used by 39.6% of the sampled individuals. In comparison, manufacturing NT are less common.

Table 1 shows NT use by sex, education, experience, tenure groups, occupation, sector (for a subsample of industries), status and size of the firm. In particular, it appears that women tend to use computers and communications NTs more heavily than men whereas manufacturing NT are more frequently used by men. Furthermore, computer use is positively related to general education (as opposed to technical or vocational education). This is most apparent when one compares computer use for workers with a junior high school diploma (61.9%) to workers with a vocational junior high school diploma (37.5%). More generally, educated workers use communication NT more but not video, laser, instruments (our fourth group). Hence, the structure of the French education system, split between general and vocational curriculae, has some effect on the way workers are allocated to machines.[5] Note also that computer use is more prevalent for younger workers as well as for workers with more tenure; as a consequence, frequent firm changes act against computer use.

The survey not only reports NT use but also gives the date of the worker's first use of this type of equipment. However, this information is only available for computers and manufacturing NT (robots and NC). Hence, for these two groups of NT, we are able to compute a measure of experience with each of these two NT for every individual. This piece of information allows us to build a dynamic measure of NT use. Suppose that a worker declares 1 year of experience with computers in 1993, then 1992 is the first year the worker ever used this equipment and, therefore, in 1991, this employee was not a computer user. Hence, between 1991 and 1992, the computer-use indicator in the wage regression changes from zero to one. Furthermore, between 1992 and 1993, experience with computers also changes from zero to one. Now, consider another individual who answered "no" to the question on computer use at the TOTTO survey in 1993. In both years 1991 and 1992, this individual is counted as not using a computer. Because of the sample design, an individual in our longitudinal sample can change from non-NT user to

[5] Notice the rather large proportion of workers with no education in table 2. The French education system indeed generated many dropouts.

Table 2
Descriptive Statistics for 1993 for Workers in the Longitudinal Sample (*N* = 9,345)

Variable	Mean	Standard Deviation
Logarithm of monthly wage (in French francs)	8.936	.490
Gender (= 1 for male)	.525	
Marital status (= 1 for married)	.717	
Education degree:		
No diploma	.188	
Elementary school	.135	
Junior high school	.075	
Vocational junior high school	.320	
Vocational high school	.075	
High school	.050	
Technical college or undergraduate college	.082	
Graduate college	.077	
Region (= 1 for living in Ile de France)	.208	
Experience (in years)	23.780	10.588
Use of NT and employment status:		
Uses computers, fixed-duration job contract	.005	
Uses computers, indefinite-duration job contract	.396	
Uses robots or NC	.047	
Uses communication NT	.412	
Uses video, laser, computerized measurement instrument	.191	
Uses no NT, fixed-duration job contract	.005	
Tenure in using computers (0 for nonusers)	2.085	3.591
Tenure in using computers (computer users only)	5.208	3.994
Tenure in using robots or NC (0 for nonusers)	.254	1.529
Tenure in using robots or NC (users only)	5.379	4.690
Tenure (in years)	13.003	9.236
Occupation:		
Manager	.104	
Middle-level white collar	.254	
Lower-level white collar	.303	
Skilled blue collar	.234	
Unskilled blue collar	.105	
Sector, status, and size of the firm:		
Food industries	.034	
Equipment goods industries	.158	
Telecom and transport industries	.186	
National or local government	.295	
Public firm	.057	
Private firm, over 500 employees*	.267	
Private firm, 50–499 employees	.164	
Private firm, less than 50 employees	.217	
Part-time worker	.118	

SOURCE.—Enquête Emploi 1993.
NOTE.—NT = new technologies; NC = numerical command.
* Include firms with unknown size.

NT user but not the reverse. Errors induced by this type of mismeasurement will be discussed at length in the next section.

Table 3 presents some statistics for the subsample of workers who are both in the basic longitudinal sample and in the quarterly Labor Force Survey (8,288 individuals of the original 9,345). All reported (and non-

Table 3
Descriptive Statistics (Sample Matched with Quarterly LFS), N = 8,288

Variable	Mean	Standard Deviation
Uses a computer (long-term contract)	.380	
Uses a computer (short-term contract)	.010	
Does not use a computer (short-term contract)	.003	
Experience with computer	1.982	3.555
Uses a robot or an NC machine	.048	
Experience with a robot or an NC machine	.245	1.499
Uses video or laser	.194	
Uses fax or minitel	.405	
Employed in March and not employed in June 1993	.012	
Employed in March and not employed in September 1993	.021	
Employed in March and not employed in December 1993	.028	

SOURCES.—Enquête Emploi, 1993; Enquête Emploi Trimestrielle, 1993.
NOTE.—LFS = Labor Force Survey.

reported) statistics on NT use are virtually identical to those reported for the whole sample. We also give the proportion of workers who became nonemployed at one of the quarterly surveys—June, September, or December 1993—while all were employed in March of the same year.

