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THE SOCIOLOGY OF WORK AND OCCUPATIONS

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Abstract

I review recent studies of work and occupations. Most of this work proceeds at the individual level, studying individual characteristics of workers, qualities of the work experience, and, to a lesser extent, stages of the work experience. Structural analysis is less common and often treats structural phenomena as aggregates rather than emergents, except in the area of labor relations. A substantial literature—probably a third of the total—examines particular occupations. In general the literature is divided into two "sides"—one focused on gender, inequality, and career/life cycle issues, the other on unions, and industrial and labor relations. Between these are smaller foci on theoretical issues and on general structures of work. I conclude that with the possible exceptions of Marxism and the study of professions, subfields of work and occupations lack the synthetic theory that would enable synthesis of empirical results. I also consider the twofold role of politicization in the area: the positive role of driving empirical investigation of new areas, the negative one of taking its own politics as unproblematic.

INTRODUCTION

An Annual Review author must cheat either breadth or depth. I cheat depth. To limit my topic to occupations alone is absurd. Changes in occupations cannot be construed without the work system that enfolds them. That normal sociology isolates social facts from their contexts—as status attainment separated achievement from structural change—only underscores the point, for what has stunted sociological knowledge if not this isolation? I therefore begin by situating the work and occupations (WO) literature historically and

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disciplinarily. An exploratory quantitative analysis of the field then leads into reviews of selected areas within it.

THE LOCATION OF THE WORK AND OCCUPATIONS LITERATURE

The modern sociological literature on WO has two ancestries. The first includes Everett Hughes and his students. Hughesian studies of work rested on field study, Chicago-style ecological institutionalism, and a social psychological focus inherited from W. I. Thomas and Robert Park. The ecological institutionalism combined with field study in the literature's long-familiar emphasis on professions, while the social psychological concern ultimately bore fruit in its emphasis on job satisfaction.

The Hughes tradition relates ambiguously to the other line of sociological inquiry into work—what is loosely called industrial sociology. [This is the tradition that is seen by Simpson (1989) as the core of WO.] Industrial sociology comprises diverse work institutionally derived from the human relations school of management; it takes formal shape in the field of industrial relations (IR). Methods differ between the IR and the Hughesian traditions; mutual citation is low and theoretical integration unusual. Few topics have drawn sustained attention from both literatures.

The split is easily understood. The Hughesians saw industrial sociology as "applied sociology," an orientation from which they had begun withdrawing by the 1950s (Smigel 1954). Industrial sociology was also closer to the organizations and bureaucracy literatures, always somewhat prescriptive and gradually moving into applied settings (i.e. business schools) throughout the postwar period. Writings on workplace participation well illustrate these differences. Driven by applied concerns and tied to formal theories of organization, this work has been ignored in the sociological literature on WO, although it is central to the IR literature.

In the years immediately after World War II, professions were the central focus of the WO literature. (For histories, see Cohen 1983, Smigel 1954, Roth et al 1973, Hall 1983.) There was little on blue collar work. The shift towards psychological concerns—job satisfaction, alienation, and so on—took place in the 1950s. Like the parallel rise of the new statistics, this shift was part of the gradual move away from institutional or structural accounts (of work as of most other things) toward individualistic ones. Simpson (1989) writes that 1950s research often portrayed workers as controlled by larger structural forces rather than as independently acting, the latter image being characteristic of earlier, observation-based research. She infers that the 1950s therefore inaugurated a period of serious structural analysis. Quite the contrary. The abstractions of Parsons simply screened the refocusing of studies of work on the individual level. Structural analysis now meant studying individual flows within a constant (controlling) structure (changes in employment levels, mobility in socioeconomic hierarchies, etc.) This change—from conceiving structures as constructed, active, and contingent to conceiving them as dead but constraining—was driven largely by the methodological paradigm, as I have argued elsewhere (Abbott 1992). The 1950s' new interest in careers also grew from this individualism in structuralist disguise.

These changes were consecrated in the 1960s by the rise of status attainment and the publication of *The American Occupational Structure* (Blau & Duncan 1967). By assuming well-defined, constant occupations with well-defined statuses (artifactually shown to be "constant" by Hodge et al 1963—on which see Burrage & Corry 1981 and Coxon & Davies 1986), these new analyses of stratification negated en passant the ecological, processual approach to occupations, and indeed to work in general, allowing only for demographic movement within fixed structure. The status attainment tradition also firmly established occupational status mobility as a, or the, central substantive topic of the sociology of WO, encircling it with a set of individual determinants, above all gender, which became one of the two central topics of WO research by the late 1970s (Hall 1983).

By the 1980s, the evolving WO tradition had come to look much as it does today. The only institutional subfield of real strength was the study of professions. To be sure, Braverman (1974) had resuscitated structural analysis in his justly celebrated book. But the DOT's scales so transcendentalized "skill" that the ensuing research ignored changing structure altogether. In such an environment, individual-level topics like mobility, status, satisfaction, commitment, aspirations, and choice elicited most of the research. The most visible subareas of WO research were mobility and gender, the latter understood basically in status attainment terms even after Hochschild's brilliant book (1983). There were structural-level conceptions, to be sure, like labor markets. But labor markets were defined in terms of individual properties—mobility chances between areas—rather than conceptualized as emergent structures (as markets were more generally by structuralists like Harrison White 1981).

The WO literature has generally been isolated from research on work in other disciplines. Thus, the enormous (1000 articles per year) economic literature is seen as excessively abstract, although eventually the human capital approach it pioneered became a competitor of status attainment. Similarly, although a few social histories beguiled sociologists—Hareven's *Family Time and Industrial Time* (1982) and Tilly & Scott's *Women, Work, and Family* (1978), for example—the majority were ignored. For example, uncited in the WO literature in sociology (in the *1990 Social Science Citation Index*) were Montgomery's *The Fall of the House of Labor* (1987), Graebner's *History of*

Retirement (1980), Keyssar's *Out of Work* (1986), Licht's *Working for the Railroad* (1983), and Stearns's *Paths to Authority* (1978). Similarly ignored were historical studies of family and external divisions of labor such as Pahl's *Divisions of Labour* (1984), Hall et al's *Like a Family* (1987), and Lowe's *Women in the Administrative Revolution* (1987). Helmbold & Schofield (1989) and Rose (1986) give a sense of the size and richness of this unknown literature.

The ignorance of both gender and labor history in WO stems from longstanding anti-structural bias. Union-membership, like gender, was easily treated as a variable, metamorphosed from a social or cultural construct into an attribute of an individual. Only the institutionalist students of professions made serious use of the new historical work (as in Larson 1977 or Abbott 1988). Indeed, most sociologists of work who did use the historical literatures were historical sociologists, identified with the relatively theoretical, structural, and somewhat leftist cast of that field.

In short, the sociology of WO has for the last 20 years pursued a fairly narrow range of topics. It has focused on individual behavior and its immediate contexts, looking at psychological, personal, and social antecedents and consequences of work behavior. It has largely ignored other bodies of inquiry into work. There have been few attempts at general theoretical analysis outside Marxist writings and perhaps the sociology of professions. It is a sad fact that much or most of the exciting study of work today happens outside sociology's provenance and even its interest.

THE TOPOLOGY OF THE WORK AND OCCUPATIONS LITERATURE

Nonetheless, sociology does produce an awesome amount on work. *Sociological Abstracts* (SA) lists about 500 articles and 70 dissertations per year on WO broadly defined, either under "sociology of occupations and professions" or under "jobs, work organization, workplaces, and unions." I had a research assistant code these articles for the entire years 1990 and 1991. For each article, I have its general character (theoretical or empirical), country of major concern (if empirical), methods (if given), number of cases (if given) and a set of major keywords covering the central topics. Since I believe in internationalism, I did not rule out foreign language articles. Unless otherwise noted, descriptive remarks following are based on this data.

Slightly more than half the articles abstracted in SA appear in sociology journals. Another 9% appear in psychology journals, 7% in economics journals, 6% in business journals (where I counted the labor relations journals), and 2–4% each in health, law, education, and anthropological journals. Another 14% appear in a variety of other areas—mostly interdisciplinary journals like *Annales ESC*.

About two thirds of these articles are empirical. The proportion of theoretical work is highest among the economics articles (45% theoretical), then sociology (40% theoretical), but much lower in business (25%) and psychology (10%). The typical empirical sample is largest in business studies ($n = 450$), then sociology (350), economics (250), and psychology, education, and anthropology (150). Sociology produces most of the really large studies, perhaps because I have included demography in sociology. But the modal study in all fields examines 100 or 200 doctors or teachers or some other type of worker.

Fields use diverse data. Economists rely more on published information (45%) than on surveys and questionnaires (25%) or interviews (20%). By contrast, sociologists rely first on surveys and questionnaires (45%), then on published information (30%), then on interviews (15%). Business and psychology both rely mainly on surveys and questionnaires (65%), with the rest mostly from published information in business and from interviews in psychology. Hughesian-style field study has nearly disappeared. Psychology leads with about 10% observation studies, while sociology, economics, and business all hover around 5%. It is in the other, minor literatures that observation survives; about 25% of empirical articles on work in anthropological or educational journals involve observation.

Most empirical studies consider one country. There are 40 comparative studies (out of 663 empirical studies), 25 of them two-country. But the literature is cosmopolitan (partly because I regard the overseas literature as relevant, of course). Of 523 studies with identifiably national datasets, only 40% (205) primarily concern the United States. Another 12% (64) concern the United Kingdom, followed by Canada (6%, 31), Germany (4%, 23), the former USSR (4%, 19), Australia and Finland (3%, 14), the Netherlands (3%, 13), and other countries. Language barriers cause the surprising absence of Japan as a principal country of analysis (a total of 4 articles in these 1100).

I now turn to topics studied. That the importance of topics varies somewhat across these literatures tells us much about how *Sociological Abstracts* selects articles for inclusion, a selection that I think reflects the tastes of the WO literature. (The reader should recall that an article can have several topics. Also, I used general headings for unspecified or "mixed" topics; these are not included in the lists immediately following.) In sociology, the most important single topic is gender, which is considered by about 20% of the sociology articles, much as in Hall's (1983) figures for the early 1980s. The next most important substantive topics are individual status mobility, unions, and income (7% each), labor and social control of work (6% each), work organizations and labor markets (5% each), and race, inequality in work experience, unemployment, technology, and manufacturing employment (4% each).

In other fields concentration is much greater, reflecting selection. Nearly half (42%) of the economics articles are on unions, another quarter (22%) concern IR, and about 10% each concern unemployment and collective bargaining. (The overall economic literature on work is distributed quite differently.) The business literature is ransacked for the same topics, unions (22% of business articles), wages and management (each 13%), and collective bargaining (11%). In psychology, gender is again the top topic (16% of psychology articles), followed closely by stress (15%), then by job satisfaction (11%), psychological consequences of work (9%), individual status mobility, doctors, and unemployment (7% each). Since the chosen collateral literatures reflect sociology's own major concerns in the study of work, I shall henceforth discuss the overall SA sample as a coherent whole.

I begin with a discussion of the comparative importance of topics. There are four general areas: general issues, individual level topics, structural level topics, and particular occupations or areas of employment.

Theory is the most important general issue, although a relatively small fraction (about 16%) of the literature concerns theoretical issues. (In what follows the percentages are always percentages of all articles, n = 1100.) Most of these articles are actually theoretical analyses of more specific areas; articles about theory alone are unusual. There is also a small literature (5% of the total) dealing with cultural images of work, often closely tied to theoretical analyses. The cultural literature is very diffuse, spread over many areas of substantive concern, rather than concentrated into "a literature" on the symbolic structure of work.

The second general area is research at the individual level. The first major subcategory here is study of individual characteristics of workers, in which most effort (22%) is concentrated on ascribed characteristics (like gender, race, age) rather than achieved ones (education, skill, and sexual orientation—3%.) Education appears often as a control variable, but it is seldom more than that. Age (2%), too, is a control variable rather than a central focus in most cases.

Among articles on (individual) qualities of the work experience, the major interests are psychological (including stress, motivation, and job satisfaction—10%) and economic (wages and fringe benefits—6%). There are also concerns for general qualities of work (working conditions and inequality in general—5%, mostly on inequality) and for health consequences of work (5%). A smaller fraction (3%) concerns social qualities of work (e.g. lifestyle).

Relatively less attention is paid to stages of the individual work experience. Here the major topic is unemployment (5% = 51 articles), followed distantly by careers (29), career choice (20), and employment opportunities (14). Surprisingly, there is little about layoffs or retirement.

A third general area is structural analysis. A substantial part of the literature considers some general structure (e.g. labor, management, occupations, bureaucracy, markets—17%), general process (e.g. individual mobility, migration, labor movement—10%), or general indicator (e.g. productivity, industrial dominance—5%) of the work system. The important structures are labor, management, and (individualistically conceived) labor markets. The important process is individual status mobility. The important indicator is productivity. The shadow of *The American Occupational Structure* falls even on this "structural" level.

Only about 10% of the literature concerns the division of labor. Most of this work is about technology (40 articles) on the one hand or about the twin divisions of labor of home and work (26 articles) on the other. A slightly larger fraction of the literature (14%) studies the social control of work, much of it covering several issues at once (64 articles), although there are specific foci on ethics (40) and professionalism (18). Surprisingly, there is little or no focus on the temporal structure of work (1%), including such topics as flex-time, part-time work, moonlighting, and job-sharing. There is much speculation about the role of temporal structure, particularly in relation to the gender wage gap, but little research.

Finally, in this structural area, about one fifth (18%) of the literature deals with topics in what is usually called industrial and labor relations. Unions figure in one tenth (113) of the articles (second only to gender), but there are also many articles on IR generally (52), and not a few on collective bargaining (21) and strikes (20).

The gap between structure and individual characteristics is bridged in part by my fourth general area, writing that considers a particular occupation, occupation type, or sector of employment. About a third of articles concern a limited group of workers. Some 10% concern a general category like white or blue collar or professionals. Fully a quarter concern a particular type of occupation. Of these the most common are doctors (54 articles), lawyers (27), nurses (25), teachers (22), university faculty (22), engineers (16), social workers (13), miners (12), scientists (9), and journalists (6). The literature's love of professions, indeed of itself (academics tie for fourth), is untarnished by time.

It is much less common (15%) to focus on a specific area of employment. Of these the most common is manufacturing (56 articles), with academia (24) second, and government (20) third. Home work (9 articles) nearly disappears, as does agriculture (There is, of course, a separate SA heading for rural sociology.)

To uncover the geography of the WO literature, one must move beyond marginals to cooccurrence. To do this, I have analyzed PAIRS of topics. For example, the most common pairing of topics in this data set, not surprisingly, is gender and wages, a combination considered in 25 articles. (Other topics can of course be considered in these articles as well.) Eighteen articles consider gender and inequality in general, while 17 consider both general inequality and wages in particular. I have considered all pairings of topics that appear in more than five articles, an arbitrary but useful cutoff. There are 52 such pairings.

That the network of topics includes 39 topics for these 52 connections tells us that the literature is not organized as a small group of central topics considered again and again in varying combinations. Quite the contrary, the pattern is of three or four principal topic areas largely disconnected from each other, each with its own hinterland of specialty topics.

As we have seen above, the two principal topic areas are gender and unions/IR. Astonishingly, fewer than six articles consider gender in the context of unions or industrial relations. Yet the relation of gender and work was centrally structured by unions, as a brilliant historical literature on the "family wage" has shown. Our ahistorical, individualistic research has forgotten that gender is more than being female in a regression equation.

Six topics are directly connected to gender and to nothing else. (I give in parentheses the number of joint appearances in articles.) They are: careers (8), career choice (9), family roles (14), stereotypes (6), sexual discrimination (10), and lawyers (7). It is surprising that there aren't more internal connections, since, with the exception of lawyers, these topics seem all of a piece. In addition, three topics are connected to gender and to one or more other topics that have no further connections: job satisfaction (8 to gender, 6 to nursing), academic employment (6 to gender, 9 to university faculty), and unemployment (6 to gender, 8 to age, 6 to labor markets). It is clear from this pattern that sociologists treat the issue of "stages of work experience" basically as a gender issue, not as a general one. With the legal retirement limit rising, actual retirement age falling, and sweeping layoffs occasioned by the recession and international restructuring, one would expect a more general interest in career contingencies than merely in the problem of family and work (Doeringer 1990).

As I have already noted, gender is the biggest member of a loose clique of topics—gender, wages, general inequality, race, and individual status mobility—that dominates this "side" of the literature. Off to one side of this clique is a less important core topic, general theory, connected to gender by nine common appearances and via the topic of professions, which has six joint appearances with each. Theory has four topics connected only to itself: capitalism (8 ties), professionalism (6 ties), intellectuals (6 ties), and cultural images (7 ties). In this second cluster, then, are the principal theoretical literatures on work, the Marxist literature with its focus on capitalism, the professions literature, and the newer, somewhat chaotic literature on cultural images and work.

The other principal nexus of topics concerns industrial and labor relations. The topic of unions is connected to IR by 14 direct ties, as well as via manufacturing employment (8 ties to unions, 6 ties to IR) and via collective bargaining (11 ties to unions, 6 ties to IR). This is a more clique-like structure; many articles consider several of these topics together. The identity of the union literature with manufacturing is curious, since unionization in that sector has plunged in the United States, to be replaced by public sector unionism (AFSCME and NEA) that is relatively more female. Again the lack of a gender connection is surprising.

The union topic area lacks the "hinterland topics" that gender has. It has only one such tie, to social control of work (6 common appearances). However, social control of work in turn relates to worker health (6 ties) and to doctors (9 ties), which in turn has 6 ties to ethics. The control theme thus has two faces: one involved with labor/management relations, the other with work practices in particular occupations. (For want of space, I do not review this control literature below.)

Between unions/IR and gender lie a number of broker topics. That is, articles appear about unions and these topics, as about gender and these topics, but not about all three together. One set of brokers are general work structures; labor (8 ties to gender, 7 ties to unions) and management (6 ties to gender, 6 ties to unions, 6 ties to IR). The two are tied to each other 7 times, and labor is not surprisingly tied to unions via the topic of organization, which has 6 ties with each. Again, the principal conclusion here is a negative one. One literature sees an important articulation between gender and labor and management while another studies the obvious connection between labor and management and the formal structuring of IR and unions. But the two literatures don't communicate.

The other principal bridge between the gender topic area and the unions/IR one is brokered by race and by the "other" category of specific occupations. Again, this means that gender is often considered within the context of a single occupation (11 ties), as is unionism (7 ties), but the two are not considered simultaneously. Similarly, race and gender (14 ties) are considered together, as are unions and race (8 ties). Further, race is connected via its 10 ties with general inequality to the tight little grouping of gender, wages, and general inequality that is the heart of the gender literature on work. There is also a separate tie between race and gender via individual status mobility (7 ties to race, 11 ties to gender).

In addition to showing the major holes in the literature, this network structure identifies the major discrete areas that require survey here. I review the literature's actual findings under these headings, using merely exemplary titles. There is not space for everything relevant.

SPECIAL TOPICS

Gender and Career Contingencies

Gender's influence on job satisfaction seems peculiar. Women are more satisfied than "objective" criteria suggest they ought to be, but several studies show that this does not reflect use of different job-rating criteria. The difference seems to lie in choice of reference groups, women often comparing themselves only to other women (Loscocco 1990, Loscocco & Spitze 1991, Hodson 1989).

The gender and career/career-choice literature focuses mainly on the sex-segregation of occupations and on the behavior of women in traditionally male occupations. Reskin & Roos (1990) attribute segregation to the intersection of employer ratings of potential employees with worker ratings of prospective jobs. This is actually a general theory of labor markets treated only in its gender connection, a limitation some writers have also noted in the comparable worth literature. Bradley (1989) also covers this area, as does a special issue of *Work and Occupations* (18:4, 1992). The literature on women in traditionally male occupations focuses on the (to the authors) surprising similarity of these and other working women, particularly in reasons for occupational choice, working, etc. Authors are also surprised at the persistent strength of SES effects and economic need on occupational aspirations and choice (Timmins & Hainesworth 1989, Poole et al 1990, Padavic 1991).

Another small literature considers work discrimination and harrassment at work. Considering the political importance of the topic, the lack of research is surprising. There are a few ethnographies of harrassment at work (Yount 1991, Gruber 1989), and a nice history of overt discrimination in compensation programs (Kim 1989). A number of historical works (like Lowe 1987 and the Reskin & Roos 1990 case studies) consider the mechanics of deliberate feminization of occupations.

A justly large literature considers the relation of family and work. A variety of articles consider usual topics like the need of complex compromises, the employer preference for single women (Peterson 1989), and the role of reconceptions in shaping perceptions of working mothers (Etaugh & Nekolny 1990). Several books emphasize the importance of complex contingencies in shaping the articulation of family and work (Stichter & Parpart 1990, Beach 1989, Selby et al 1990). Many also emphasize the enormous influence of differing cultural traditions on this mediation (Stichter & Parpart 1990, Standing 1991, Ramu 1989). Several of these (e.g. Ramu 1989) show the enormous persistence of traditional attitudes in women undertaking modern work. Overall, this literature shows that analyses based on the half-century transformation of American mothers into wage workers cannot produce a general model for the relation of family and extra-family divisions of labor. Not only are there many other ways of working this relation out, but those other ways point out things earlier analysts missed in the American experience. One cannot discuss this issue without a full analysis of work and family culture, of complexes of structural forces that impinge on the local work world, and of the creative response of family members—men and women alike—to them. Serious readers of Tilly & Scott will recognize this as an old lesson. But parts of the literature continue as if they hadn't learned it.

Gender and Specific Occupations/Areas of Employment

Articles about gender and particular occupations are of two kinds. One kind compares men and women, generally finding the expected differences—in rewards (persisting after controls) and attitudes. Ott (1989) finds differences in the rewards of men and women under different kinds of gender mixes. Martin (1990) finds differences in the attitudes of men and women judges. Roach (1990) notes differences in achievement by men and women in law. Yoder et al (1989) note differences in academic hiring consequent on departmental gender mix. The second kind of article compares women to other women and finds extensive variety among them. Gray (1989) finds differences in nurses' organizational commitment reflecting feminist ideology and presence of children. Rosenberg et al (1990) distinguish several types of women lawyers, while Briscoe (1989) notes varying attitudes of women lawyers toward entering politics. Henry (1990) is worried by the antifeminist scripts of senior women academics. In the dozens of articles here sampled, these two strategies are never conducted concurrently, so that varieties of women are compared to varieties of men. This is clearly a major problem.

In books on individual occupations, many of these themes recur. Kingsolver (1989), Kesselman (1990), and Fine (1990) all testify to the complex responses of women in particular occupational settings. Most important, all three testify to the centrality of forming a consciousness (both personal and corporate), making a set of symbols, and coming to an appraisal of the situation. Here, too, we find diversity of women's responses (as in Abir-Am & Outram 1987 on women scientists).

The antinomy between the simple male/female opposition and the diversity of women when considered alone is the basic conundrum of this literature, indeed, of the gender and work literature as a whole. The notion of women's oppression is so central to most writers in these literatures that they are bewildered or angered when their subjects reject it (as in the elitism of some of Abir-Am & Outram's scientists or the acceptance of clerical subordination in Fine). Yet at the same time writers see enormous variety in the responses of women to situations, a variety that probably—few have studied this question sympathetically—occurs similarly among men. A reminder that other solidarities can supersede gender comes from Hine's (1989) book on black nurses. Like the literature on gender and career, then, the literature on gender and occupations is vibrant and exciting, if occasionally one-sided. The best of it shows conclusively how the structures of culture, tradition, economy, class, and social organization create frameworks within which women (and sometimes men) transform their world of work, indeed their whole lives, and, ultimately, those structures themselves. This literature proves that the static approach of the individualist statistical studies can tell us little or nothing about why the relation of gender and career, family and work, looks the way that it does.

Gender, Race, Wages, Inequality, and Status Mobility

One subtopic commands a substantial portion of this literature: comparable worth (CW). Many of the writers on CW are not sociologists; CW is an applied topic most often researched in business schools, by professors of organizational behavior and psychology (only one author in the 1989 special issue of *Journal of Social Issues* on CW is a sociologist). Most articles on CW are frankly prescriptive. Their politics is well analyzed by Hegtvedt (1989), who also locates the CW literature within the theoretical literature on fairness. Some of the empirical work notes, again, the disturbing toleration of wage gaps by women (Major 1989), while others have noted that evidence for direct bias in job evaluation is fairly rare (Mount & Ellis 1989) although historical factors (Kim 1989) and knowledge of existing wage differences (Mount & Ellis 1989) provide avenues for indirect bias. By far the best studies of CW are the three book length case-studies (Acker 1989, Blum 1991, and Evans & Nelson 1989), which provide cautionary tales about the complexities of actual politics, the difficulties between elite feminist reformers and workaday women, the complex cross-cutting of gender and class interests, and (here at last) the intricate relations between CW advocates and the unions. These books are all thought-provoking analyses.

Beyond the literature on CW, the articles on gender, race, and various forms of inequality are a cacaphony. Their time frames run from 50 years to 20 years to 5 years to none. Their spatial units run from countries to industries to states to firms to jobs within firms. We can draw no general conclusions from combining these articles because we have no theory saying how these different levels and periods of social reality relate to one another. Fillmore's (1990) 50-year study of Canadian census documents may support a reserve army theory of women's labor force participation, but how do we reconcile that with Rosenfeld & Kalleberg's (1990) equally interesting cross-sectional finding that countries with more occupational sex-segregation can have more equal income distributions (Sweden)? We have state-level census data showing that unionization lowers wage inequality, panel survey data showing that gender gaps have worsened, firm-level data showing blacks and women do badly, occupational data showing that although selection bias lowers the effect, women in female-dominated occupations still make 6 to 15% less than equivalent women in other occupations, and so on and so on. The only common theme of these results is that women always do somewhat worse than men. Politically, such findings are useful; intellectually, they are old news. Without a theory linking large-scale change to local structures and local structures to individuals, we cannot synthesize such findings. There is obvious concern that classical Marxism, which used to provide a general theory for some people, just doesn't work.

Gender and Labor/Management

There is a very small literature on gender and management, mostly on women managers. Thus, Chusmir & Mills (1989) note differences in conflict resolution styles but feel that job level makes more difference than gender. Wiley & Eskilson (1990) show the influence prospective images are likely to have on gender management styles. There is also a literature on management careers and gender, which has much the shape suggested above. There are also a few books on gender and labor. Gabin's (1990) excellent history of women and the United Auto Workers gives a detailed, practical portrait of women's successes and failures on the inside, as do Kingsolver's (1989) wonderful book on women in a mining strike and a number of the books on CW. This area of the relation of women to the principal corporate structures of the work world is understudied in WO, although there is some writing on it in the management literature proper.

Unemployment vis à vis Gender, Age, and Labor Markets Generally

The literature on unemployment (UE) is extensive and cosmopolitan, the latter fact reflecting the larger size and greater political importance of UE in Europe. The literature is chiefly focused on age and labor market policies. There is some question whether increased female labor force participation has increased UE generally, with mixed evidence (McCarthy 1988, Furaker et al 1990). DeBoer & Seeborg (1989) attribute the sharp convergence of male and female UE rates partly to changing labor force attachment and partly to the interaction of occupational sex segregation with sectoral differences in UE.

A large literature considers UE and age, focusing on the beginning and ending of the work life course. There is increasing attention to the fuzziness of entry into and departure from the labor force. Hartley (1989) considers the impact of youth wage rates on labor force entry; Singell & Lillydahl (1989) consider various definitions for youth UE; Sullivan (1989) considers the interaction of crime and work in youth; and Weis (1990) examines a high school's responses to deindustrialization. An even larger literature considers retirement, now recognized to proceed by fits and starts. (Weiller 1989 gives a historical background.) Some consider the response of older workers to career displacements (Rife & First 1989), while others work at disentangling early retirement from UE (Casey & Laczko 1989), itself complicated by older workers' interpretation of their segregation into special labor markets (House 1989). Schulz et al (1991) and Szeman (1989) study the complex interactions of policy in this area, the latter being an elegant study of the emergence of UE in a pensioners' labor market.

Labor market policy provides the other major theme in this area. A number of writers emphasize the wide diversity of policies (Evers & Wintersberger 1990, Grahl & Teague 1989) while noting that different policies may yet produce similar UE rates. A surprising number of works refer, directly or indirectly, to the cultural construction of unemployment, by workers and policy makers. This construction can be analyzed at the national level (Janoski 1990), within a particular policy (Miller 1991), or among individual workers (Pappas 1989, Mandler 1990. See also Salais et al 1986). Work on UE is beginning to be complemented by sophisticated work on structures of employment itself. Korver (1990) and Jacoby (1991) provide general histories of forms of American employment, while DiPrete (1989) gives a detailed analysis of the Federal civil service.

Unions and Labor Relations

The literatures clustered around the union and IR topics are much more international than the literatures around gender. This reflects not only the currently declining fortunes of American unions, but also the greater institutionalization and political importance of both organized labor and labor policies in other advanced countries.

There are many case studies. Bracho (1990) examines the demise of a Mexican cadre union at the hands of peasant and worker confederations. Berger (1990) studies gender conflicts in a South African canning union, noting the interaction of anti-apartheid politics and union politics. Barber (1990) studies a fish processing plant union, emphasizing that labor action is possible only when larger structural circumstances interact with local cultural values to provide opportunities for locally meaningful action. The emphasis on interaction of micro and macro reaches its peak in Cornfield's (1990) massive study of the furniture workers' union, where status conflicts generated by external pressures drive union change.

A number of studies consider broader issues like the emergence of overall styles of IR. Patmore (1988) tells how arbitration procedures in Australian railroading created openings for union activity, while Kelly (1988) considers the much later emergence of tripartite structures in Australian IR. Hertle & Kadtler (1990) see the move of German paper and ceramics unions toward industry-wide policy as key to union success. Teage (1989) considers attitudes of European workers toward transnational unions possible under the EC (attitudes are mixed.) Here too, the theme of culture makes an appearance. Cohen (1990) attributes much of the peculiarity of American labor relations to the influx of British workers into a labor system that had had no phase of large-scale domestic production, although Haydu (1989) finds that effects of changes in production patterns swamp those of cultural differences in his study of open shops early in the twentieth century.

The literature on collective bargaining proper is another of those relatively complex literatures whose chief implication is that complex conjunctures are central. Miller & Canak's (1991) case study of public sector collective bargaining in Florida provides a good example, explaining why local conditions undermined the "normally expected" effects of the "usual variables." Public sector collective bargaining groups are equally a focus for Troy (1990), who wants to explain their strength, and Gagnon (1990), who wants to explain their divisions. There are the usual attempts to explain the dominance of bread and butter issues for American unions (e.g. Taplin 1990) when contrasted with broader, neocorporatist bargaining in Europe.

More general work continues the same themes. Clark's (1989) study of American union decline in the 1980s again focuses on a conjunction, in this case of internal tensions, economic restructuring, and conscious anti-union policy. Baglioni & Crouch's (1990) review of European IR sees less convergence of forces and consequently more various outcomes, although noting the general shift in industrial relations toward management initiative and dominance.

Also repeated in this broader literature is the emphasis on the culture, the symbolic and subjective representation of work. Sometimes, this involves direct study of worker attitudes to work itself, as in Lincoln & Kalleberg's (1990) quantitative attempt to distinguish Japanese & American attitudes toward work. In a different style, Vallas (1991) opposes standard hegemony theories of worker attitudes toward management, while Bodnar (1989) turns to detailed oral histories to reconstruct collective constructions of life in an automobile plant. A variety of work considers political attitudes of workers and managers, the adversarial attitude often being singled out (particularly in applied literatures) as "problematic." [See Peck & Hollub (1989) for an empirical study of this situation, and Lambelet & Hainard (1989) for a theoretical one.] Survey studies of workers document precisely that Michelsian view of union leaders (Golden 1990) whose error Cornfield (1990) was at pains to show in his book.

Study continues of worker participation; essays in Sirianni (1987) provide a useful cross-section of these. Grootings et al (1989) also review the area. A more analytical attempt is by Tsiganou (1991), who places all the new participative schemes in a common and explicit comparative framework. A few writers (e.g. Wever 1989) have begun to examine the specific conditions for participation schemes with standard methods. Results seem to suggest as crucial the conjunction of union security, union economic leverage, and some common vision between labor and management. The absence of these may explain the concern, here and there in the literature, that the current wave of worker participation schemes is over.

Unions and Race

The exciting and angry literature on unions and race is largely within the discipline of history. It is today making a transition that the historical literature on labor and gender made more than a decade ago: a transition in which the "good guys" of the new labor historians are recognized to have partaken not only of the best but also of the worst of their cultures. Asher & Stephenson (1990) provide a collection looking on the positive side of race relations within the labor movement. Hirsch's (1990) immensely detailed study of Chicago labor focuses on culturally rooted ideologies and gives a more complex, and on balance more negative, picture. There has been frank debate about racism in the unions, particularly in the UAW, sparked by the work of Hill (1987, 1989). Responses to Hill's analysis range from mild apologetics through agonized fence-sitting to angry denial. The reader may choose his or her own version. (See various writers in *New Politics* 1:3, 1987, and *International Journal of Politics, Culture and Society* 2:3, 1989.)

Work and Culture

The area of work and culture has several parts. The first concerns ethnicity. Most study of ethnicity and work is done in books, perhaps because what we often take as a simple variable breaks down, on serious examination, into a diverse bundle of phenomena. Studies here include Jesuadson's (1989) examination of the interaction of state, ethnic groups, and external forces in Malaysia, Bourgois's (1989) study of the diversity of ethnic groups on a banana plantation, and Peled's (1989) analysis of class and ethnicity among Jewish workers of the Russian Pale. As is usual in books, there is emphasis on complexity and contingency, although all three take environing economic forces as exogenous and all emphasize (especially Peled) the construction of ethnicity in the face of work interactions. A somewhat related literature takes up the impact of national level cultures on transnational phenomena like capitalism, a topic reviewed by Clegg & Redding (1990). (See also Gullick 1990 on expatriate managers.) There is much writing, now more now less applied, on "organizational culture" (e.g. Paules 1990, D'Amico-Samuels 1990).

By far the most exciting study of culture and work concerns the imagining of work itself. Some of this study has concerned occupations (e.g. Symonds 1991, Whalley 1987). But the most exciting work concerns occupational statistics. A handful of people—Margo Anderson (Conk 1980), Simon Szreter (1984, 1992), and others—have definitively shown that we cannot take census occupational categories as immediately meaningful, either synchronically, as in the DOT, or diachronically, as in the quixotic effort of the Minneapolis 1880 Census Project to render "comparable" all US occupational classifications back to 1880. In France, official statisticians themselves lead the way in this reanalysis of occupational categories; Alain Desrosières of INSEE invented the 1982 occupational classification, and he himself (Desrosières & Thévenot 1988) has written the analysis deconstructing it. Robert Salais and others (1986) have written an equally brilliant book on the concept of unemployment. [Work on this area has appeared in such diverse places as Finland (Kinnunen 1988) and Portugal (Ravara 1988), work I am sadly unable to read.] A central driving force in this reappraisal of occupational categories has been recognition of their facile occlusion of women's work, a subject investigated at length by Higgs (1987), Folbre & Abel (1989), and others. Beechey (1989) has recently reopened the topic with a focus on its implications for cross-national comparisons of womens' work. It is absolutely certain that analysis of the culture of work will be one of WO's major future topics.