IV. NT Workers and Wages

A. The Results

Table 4 presents our estimation results in the cross-section dimension. In column 1, we present a close comparison to Krueger (1993): the 1993 log wage is explained by education, experience, sex and marital status (interacted), region of residence, a part-time indicator, tenure, and a computer-use indicator.[6] In column 4, we present a close comparison to Di Nardo and Pischke (1997): these authors added indicators for the use of calculator, telephone, and pencils as well as an indicator for sitting while working to Krueger's specification, so we add here a fax-use indicator, a minitel-use indicator, two very simple techniques requiring no particular skills, as well as a robot-use indicator and a laser-use indicator to the set of explanatory variables of column 1. While in columns 2 and 3, we present estimates using all the available information. In particular, we decompose returns to computer and robots into a constant part and a part related to experience. In addition, estimates in

[6] Tenure is not included in Krueger's regression. On the other hand, it includes both union status and race indicators. There is no equivalent of the first variable in the data, but, in France, jobs of union workers are identical to those of nonunion workers, even within firms. The introduction of citizenship indicator, instead of race, did not change the results.

Table 4
The Effect of New Technologies on Pay: Cross-Section Results, N = 9,345

	Cross Section				
Dependent Variable, ln(Monthly Wage)	Krueger (1993) (1)	(2)	Firm-Fixed Effects (3)	Di Nardo and Pischke (1997) (4)	Krueger (1993) (5)
Uses a computer (yes = 1)	.1824	.0700	.0809	.0979	.1612
	(.0076)	(.0128)	(.0143)	(.0089)	(.0088)
Uses a robot (yes = 1)	. . .	.0197	−.0171	.0249	. . .
		(.0306)	(.0344)	(.0162)	
Uses fax or minitel (yes = 1)	. . .	.0804	.1359	. . .	. . .
		(.0084)	(.0091)		
Uses fax (yes = 1)	. . .	. . .	. . .	.1204	. . .
				(.0093)	
Uses minitel (yes = 1)				.0470	
				(.0091)	
Uses video or laser (yes = 1)		.0197	.0436	.0711	
		(.0084)	(.0095)	(.0090)	
Experience with computer		−.0013	−.0010	. . .	
		(.0033)	(.0038)		
Experience with computer^2		.0001	.0002	. . .	
		(.0002)	(.0002)		
Experience with robot		.0057	.0131	. . .	
		(.0090)	(.0102)		
Experience with robot^2		−.0004	−.0009	. . .	
		(.0004)	(.0005)		
Tenure	.0106	.0119	.0112	.0110	.0161
	(.0012)	(.0011)	(.0012)	(.0012)	(.0014)
Tenure^2	−.0006	−.0014	−.0009	−.0008	−.0015
	(.0004)	(.0003)	(.0004)	(.0003)	(.0004)
Experience	.0163	.0108	.0155	.0155	.0148
	(.0015)	(.0013)	(.0014)	(.0015)	(.0016)
Experience^2	−.0022	−.0017	−.0024	−.0021	.0022
	(.0003)	(.0002)	(.0003)	(.0003)	(.0003)
R^2	.5479	.6350	.6384	.5634	.4954

Source.—Enquête Emploi, 1991–93. Columns 1–4 use 1993 data; col. 5 uses 1991 data.
Note.—Standard errors in parentheses. Models 1, 4, and 5 also include years of education (and square), a part-time effect, a sex effect, a married effect, a married female effect, a region effect (= 1 for Ile de France), and size of firms and government agencies (five indicators). Models 2 and 3 also include regional effect (= 1 for Ile de France), part-time effect, size of firm effects, government agencies effects, sex effect, eight education effects, short-term contract effect (interacted with computer use), five occupation effects, and 14 sector effects. Finally, model 3 also includes 1,016 firm effects.

column 3 include more than 1,000 firm-fixed effects. Finally, in column 5, we present estimates of the Krueger equation using 1991 data instead of 1993 data as in column 1. These final estimates will be used in our measurement errors subsection.

First, similar to Krueger or Di Nardo and Pischke, we find that computer users are better compensated than nonusers. The coefficient, 0.1824, falls exactly within the range given by the latter authors for the United States and is slightly larger than the one given for Germany. Looking at the french "pencils," the fax and the minitel (on-line Yellow Pages), we find that minitel and, more importantly, fax users are better compensated than nonusers; a first indication that unobserved heterogeneity matters and that the wage differential associated to computer use does not reflect true returns (see also Gollac 1993). Furthermore, the computer coefficient decreases by one-half when other NTs are included (compare col. 1 with other columns). When these returns to computers are decomposed into a constant and a quadratic function of experience with computer, returns to experience are not significantly different from zero.[7]

Table 5 presents our estimates of the basic longitudinal equation,

$$\ln w_{it} = \alpha \mathrm{Comp}_{it} + X_{it}\beta + \sum_k 1_i(k) e_k + \varepsilon_{it},$$

where X_{it} are time-varying observables for worker i, where $1_i(\cdot)$ is an indicator for individual i, and where ε_{it} is the error term. The first column presents the results of the first specification while the second column also includes firm-fixed effects (eq. [4]). Both give a common answer: when workers change their status from non-NT user to NT user, their wage does not immediately increase. Furthermore, returns to experience with computers are not zero. The coefficients of the quadratic function given in table 5 do not seem to display a clear pattern. So, we replaced this quadratic function with indicator functions for each year of experience. Results—not reported here—are imprecisely estimated but show that returns are maximal—2%—after 1–3 years of experience with computers. After 3 years, they become nonsignificantly different from zero.