Professions

Professions continue to be the most effectively theorized of occupations. To be sure, a few individual level studies are found (Johnson 1990 and Jolly et al 1990), particularly on gender matters. And the steady flow of case studies continues (Crawford 1989, Curry 1990, Galanter & Palay 1991, Landon 1990, Stebbins 1990), as does comparative study, at least of lawyers (see the three volumes of Abel & Lewis 1989). But theorizing dominates. After nearly two decades under the "professional dominance" paradigm, sociologists of medicine finally recognized that medicine's precarious position demanded more complex theories (see the special issue of *Milbank Quarterly* 66:Supplement 2, 1988). A special issue of *Sociologie et Sociétés* (20:2, 1988) shows far more bravura. Catherine Paradeise (1988) construes professions in terms of labor markets, while Magali Sarfatti Larson (1988) turns to Foucauldian fields of discourse and Elliott Krause (1988) to relations between state and profession. Of these three lines of attack, Krause's appears to be most prominent today, partly because of policy debates on professionals within the EC. (See also essays in Torstendahl & Burrage 1990 and Burrage & Torstendahl 1990). We can also expect more studies of gender and professions, although the mechanics of women's exclusion and of the relations between male and female professions are pretty well-studied. As yet unstudied in any real detail is the gendering of the concept of expert itself in the nineteenth century.

Abbott (1988) attempted to recast the area with three basic arguments: that professions could not be studied individually but only within an interacting system, that a theory of professions had to embrace not only culture and social structure but also intra-, inter-, and transprofessional forces, and that the development of professions would necessarily be a matter of complex conjunctures. None of these arguments has had much impact. The jurisdictional studies Abbott called for have not appeared; linear studies of individual professions continue to dominate (e.g. Wenocur & Reisch 1989, Junqueira Botelho 1990, Brain 1991). Moreover, recent studies have emphasized either the cultural (Brain & Larson) or the social structural (like most work on relations with the state), and none systematically pursues multilevel analysis. Finally, although ridiculed by Abbott, the search for "determining variables" continues (e.g. Raelin 1989).

Theory

Capital is surely the archetypical examination of work: a study ranging from micro to macro, from social structure to (once in a while) culture, systematically drawing together a theory of economy, of work, of organization, and of association and placing the whole within a formal conception of the historical process. It is therefore not surprising that most synthetic studies of work are Marxist in lineage if not in fact. Yet much of what seems general theoretical work is simply macro rather than synthetic. Thus we have literatures on macro topics like classes (Zeitlin 1989, Swenson 1989, Laba 1991), organizations (Fligstein 1990, Chandler 1990), and technology (Wright et al 1987, Ling 1990, Nash 1989, Morgan & Sayer 1988). There are also various critiques of economic reasoning (Block 1990, Friedl & Robertson 1990). But true multilevel work is mainly within case studies, rather than in the theoretical literature itself. In particular, the problem of transnational capitalism's impact on individual localities has drawn some interesting multilevel work.

CONCLUSION

The theoretical problem of reconciling the micro and the macro is general in sociology and crucial in WO, as Kalleberg (1989) recognizes. But our official theorists do little more than relabel the problem with new words like structuration. A serious theory of micro to macro can emerge only from an empirical area. In WO, it will emerge from people working with multilevel data on work: data that brings together exact career information (micro), network structure among careers and jobs (meso), and occupational/organization level information (macro) on occupations and work structures in conflict and process. At present, only historians and historical sociologists have taken the time to gather such datasets, and usually only on professions or skilled laborers. In the meantime, lack of theory covering multiple levels in both space and time makes most pieces of quantitative research on work mutually irreconcilable and hence meaningless. It remains true that for all our modern insights, there is only one truly general theory of work, that of Marx.

Yet all the formal attractions of Marx's work cannot hide its present problems—its assumptions about the nature of work (see Seidman 1991) as much as its magnificent failures as a guide for policy. And ironically, although the formal beauty of Marx's theories means little to the individualist literature on work, in the hands of many current mainstream workers the spiritual values of his theory—the union of theory and practice and the hatred of oppression—have become that literature's chief cornerstone. For the last decade or more, the best study of work has without question come from researchers whose passion to right wrongs spoke in every sentence. In my view, that commitment is beginning to obscure our understanding. That studies attend to diversity among women only when men are completely off-stage is one indicator, as is the common treatment of men as a unified, homogenous, oppressive group. Another is the insistence that "there is no sorrow like my sorrow," for example, the peculiar focus on comparable worth across gender to the exclusion of other differences (q.v. Mount & Ellis 1989, Hine 1989.) Still another is the common anger at research subjects, both past and present, who refuse to recognize what we now "know" to be their oppression. More fools they.

The move of historical studies of gender and work toward explicit recognition of the complex, contingent responses of men and women to changes in their world is a move in the right direction. Like Marxist study of work before it, feminist history of work has been led by its focus on lived experience to recognize the complex contradictions of life that ideologies often ignore. But there is a long way to go. At the deepest level, those focused on oppression have to confront anti-hierarchicalism for the culture-bound, Western ideology that it is, perhaps by frankly confronting what kinds of hierarchy they might consider just. This problem may be under discussion off-stage, in theoretical writing I don't know. But here in the area of WO, our day-to-day experience is that most of the really exciting work—the work with new data, new questions, and new solutions—has a consistently politicized tone, while most of the apolitical work is intellectually sterile. We die between.

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Chapter 40

THE ANALYSIS OF LABOR MARKETS USING MATCHED EMPLOYER–EMPLOYEE DATA

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Abstract

Matched employer–employee data contain information collected from households and individuals as well as information collected from businesses or establishments. Both administrative and sample survey sources are considered. Both longitudinal and cross-sectional applications are discussed. We review studies from 17 different countries using 38 different systems for creating the linked data. We provide a detailed discussion of the methods used to create the linked datasets, the statistical and economic models used to analyze these data, and a comprehensive set of results from the different countries. We consider compensation structure, wage and employment mobility, and the relation between firm outcomes and worker characteristics in detail. Matched employer–employee data provide the empirical basis for further refinements of the theory of workplace organization, compensation design, mobility and production; however, the arrival of these data has been relatively recent.

JEL codes: J3; J6; C1; C8

1. Introduction

> On the empirical side of these questions, the greatest potential for further progress rests in developing more suitable sources of data on the nature of selection and matching between workers and firms. Virtually no matched worker-firm records are available for empirical research, but obviously are crucial for the precise measurement of job and personal attributes required for empirical calculations. Not only will the availability of such data produce sharper estimates of the wage-job attributes equalizing differences function but also will allow more detailed investigations of the sorting and assignment aspects of the theory, which have not received sufficient attention in past work. (Rosen, 1986, p. 688).

> The recent stress on the role of specific as opposed to general human capital and the development of agency theories of the employee–employer relationship may result in the modification of some of the received doctrines but these theories also serve to enrich the scope of the theory by pointing towards interesting and potentially important connections between wages, job mobility and institutional practices. Future progress in this area will hinge crucially on the development of data which links information on the individual characteristics of workers and their households with data on the firms who employ them (Willis, 1986, p. 598).

In the decade since Sherwin Rosen and Robert Willis wrote these words, economists have made enormous strides in finding and using matched employer–employee data. This chapter reviews about 100 studies from more than 15 different countries. Virtually all of these papers have been written in the last 5 years and many are still only available in working paper form. As this chapter was being prepared more than 40 new papers using matched employer–employee data appeared as a part of a conference organized specifically to investigate this issue.[1]

From the many papers that we discuss below, two broad themes emerge. The first is the relative importance of person and firm variables in the determination of compensation. The second is the relative importance of individual mobility in relation to firm-specific employment adjustments. These questions have now been addressed by dozens of researchers. In contrast to many other areas of empirical labor economics, the results we discuss on these questions have largely been estimated from European, and not American, matched employer–employee data, a situation that was foreshadowed by evident advance of the European statistical systems in providing support for the microeconometric analysis of human resource decision making.[2] It is clear from the degree of professional interest in these research efforts that the availability of the type of data Rosen and Willis called for in the original handbook has already produced many important new results.

2. The different types of matched employer–employee datasets

In order to describe the potential that matched employer–employee datasets offer for labor economists, we begin by describing the datasets that exist and some of the basic applications analyzing compensation, mobility, unemployment insurance and other aspects of the labor market. Table 1 presents a complete summary of each of the datasets we describe as well as basic references for further information and applications.

[1] The International Symposium on Linked Employer–Employee Data was held on May 21–22, 1998 in Washington, DC. The preliminary versions of papers from this conference are discussed in this chapter. See Lane et al. (1997a) for an earlier review.

[2] See Abowd and Kramarz (1996b).

Two important dimensions distinguish the matched employer–employee data that we present. First, some are cross-sectional datasets while others are longitudinal. Second, some sampling designs focus on the employee while others use the firm as the primary unit of analysis.[3] When considering issues of representativeness, we show that certain samples, with a longitudinal component, are representative of the target population in the cross-section without being dynamically representative. In particular, certain sampling techniques do not permit entry and exit of individuals from the labor market and/or entry and exit of firms, phenomena which cannot be ignored with matched employer–employee.

Most labor economists are not familiar with the methods used to construct matched employer–employee data. We have, therefore, taken some care to describe the technical details so that potential users of these data can use this chapter to select data sources that are appropriate for the questions they wish to investigate.

2.1. *Representative cross-sections of firms with representative data on workers*

We begin with the basic design of datasets in which both the sample of firms and the sample of individuals are cross-sectionally representative of the population under study. We start by describing the French program since it follows closely a structure that has been widely adopted across Europe. The Wage Structure Surveys (Enquête sur la Structure des Salaires, ESS), performed by the French National Statistical Institute (INSEE) in 1986 and 1992, were initiated in 1966 by the European Statistical Office (ESO). However, after the 1966, 1972 and 1978 surveys, the ESS was abandoned by the ESO. INSEE decided to resume this survey because of the importance of the information collected at each round and the uniqueness of the statistical design. The ESS collects data on the structure and amount of individual compensation within a sample of establishments from the manufacturing, construction and service industries.

The sampling frame has two stages: at the first stage, production units are sampled; at the second stage, individuals employed at these sampled units are sampled. The target population is all establishments with at least ten employees in general industry. In the construction and in the service industries, the first stage sampling unit is the firm. Furthermore, agriculture, transportation, telecommunication and services supplied directly to individuals are excluded from the scope of the ESS except for insurance, banks and all industries where services are also supplied directly to firms. The universe is constructed from the SIRENE system, a unified database recording all existing establishments and firms in France. The sampling rates are stratified according to the industry, the region, and the size of the unit – from unity for the establishments above 500 employees to 1/48 for establishments between 10 and 20 employees. The sampling frame for the employees at sampled units is based on the employee's year and month of birth. The sample is exhaus-

[3] Hildreth and Pudney (1999) provide an interesting methodological discussion of the statistical properties of many of these methods of creating matched datasets.

Table 1
Comparison of matched employer–employee data sources from different countries

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
Algeria	Algiers Regional Manufacturing Establishments	42 manufacturing enterprises in the Algiers area, 1,000 employees of these firms	1992	Daily wage, employee demographics, education, seniority, other work experience; employer information is limited to detailed industry and employer ID	Data for a developing country	Chennouf et al. (1997)
Australia	Australian Workplace Industrial Relations Survey	Probability sample of workplaces with 20 + employees. Probability sample of employees at that workplace	1995, (earlier survey without the employee questionnaire: 1990)	Workplace qualitative and interval information on wages, profits, productivity, competition. Detailed employment and industrial relations data. Individual data on wages, hours, demographics and unions	Focused on industrial relations issues. Close collaboration with the British Workplace Industrial Relations Survey permits comparative analyses	Callus et al. (1991), Morehead et al. (1997), Alexander and Morehead (1999), Blanchflower and Machin (1996)
Austria	Social Security Files Sample (SSFS, Austria)	Probability sample of firms (1/50)	1975–1991	Simple individual demographic variables, detailed earnings and labor force variables; establishment and firm IDs from the SSFS	Exhaustive within establishments; no longitudinal information on individuals in the sample	Winter-Ebmer and Zweimüller (1997)

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
Belgium	Social Security Administrative Records	Universe of private firms	1978–1985	Individual earnings histories, demographic variables, and broad occupation, firm-level accounting data for larger firms	Universe of private employees permits detailed mobility analyses	Leonard and Van Audenrode (1996, 1997), Leonard et al. (1999)
Canada	Workplace and Employee Survey (WES)	Clustered probability sample of establishments	1996 (pilot), 1997	Detailed establishment information from the human resource manager; detailed demographic, labor force, and earnings variables; establishment IDs from Statistics Canada	Designed to collect longitudinal information on establishments (including birth, death, and mergers); no longitudinal information on workers	Picot and Wannell (1996)
Canada	T-4 Supplementary Tax File	1% simple random sample of individuals ever filing a tax return	1975–1993, ongoing	Individual age, sex, taxable earnings, taxpayer ID and employer ID	Very long employment histories	Morissette and Berube (1996), Baker and Solon (1997)
Denmark	Integrated Database for Labor Market Research (IDA)	Universe of the Danish population based on the person ID used in Danish government registers	1970, 1980–1994, ongoing	Detailed demographic and labor force variables; employer reported earnings; employer IDs from the Danish establishment register	Complete census; individuals who are unemployed or not in the labor force are included	Albaek-Sorensen (1999)

Finland	Employment Register matched with manufacturing establishments in Register of Establishments	Census of employed persons and census of manufacturing establishments and plants with 5 + employees	1988–1994, ongoing but 1995 changes in industrial register present comparability problems	Earnings, other income, education, demographics, employment history for workers; Output, value added, inputs, price indices, some capital measures	Because both sides of the match are based on registers, the coverage is very good when supplemental data from other sources are added	Laaksonen et al. (1998)
France	Déclaration Annuelle de Données Sociales (DADS), formerly DAS; Bénéfices Industriels et Commerciaux (BIC); Bénéfices Réels (BRN), Echantillon Démographique Permanent (EDP); Enquête Structure des Emplois (ESE)	1/25th of private and semi-public workforce (born October, even years); supplemental individual data for 1/10th from the EDP; employer information from BIC (larger enterprises), BRN (smaller enterprises) and ESE (establishments)	1976–1995, ongoing	Individual data: earnings, days worked, payroll taxes, occupation, industry, demographic data, education, detailed individual data from EDP; longitudinal firm data from sources keyed to Siren/Siret: production, value added, operating income, assets, employment, imports, exports, prices	Based on a set of databases that permits any data coded by firm or person identifier to be added. Longitudinal for firms and individuals	Abowd et al. (1999a), Kramarz and Roux (1998), Margolis (1999)
France	Déclaration Mensuelle de Mouvements de Main d'Oeuvre (DMMO), matched to the sources cited above	All establishments with 50 + employees complete the monthly questionnaire,	1987–1990, ongoing	Individual data: demographics, type of contract, type of entry, skill-level for all entries, seniority at exit, type of exit,	Permits monthly analysis of employment flows as well as job creation and destruction	Abowd et al. (1999b)

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
		which includes the reports on each movement for the individual employees		and skill-level for all exits; establishment data: employment, skill distribution, profit/loss		
France	Enquête Emploi (EE) and Enquête sur la Technique et l'Organisation du Travail auprès des Travailleurs Occupés (TOTTO)	Clustered probability sample of domiciles 1/300, longitudinal with 3 years in sample. Employer identifiers since 1990	1990–1996, ongoing1987 and 1993 new technologies supplement	Full complement of household-based labor force variables; periodic topical supplements; longitudinal firm data from sources keyed to Siren/Siret; new technologies supplements have a full complement of computer and computer-assisted production questions	Overlapping samples with 3-year rotation groups permit dynamic analyses; employer IDs for establishments (Siret) permit linking to establishment or firm data	Entorf and Kramarz (1997, 1999), Entorf et al. (1999), Kramarz (1997)
France	Enquête Structure des Salaires (ESS)	Probability sample of establishments with 20 or more employees; Probability sample of employees at the establishment	1967, 1978, 1986, 1992	Very detailed job and compensation descriptions, earnings; establishment level work place organization; longitudinal firm data from sources keyed to Siren/Siret	Large representative samples of employees within establishment; employer IDs for establishments (Siret)	Rotbart (1991), Kramarz et al. (1996)

France	Enquête Formation Qualification Profession (FQP)	Clustered probability sample of domiciles 1/1,000	1993	Full complement of labor force variables; detailed education and training, apprenticeships; retrospective from 1988; longitudinal firm data from sources keyed to Siren/Siret	Employer IDs for establishments (Siret) permits addition of other information	Goux and Maurin (1997)
Germany	Beschaftigungs-stichprobe (BS) matched with the Leis-tungsempfanger-datei (LD)	Probability sample of the Historikdatei (HD), 1/100th sample, of the Bundesanstalt für Arbeit (BfA)	1975–1990	Simple individual demographic variables (sex, education, nationality), gross earnings; benefits for the unemployed; employer IDs from the HD	Longitudinal information on individuals even when unemployed or on training programs; plant-level statistics from the HD	Bender et al. (1996), Dustmann and Meghir (1997)
Germany	Gehalts- und Lohnstruktur-erhebung (GLS) matched to social insurance registry	Multistage probability sample of establishments, probability sample of employees. Lower Saxony.	1990, 1995	Employer supplied detailed data on the structure of compensation and conditions of employment in October. Demographic, education, occupation for employee	Two representative cross-sections, 5 years apart with very similar structure to ESS in France	Stephan (1998)

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
Italy	Ricerche e Progetti (R&P)	Universe of private firms (industrial and service) and records of self-employed	1985–1991, ongoing	Social security earnings, wage supplements, months, weeks or days paid, occupation, employment contract type	Longitudinal information on workers and firms, transitions to self-employment can be included. Availability of universe permits different sampling schemes	Contini et al. (undated)
Japan	Establishment Census (EC) matched with Basic Survey on Wage Structure (BSWS), Census of Manufactures (CM) and Census of Commerce (CC)	EC is a census of establishments with 5 employees or more (10 + in public establishments). BSWS, see below, CC every 3 years and CM annual	1991–1994, ongoing, not all years for all sources	Wages, bonuses, seniority, occupation, employee demographics and education from BSWS, detailed business data from CM and parts of CC, varies by industry	Some longitudinal information on both workers and firms	Hayami and Abe (1998)
Japan	Basic Survey on Wage Structure (BSWS)	Probability sample of all establishments with at least 5 employees or government sector if covered by the National Enterprise Labor Relations Law or by the Local Public Labor Relations Law and at least 10 employees	1982–1994, ongoing	Simple information on the establishment; simple individual demographic and labor force variables, detailed earnings	Large sample within establishments	Abe and Sofer (1996)

Nether-lands	Wage Survey, Production Survey, RD Survey and Survey of Manufacturing Technology	Probability sample of firms, simple random sample of employees at each firm	1979, 1985, 1989, ongoing	Detailed individual and job characteristics, gross weekly earnings, hours worked per week, firm data on inputs, outputs, value added, profits, R&D activities (larger firms), computer technologies used	Very comprehensive set of repeated cross-sections with detail on both the individual and the firm	Boon (1996)
Nether-lands	Ministry of Social Affairs and Employment (AVO)	Probability sample of firms, probability sample of employees of those firms	1993, 1994	Detailed salary information, separation reasons, demographic data, seniority; firm level aggregates of these variables and employment	Two observations (successive Octobers) for each employee	Hassink (1999)
Norway	Employer–employee Register and Education Register	Universe of the Norwegian population based on the person ID used in Norwegian government registers	1986–1994, ongoing	Detailed demographic and labor force variables; employer reported earnings; employer IDs from the Norwegian Employer–employee establishment register	Not a sample; individuals who are unemployed or not in the labor force are included	Salvanes and Forre (1997)

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
Portugal	Social Security Files Sample (SSFS, Portugal)	Clustered probability sample, 1/5 of all firms	1983–1992, ongoing	Simple individual demographic variables, detailed earnings and labor force variables; establishment and firm IDs from the SSFS	Exhaustive within establishments; no longitudinal information on individuals in the sample	Ministério do Emprego e da Segurança Social (1993), Cardoso (1997)
Sweden	Register of Income Verifications, Register of Jobs and Other Activities, Register of Employment, Register of Enterprises and Register of Establishments	All registers are censuses of the relevant population.	ongoing, dates depend upon specific application	Earnings and income, job characteristics, demographics, enterprise and establishment characteristics	Surveys with more detailed information can be linked to any of the component registers	Tegsjö and Andersson (1998)
Sweden	Labor Force Survey (Arbetskraft-sundersök-ningarna, AKU) matched with the Registers of Employment, Enterprises and Establishments	Probability sample of households, census of business establishments with at least one employee	1987–1993, ongoing	Detailed employment data from the AKU, other data as described above	Makes use of the register system described above	DiPrete et al. (1998)

UK	Panel Study of Manufacturing Establishments (PSME)	Probability sample of establishments, most recently hired employee and one randomly sampled production employee	1994, 1995	Detailed employer information on the personnel, financial, investment policy; detailed demographic and earnings variables on individual	Longitudinal in the establishment, employees are not followed due to legal restrictions in the UK	Hildreth and Tremlett (1994, 1995)
UK	New Earnings Survey (NES), Joint Unemployment and Vacancies Operating System (JUVOS), Annual Census of Production (ACOP)	1/100 sample of employees enrolled in the Pay As You Earn (PAYE) tax system linked to administrative universe of unemployment system and universe of firms with 100 or more employees. Probability sample of smaller firms	1994, 1995	Employee information on weekly earnings, demographic data, unemployment spells. Firm information on inputs, production, profitability	The links can be used along several dimensions to follow individuals in and out of employment and/or to follow individuals from firm to firm	Hildreth and Pudney (1999)
UK	Workplace Employee Relations Survey	Probability sample of workplaces, 25 employees per workplace	1997, earlier surveys without the employee segment: 1980, 1984, 1990, called Workplace Industrial Relations Survey	Workplace qualitative and interval information on wages, profits, productivity, competition. Detailed employment and industrial relations data	Focused on industrial relations issues. Close collaboration with the Australian Workplace Industrial Relations Survey permits comparative analyses	Cully (1998), Blanchflower and Machin (1996)
UK	British Household Panel Study	Probability sample of households, details of employer data not available	in progress	Detailed data on individuals in the household, including labor market earnings, education and demographics.	Design permits analysis of the effects of employer variables on household outcomes	Hildreth, private communication

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
United States	Worker-Employer Characteristics Database (WECD) and New Worker-Employer Characteristics Database' (NWECD)	Manufacturing establishments from the Longitudinal Research Database (LRD), a probability sample; matched to 1990 Census of Population long form responses; NWECD establishments from the Standard Statistical Establishment List (manufacturing and non-manufacturing)	1990 match	WECD: longitudinal data on establishments income, balance sheet, investments; NWECD: employment and sales; Both datasets: full complement of labor force variables and household variables from the Census of Population	Very large samples within establishments	WECD, Troske (1998); NWECD, Bayard et al. (1998)
United States	State Unemployment Insurance Systems	Simple random samples from state unemployment insurance records Mexico, Pennsylvania, South Carolina, and Washington	various years, matched on the individual ID from Georgia, Idaho, Louisiana, Maryland, Missouri, New	Earnings and employment data required to calculate UI benefits; employer UI-related data (tax rates, taxable compensation); employer IDs from the federal employer ID system	Some states have labor force variables (sex, education, etc.) for a subsample who received UI benefits, other states have demographic data for representative samples, others have no demographic data	Jacobson et al. (1993), Anderson and Meyer (1994)

United States	Maryland Unemployment Wage Records, Current Population Survey, Standard Statistical Establishment List (SSEL)	Universe of Maryland UI records matched to CPS (probability sample); Employers matched using the SSEL	1985–1997	Individual and household data from the Current Population Survey including demographic, earnings, education; establishment data from the SSEL (employment, payroll, sales, industry)	Limited to the State of Maryland. Longitudinal information on UI earnings matched to longitudinal information on the SSEL	Lane et al. (1999)
United States	National Longitudinal Survey of Youth '79 (NLSY-79)	Clustered probability sample of individuals aged 14–21 on January 1, 1979	1986–1994	All variables from the public-use NLSY files; employer IDs from private lists, Compustat and CRSP	Unique IDs for all available employers on the NLS; employer data for publicly-held firms, some data for governments	Abowd and Finer (1998)
United States	Survey of Consumers (University of Michigan Survey Center)	Clustered probability sample	September 1991-March 1992	All variables in the Survey of Consumers; employer IDs from Dun and Bradstreet	Some employer data for 700 matches	Brown and Medoff (1996)
United States	Employment Opportunity Pilot Project (EOPP) and Multi-City Study of Urban Inequality (MCSUI)	Probability sample in metropolitan areas, data on a representative employee and the most recently hired employee	1982, 1993 (repeated the design of 1982 survey)	Detailed employer information from the human resource manager; labor force variables on individual	Heavy focus on training and training related variable	Bishop et al. (1983), Holzer and Reaser (1996).

Table 1 (*continued*)

Country	Name	Sampling plan	Dates	Main variables	Unique features	References
United States	Continuous Work History Sample (CWHS) and Longitudinal Employer–employee Database (LEED)	1/100 sample of Social Security earnings reports	1957–1972, other files continue	Social Security earnings, total employment in the firm, basic demographic variables, some schooling, hours and weeks worked information. Employer and employee identifiers	Most extensive US sample. Internal Social Security files are produced on an ongoing basis	Smith (1989), Topel and Ward (1992)
United States	Survey of Employer-Provided Training (SEPT95)	Probability sample of private establishments with 50 + employees, two employees per establishment	1995	Detailed training information at the establishment level, earnings, seniority, training and demographics for employees	Design permits analysis of both establishments and individuals for population training models	Bureau of Labor Statistics (1996)
United States	Longitudinal Research Database (LRD) and the National Labor Relations Board (NLRB) files	Probability sample of manufacturing establishments (LRD) matched with the annual NLRB election data	1977–1989	Detailed establishment data on inputs, production, costs, production and non-production employment. NLRB election data describe the proposed bargaining unit and election results	Match of union representation vote data to detailed history of the establishment permits studies of effects of new unionization on factor use and production. Establishment data available from 1963	LRD, McGuckin and Pascoe (1988); match to NLRB, Lalonde et al. (1996)

United States	White Collar Pay Survey (WCP) supplement	Probability sample of establishments, simple random sample of employees in certain white collar occupations	1989 (service), 1990 (goods)	Detailed earnings and components of compensation from employer survey, starting pay, demographics, education, seniority	Sample focuses exclusively on white collar occupations	Bronars and Famulari (1997)

tive in small units and the sampling rate is 1/24 in the largest establishments (above 5000 employees).

In the 1986 version of the survey, annual and October compensation are available for each sampled employee. The October compensation for each employee includes all employee and employer-paid benefits but excludes non-wage benefits. It can be decomposed into total wage, overtime compensation and October-specific bonuses. The total annual compensation includes all benefits and bonuses, even those not paid on a monthly basis. Finally, information on the method of pay is given (time versus piece rates, for instance). In 1992, total annual compensation, decomposed as described above, is available but the October compensation is not decomposed.

In both versions of the ESS, occupation, firm-specific seniority, age, country of origin, hour schedule (number of hours and shifts), days of absence are measured for the employee. In addition to this individual-level information, the surveyed unit gives the following information: total employment, existence of shifts and night work, existence of a firm-level agreement, of a branch-level agreement. Since some questions in the 1986 and 1992 versions of the ESS were not formulated identically, the two surveys are not always comparable.

The basic research data files for the ESS contain 16,239 establishments with 678,798 interviewed employees in 1986 and 15,858 establishments with 148,976 interviewed employees in 1992. More detailed technical information on the 1986 version of the ESS is available in Rotbart (1991). The technical report on the 1992 version of the ESS is not yet available.

Salary structure surveys with the same structure as the ESS exists in most EC countries, for instance in Germany (see Stephan, 1998) and the United Kingdom. Unfortunately, the statistical offices in charge of collecting and storing these data have been generally reluctant to let researchers access them. In France, however, the policy for non-INSEE researchers has been more generous (see Arai et al., 1997, among others). Statistics Canada is now in the process of building such a dataset called the Workplace and Employee Survey. Data collection should be completed by the end of 1997. A pilot, designed to be one-fifth of the production version, was conducted in 1996 with approximately 1000 establishments and 6000 workers (see Picot and Wannell, 1996). In the United Kingdom, the Office for National Statistics now allows contracted researchers access to these confidential data.

Salary structure data also exist in Japan, based on a annual survey called the Basic Survey on Wage Structure (see Abe and Sofer, 1996). The universe of establishments sampled every year includes all establishments of the private sector with at least 5 employees and the public sector establishments if covered by the National Enterprise Labor Relations Law or by the Local Public Labor Relations Law and at least 10 employees. Each year, approximately 70,000 establishments with 1.4 million workers are sampled. The survey is conducted during the month of July, with information recorded about the month of June (apart from annual bonuses, which come from the previous fiscal year). General information about the establishment is collected: industry, size, product, enterprise to which the establishment belongs, entry wage for the youngest hires. Information on individual workers includes: sex, age, education, type of contract, number of days and hours worked, experience, job position, June earnings (before taxes), and annual bonuses.

2.2. Representative cross-sections of firms with non-representative data on workers

In this type of data, a sample is designed to be representative of the cross-section of firms (or other business units) in a given year and data on some workers are collected. Some of the surveys have longitudinal or panel components but the sampling frame was, nevertheless, constructed using a universe that was fixed at a particular date. Hence, they are not dynamically representative even though they are representative over time of the business units and employees at risk to be sampled at that date.

The best example is the European Commission-sponsored research data collected in the United Kingdom, called the Panel Study of Manufacturing Establishments (PSME). The description is based on Hildreth and Tremlett (1994, 1995). The stage of the sample is based upon an establishment universe called a business location and defined as the activities of a single employer at a single address. The sample of business locations is based on British Telecom's (BT) business line records. If an establishment has a business telephone line, it is included in the population at risk to be sampled. As seems natural given the origin of the sample, the BT sample provided a contact phone number as well as an establishment name, and address. This allowed the interview to be conducted over the telephone. BT also reported the industry classification as well as size of the establishment. The sample was restricted to manufacturing establishments only (Divisions 1–4 of the 1980 Standard Industrial Classification, SIC code). Using this information, the frame was stratified according to area, size, and industry. Details of the sampling scheme can be found in Hildreth and Tremlett (1994). The initial sample comprised 881 establishments of which nearly a quarter (23%) was found to be out-of-scope for the survey.

From the original 881 establishments, 682 were in the scope for interview. Interviews were conducted between February and April 1994 using Computer Assisted Telephone Interviewing (CATI). The average interview lasted 45 min and was conducted by interviewers at the Social and Community Planning Research (SCPR) telephone interviewing unit. The questionnaire covering a range of areas of the establishment operation: ownership and control, markets and products, innovation and investment, employment and human resources, financial performance, and, finally, detailed information on two workers – the most recent hire and a randomly selected employee. There were several respondents at each establishment – the Chief Executive or Senior Manager, the Personnel or Human Resources Manager, and the Chief Accountant or Financial Director. Of the original sample of 682 establishments within scope for the survey, 430 completed interviews, of which 398 have consistent information on the establishment. Not all establishments gave complete worker information on both of the employees. The number of observations for the worker selected at random from the list of production line employees is 339 while the number of observations for employees selected as the most recent hire is 346. Only 312 establishments have complete information for both workers.

The Employment Opportunity Pilot Projects (EOPP) employer survey for the US is based on a very similar sampling scheme as the PSME (the description is based on Bishop, 1994). The survey covers a sample of 3412 employers. It was sponsored by the National Institute on Education (NIE) and the National Center for Research in Vocational Education (NCRVE). Interviews were conducted between February and June 1982. This survey was a two-wave longitudinal survey of employers from selected geographic areas across the country. The ES-202 list of companies paying unemployment insurance taxes provided the sampling frame for the survey. Establishments in industries with a relatively high proportion of low-wage workers have been oversampled. The survey was conducted over the phone and obtained a response rate of 75%.

The second wave tried to interview all of the respondents from the first wave survey. Approximately 70% of the original respondents completed surveys for the second wave. Seventy percent of the establishments have fewer than 50 employees and 12% have more than 200 employees. In large organizations, the main respondent was most often the personnel officer in charge of hiring. Employers who received the full questionnaire were asked to select the "last new employee your company hired prior to August 1981 regardless of whether that person is still employed by your company." A total 818 employers could not provide information for a recent new hire. The employers who provided information on one new hire were also asked to provide data on a second new hire in the same job but with a different amount of vocational education. Of the 2594 employers who provided data on one new hire, 1511 had not hired anyone else in that job in the last 2 years, and 424 had not hired anyone with a different amount of vocational training for that position in the last 2 years. As a result, data are available for 659 pairs of individuals who have the same job at the same establishment. Missing data on specific questions used in the model reduce the sample to about 480. The questionnaire focused primarily on training activities on the job. See Bishop (1994) for more information on the questionnaire.

2.3. Representative cross-sections of workers matched with longitudinal data on firms

A representative cross-section of workers is often matched with longitudinal data on the employing firms. The data source for the individual workers and the source for the employing firms are not generally coordinated ex-ante, as was the case for the data described in Sections 2.1 and 2.2. In the United States, the Longitudinal Research Database (LRD) – a panel of manufacturing establishments (see McGuckin and Pascoe, 1988) – has been linked by Troske (1998) with the 1990 Decennial Census of Population. In France, the supplement to the 1987 Labor Force Survey on New Technologies contains the firm identifier and the establishment identifier number for most employed workers, which permits researchers to match with the Echantillon d'Entreprises (based on the BIC), a dynamically representative sample of French firms or the Enquête Structure des Emplois (ESE) (see Entorf and Kramarz, 1997, 1999).

We first describe the Worker-Establishment Characteristics Database (WECD) based on Troske (1998). The data for workers were extracted from the 1990 Sample Detail File (SDF), which consists of all households questionnaires from the 1990 Decennial Census of Population long form. The data for establishments come from the 1990 Standard Statistical Establishment List (SSEL), a register of all establishments active in the US in 1990. From the SSEL, a 4-digit SIC code giving the establishment primary industry and a geographic code giving location were extracted. Only manufacturing establishments were retained. Equivalent industry and location information was obtained for the individuals in the SDF through individual responses coded by the Census Bureau (using Census industry codes, however). All workers employed in manufacturing in 1990 who responded to the long form are in the sample file. The number of individual observations is 4.5 million. These individuals were at risk to be matched to an employing establishment.

The matching procedure has four steps. First, Troske standardized the geography and industry definitions across the two data sources. Second, he eliminated all establishments that are not unique in each location-industry cell. Third, he assigned a unique establishment identifier to all workers located in the same location-industry cell. Fourth, he eliminated all matches based on imputed industry or location data in the Census of Population.

To understand the first step, one must know that each Census of Population geographic code consists of a region code, a state code, and a county code. Each county code is further divided into incorporated and unincorporated areas. Each incorporated area gets a unique place code. Finally, in highly populated places, a further subdivision, blocks, is added. Since the 1990 SSEL only contains place codes, which are not the same as these Census of Population location codes, Troske used the Census Bureau's Address Reference List (ARF) to assign blocks to the 1987 SSEL which was then matched to the 1990 SSEL. In addition, the Standard Industrial Codes (4-digits) were recoded into the Census Industry Codes (3-digits).

The second step forces Troske to use only establishments that meet one of these three criteria:

- Establishments that are unique in an industry-block cell;
- Establishments in the same industry-place cell with missing block codes when all other establishments in the same industry-block cell have valid block codes;
- Establishments unique in the industry-place cell.

In the third step, Troske matched individuals using industry-block codes (first group above). Next, all remaining workers were matched to establishments with identical industry-place codes (next two groups). All matches in which the industry or the geographic code were imputed were deleted. Finally, all matches for which the total number of matched workers exceeded the establishment employment were deleted. The resulting dataset contains 200,207 workers employed in 16,197 manufacturing establishments. Troske (1998) describes various tests of the quality of the WECD. On average, 16% of an establishment's work force is included in the WECD. This match rate is correct given the sampling frame of the SDF.