The introduction of firm-fixed effects has no effect on the estimated coefficients.[8] This is consistent with Abowd, Kramarz, and Margolis's (in press) findings for France as well as those of Abowd, Finer, and Kramarz (1996) for the United States: firm compensation policies (as captured by the firm-fixed effects) are not highly correlated with individual observables and individual-fixed effects. The introduction of these firm-fixed

[7] Estimation with indicator functions for each year of experience—not reported here— displays the same flat pattern in this cross-section dimension.

[8] As indicated in Abowd, Kramarz, and Margolis (in press), such firm-fixed effects can only be separately identified from worker-fixed effects when at least one worker in the firm quits for another firm in the sample. Here, we are able to identify 494 of the 1,016 firm dummies.

Table 5
The Effect of New Technologies on Pay: Longitudinal Results

Dependent Variable, ln(Monthly Wage)	Individual-Fixed Effects (5)	Individual-Fixed Effects with Firm-Fixed Effects (6)
Uses a computer (yes = 1)	.0105	.0112
	(.0082)	(.0084)
Uses a robot (yes = 1)	.0423	.0368
	(.0222)	(.0225)
Experience with computer	.0047	.0034
	(.0045)	(.0046)
Experience with computer^2	−.0006	−.0006
	(.0003)	(.0003)
Experience with robot	−.0132	−.0132
	(.0116)	(.0116)
Experience with robot^2	.0002	.0001
	(.0008)	(.0008)
Tenure	.0039	.0035
	(.0010)	(.0011)
Tenure^2	−.0007	−.0006
	(.0003)	(.0003)
Experience	.0468	.0502
	(.0047)	(.0048)
Experience^2	−.0025	−.0026
	(.0006)	(.0007)
R^2	.9126	.9160

Source.—Enquête Emploi, 1991–93.
Note.—N = 27,893 (both models). Standard errors in parentheses. Models 5 and 6 include an indicator for year 1991, size of firm effects, government agencies and firms effects, short-term contract effect (interacted with computer use), five occupation effects, 14 sector effects, experience (and square), seniority (and square), and 9,344 individual effects. Model 6 also includes 1,045 firm effects of which 494 are identified.

effects have, as observed in Abowd, Kramarz, and Margolis (in press), little effect on the R^2 of the regressions (corrected for the degrees of freedom). In fact, most of the increase in R^2 comes from the introduction of individual dummies. The numbers we obtain using our 3 years of data (91% of the variance in wages) are also consistent with what was obtained on a different data set with many more individuals (1,000,000) and years (12) but many less individual variables (81% of the variance was explained in the Abowd, Kramarz, and Margolis's analysis).

The introduction of indicator variables for each year of experience into equation (4), that is, with both person- and firm-fixed effects yield the same outcome as without firm-fixed effects; the maximal total returns equal 2%. In one of the few studies on the evolution of wage inequality based on matched individual-firm information, Kramarz, Lollivier, and Pelé (1996) demonstrated that within-firm wage inequality increased between 1986 and 1992 in France (see Davis and Haltiwanger [1991] for the United States). However, only a minor part of the increase could be explained using conventional firm-level variables. Our analysis, which incorporates firm-fixed effects, shows that the introduction of NT is partly responsible for this within-firm widening of wage differentials.

In contrast to the cross-section estimates, the introduction of robots seem to have a small direct effect on wages in the longitudinal dimension (4%, only marginally significant). Notice however that it disappears when firm-fixed effects are introduced, which means that robots are used in relatively high-paying firms (see also col. 3 in table 4 with similar results in the cross-section dimension).

Table 6 presents estimates of variants of the basic equation. The first two columns examine the interaction of computer use and education. The first column gives the cross-section estimates, the second column shows the individual-fixed effects estimates. The last four columns present robustness checks of our previous estimates of the longitudinal equation. The third column is identical to column 5 of table 5 except that occupation indicators are not included. Since information on experience with computers is restricted to the year of first use (no data on the month within the year), computation of experience with computers is subject to a 1 year uncertainty. Hence, column 4 of table 6 examines the effect of a change in the measure of experience with computers, we substract 1 year to the previous measure, on this basic equation (col. 5 of table 5). In the fifth column, the same basic equation is estimated, however the sample excludes all workers who declared a value for their experience with computers equal to their firm-specific tenure. Finally, the last column reestimates the same basic model but distinguishes between two types of computers, "microcomputers" and "terminals connected to a mainframe."

If we turn to the first two columns where the effects of education are analyzed, it appears that the premium associated with computer use goes to low-education workers (the premium is approximately equal to 10% in the cross section). It is not significantly different from zero for the two other groups of education. However, the second column supports results of table 5; in the longitudinal dimension, the premium disappears. Other robustness checks confirm this conclusion. First, without occupation indicators, our results are virtually identical to those of table 5. When the measure of tenure is translated by 1 year to reflect the fact that the survey took place in March 1993 (col. 4), we find a small and significant effect—1.5%—that corresponds to estimates mentioned previously with indicators for each year of experience with computer. Accordingly, maximal return for computer use—2%—comes after 2 years of experience.[9]

Cross tabulations reveal that approximately 20% of workers report an

[9] These results are based on nonreported regressions.

Table 6
The Effect of New Technologies on Pay (Variants)

Dependent Variable, ln(Monthly Wage)	Cross Section by Education (1)	Individual-Fixed Effects by Education (2)	Individual-Fixed Effects without Occupation Dummies (3)	Individual-Fixed Effects and Experience with Computer Minus 1 (4)	Individual-Fixed Effects Excluding Workers with Experience with Computer Equal to Tenure (5)	Individual-Fixed Effects with a Distinction for Microcomputer and Terminal (6)
Uses a computer	...	...	.0111 (.0082)	.0146 (.0074)	.0072 (.0090)	...
Uses a computer (low education)	.0937 (.0137)	.0069 (.0100)	...	...	...	...
Uses a computer (middle education)	.0104 (.0216)	.0210 (.0157)	...	...	...	...
Uses a computer (high education)	.0316 (.0191)	.0111 (.0141)	...	...	...	...
Uses a microcomputer	...	...	...	...	...	.0162 (.0088)
Uses a terminal	...	...	...	...	...	−.0053 (.0128)
Experience with computer	−.0009 (.0033)	.0048 (.0045)	.0046 (.0045)	.0030 (.0042)	.0059 (.0051)	.0045 (.0045)
Experience with computer^2	.0001 (.0002)	−.0006 (.0003)	−.0006 (.0003)	−.0006 (.0003)	−.0007 (.0004)	−.0006 (.0003)
Uses a robot	.0206 (.0306)	.0429 (.0222)	.0408 (.0223)	.0290 (.0193)	.0215 (.0249)	.0412 (.0223)
Experience with robot	.0054 (.0090)	−.0131 (.0116)	−.0133 (.0116)	−.0110 (.0107)	−.0161 (.0118)	−.0132 (.0116)
Experience with robot^2	−.0004 (.0005)	.0002 (.0008)	.0001 (.0008)	.0001 (.0008)	.0003 (.0008)	.0001 (.0008)
R^2	.6341	.9126	.9123	.9123	.9168	.9123

SOURCE.—Enquête Emploi, 1991–93.
NOTE.—Standard errors are in parentheses; col. 1 is estimated using 9,345 observations; col. 5 is estimated using 22,357 observations; all other columns use 27,893 observations.

experience with computer equal to their firm-specific tenure.[10] If many individuals erroneously report the date of entry in their new firm as the date of first use of a computer, the simultaneity of these two changes may affect our estimated returns to computer use and may make it difficult to separately identify an increase in wage because of a change of firm from an increase in wage because of the introduction of a new machine. Hence, column 5 presents estimates of the same equation based on a sample from which all such workers (1,861 individuals) have been removed. Once more, results remain identical. Finally, distinguishing between microcomputers and terminal (connected to a mainframe) is relevant since the coefficient for microcomputers is marginally significant—microcomputer use induces a wage increase of roughly 2%—while terminal use has no effect on wage.

In the same statistical spirit, we reestimated most of our equations correcting for possible heteroskedasticity bias (White 1980). This correction is slightly complicated here by the panel dimension: the matrix to use is block-diagonal (one block correspond to two or three observations for the same individual) instead of being diagonal as in the usual case. All standard errors are identical or virtually identical to those given in our tables and not reported here for this reason.

Finally, we also estimated the model in first difference, restricting our analysis to observations in which individuals changed their status from nonusers to computer users under our sample period, that is, between March 1991 and March 1993.[11] Once more, the immediate returns to computer use are zero while total returns are at most 2% (only marginally significant).

B. A Discussion of the Results: Measurement Errors and Other Interpretations

Our estimates show that, in a longitudinal regression, the coefficient of the computer-use indicator is much smaller than in the cross section. A first explanation could be the small number of workers changing status from nonuser to user under the sample period. This issue is examined in the next paragraph. A second concern could be measurement error coming from the retrospective nature of our data. Indeed, measurement error would imply a bias toward zero of the estimated coefficient. This issue is examined in most of the remaining part of this section. The final paragraphs of this subsection are devoted to other potential interpretations of our results.