Different measures of average earnings per employee result from aggregating individual data to the establishment level and calculating per employee averages directly from the LRD. These earnings measures are positively and significantly correlated. An analysis of the structure of the establishments shows that large plants and plants located in urban areas are over-represented in the WECD. This induces overrepresentation of white, male, and educated workers in comparison to the original SDF data.[4]

The techniques used to create the WECD have been extended by Bayard et al. (1999) to create a cross-sectional matched employer–employee dataset that includes both manufacturing and non-manufacturing establishments. The new dataset, which is called the New Worker-Employer Characteristic Dataset (NWECD), has not yet been as widely used in empirical analyses; however, the addition of non-manufacturing establishments greatly extends the potential of these data. The authors obtained their individual and household data from the same 1990 decennial Census of Population SDF file described above. The information on establishments was taken from the Bureau of the Census Standard Statistical Establishment List (SSEL), which provides the sampling frame for Census Bureau surveys of establishments in virtually all regions and industries. The main difference between the NWECD and the original WECD is the breadth of establishment data available. The SSEL has only limited employer information (employment, payroll, sales and industry), whereas the WECD, which permits access to all of the LRD data but for manufacturing only, contains detailed longitudinal information on establishments that appear in the LRD.

Entorf and Kramarz (1997, 1999) have constructed similar data for France by matching four different INSEE sources. The basic sources are the "Enquête Emploi," 1985–1987, a single rotation group from the French Labor Force Survey, and the"Enquête sur la Technique et l'Organisation du Travail auprès des Travailleurs Occupés" (TOTTO) from 1987, a supplement to the labor force survey, which asked questions about the diffusion of new technologies and the organization of the work place. In addition to the usual questions on labor force surveys (earnings, wage rates, tenure, age, education, etc.) the supplement contains a rich source of information on the use (e.g., intensity and experience) of microcomputers, terminals, text processing, robots and other well specified groups of "New Technologies." Likewise, questions concerning the hierarchy of labor and working-time schedules help in drawing more detailed conclusions concerning the impact of new technologies than would be possible by the analysis of usual labor force surveys.

Additional information on employing enterprises (a business unit in American terminology, not an establishment) for individuals in the EE and TOTTO was added using the standardized Siren enterprise identification number, which was coded for the first time in an INSEE survey for this particular year (1987) and survey (TOTTO). This feature of the French INSEE classification system enables the researcher to employ information from corresponding firm-level surveys (such as profits and share of sales going to exports, for instance). Entorf and Kramarz used information from the 1985–1987 period. No informa-

[4] Hildreth and Pudney (1998) consider likelihood corrections for these kinds of sampling problems.

tion on the employing firm in years 1985 and 1986 is available for workers who changed firms between these dates and 1987. Entorf and Kramarz use two additional sources: the "Bénéfices Industriel et Commerciaux" (BIC) and the "Enquête sur la Structure de l'Emploi" (ESE). From the first source, which collects annual information on balance sheets and employment, they use the measure of the annual average full-time employment, the total capital in the firm as the sum of debt and owners' equity (this sum is equal to total assets in the French accounting system), the annual operating income, and, finally, the export ratio computed as the ratio of the firm's exports to its sales. From the second source, which collects information on the employment structure, they compute a proportion of engineers, technicians and managers in the work force and a proportion of skilled workers in the work force, both expressed as ratios using the employment measure described above.

The survey "Enquête sur la Technique et l'Organisation du Travail auprès des Travailleurs Occupés" (TOTTO) was performed in March 1987. It covers a total of about 20 million individuals in civilian employment. The probability of being selected is 1/1000; thus the survey contains about 20,000 workers. Questions concerning the organization of the workplace were asked to wage-earners and salaried employees only, questions concerning the use of "New Technologies" were asked to all members (including civil-servants) of the civilian work force (according to the definition of the OECD). The sample used for cross-section estimation consists of 15,946 wage-earners and salaried employees, based on TOTTO. The longitudinal sample where individual workers are followed at least 2 years and at most 3 years has 35,567 observations. When merged with firm-level information, the cross-section dataset includes 3446 individuals and the longitudinal dataset reduces to 7965 observations. The firm-level data are based on a panel of firms covering the years 1978–1987. The firm-level information comes from an exhaustive sample for large firms (more than 500 employees) and an INSEE probability sample plan for smaller firms. The sample plan provides a weighting variable which is used in subsequent estimation in order to estimate the variance–covariance matrix that is representative of the population of individuals (such that the bias arising from the higher probability of large firms to be in the sample can be offset).

Starting in 1990, most individual-level surveys performed at INSEE contain the same firm-level identification number, the Siren; mentioned above. This means that matched worker-firm data are available on a regular basis. For instance, the "Formation, Qualification, Profession" 1993 survey on education and continuous training has the employing firm for more than 90% of the employed workers in the dataset (see Goux and Maurin, 1997). We will also examine later longitudinal uses of the French Labor Force Survey, a 3-year rotating panel for which the Siren is available in every year a worker is employed.

Other interesting examples of representative cross-section data for the employee matched to longitudinal data for the firm include the Portuguese file used by Cardoso (1997) and the British file created by Hildreth and Pudney (1997). Cardoso used Social Security files (see the discussion in Section 2.4), to construct a random sample of 20% of the firms, stratified by economic activity. For each such firm, information on workers employed in a given year is available–sex, age, skill, occupation, schooling, tenure, earnings split into different components (base pay, bonuses, tenure-related pay, overtime pay), and hours. The sample of firms is designed to be dynamically representative of the Portuguese economy (starting in 1982). Hence, firms were initially sampled in 1983, the first year available. Then, all sampled firms were followed until their death. All new firms are at risk of being sampled at most once. Sampling frames like the one used to construct the Portuguese data make it difficult to follow the workers from firm to firm since the plan does not ensure the presence of the same workers from year to year. The Portuguese data have been used primarily to assess firm-specific wage inequality at different dates (see Cardoso, 1997). Hildreth and Pudney (1999) use the New Earnings Survey (NES), the Joint Unemployment and Vacancies Operating System (JUVOS), and the Annual Census of Production (ACOP), all for the United Kingdom. The different data sources permit dynamic links but the ACOP rules for sampling establishments changed between 1994 and 1995, creating difficulties for longitudinal analyses.

2.4. Representative matched worker-firm panels (administrative origin)

Many matched employer–employee datasets are based on administrative files. In this section we discuss some leading examples.

Every state in the US, except New York, maintains very complete information on quarterly employment and earnings so that the State Employment Security Agency (or State Unemployment Insurance Agency, depending on the state) can manage the state unemployment benefits program. The exact details of these programs may vary from state to state. However, such UI wage records cover almost all of the employment (at least 90% of the work force but more in some states). Self-employed individuals are never covered. Other categories, such as federal and military personnel, employees of the US postal service, railroad employees, employees of religious and philanthropic organizations, those who receive only commissions, and some agricultural employees may not be covered in some states (Maryland is an example; see Burgess et al. 1999).

Starting with the base UI earnings files, the different states have constructed random samples of the eligible work force. The sampling rate varies by state: 5% in Pennsylvania, 10% in Washington State to 100% in Maryland. Eight states participated in an early attempt to coordinate such data, the Continuous Wage and Benefit History Project (Georgia, Idaho, Louisiana, Missouri, New Mexico, Pennsylvania, South Carolina, and Washington, see Anderson and Meyer, 1994, who use these datasets for the period 1978–1984). Apart from the wage amount received by workers (total wages, including tips, commissions, and bonuses, up to a ceiling of $100,000 that may depend on the state), each quarterly record includes a person identifier, a firm identifier–the federal employer identification number (FEIN), and some other firm characteristics such as the industry (4-digit SIC), average monthly employment, total wages, taxable wages and tax rate as computed by the State Agency.

Unemployment benefit claim records for any worker who filed for UI are also available in certain states (for an example, see Anderson and Meyer, 1997. These datasets contain, for each claim filed, the worker's personal identifier, the date the claim was filed, the first pay date and the exhaustion date, the total amount of benefits paid, the reason for work separation, as well as personal characteristics (age, sex, race, schooling). In addition, it is possible in some states, for some firms (mostly publicly traded firms) to merge with financial data using the FEIN. Even though this is possible for only a small fraction of firms – the largest, in general – more than half of the workers are employed in such companies. Hence, financial data, balance-sheet information may be available for a large share of the records at hand.

Lane et al. (1999) recently completed a pilot project in which the information from the State of Maryland UI wage records was matched to data from the Current Population Survey (also called the monthly household survey in the United States) and the Standard Statistical Establishment List (SSEL, Bureau of the Census). The use of data from the Current Population Survey provides demographic, educational and other individual and household data to complement the earnings history in the UI wage records. The SSEL provides longitudinal, but limited, data on the employing establishments.

Topel and Ward (1992) use the Social Security earnings reports made by employers to the Social Security administration, a Federal version of data similar to the state UI reports. The Longitudinal Employer–Employee Data (LEED) contain quarterly information for over one million individuals for the period 1957–1972. In addition to employee and employer identifiers, available individual characteristics are the age, the race, and the sex. Earnings are reported on a quarterly basis (see Smith, 1989). According to Topel and Ward (1992), top-coding problems, common with US Social Security-based data, are minimized because of the quarterly reporting. Jacobson et al. (1993) use both types of data – UI and Social Security – or a subsample of the Pennsylvanian displaced workers that they analyze.

The administrative source from which similar French data files were constructed are derived from records received by the Tax and Social Security Authorities in order to compute the wage-related taxes, to cross-check with employees' own income tax reports, to compute employers' contributions to Social Security, and to manage employees' individual accounts for entitlements to pensions and health benefits. INSEE also receives these files, called the Déclaration Annuelle de Données Sociales (DADS). As in the US, the coverage is very broad, every employer except those employing only domestic staff must report. INSEE files exclude agricultural workers as well as government employees from the statistical operations (all of whom have special social security systems). Information on the establishment consists of: Siren (firm) and Siret (establishment) identification numbers, address, 4-digit industry code (APE), work force (December 31), and total wage bill. For each individual employee, INSEE receives the name, national identity number, occupation, number of hours (since 1993), start and end dates of the employment period, employment status (full-time, part-time, home work, irregular), total compensation (before as well as after deduction of social security contributions), total benefits in kind, and total allowances for business expenses. Because of the work load that the data entry imposes, not all of this information is accessible at all dates. For instance, the employer identifier is only available starting in 1976. The start and end dates of the employment period are not on the research files that, for instance, Abowd et al. (1999a) have used (they only have its length). Starting in 1964, only those workers born in October of an even year were kept in the research files, resulting in a 1/25 sample of the private and semi-public sector employees. The file used in Abowd et al. (1999) includes more than 1.1 million individuals and 500,000 firms for the period 1976–1987. Kramarz and Roux (1998) have extended the dataset to 1995. This new dataset includes approximately two million individuals and one million firms.

Because of the centralized nature of the French statistical system, identical identifiers (firm or individual) can be found in different data sources. It is therefore very easy to match establishments from the DADS with other firm-level or establishment-level data sources such as balance-sheet information. It is possible, subject to the approval of the "Commission Nationale Informatique et Liberté" (CNIL), to match the DADS with other individual level datasets using the person identifier. However, due to the CNIL policy, this has not been done frequently in the past. The most important example is the match between the DADS and the Echantillon Démographique Permanent (EDP). The EDP collects for 1/10th of the population, information drawn from Civil Status registers on marriage, births, deaths, as well as data from the decennial Censuses of Population (in particular, completed education).

The French Déclaration Mensuelle de Mouvements de Main d'Oeuvre (DMMO), used by Abowd et al. (1999b), is another administrative data source in which all establishments with 50 or more employees register all hiring or separations every month. Information on the workers includes age, sex, type of contract, type of entry (shortterm contract (CDD), longterm contract (CDI), or transfer from another establishment of the same firm), skill-level for all entries and age, sex, seniority at exit, type of exit (end of shortterm contract, quit, retirement, firing for cause, firing for economic reasons, transfer to another establishment of the same firm), military service, death, and skill-level for all exits. These movements are usually aggregated at the monthly level by categories of entry/exit and skill-level. Notice that no wage information is available. The data source includes the establishment identifiers required to link to other information on the establishment and enterprise, including employment structure. Thus the data are dynamically representative of establishments and of mobile workers.

Danmarks Statistik has constructed a similar database with longitudinal information on workers and their establishments (IDA, see Leth-Sørensen, 1995) based on administrative registers on individuals. All persons in the population are covered, irrespective of their labor market status, and are identified by their person ID. Starting in 1980, annual information on each person's labor situation at the end of November is available. For persons born after 1960, there are also references to the person IDs of their parents. Notice also that, since the 1970 Census of Population was the first ever to include this person number, it is possible to get information back to 1970. For all employed workers, the employing establishment identifier is known. The information available at the establishment level consists of: years of operation, industry, and location. Many individual characteristics are collected: sex, age, family and marital status, education, employment experience, unemployment history, income, full-time or part-time job, hourly pay, seniority. There are, however, no other data on firms such as balance sheets, production, factor use or financial information. The same kind of data, based on individual registers, are also available in Norway (see Salvanes and Forre, 1997) and Sweden (Tegsjö and Andersson, 1998).

In Japan, an establishment register called the Establishment Census forms the basis of matched data that is dynamically representative. The information in the establishment census has been matched to wage information in the Basic Survey on Wage Structures, a probability sample of establishments with 5 or more employees. Other information on the firms is taken from periodic censuses of manufacturing and commercial establishments. See Hayami and Abe (1998) and Abe and Sofer (1996) for details.

In Germany, starting in January, 1973, in order to collect all the necessary information for unemployment insurance and health-retirement payments, employers have been required to report information regarding any employment relation subject to social security contributions (more specifically, at the beginning, at the termination, and on December 31st for any employee). The reporting form, known as the Historikdatei (HD), is collected by the Bundesantalt für Arbeit (BfA). A 1% sample of the HD has been used by the Institut für Arbeitsmarkt und Berufsforschung (IAB) to construct a research dataset called the Beschäftigungsstichprobe (BS), from January 1, 1975 to December 31, 1990. The information reported in every record includes sex, nationality, education, gross earnings over the spell (with both left- and right-censoring because of the floor and the ceiling in the base formula for the computation of contributions), reasons for interruption of the spell (maternity leave, military service). As in other countries, self-employed individuals as well as civil-servants are not covered by the data. The HD comprises 79% of the labor force in 1979 (see Dustmann and Meghir, 1997, for further references to this data file). In addition to the BS file, the IAB has added information from another administrative data source, the Leistungsempfangerdatei (LD). The LD provides information on all spells that resulted in benefits from the BfA: unemployment benefits, unemployment assistance, and payments while in training program. Individuals can be followed from employment to registered unemployment spells. The IAB dataset (i.e., the BS plus the LD datasets) also contain a plant and a firm identifier. Using the entire HD dataset, aggregate individual characteristics have been created at the establishment-level, making firm size and within-firm educational structure available.

Similar data are also available in Austria (see Winter-Ebmer and Zweimüller, 1997) and in Italy (see Contini et al., undated). For Belgium, the data used by Leonard and Van Audenrode (1996, 1997) are based on Social Security declarations for the national pension system of private sector workers and cover the period from 1977 to 1985.

2.5. Representative matched worker-firm panels (statistical surveys)

The French Labor Force Survey (Enquête Emploi, EE) is conducted every year by the

French National Statistical Institute (INSEE). Because this survey routinely includes the employer identifier (firm and establishment), it has become a standard for matched employer–employee database upon labor force surveys.

The universe of individuals sampled in EE includes all ordinary households in metropolitan France. In 1990, INSEE started a new series of March EEs, administered to the household sample every March for three consecutive years using a sampling frame based on the 1990 census. The sampling rate is 1/300. There are three rotation groups, so the sample is refreshed by one-third every year. Each year, a supplement (enquête complémentaire) is administered to the outgoing rotation group, one-third of the sample. Because the sampling technique is based on housing in tracts built in French territory with further inclusions or modifications in case of construction or reconstruction of buildings not known at the 1990 Census of Population, it is possible to have a dynamically representative survey (see INSEE, 1994 for all the technical details on the survey methodology).

The data collected in the EE include both standard and more unusual questions from labor force surveys–wage, country of origin, sex, marital status, number of children and their ages, region of residence, age, detailed education, age at the end of education period, occupation (4-digit classification), father's last occupation, mother's last occupation, employment status (employed, unemployed, inactive), usual number of hours, seniority in the employing firm, sector and size of the employing firm, nature of the contract (shortterm, longterm, program for young workers (stage)) for each of the individuals in the sample. Furthermore, each employed individual is asked the name and address of the employment location. 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 links the employer to the rest of the French statistical system. The first nine digits represent the enterprise to which the establishment belongs, based on an economic and not a financial definition. Employer Siret number can be coded in the EE for more than 90% of the workers. Hence, it becomes possible to use this type of dataset in the same fashion as the DADS was used in Abowd et al. (1999a) (see Goux and Maurin, 1999). In particular, the EE can be matched with other firm level datasets as the Echantillon d'Entreprises (based on the BIC), the Déclaration de Mouvements de Main d'Oeuvre (DMMO), a record of all entries and exits in all establishments with at least 50 employees (see Abowd et al., 1999b). Such matches have been performed by Entorf et al. (1999) to study New Technologies or Kramarz (1997) to analyze trade, wages, and unemployment.

In the United States, a longitudinally representative matched employer–employee data file has been created for the National Longitudinal Survey of Youth 1979 Cohort (NLS-Y).[5] The description is based on Abowd and Finer (1998). The creation required the resolution of two conceptual difficulties and one procedural problem. The conceptual difficulties were (1) defining an employer and (2) specifying the level of aggregation to

[5] As Hildreth and Pudney (1998) note, samples of this structure are representative for the target age group of the population but the resulting sample of employers is not necessarily representative of employers.

use on the employer unit. The procedural problem is to find a method for performing the analysis that is consistent with the confidentiality requirements that have been specified for NORC and the Center for Human Resource Research at Ohio State University, the two primary contractors for the survey.

The simplest and most comprehensive definition of an employer is any organization for which the respondent completed the employer questionnaire during any year of the NLS-Y. For the purposes of preparing the matched data file, this definition maximizes the number of employers for which information would be available. Employers are divided into primary employers (main job; full- and part-time employees) and secondary employers (no main job or several part-time jobs). Ultimately, all types of employers will be covered; thus, private for-profit employers (firms), public sector employers (units of government) and private not-for-profit employment (other organizations) would all be included in the file. Some summary measures about the employer (size, type) are available for all types of employers. Other measures (sales, profits, assets) are only available for some private for-profit employers. Detailed analysis of the characteristics of the employing firms, therefore, requires careful attention to the type of firm. The level of aggregation to use for the employer depends upon the purpose of the analysis and the prospects for collecting data at that level of aggregation. Three potential definitions are possible: establishment (the physical location where work occurs), business or governmental unit (the economic entity at which decisions are made concerning employment, investment, etc.) or company/governmental aggregate (the entity required to disclose information to public sources). Currently, the employer identifier file includes an ID for the company/governmental aggregate and the business/governmental unit, where possible. This level of identification permits merging information about companies and lines of business from sources like Compustat and Dun & Bradstreet. More specifically, approximately 49,000 unique employer names were checked for relevant (time period consistent) matches in a variety of public sources.

This matching process was done in several phases. First, the raw files of the NLS-Y for the years 1986–1994 were accessed to acquire the employer names for up to 5 employers per year. The first stage match attempts to match the respondent employer names with employer names in the Compustat (Standard and Poors) and CRSP header files. There were approximately 159,000 non-blank employer name fields for the years 1986–1994. Government coded employers, self-employed jobs, and employers with less than 50 workers were all eliminated. This left exactly 48,422 unique employer names eligible for match. These employer names were placed in a database with the Compustat headers and CRSP name histories. One by one, the respondents employers were checked against the Compustat headers. At the end of this process, around 8000 employer names were matched with Compustat and CRSP employers. These unique names accounted for roughly 18% of the master list of employer names. In addition to checking for matches, unmatched records were coded for additional checking, military employer, and public or non-profit. Unmatched small employers are left initially unmatched. The second stage match was used to double-check suspicious first stage matches, and further match unmatched first stage names that may be subsidiaries of publicly-traded parent companies. A total of 9000 such names were resolved using the Directory of Corporate Affiliations for several years both in printed and machine readable formats. These second-stage names were then recoded to reflect their status as private companies, subsidiaries of public parents, franchises, help-supply services. The third phase of the match procedure was to check improperly coded government, non-profit, and military employers. Approximately 2000 employer names were checked and coded as religious organizations, military, federal government, state government, local government, and educational institutions. In addition, private and non-profit health care facilities were reserved for the future processing. The fourth stage consisted of internal matching of companies with no publicly disclosed parent that appear multiple times.

2.6. *Non-representative cross-sections and panels of workers and firms*

Not all datasets matching workers with their firms were designed by statistical agencies with the avowed goal of representativeness of the set of workers or firms in a country, a state, or any geographic unit of some importance. This is most apparent the matched job-firm data that have been studied by Groshen (1996), who uses employer-based salary surveys in many of her papers. In this subsection, we give examples of such datasets. Our requirement for discussion herein is that multiple firms in which multiple workers or jobs are surveyed be present.

Employers have conducted salary surveys for many years in which they collect matched worker (or job) and firm information. We base our description of the American salary surveys on Groshen (1996). Salary surveys are used by large employers as a source of information on external wage opportunities of the workers they employ. These employers are very different in nature and scope. Groshen cites the following examples: "the federal government, most of the regional Federal Reserve banks, Hay Associates, Inc., the American Hospital Association, the National Association of Business Economists, and the American Association of University Professors." Access to the data is generally granted to the members of the collecting association, which may entail a large fee or to clients (in the case of Hay Associates, Inc. for instance).

These datasets contain annual information on wages, including bonuses and incentives, of all persons with a job in predefined occupations. They also have information on the participating employers themselves: industry, total employment, and firm-specific compensation policy elements. As is obvious from the list of organizations that collect Salary Surveys, the coverage largely depends on the purpose of the user. Groshen notes that "if a survey is geographically based, then the occupations covered will be those commonly found and most comparable across industry: usually clerical, administrative, maintenance, and managerial positions." Hence, occupations such as secretaries, drivers, painters, accountants are often included. She adds that "These surveys have the advantages over industry and professional wage surveys that they allow control for regional wage differences, they include many different industries, and they are longitudinal in establishments. While they do not cover all occupations, they do cover a broad mix...."

Industry-based surveys differ in their scopes. They generally cover a large fraction of those workers employed in a particular industry. This allows jobs and occupations to be very precisely defined. In particular, for blue-collar workers, information on training, on machines, and tools needed or used in the job is available.

Profession-based surveys focus on one narrowly-defined occupation and tend to be national in scope since professions have generally a national market, the characteristics of which the survey organizers want to know. In particular, information on the educational background and employment experience of the participants is often collected. In his chapter on executive pay, Murphy describes many of these surveys for CEOs.

All these different types of salary surveys have several common features. First, the description of the job is very detailed, "two to three paragraphs long, and specify the responsibilities, training requirements, how the job is done, what is produced, position in the corporate hierarchy, the occupation of direct supervisor, and number of supervisees," according to Groshen. Furthermore, the jobs may be classified into job families defined as all members of a career path. Finally, demographic information is usually not collected.

Although salary surveys are an important source of information in the private sector of the American economy, Groshen is one of the few to use these for research purposes. A number of researchers, however, have recently made use of similar Bureau of Labor Statistics surveys of occupational or industrial salaries matched to employer information. These are noted in Table 1 and discussed in the relevant sections below. Most of the design features noted by Groshen apply to these surveys as well.

Brown and Medoff (1996) describe a dataset for which individuals interviewed for the Survey of Consumers, a survey run by the Survey Research Center at the University of Michigan, between September 1991 and March 1992 were asked to complete a supplement on their employer. Supplementary questions were only asked to workers with a private-sector employer. These questions included workers' experience, seniority, occupation, and wage rate as well as information on the employer, more specifically, the collective bargaining status, the number of employees, the industry, the age of the business, fringe benefits, personnel policies, and related features of the workplace. The sample has 1410 private-sector workers of which 1168 gave information on the name and address of their employer. Brown and Medoff asked Dun & Bradstreet to locate the employer and, when located, to give the establishment and the company employment, the age of the business, and the industry. All of 863 reported matches were hand-checked, generating a set of 701 "clean matches" as described in Brown and Medoff (1996). Employers in those clean matches are larger and older (longer in business) than the employers in the original sample.

A recent study by Chennouf et al. (1997) uses matched worker-firm data for a small sample of Algerian firms in the Algiers region. This dataset comprises 42 firms from diverse manufacturing industries and 1007 employees. The available individual characteristics are the wage, the number of days worked, the education level, seniority, experience, age, sex, and the marital status. On the firms themselves, apart from the industry and private/public status, variables are mostly defined as an aggregation of the individual characteristics of workers employed at that firm (average seniority, experience, and education).

3. Statistical models for matched employer–employee datasets

3.1. The basic linear model

Virtually all of the papers that we discuss below use a variant of the linear model that can be identified with matched employer–employee data:

$$y_{it} = x_{it}\beta + \theta_i + \psi_{J(i,t)it} + \varepsilon_{it}, \tag{3.1}$$

in which y_{it} is some measured outcome (compensation, layoff event, etc.) for the individual $i = 1, \ldots, N$ at date $t = 1, \ldots, T$; x_{it} is a vector of P time-varying exogenous characteristics of individual i; θ_i is a pure person effect; $\psi_{J(i,t)it}$ is a pure firm effect for the firm at which worker i is employed at date t (denoted by $J(i,t)$), and ψ_{it} is a statistical residual. Assume that a simple random sample of N individuals is observed for T years. The firm and person effects in Eq. (3.1) can be decomposed into components relating to seniority and non-time-varying personal characteristics as follows:

$$\psi_{jit} = \phi_j + \gamma_j s_{it}, \tag{3.2}$$

where s_{it} denotes individual i's seniority in firm $j = J(i, t)$ in year t, ϕ_j denotes the firm-specific intercept, and γ_j is the firm-specific seniority coefficient; while

$$\theta_i = \alpha_i + u_i\eta, \tag{3.3}$$

where u_i is a vector of non-time-varying measurable personal characteristics, α_i is the person-specific intercept, and η is the vector of coefficients on the non-time-varying personal characteristics.

In matrix notation we have

$$y = X\beta + D\theta + F\psi + \varepsilon, \tag{3.4}$$

where X is the $N^* \times P$ matrix of observable, time-varying characteristics, D is the $N^* \times N$ matrix of indicators for individual $i = 1, \ldots, N$, F is the $N^* \times J$ matrix of indicators for the firm at which i works at date t (J firms total), y is the $N \times 1$ vector of outcomes, ε is the conformable vector of residuals, and $N^* = NT$. Balanced samples are not necessary but simplify the discussion of the statistical models. The firm effect can also have higher dimension, as for example in Eq. (3.2), but we use this simpler form for the discussion herein.

The parameters of Eq. (3.4) are β, the $P \times 1$ vector of coefficients on the time-varying personal characteristics; θ, the $N \times 1$ vector of individual effects; ψ, the $J \times 1$ vector of firm effects; and the error variance, σ_ε^2. The parameter θ includes both the unobservable (to the statistician) individual effect and the coefficients of the non-time-varying personal characteristics. Eqs. (3.1) and (3.4) are interpreted as the conditional expectation of individual outcomes given information on the observable characteristics, the date of observation, the identity of the individual, and the identity of the employing firm. In this section we want to make precise the interpretation of Eq. (3.4) under classical least squares when some of the effects, β, θ, and ψ are missing or are aggregated into linear combinations. The discussion draws heavily on Abowd et al. (1999a).

3.2. Aggregation and omitted variable biases

The omission or aggregation of one or more of the effects in Eq. (3.4) can change the meaning of the other effects in important and subtle ways that are not always clear from the specific equation that various authors have estimated. Variations in the set of conditioning effects, which give rise to omitted-variable biases, are one source of confusion about the interpretation of the statistical parameters. The use of different linear combinations of the effects in Eq. (3.4), which gives rise to aggregation biases, is another source of differential interpretations for the parameters. These are considered in turn.

When the estimated version of Eq. (3.4) excludes the pure firm effects (ψ), the estimated person effects, θ^*, are the sum of the pure person effects, θ, and the employment-duration weighted average of the firm effects for the firms in which the worker was employed, conditional on the individual time-varying characteristics, X:

$$\theta^* = \theta + (D'M_X D)^{-1} D'M_X F\psi, \tag{3.5}$$

where the notation $M_A \equiv I - A(A'A)^{-1}A'$ for an arbitrary matrix A. Hence, if X were orthogonal to D and F, so that $D'M_X D = D'D$ and $D'M_X F = D'F$, then the difference between θ^* and θ, which is just an omitted variable bias, would be an $N \times 1$ vector consisting, for each individual i, of the employment-duration weighted average of the firm effects ψ_j for $j \in \{J(i, 1, \ldots, J(i, T)\}$:

$$\theta_i{}^* - \theta_i = \sum_{t=1}^{T} \frac{\psi_{J(i,t)}}{T}.$$

The estimated coefficients on the time-varying characteristics in the case of omitted firm effects, β^*, are the sum of the parameters of the full conditional expectation, β, and the omitted variable bias that depends upon the conditional covariance of X and F, given D:

$$\beta^* = \beta + (X'M_D X)^{-1} X'M_D F\psi.$$

Similarly, omitting the pure person effects (θ) from the estimated version of Eq. (3.4) gives estimates of the firm effects, ψ^{**}, that can be interpreted as the sum of the pure firm effects, ψ, and the employment-duration weighted average of the person effects of all of the firm's employees in the sample, conditional on the time-varying individual characteristics:

$$\psi^{**} = \psi + (F'M_XF)^{-1}F'M_XD\theta. \tag{3.6}$$

Hence, if X were orthogonal to D and F, so that $F'M_XF = F'F$ and $F'M_XD = F'D$, then the difference between ψ^{**} and ψ, again an omitted variable bias, would be a $J \times 1$ vector consisting, for each firm j, of the employment-duration weighted average of the person effects θ_i for $i \in \{J(i,t) = j \text{ for some } t\}$:

$$\psi_j^{**} - \psi_j = \sum_{i=1}^{N}\sum_{t=1}^{T}\left[\frac{\theta_i 1(J(i,t) = j)}{N_j}\right],$$

where

$$N_j = \sum_{i=1}^{N}\sum_{t=1}^{T} 1(J(i,t) = j),$$

and the function $1(A)$ takes the value 1 when A is true and 0 otherwise. The estimated coefficients on the time-varying characteristics in the case of omitted individual effects, β^{**}, are the sum of the parameters of the full conditional expectation, β, and the omitted variable bias that depends upon the covariance of X and D, given F:

$$\beta^{**} = \beta + (X'M_FX)^{-1}X'M_FD\theta. \tag{3.7}$$

Almost all existing analyses of equations like (3.4) produce estimated effects that confound pure person and pure firm effects in a manner similar to that presented above. The possibility of identifying both person and firm effects thus allows users of matched employer–employee data to reexamine many important topics in labor economics using estimates that properly allocate the statistical effects associated with persons and firms. Of course, other identification issues also arise, such as in the estimation of person effects, so that longitudinal matched data are usually required.

3.3. Identification of person and firm effects

Although Eq. (3.1) is just a classical linear regression model, the full design matrix $[X\ \ D\ \ F]$ has high column dimension. The cross-product matrix

$$\begin{bmatrix} X'X & X'D & X'F \\ D'X & D'D & D'F \\ F'X & F'D & F'F \end{bmatrix} \tag{3.8}$$

is patterned in the elements $D'D$ and $F'F$. The separate identification of the individual and firm effects requires the presence in the sample of individuals who move from firm to firm. The individual and firm effects are both identified whenever an individual that appears in the sample works for a firm that employs at least one individual, also in the sample, who moves to another firm, which, necessarily, also appears in the sample. The simplest example of the complexities of identification in this model can be seen by considering an example in which there are three individual (1, 2, and 3), two firms (A and B) and two time periods. Suppose that individual 1 is continuously employed at firm A, individual 2 is continuously employed at firm B, and individual 3 moves from firm A to firm B. Then all three individual effects are identified (subject to the usual identification restriction that they sum to zero) and both firm effects are identified (again, subject to the usual identification condition that they sum to zero). If individual 3 is not mobile (stays at firm A), then firm effect B cannot be distinguished from person effect 2 and person effects 1 and 3 are entirely within firm effect A.

There are many computational difficulties associated with inverting the matrix (3.8). These computational problems are directly related to the fact that the basic statistical model is neither hierarchical nor balanced. Thus, projecting onto the columns D, the method usually called "within persons estimation," leaves a high-dimension unpatterned, non-sparse matrix to invert for the solution for β and ψ. Similarly, projecting onto the columns of F, the method usually called "within firms estimation," leaves a high-dimension unpatterned, non-sparse matrix to invert to solve for β and θ. Clearly, the usual computational methods for least squares estimation of the parameter vector $[\beta' \;\; \theta' \;\; \psi']'$ are not generally feasible. Hence, one usually cannot compute the unconstrained least squares estimates for the model (3.1). Correlated random effect models, which permit the estimation of all effects without restricting the design matrix in Eq. (3.8), also require solution of the full least squares normal equations (see Scheffé, 1959; Searle et al. 1992). See Abowd et al. (1999a) for a detailed discussion of the identification and estimation issues in models using Eq. (3.1).

3.4. Aggregation and omitted variable biases for inter-industry wage differentials[6]

Define a pure inter-industry wage differential, conditional on the same information as in Eqs. (3.1) and (3.4), as κ_k for some classification $k = 1, \ldots, K$. By definition, pure firm effects are fully nested within pure inter-industry effects so that κ_k can be represented as an employment-duration weighted average of the firm effects within the classification k:

$$\kappa_k \equiv \sum_{i=1}^{N} \sum_{t=1}^{T} \left[\frac{1(K(J(i,t)) = k)\psi_{J(i,t)}}{N_k} \right],$$

where

$$N_k \equiv \sum_{j=1}^{J} 1(K(j) = k) N_j$$

and the function K(j) denotes the classification of firm j. If we insert pure inter-industry effects as the appropriate aggregate of the firm effects in Eq. (3.1), then the equation

[6] This subsection draws heavily on Abowd et al. (1999a).

becomes

$$y_{it} = x_{it}\beta + \theta_i + \kappa_{K(J(i,t))} + (\psi_{J(i,t)} - \kappa_{K(J(i,t))}) + \varepsilon_{it}$$

or, in matrix notation as in Eq. (3.4),

$$y = X\beta + D\theta + FA\kappa + (F\psi - FA\kappa) + \varepsilon, \tag{3.9}$$

where the matrix A, $J \times K$, classifies each of the J firms into one of the K categories; that is, $a_{jk} = 1$ if, and only if, $K(j) = k$. The parameter vector κ, $K \times 1$, may be interpreted as the following weighted average of the pure firm effects:

$$\kappa \equiv (A'F'FA)^{-1}A'F'F\psi,$$

and the effect $(F\psi - FA\kappa)$ may be re-expressed as $M_{FA}F\psi$. Thus, the aggregation of J firm effects into K inter-industry effects, weighted so as to be representative of individuals, can be accomplished directly by estimation of Eq. (3.9). Only rank$(F'M_{FA}F)$ firm effects can be separately identified; however, there is neither an omitted variable nor an aggregation bias in the classical least squares estimates of (3.9).