[10] Tenure is expressed in months for the first 3 years. Equality between the two measures has to account for this fact and, therefore, implies rounding.

[11] This regression—not reported here—was suggested by a referee.

One hundred and eighteen individuals declared 1993 as their date of first use (i.e., they started between January 1 and March 31), while 455 declared 1992, and 443 declared 1991 for this same date of first use. Under reasonable stationarity assumption, ¼ of those declaring 1991 started before the date of the 1991 survey. This yields an estimate of 905 (= 1,016 − 111) individuals who changed states from nonusers to computer users while 117 workers became robot users. These numbers are equivalent to those obtained for the period 1985–87 (Entorf and Kramarz 1997, in press). In addition, as was already shown in these two articles, the new users do not differ from the rest of the population, they are only slightly younger (3 years younger) and therefore less experienced than the whole population. So, it appears that there is a sufficient number of workers changing status from nonusers to users in order to identify the effects of computer use on wage.

The number of starters in each year is computed using the date of first use of computer, a piece of information that plays a crucial role in our analysis. This date of first use is, by nature, retrospective information. Any measurement error in this variable would be exacerbated by using a fixed-effect estimator. In Entorf, Gollac, and Kramarz (1997), we discussed at length potential sources of measurement errors. In the same paper, we also compared results from this data source with the 1994 French version of a European survey on computer use labelled TOTTO-Europe as well as with the two previous surveys on computer use from 1987 and 1991. We can summarize the results of this comparison as follows.

A first difficulty could come from the questionnaire itself. In the TOTTO supplement to the Labor Force Survey, the question on the date of first use is asked separately for microcomputers and terminals. We used the earlier of these two dates. The question is formulated as follows: "You, personally, when did you first use this machine?" The instructions that were given to the interviewers when asking this question are as follows:[12] "For the microcomputer or the terminal, the question refers to the first use of a machine of this type. Indeed, . . . software is more important than the machine itself . . . . This date may well be earlier than the date of entry in the firm at which the worker is presently employed." By contrast, the French version of the 1994 TOTTO-Europe[13] contained the following question: "You, personally, when did you first use a computer?" This phrasing eliminates potential ambiguities of the previous question. The TOTTO-Europe also contained a question on previous use

[12] Notice that the interview took place at the domicile of the interviewee and not by telephone.

[13] See the data appendix for a brief description of this survey.

of computers for those who did not use one at the date of the survey. Using all these different bits of information gives a very consistent picture. First, a rough estimation of the rate of change of computer users on the period is roughly equal to 10%. Second, the annual rate of computer quits (i.e., from users to nonusers, which we do not capture in the Labor Force Survey) is slightly inferior to 2%. This is only one-fifth of the rate of change in computer use. Hence, unless the wage loss when stopping to use computers is very asymmetric (either much larger or much lower than the premium at entry), which is very unlikely given all the available evidence, this result shows that our previous estimates are at most mildly biased by the absence of information on computer quits. Third, a comparison of the number of workers who declare that the date of first use is identical to their entry in the firm shows some overrepresentation in the 1993 survey, 23%, against 17% in TOTTO-Europe. But, our variants of table 6 have shown that this has no effect on our estimates.

Indeed, all numbers seem to add up and confirm the overall quality of the answers to this question. We now estimate the effect of this precise type of measurement error—a small fraction of workers declare the wrong date of first use—on our estimated returns to computer use.

To understand our approach, consider the following measurement error equation:

$$\mathrm{Comp}_{it} = \mathrm{Comp}^{*}_{it} + \upsilon_{it},$$

where Comp^{*}_{it} denotes true computer use while Comp_{it} is the measured computer use. Assume also that the true and the measured variables are equal at the date of the survey (so that there is only retrospective error). Then, at the date of the survey, the cross-section coefficient of the computer-use variable should only be biased by the presence of unobserved worker characteristics (that are eliminated in the fixed-effect estimation). But, at any previous date, the bias in the cross-section estimates of the same equation should stem from two sources: first, the same unobserved heterogeneity problem and, second, and crucial for us, the bias coming from the measurement error in the date of first-use variable. Notice that this last bias should be negative. So, assuming that the covariance between computer use (conditional on all other observed covariates) and unobserved heterogeneity is constant across periods (for us, between 1991 and 1993), a simple comparison of the two cross-section estimates (1991 and 1993) should help us assess the magnitude of the measurement error bias. Results for 1991, in which computer use is computed from the date of first use reported in 1993, are presented in the last column of table 4. The coefficient of the computer-use indicator is equal to 0.1612, slightly lower than the one obtained in 1993 (0.1824). These coefficients are only slightly different. As predicted from the above discussion, the 1991 coefficient is smaller than the 1993 one, but the difference is very small and only marginally significant given the respective standard errors on the coefficients. This analysis confirms the previous conclusion that measurement errors appear to be a minor concern and do not invalidate our results.