Estimates of inter-industry effects, κ^*, that are computed on the basis of an equation that excludes the remaining firm effects, $M_{FA}F\psi$, are equal to the pure inter-industry effect, κ, plus an omitted variable bias that can be expressed as a function of the conditional variance of the inter-industry effects, FA, given the time-varying characteristics, X, and the person effects, D:

$$\kappa^* = \kappa + \left(A'F'M_{[D|X]}FA\right)^{-1}A'F'M_{[D|X]}M_{FA}F\psi, \tag{3.10}$$

which simplifies to $\kappa^* = \kappa$ if, and only if, the inter-industry effects, FA, are orthogonal to the subspace $M_{FA}F$, given D and X, which is generally not true even though FA and $M_{FA}F$ are orthogonal by construction. Thus, it is not possible to estimate pure inter-industry wage differentials consistently, conditional on time-varying personal characteristics and unobservable non-time-varying personal characteristics, without explicit firm-identifiers unless this conditional orthogonality condition holds. A similar argument applies to the estimates of β. Industry effects as defined by Eq. (3.10) are directly comparable to those estimated by Krueger and Summers (1988) when they include person effects.

When the estimation of Eq. (3.9) excludes both person and firm effects, the estimated inter-industry effect, $\kappa_k{}^{**}$, equals the pure inter-industry effect, κ, plus the employment-duration weighted average residual firm effect inside the category k, given X, and the employment-duration weighted average person effect inside the category, given the time-varying personal characteristics X:

$$\kappa^{**} = \kappa + (A'F'M_XFA)^{-1}A'F'M_X(M_{FA}F\psi + D\theta),$$

which can be restated as

$$\kappa^{**} = (A'F'M_XFA)^{-1}A'F'M_XF\psi + (A'F'M_XFA)^{-1}A'F'M_XD\theta. \tag{3.11}$$

Hence, the raw inter-industry effects consist of the sum of the properly-weighted average person effect and average firm effect, conditional on X. Thus, analyses that exclude person effects confound the pure inter-industry wage differential with an average of the person effects found in the category, given the measured personal characteristics, X. The inter-industry wage differentials in Eq. (3.11) are directly comparable to those studied by Krueger and Summers (1988) when person effects are omitted.

3.5. Aggregation and omitted variable biases for inter-person wage differentials

Another line of research attempts to explain inter-personal wage differentials conditional on firm effects without explicit controls for unobservable personal heterogeneity. None of the studies in this strain of the wage-determination literature includes both pure person and pure firm effects, as defined in Eq. (3.1) or (3.4) above. In our notation, studies like Groshen (1991a) estimate ψ^{**}, from Eq. (3.6), and β^{**}, from Eq. (3.7).

3.6. Firm-size wage effects

The repeated finding of a positive relation between the size of the employing firm and wage rates, even after controlling for a wealth of individual variables (see Brown and Medoff, 1989), has also generated many alternative interpretations. Properly modeled, the firm-size wage effect can also be fully decomposed using matched employee–employer data. Using our notation, a firm-size effect, δ, can be modeled using a matrix S, $J \times R$, that maps the size of firm j into R linearly independent functions of its size. Using the same methods as above, we express the wage equation, Eq. (3.4), as

$$y = X\beta + D\theta + FS\delta + M_{FS}F\psi + \varepsilon, \tag{3.12}$$

so that the pure firm-size effects are related to the underlying pure firm effects by the equation:

$$\delta \equiv (S'F'FS)^{-1}S'F'F\psi. \tag{3.13}$$

The firm-size effect is also an aggregation of the pure firm effects and can be analyzed using the same tools that we used for the inter-industry wage differential. The raw firm-size wage differential, δ^{**} (in our notation), can be represented as

$$\delta^{**} = (S'F'M_XFS)^{-1}S'F'M_XF\psi + (S'F'M_XFS)^{-1}S'F'M_XD\theta, \tag{3.14}$$

which can be interpreted as the sum of the firm-size, employment-weighted average firm effect and the similarly-weighted average person effect, conditional on personal characteristics, X, and firm size, FS.

3.7. Other methodological issues

There are a variety of technical statistical issues surrounding the use of different sampling frames to construct matched employer–employee issues. Recently, several teams of authors have begun to examine these issues. Hildreth and Pudney (1997, 1999) examine the issues of non-random missing data, choice based sampling induced by the matching process and correlated random effects modeling of the heterogeneity. They provide full likelihoods for the hierarchical case (individual effects are fully within firm effects) and some likelihood models for non-hierarchical case (individuals move from firm to firm within the sample). Abowd et al. (1999c) address the issue of non-random missing data following the match. Dolton, Lindeboom and van den Berg (1999) address the issues of non-random missing matches (of the employer or employee), attrition and endogenous sampling. Mairesse and Greenan (1999) consider the problem of modeling employee and employer behavior when only a single employee observation is available per firm, as is common in matched training surveys.

4. From theoretical models to statistical models: potential interpretations of the descriptive models

We illustrate the relation between structural heterogeneity in the populations of workers (heterogeneous abilities or tastes) and firms (heterogeneous efficiencies or technologies) and the statistical heterogeneity in Eq. (3.1) using four economic models with very simple population structures. In each case we derive the conditional expectation of individual compensation given the identity of the employing firm and the individual. We then relate the parameters of this conditional expectation to the statistical parameterization above.

4.1. Measurement of the internal and external wage

Virtually all economic models of labor market outcomes require an estimate of the opportunity cost of the worker's time. In simple, classical equilibrium models without unmeasured person or firm heterogeneity, this generally corresponds to the measured wage rate. In models of wage determination such as quasi-rent splitting or imperfect information (efficiency wage and agency models), unmeasured statistical heterogeneity (person or firm) breaks the direct link between the observed wage rate and the opportunity cost of time. Moreover, such models usually make an explicit distinction between the compensation received and the wage rate available in the employee's next best alternative employment. The statistical model in Eq. (3.1), while not derived from an explicit labor market model, contains all the observable elements from which non-classical labor market models derive their empirical content. Indeed, the simplest definition of the components of the external and internal wage rate based a structural model leading to Eq. (3.1) is given by the following model:

$$y_{it} = x_{it}\zeta + \nu_{it},$$

where $\{x_{it}, \nu_{it}\}$ follows a general stochastic process for $i = 1, \ldots, N$ and $t = 1, \ldots, T$ with

$$\mathrm{E}[\{x_{it}, \nu_{it}\}\{x_{ns}, \nu_{ns}\} \mid i, n, s, t, J(i,t), J(n,s)] \neq 0 \qquad \text{iff } i = n \text{ or } J(i,t) = J(n,s).$$

Then,

$$\theta_i = \mathrm{E}[x_{it}\zeta + \nu_{it} \mid i] - \mathrm{E}[x_{it}\zeta + \nu_{it}]$$

and

$$\psi_j = \mathrm{E}[x_{it}\zeta + \nu_{it} \mid J(i,t) = j] - \mathrm{E}[x_{it}\zeta + \nu_{it}].$$

4.2. A matching model with endogenous turnover

This model is based on Jovanovic (1979). Suppose that workers are homogeneous. There are two types of firms, m and n, and two periods. In type m firms a worker's marginal product and wage rate are always w^*, and employment is always available in a type m firm. In type n firms there is a matching process. Worker i's productivity is $w^* + \varepsilon_{in}$ in both periods with ε_{in} drawn from a binomial distribution $B(-H, H, 1/2)$. The matching outcome, ε_{in}, unknown to both the worker and the firm at the beginning of the first period of employment, is realized at the end of the first period and becomes public information. Workers are offered contracts at the beginning of the first period of the form (w_1, w_2) and workers may leave firm n at the end of the first period. All workers are risk-neutral and earn no rents. The equilibrium contract for firms of type n is $(w^* - H/2, w^* + \varepsilon_{in})$. All workers in type n firms with a bad matching outcome $-H$ quit to type m firms.

To simplify the model, we consider a stationary situation with nine workers who live for two periods each, three born in period 0, three born in period 1, three born in period 2. Two workers in each generation enter type n firms, one worker in each generation enters a type m firm. Of the two workers who entered type n firms, let one draw a positive matching outcome and the other draw a negative matching outcome. The worker with the negative matching outcome leaves the type n firm for a type m firm when the matching parameter is made public.

The structure of the data implied by this theoretical model is shown in Table 2. This corresponds to the following parameter values in the descriptive model:

$$\mu = w^*,$$

where μ is the overall mean;

$$\alpha_i = 0, i = 1, \ldots, 9,$$

where α_i is person i person-effect;

$$(\phi_m, \gamma_m) = (0, 0),$$

for the type m firm compensation policy; and

$$(\phi_n, \gamma_n) = \left(-\frac{H}{2}, \frac{3H}{2}\right),$$

for the type n firm compensation policy.

4.3. A rent-splitting model with exogenous turnover

Suppose there are four different individuals, two types of firms, m and n, and two time periods. Each of the two firms earns quasi-rents of q_{it}, and the quasi-rents are split by negotiation so that the workers receive a share s_j of the quasi-rent in firm j. Suppose that each firm employs two workers. With probability one, exactly one worker is randomly selected to separate from the period one employer and be re-employed at the other firm in the second period. All information about the workers and firms is known to those parties but not to the statistician. All workers are included in the data sample and the typical worker has wages of the form

$$y_{it} = x_i + s_j q_{jt},$$

where x_i is the measure of wage rate heterogeneity, i.e., the worker type, q_{jt} follows a binomial distribution $B(-Q, Q, 1/2)$, $i = 1, \ldots, 4, j = m, n$, and $t = 1, 2$.

Table 3 shows the relation among the theoretical parameters, x_i, s_j, and Q, and the statistical parameters of Eq. (3.1) for each worker and each period. The model cannot be solved exactly. Thus, we use these relations to solve, by least squares, the moment equations that determine the relations between the statistical parameters and the model parameters. This yields

$$\mu = \frac{1}{4} \sum_{i=1}^{4} x_i,$$

where μ is the overall mean;

$$\alpha_1 = \frac{1}{4}\left(-3s_m Q - s_n Q - \sum_{i=1}^{4} x_i\right) + x_1,$$

$$\alpha_2 = \frac{1}{4}\left(-s_m Q - 3s_n Q - \sum_{i=1}^{4} x_i\right) + x_2,$$

$$\alpha_3 = \frac{1}{4}\left(s_m Q + 3s_n Q - \sum_{i=1}^{4} x_i\right) + x_3,$$

$$\alpha_4 = \frac{1}{4}\left(3s_m Q + s_n Q - \sum_{i=1}^{4} x_i\right) + x_4,$$

where the α_i are the four person effects;

$$(\phi_m, \gamma_m) = \left(\frac{(s_n - s_m)Q}{4}, 2s_m Q\right)$$

and

Table 2
Matching model with homogeneous workers[a]

Individual	Wage period 1	Wage period 2
1	$y_{11} = \mu + \alpha_1 + \phi_m = w^*$	$y_{12} = \mu + \alpha_1 + \phi_m + \gamma_m = w^*$
2	$y_{21} = \mu + \alpha_2 + \phi_m = w^*$	
3	$y_{31} = \mu + \alpha_3 + \phi_m + \gamma_m = w^*$	
4		$y_{42} = \mu + \alpha_3 + \phi_m = w^*$
5	$y_{51} = \mu + \alpha_5 + \phi_n = w^* - (H/2)$	$y_{52} = \mu + \alpha_5 + \phi_n + \gamma_n = w^* + H$
6	$y_{61} = \mu + \alpha_6 + \phi_n + \gamma_n = w^* + H$	
7	$y_{71} = \mu + \alpha_7 + \phi_n = w^* - (H/2)$	$y_{72} = \mu + \alpha_7 + \phi_m = w^*$
8		$y_{82} = \mu + \alpha_8 + \phi_n = w^* - (H/2)$
9		$y_{92} = \mu + \alpha_9 + \phi_n = w^* - (H/2)$

[a] Individual 1 enters type *m* firm in period 1; individual 2 entered type *m* firm in period 0 (before period 1); individual 3 entered type *n* firm in period 0 (before period 1), had a negative matching outcome and left for a type *m* firm; individual 4 enters type *m* firm in period 2; individual 5 enters type *n* firm in period 1, has a positive matching outcome; individual 6 entered type *n* firm in period 0 (before period 1), had a positive matching outcome and remained in type *n* firm for period 1; individual 7 enters type *n* firm, has a negative matching outcome and leaves for a type *m* firm in period 2; individuals 8 and 9 enter type *n* firm in period 2.

$$(\phi_n, \gamma_n) = \left(\frac{(s_n - s_m)Q}{4}, -2s_nQ\right)$$

are respectively the type *m* and type *n* firms' policies.

4.4. An incentive model with unobserved individual heterogeneity

Following Kramarz and Rey (1995) , consider workers who are heterogeneous with respect to a parameter $q \in [0, 1]$, which is known to them but not known to the firms. Suppose, furthermore, that there are two types of firms, *m* and *n*, that differ according to their technology, and that there are two time periods. At type *m* firms, workers are hired for one period and have a level of productivity y^* regardless of their q. At type *n* firms, workers are hired in period one, produce y regardless of their q, and choose an effort level, either 0 or E, to exert during on-the-job training. At the end of the first period, workers in firm type *n* take a formal, verifiable test. If worker q exerts effort E, the test is passed with probability q. Otherwise, the test is passed with probability kq, where $(0 < k < 1)$. At the beginning of the second period, the firm decides which workers to keep and the workers may leave on their own. Workers who exert effort E have a level of productivity in the second period of $y + \tau_q$ if they remain in a type *n* firm.

There are many type *m* firms and two type *n* firms, which compete for workers in both periods. Workers in type *m* firms always receive a wage w^*. Workers in type *n* firms are offered a wage contract $(w_1(q),w_2(q),b(q))$, where $w_1(q)$ is the first period wage, $w_2(q)$ is the second period wage, and $b(q)$ is the bonus paid to those who pass the test. In equilibrium

Table 3
Rent-splitting model[a]

Individual	Wage period 1	Wage period 2
1	$y_{11} = \mu + \alpha_1 + \phi_m = x_1 - s_m Q$	$y_{12} = \mu + \alpha_1 + \phi_m + \gamma_m = x_1 + s_m Q$
2	$y_{21} = \mu + \alpha_2 + \phi_m = x_2 - s_m Q$	$y_{22} = \mu + \alpha_2 + \phi_n = x_2 - s_n Q$
3	$y_{31} = \mu + \alpha_3 + \phi_n = x_3 + s_n Q$	$y_{32} = \mu + \alpha_3 + \phi_n + \gamma_n = x_3 - s_n Q$
4	$y_{41} = \mu + \alpha_4 + \phi_n = x_4 + s_n Q$	$y_{42} = \mu + \alpha_4 + \phi_m = x_2 + s_m Q$

[a] The quasi-rent is $-Q$ in type m firm in period 1 and q in period 2. The quasi-rent is q in type n firm in period 1 and $-Q$ in period 2. Individual 1 works in type m firm in both periods. Individual 2 works in type m firm in period 1 and in type n firm in period 2. Individual 3 works in type n firm in both periods. Individual 4 works in type n firm in period 1 and in type m firm in period 2.

all firms of both types make zero profits because of the competition to attract workers. Furthermore, if $y + \delta(y + \tau_q)$ is convex in q (δ being the rate of discount of future earnings), the equilibrium contract will be such that $w_1(q) = y - qb(q)$, $w_2(q) = y + \tau_q$, and

$$b(q) = \frac{\mathrm{d}}{\mathrm{d}q}(y + \delta(y + \tau_q)).$$

All workers with type q, $q \geq p$, will choose to enter one of the type n firms and will choose to exert effort E when $b(p) \geq E/(1 - k)p$.[7] To simplify the model, we suppose that $\tau_q = \tau(Q^2/2)$ and that parameters are such that $p = 1/3$. We also suppose that there are nine workers, three of whom are employed by type m firms and the remaining six work in type n firms.

Table 4 shows the wage of every individual in each firm and in each period in terms of the theoretical model, as well as in terms of the descriptive model. These equations can be solved in order to express each parameter of the descriptive model using parameters of the theoretical model. As in the rent-splitting model, the solution is not exact–we must use least squares to express the function of the theoretical parameters that is closest to the statistical parameter. To see why, consider the workers in type n firms. Individual 7 passed the test and, consequently, received a bonus. This result generates a seniority slope for individual 7. Individual 8 did not pass the test and therefore received no bonus in period 2. Thus individual 8 has a different seniority slope in the same firm. The statistical parameter γ_n measures the average seniority slope in the firm n. Thus, the resulting estimated seniority slope will be the least squares estimate of the average of the two slopes. We illustrate these solutions for all the statistical parameters below.

The overall mean, μ, is given by the following:

$$\mu = \frac{\delta\tau}{18}\sum_{i=4}^{7} q_i\left(1 - \frac{q_i}{2}\right) - \frac{\delta\tau}{18}\sum_{i=8}^{9}\frac{q_i^2}{2} + \frac{w^*}{3} + \frac{2y}{3}.$$

[7] Proofs of all these assertions can be found in Kramarz and Rey (1995).

The individual effects, α_i, $i = 4, 5, 6, 7$ are

$$\alpha_i = \frac{\delta\tau}{24}\left[\sum_{j=4,j\neq i}^{7} q_j\left(\frac{q_j}{2} - 2\right) + 5q_i(2 - q_i) + \sum_{j=8}^{9} q_j^2\right], \qquad i = 4, 5, 6, 7$$

and those for individual $i = 8{,}9$ are

$$\alpha_i = \frac{\delta\tau}{24} \sum_{j=4,j\neq i}^{7} q_j(q_j - 2) + q_k^2 - 5q_i^2],$$

where $k = 8, 9$, $i \neq k$. Finally, the individual effects for $i = 1, 2, 3$ and the firm effects for m are not separately identifiable, since there are no movements between firms. We arbitrarily set

$$\alpha_i = 0, \qquad i = 1, 2, 3$$

for these individuals, implying a firm effect of

$$\phi_m = \frac{\delta\tau}{36}\left[\sum_{i=4}^{7} q_i(q_i - 2) + \sum_{i=8}^{9} q_i^2\right] + \frac{2w^*}{3} - \frac{2y}{3}.$$

For type n firms, we have

$$\phi_n = \frac{\delta\tau}{36}\left[\sum_{i=4}^{7} q_i(-5q_i - 2) - 5\sum_{i=8}^{9} q_i^2\right] - \frac{w^*}{3} + \frac{y}{3}.$$

The seniority slopes are

$$\gamma_m = 0$$

for firm m and

$$\gamma_n = \frac{\delta\tau}{12}\left[\sum_{i=4}^{7} q_i(3q_i + 2) + 3\sum_{i=8}^{9} q_i^2\right]$$

for firm n.