Now, it is obvious that the model presented in the second section is not the only potential explanation of the results described in Section IV. More specifically, a first line of reasoning could go as follows. Suppose that there are two types of firms, for instance, firms with high rents and firms with low rents. Suppose now that workers are able to capture some of these rents, partly as wages, partly as computers. Then, we might observe the type of cross-section results that were found previously: NT users are better paid than non-NT users. An equivalent situation could emerge from an efficiency wage story: high-tech firms prefer a lower turnover rate and, therefore, pay higher wages than low-tech firms. Of course, high-tech firms use more computers than low-tech firms. In both situations, a spurious (positive) correlation would exist in any cross-section equation that we have presented in table 4.

To evaluate these potential explanations of the high wage of computer users, we pursue the following strategy. We take equation (2) and add a measure of the firm- or establishment-specific economic situation (for the first story) or the turnover rate (for the second story) in the same year. If any of the alternative explanations given above is correct, then the spurious correlation between computer use and wage should disappear.

Remember that, for most workers, we have the employing establishment identifier (the so-called SIRET number). Hence, we can match the individual data with establishment data. To approximate both the establishment-specific business conditions and the turnover rate, we use the DMMO (see above and the data appendix), which gives information on hiring, quits, and different types of terminations as well as total employment. From these variables, we computed an entry rate, an end of short-term contracts rate, a quit rate, an "economic" terminations rate, and an "other types" of terminations rate. We add these variables to our cross-section regressions. All new estimates of the computer-use coefficient are identical to those presented in table 4 (and, therefore, not reported in other tables). Hence, we conclude that none of the above explanations of the positive correlation between wage and computer use observed in the cross section holds.

The same type of exercise can be undertaken in the longitudinal dimension. Assume that firms experience profit shocks. Consider now firms that experienced positive shocks in a previous period and, therefore, bought computers for some of their workers. If these firms are more likely to experience negative shocks, then computer workers—even if they are more productive and entitled to a wage increase—may receive no wage increase because of these bad business conditions. Hence, this may induce a downward bias to the overall returns to experience with computers.

To examine this issue, we pursue a similar strategy as the one described above for the cross section. We add the same set of variables that should control for the time-varying business conditions faced by the establishment. Then, we run regressions with both individual- and firm-fixed effects. Results are identical to those described previously for table 5 and, thus, are not reported in other tables. Hence, we conclude that these alternative explanations of our result, which could indeed provide downward biased estimates of the returns to computer use, do not hold.

V. NT Workers and Unemployment

Our previous section has demonstrated that workers who use NT, computers in particular, are selected among high-wage workers. Returns to computers amount to approximately 2% after 2–3 years. Hence, the difference between the cross-section estimates (between 15% and 20%) and the longitudinal estimates must come from the selection among workers with otherwise identical observable characteristics by their employing firm.

To go one step further, we may wonder whether computer use per se protects workers from job losses. To examine this issue, we use our supplement of the Labor Force Survey on New Technologies (i.e., TOTTO) matched with the quarterly Labor Force Survey for 1993. Hence, for most workers employed in March 1993 (8,288 of the 9,345 individuals in the original sample), we know the employment status in June, September, and December of the same year.[14] We first estimated equation (4),

$$\Pr[\text{unemp}_{it} = 1 | e_{it_0} = 1] = \Phi(\alpha \text{Comp}_{it_0} + X_{it_0}\beta), \tag{4}$$

where $\text{unemp}_{it} = 1$ denotes that worker i is unemployed at date t (after t_0), where X_{it_0} are observables for worker i, where $e_{it_0} = 1$ denotes that worker i was employed at date t_0, and where Φ denotes the probit function (standard normal c.d.f.). This equation was estimated jointly with equation (1) as well as by itself. For the joint estimation of (1) and (4), the variables that are included in the computer allocation equation (1) but excluded from the job-loss equation are defined as follows. For each

[14] The absence in the panel of workers having changed residence after March 1993 could induce biases in our estimates. However, changes of residence in France are not frequent, much lower than in the United States, and, more importantly, workers who lose their jobs seldom change residence in France.

worker i employed in firm j, we compute the proportion of workers in our sample other than i who are employed at the same firm j and who use a computer, or a robot, or a telecommunication machine, or another type of NT. This gives us four different variables for each worker. Notice that each variable is computed without taking i into account. Hence, they do not suffer from the "Reflection Problem" (see Manski 1993). Obviously, there are good reasons to believe that the existence of other computer users in the same firm has a positive effect on each worker's probability of using computer. Indeed, estimates show a positive, and significant, relationship (not reported here). Then, a correction term (see Van de Ven and Van Praag 1981) is added in equation (4), a probit equation, to control for selection biases.

Both sets of results are presented in table 7. Both have the same flavor. Workers who use computers, and only those workers, are protected in the short run from job losses. The unemployment probability for computer users is lower both in June and September 1993, but they are not protected anymore in December of the same year. Indeed, 1993 was a bad year for employment, maybe the worst in the last 10 years, and the second semester was the trough in the employment cycle; the number of unemployed workers went over 3,000,000 for the first time during this semester. When bad business conditions last, all workers suffer from job losses.

In addition, none of our correction term is significantly different from zero. Most of the selection bias appears to be captured by the computer-use variable. To check the robustness of this result, we estimated many variants of equation (4). In particular, in order to capture unobserved heterogeneity, we included the estimated residual $\hat{e}_i$ of equation (3) as described in equation (5):

$$\Pr[\text{unemp}_{it} = 1 | e_{it_0} = 1] = \Phi(\alpha \text{Comp}_{it_0} + X_{it_0}\beta + \gamma \hat{e}_i). \qquad (5)$$

This residual captures whether a worker is a high-wage or a low-wage worker given his or her observed characteristics. Once more, none of the above results are changed. As additional checks, we estimated multinomial logits with the different sequences of employment and nonemployment in each of the quarters as dependent variables. Estimated results were unchanged. Finally, we matched our individuals with the DMMO files using the SIRET identification number as described above for the wage analysis. The results were identical and not reported here for this reason.

VI. Conclusion

In France, as in the United States and Germany, computer users are better paid than nonusers. The wage premium to computer use is

Table 7
Employment Status after March 1993 and New Technologies

	Employed in March					
	Unemployed in June	Unemployed in June	Unemployed in September	Unemployed in September	Unemployed in December	Unemployed in December
Uses a Computer	−.4373	−.4284	−.2762	−.2553	−.0347	−.0193
	(.1871)	(.1891)	(.1180)	(.1187)	(.1019)	(.1025)
Uses a robot or NC machine	.2337	.2309	−.0519	−.0623	−.0312	−.0376
	(.1881)	(.1884)	(.1915)	(.1921)	(.1580)	(.1583)
Uses video or laser-based instrument	.0947	.0958	.0086	.0066	.0386	.0370
	(.1482)	(.1483)	(.1131)	(.1134)	(.1000)	(.1003)
Uses fax or minitel	−.1345	−.1343	.0495	.0455	.0500	.0486
	(.1411)	(.1410)	(.1013)	(.1011)	(.0924)	(.0923)
Correction term	. . .	.1070	. . .	−.4019		−.3422
		(.3737)		(.3163)		(.2828)

SOURCE.—Labor Force Survey, 1993; Quarterly Labor Force Survey, 1993.
NOTE.—All workers were employed in March 1993. There were 8,288 observations. Probit was estimated by maximum likelihood. Standard errors are in parentheses. Explanatory variables also include sex, education (eight positions), a region dummy, a part-time dummy, occupation (six positions), size and status of the employing firm, experience and its square, tenure and its square, a part-time dummy, and a constant. NC = numerical command.

similar to those obtained by Krueger (1993) and Di Nardo and Pischke (1997). However, computer users were better compensated than non-users even before their first use of computers. The total returns to computer use do not exceed 4%—far from the 15%–20% estimated in France or in the United States in the cross-section dimension. Selection of the high-quality workers is a pervasive phenomenon when firms allocate their new technologies. Furthermore, computer users are protected from job losses in the short run, that is, as long as bad business conditions do not last too long.

These conclusions seems consistent with the following story. Wages are rigid in France. Therefore, all adjustments must go through employment changes. Indeed, skill-biased technical change, as strong in France as in the United States, has increased the demand for skilled employment. Rigidity implies that wages cannot adjust fully to their new equilibrium level (computer users should be better compensated than they can be). Hence, unskilled workers became relatively more expensive than before. This implies that workers who do not use computers or, more generally, unskilled workers, are going to lose their jobs more often than computer users or, more generally, skilled workers, all other things being equal.

This story must contain a grain of truth. However, Card, Kramarz, and Lemieux (1996) have shown that a similar argument for low-wage workers is not supported by the data: even though wages of the low-educated workers are rigid in France, changes in their relative employment were not markedly different from those observed for their American counterparts during the 1980s. Indeed, as was noted above, the U.S. labor market operates very similarly to the French one in many dimensions. The evidence brought to the fore in Di Nardo and Pischke (1997) or in Doms, Dunne, and Troske (1997) together with our results should imply that the same selection effects also operate in the United States. It becomes impossible to attribute all of the increasing inequality to the introduction of computers. Any explanation of the above facts as well as the increase in inequality should integrate other factors than education or experience. More precisely, unobserved but compensated characteristics of the workers matter. In our sample, and even more so for the low-education workers, unobserved skills are indeed complementary to computers. This induces and, even, reinforces selection among workers with otherwise identical observed characteristics. Selection determines workers' prospects in terms of wage gains as well as in terms of job losses.

Data Appendix

The French Labor Force Survey (Enquête Emploi, EE) is conducted every year by the French National Statistical Institute. The universe of individuals sampled includes all ordinary households of metropolitan France. Only some of those living in communities are sampled—excluded, for instance, are the members of religious communities, individuals living in mobile homes, or bargemen.

We use the March EE series starting in 1990 with a sampling frame based on the 1990 census. The sample corresponds to a sampling ratio of 1/300 and is renewed by one-third every year. Hence, every individual is at risk of being surveyed at most 3 consecutive years. Furthermore, the sampling technique is based on housing in tracts built in French territory with the further inclusion or modifications in case of construction or reconstruction of buildings not known at the 1990 census (see INSEE 1994 for all the technical details on the survey methodology). This introduction of new buildings (and households) is made by interviewers who are responsible for a sub-tract and interview the members of each household.

Each year, a supplement (Enquête Complémentaire) is directed at the outgoing third of the sample. In 1987 and 1993, the supplement was centered on new technologies. Unfortunately, the 1993 version that we use is not identical to the 1987 version. However, unlike in 1987 and before, the wage data that we use in 1993 are no longer categorical but rather the monthly wage net of employee- and employer-paid benefits (such as health or unemployment insurance).[15] The data drawn from the Enquête Emploi include standard questions from labor force surveys. Hence, besides the wage, we know the country of origin, the sex, the marital status, the number of children and their age, the region of residence, the age, the detailed education, and the age at the end of the education period, the occupation (four-digits classification), father's last occupation, mother's last occupation, the employment status (employed, unemployed, inactive), usual number of hours, the seniority in the employing firm, the sector and size of the employing firm, the nature of the contract (short term, long term, program for young workers [stage]) for each of the individuals in the sample. Furthermore, each employed individual is asked about his or her firm. Name and address are collected as often as possible. This information is given to the INSEE regional agencies where the SIRET (establishment identification number) is coded using the on-line SIRENE computerized system. This number is the unique establishment identifier that an establishment receives during its life. The first nine digits represent the firm to which the establishment belongs. This number can be coded in the Enquête Emploi for more than half of the workers.

In addition to these questions, the 1993 supplement on NT gives information on the use of new technologies in the firm and workplace organization as seen by the worker. More specifically, the worker is asked about work at night, on Saturdays, on Sundays, the content of the job (polyvalence, relations with the hierarchy or with other services in the same firm or with persons outside the firm, the nature of the decisions the worker has to make, the existence of quality norms, the way problems are

[15] This started with the new EE series of 1990.

handled). Finally, the worker gives detailed information on the technologies used for his or her job (see table A1). Information on experience with NT is asked only for the computer and the robot categories.

For those households of the outgoing third of the survey on whom data on NT are available, a quarterly follow-up is organized in order to compute a quarterly unemployment rate. Simple questions are asked. In particular, the activity status is collected. However, no other information, such as wage, employing establishment, type of job, is sought.

The DMMO is an administrative data source which exists for all private and semipublic establishments employing at least 50 workers. This declaration is monthly. The document began in 1975 to enforce new labor market legislation. It has been maintained since 1986 even though the "administrative authorization for separation" was abolished. The complete collection and control of this data source is only available since 1987. All the controls and coding are made by both INSEE and the Ministry of Labor. Information includes all entries and exits detailed by occupations, type of contracts (short term, indeterminate duration, transfer) for entries, type of exit (quit, firing for cause, firing for economic reasons, end-of-short-term contract), employment at the beginning and at the end of each month. The version that we use in this article is an aggregation at the year level of this data source. Every year, we have approximately 38,500 establishments that can be matched using the SIRET with the individual datasets coming from the Enquête Emploi. For each establishment, we compute the hiring rate (total hiring in the year divided by total employment at the beginning of the year), a quit rate (total quits divided by total employment at the beginning of the year, an end-of-short-term contracts rate (equivalent definition), a firing-for-economic-reasons rate (equivalent definition), a firing-for-other-reasons rate (equivalent definition). The definitions we use here are identical to those used in Thiery and Torelli (1994).

The TOTTO-Europe is a survey on computers and workplace organization that took place in 1994. The survey was financed by the European Foundation for the Improvement of Working and Living Conditions and INSEE collected the data. This survey was designed as a test for the 1995 European survey on working conditions, which was administered to the 15 member countries of European Community (EC). The sample comprises 954 individuals. Virtually all questions in the Enquête Emploi as well as in the 1993 TOTTO supplement were administered to these individuals.

Appendix A

Table A1
Definitions of NT Categories

NT Group	Detailed NT
Computers	Microcomputer
Computers	Word processor
Computers	Connected terminal
Robot	(3-dim) robot
Robot	Numerical command machine-tool
Communication NT	Minitel
Communication NT	Fax machine
Other NT	Computer-based measurement or control instruments
Other NT	Laser-based measurement or control instruments
Other NT	Computer-based medical instruments
Other NT	Laser-based medical instruments
Other NT	Laser-based machines
Other NT	Video-based machines

NOTE.—NT = new technologies.

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