Notice that the α_i of the workers in the type n firm depend upon their hidden characteristics q_i as well as the characteristics of their fellow workers. Note also that the intercept in type m firms is larger than that of type n firms. Finally, as mentioned above, the seniority slope, γ_n, in type n firms is the least squares average of the career paths in the firm, depending on the success or failure of the test.

No single economic model is likely to explain a large, diverse labor markets like the ones studied in virtually all of the papers we discuss below. Nevertheless, it is important to keep in mind that it is not always possible to make a direct interpretation of the statistical parameters (for individuals or firms) in terms of simple economic parameters. In general,

Table 4
Incentive model with heterogeneous workers[a]

Individual	Wage period 1	Wage period 2
1	$y_{11} = \mu + \alpha_1 + \phi_m = w^*$	$y_{12} = \mu + \alpha_1 + \phi_m + \gamma_m = w^*$
2	$y_{21} = \mu + \alpha_2 + \phi_m = w^*$	$y_{22} = \mu + \alpha_2 + \phi_m + \gamma_m = w^*$
3	$y_{31} = \mu + \alpha_3 + \phi_m = w^*$	$y_{32} = \mu + \alpha_3 + \phi_m + \gamma_m = w^*$
4	$y_{41} = \mu + \alpha_4 + \phi_n = y - \delta\tau q_4^2$	$y_{42} = \mu + \alpha_4 + \phi_n + \gamma_n = y + (\delta\tau/2)q_4^2 + \delta\tau q_4$
5	$y_{51} = \mu + \alpha_5 + \phi_n = y - \delta\tau q_5^2$	$y_{52} = \mu + \alpha_5 + \phi_n + \gamma_n = y + (\delta\tau/2)q_5^2 + \delta\tau q_5$
6	$y_{61} = \mu + \alpha_6 + \phi_n = y - \delta\tau q_6^2$	$y_{62} = \mu + \alpha_6 + \phi_n + \gamma_n = y + (\delta\tau/2)q_6^2 + \delta\tau q_6$
7	$y_{71} = \mu + \alpha_7 + \phi_n = y - \delta\tau q_7^2$	$y_{72} = \mu + \alpha_7 + \phi_n + \gamma_n = y + (\delta\tau/2)q_7^2 + \delta\tau q_7$
8	$y_{81} = \mu + \alpha_8 + \phi_n = y - \delta\tau q_8^2$	$y_{82} = \mu + \alpha_8 + \phi_n + \gamma_n = y + (\delta\tau/2)q_8^2$
9	$y_{91} = \mu + \alpha_9 + \phi_n = y - \delta\tau q_9^2$	$y_{92} = \mu + \alpha_9 + \phi_n + \gamma_n = y + (\delta\tau/2)q_9^2$

[a] Individuals 1–3 belong to type m firm with q_i, $i = 1,2,3$ between 0 and 1/3, individuals 4–9 belong to type n firm with q_i, $i = 4$–9 above 1/3. Individuals 4–7 pass the test and receive the bonus; individuals 8 and 9 fail.

the interpretation of a given statistical parameter depends upon all the elements of the economic model under consideration.

5. New results with matched employer–employee datasets: compensation structure

5.1. *Models with both person and firm effects*

The papers we consider in this section all estimate a variant of the full model (3.1) and then use the results to consider related sets of questions about the links between individual heterogeneity, firm heterogeneity and observable wage differentials. We consider Abowd et al. (1999a,c), Bingley and Westergård-Nielsen (1996), Burgess et al. (1997), Goux and Maurin (1999), Finer (1997), Leonard and van Audenrode (1996, 1997), and Leonard et al. (1999). Belzil (1997) estimates the full model but is concerned primarily with worker mobility, see Section 7.5 for a discussion. Entorf et al. (1999) also estimate the full model, but their focus is on computer and wages, so this article is described in Section 7.4. Pacelli (1997) estimates the full model but is concerned primarily with seniority effects; see Section 5.3 for a discussion.

Abowd et al. (1999a) provide a very complete discussion of the statistical and economic issues surrounding estimation of Eq. (3.1) for log wage rates, much of which is summarized in Section 3. In their analysis of the French data from the DADS (see Table 1), they find that person effects, without controlling for non-time-varying personal characteristics, θ_i, or after such controls, α_i, account for 60–80% of the variation in log annual wage rates while the full firm effect (including a heterogeneous seniority effect discussed in Section 5.3) accounts for only 4–9%. The two effects are not highly correlated (0.09–0.26, depending on the statistical model).

Abowd et al. (1999a) use the estimated person and firm effects to address a number of other questions. They show that raw interindustrial wage differentials, as defined in Eq. (3.11), can be decomposed into a part due to the industry-average person effect and a part due to the industry-average firm effect. The decomposition is exact when there is no estimation error in the relevant industry averages and the large sample sizes from the French data essentially eliminate this estimation error. These authors find that, for France, 90% of the raw inter-industry wage differential is explained by the industry-average person effects and between 7% and 25% is explained by the industry-average firm effect (according to the method used, the average effects are correlated at the industry level). They perform the same decomposition for the firm-size wage effect in France and the results are that 90% of the firm-size wage differential is due to the firm-size-average person effect and 25–40% is due to the firm-size-average firm effect (again, according to the estimation method for the basic effects and allowing for correlation among the firm-size averages). These differentials are examined in more detail in Abowd and Kramarz (1996a, 1998a).

Abowd et al. (1999c) examine the same questions as Abowd et al. (1999a) using two American data sources (Washington State UI and NLSY '79, see Table 1). As in France, individual heterogeneity (θ_i or α_i) is the most important source of variation in log wage rates for these American data, explaining about twice as much of the variance as firm-level heterogeneity. Again, as in France and the other countries discussed below, there is only a weak correlation between person and firm effects. The results concerning inter-industry wage differentials are again similar to those for France. Industry-average person effects are very important in explaining the raw differentials. In contrast to the results for France, however, the industry-average firm effect is also important in explaining the raw differentials, although less important than person effects.

In a series of papers, Leonard and Van Audenrode (1996, 1997) and Leonard et al. (1999), consider the wage determination process using longitudinal matched Belgian data that are capable of identifying both firm and individual effects as defined in the full model (3.1). Because their focus is on wage and employment mobility and, in particular, the interaction of individual and firm wage components on the subsequent wage and mobility of individuals, they model these effects differently. These authors use log wage equations of the form

$$\ln w_{it} = x_{it}\beta_{tJ(i,t)} + \psi^{**}_{itJ(i,t)} + \varepsilon_{it}, \tag{5.1}$$

where the firm-specific component of the wage equation contains a firm-specific effect, $\psi^{*}_{tJ(i,t)}$, and a within-firm person effect $\psi^{**}_{itJ(i,t)} - \psi^{*}_{tJ(i,t)}$. In the first paper (1996), these authors fix $t = 1984$. They show that there is considerable heterogeneity in β (coefficients on functions of age, seniority and sex, education is not available in the data) and in $\psi^{**}_{i1984J(i,1984)}$. This heterogeneity is directly related to employee mobility. Higher composite firm effects, $\psi^{**}_{i1984J(i,1984)}$, are associated with lower mobility, a result interpreted as supporting the hypothesis that the component of $\psi^{**}_{i1984J(i,1984)}$ due to the average person effect within the firm, $\bar{\theta}_{J(i,1984)}$, is less important that the effect due to the firm, $\psi_{J(i,1984)}$. Steeper profiles as a function of age or seniority are associated with lower separation rates.

In the second paper (1997), these authors estimate Eq. (5.1) using samples of non-movers and movers. The resulting parameters have the interpretation

$$\psi^{**}_{ij} = \theta_i + \psi_j, \qquad \psi^{*}_{j} = \psi_j + \bar{\theta}_j \tag{5.2}$$

$$\psi^{**}_{ij} - \psi^{*}_{j} = \theta_i - \bar{\theta}_j. \tag{5.3}$$

They find that pay is persistent, by which they mean that the components of pay, ψ^{*}_{j} and $\psi^{**}_{ij} - \psi^{*}_{j}$ estimated on the observations prior to the move are significantly related to compensation on the next job. There are two possibilities for the persistence of the effect in Eq. (5.2). Either a substantial proportion of the effect is due to unmeasured human capital, an interpretation of $\bar{\theta}_j$, or there is non-random mobility, $\psi_{J(i,t)}$ is correlated with $\psi_{J(i,s)}$ when $J(i,t) \neq J(i,s)$ because of the mobility decisions of the firms and workers. There is only one explanation for the persistence of $\psi^{**}_{ij} - \psi^{*}_{j}$, as Eq. (5.3) shows, unmeasured human capital. Leonard and Van Audenrode conclude, after considering some additional mobility evidence, that the unmeasured general human capital hypothesis is the most reasonable explanation for their results.

In the third paper, with Leonard et al. (1999), they estimate a version of Eq. (5.1) with full heterogeneity in both the observable characteristics coefficients, β, and the combined person-firm effects, ψ^{**}_{ij}. They show that there is considerable persistence in these effects by examining autocorrelation matrices. Because they do not use the longitudinal nature of the data to distinguish person and firm effects, we discuss these results in Section 5.2. They also relate the heterogeneous coefficients to productivity measures in the firm, results which we discuss in Section 7.1.

Burgess et al. (1997) analyze data from the State of Maryland unemployment insurance system (see Table 1). These authors are primarily interested in studying the effect of reallocations of workers among firms on the resulting distribution of earnings. They present several models of mobility that depend upon detailed knowledge of the parameters in Eq. (3.1). They present two methods for estimating the model. In the first, they take a subset of 4000 of the workers with 10-quarter continuous employment histories. These individuals are used to select 2426 employers who ever employed these individuals. Then, they add all of the other employees of these 2426 firms to the analysis sample but only for the quarters in which these individuals worked for the 2426 employers originally selected. The procedure is equivalent to selecting a probability sample of employers with probabilities proportional to the distribution of long term employment at a point in time. The identification of the firm effects is with respect to this sampling frame, which is not representative of the same populations as the other articles discussed in this section. This sample is used to estimate a variant of Eq. (3.1) by full least squares. They also use the methods of Abowd et al. (1999a) for comparison. Regarding the basic structure of compensation, these authors report summary statistics on the correlation between firm and worker effects (small and negative) and on the correlation between successive firm effects for movers (essentially zero). The estimated firm and worker effects are used to study the effect of worker reallocation among firms on the distribution of earnings in the State of Maryland. The individual effects (θ_i, the person effect including permanent differences in observables like education) account for 55% of the variation in log wages. Firm effects account for 35%. The small negative correlation of the firm and worker effects is associated with relatively large changes in the distribution of earnings over the period.

Bingley and Westergard-Nielsen (1996) consider the determinants of log wages using the Danish IDA (see Table 1). They adopt the specification in Eq. (3.4) but they use a random effects, say $\theta^*_i + \phi^*_j + \psi^*_{ij}$, that permits correlation between the person and firm effects but assumes that both effects are orthogonal to all observable variables. The rationale for considering this form of model stems from the method that the authors use to sample the IDA data. They construct a 5% sample of workplaces (rather than employees) and, hence, there is no observed firm-to-firm mobility. Their person effect, θ^*_i, is, therefore, defined relative to the firm in which the worker is employed, rather than relative to the employee's entire measured work history. Their firm effect is defined in a manner that includes the average person effect within the firm ($\phi^*_j = \phi_j + \overline{\theta}_j$). Finally, their interaction measures the correlation within a firm of the random person and firm effects. Thus, the authors force a hierarchical structure on the person and firm effects (see Scheffé, 1959; Searle et al., 1992) implying that their effects relegate a part of the person effect, $\theta_i - \theta^*_i - \phi^*_j - \psi^*_{ij}$, to the model residual rather than to the person effect they estimate. Keeping these statistical qualifications in mind, they find that 38% of the variance (after controlling for x_{it}) is due to the person effect, 26% is due to the firm effect and 5.8% is due to the interaction. Their commentary indicates that the person and firm effects are of approximately equal magnitude and that the correlation between the two is not strong (due to the small contribution of the interaction term) but, because of the sample design, this conclusion is not strictly comparable to the other studies in this section.

Goux and Maurin (1999) use data from the French labor force survey (see Table 1) matched with employer information to study the influence of individual and firm factors on inter-industry wage differentials. Using Eq. (3.4), these authors estimate the underlying model, identifying about 1000 firm effects and about 10,000 individual effects (over two 3-year periods), by full least squares and by a correlated random effects method. They find that person effects are more important than firm effects as components of the variance of log wages. They also find that the correlation between firm and person effects is small and negative. Goux and Maurin use the results of their statistical analysis of the components of earnings to the decomposition of inter-industry wage differentials in Eq. (3.9), these authors find that the inter-industry differences in average person effects are the main source of inter-industry wage differences in France. The part of the inter-industry wage differential explained by the firm effects is very small. There is more firm effect variation within an industry than between industries.

Finer (1997) uses the matched employer–employee NLSY '79 data (see Table 1) to estimate Eq. (3.4) directly by least squares and by a variety of other methods proposed by Abowd et al. (1999a) and Abowd and Kramarz (1999). Their full least squares results show that the person effect θ_i, and its counterpart with observable non-time-varying effects removed, α_i, explain about 35% of the variation in log hourly wages, while the firm effect $\psi_{itJ(i,t)}$, which includes a heterogeneous seniority effect, accounts for 5% of the variation. The correlation between the two effects is -0.049.

5.2. *Models with firm effects only*

In this type of work, analysts estimate a variant of Eq. (3.1) in which person fixed-effects, θ, are absent. Thus, the estimated firm effect is the sum of the true firm effect, ψ, and the firm-average of the persons effects, appropriately corrected for correlation between personal characteristics and person effects. The evidence discussed in the previous section for Danish, French, and American data, suggests that the correlation between the person effects and the individual characteristics causes a large omitted variable bias and prevents a clean interpretation of the studies discussed in this section. The introduction of plant or firm effects does not help to capture a lot of the correlation between individual effects and personal characteristics because of the low correlation between person and firm effects, again, as shown in all estimated equations discussed in the subsection above.

The papers considered in this subsection use data from a variety of countries. Two of them use French data (Kramarz et al., 1996; Pelé, 1997), two use American data (Groshen, 1991a; Troske, 1999), one uses both American and French data (Abowd et al., 1998b), one uses Belgian data (Leonard et al., 1999), one uses Portuguese data (Cardoso, 1997), and one uses German data (Stephan, 1998).

The work of Groshen (1991a, in particular, and surveyed in 1996) is an important precursor to the papers that use matched employer–employee data discussed in this section. Groshen uses employer-based salary survey data to study the role of employer effects on wages. Employer-based salary surveys contain information about the participating firms. Generally, however, the only characteristic of the employer used in the statistical analysis is the identity. Estimating Eq. (3.1) with the person effects replaced by occupation gives Groshen's primary result, which is that establishment effects are a very significant component of compensation. The papers discussed in this section try to link this finding to basic characteristics of the establishment or firm.

Kramarz et al. (1996) first document the increasing inequality in France between 1986 and 1992, the dates at which the ESS was performed. A large part of this increasing dispersion is due to firm-specific compensation policies as measured by the firm effects. Indeed, the standard deviation of this firm effect increased by almost 30% between the two dates. On the other hand, the observable characteristics explain a smaller fraction of the variance in 1992 than in 1986. Furthermore, the authors compute a specialization index proposed by Kremer and Maskin (1996) to examine whether workers with the same observed characteristics are employed increasingly in the same firms. These indices grow strongly between 1986 and 1992, implying that workers are increasingly employed in firms with other similar workers. Another important feature, also found in Cardoso (1997), is the decreasing importance of returns to seniority: the wage-setting rules rely more on experience and less on seniority.

For each firm, the authors estimate the fixed firm effects. These estimates are then used in a second set of regressions that tries to explain the level and the growth of firm-effects for all firms that are present in 1986 or 1992. In all these regressions, the establishment or firm-level variables used as independent variables are the size, the existence of a firm-level collective agreement, existence of an industry-level collective bargaining agreement, the proportion of workers employed at different skill-levels, the existence and number of shifts (in level at each date for the first two regressions and in difference (1992 minus 1986) for the growth regression). Indeed, most variables matter both in 1986 and 1992, with the size of the establishment, the proportion of highly skilled workers, and the existence of 4 or 5 shifts being the most important. Interestingly, these same variables are also best (positively) correlated the growth of the fixed-effects between 1986 and 1992. Finally, in order to investigate the firm by firm compensation policies, the authors concentrate on a subsample of 132 establishments or firms for which they have a sufficient number of observations both in 1986 and 1992 and perform firm by firm wage equations for both dates. They use the estimated coefficients (on experience, measured as the experience prior to entry in the firm, and seniority) to examine how they relate one to the other as well as their correlation with mean experience and seniority at the firm. They show that the firm-specific intercept is negatively correlated to the seniority coefficient, a feature also found in Abowd et al. (1999a). They also find that the seniority coefficient is also negatively related to the mean seniority; high-seniority firms do not reward seniority very highly. Finally, the authors show that the evolution of the mean seniority at the firm (which increased by 3 years between 1986 and 1992) is negatively correlated to evolution in the mean experience (which only slightly increased) which shows that firms reduced drastically their hiring of young workers and separated mostly from workers with little seniority.

Cardoso (1997) used a very similar dataset (described above) to examine related issues. More specifically, she tried to understand the origin of the increase in wage inequality in Portugal, an increase that started between 1983 and 1986, a timing that is identical to what was observed for France (Kramarz et al., 1996). In addition, she showed that most of this increasing inequality occurred within firms rather than between firms. Therefore, she tried to identify the dimensions along which this within-firm inequality developed. First, she computed a specialization index as in Kramarz et al. but, in contrast with what was found for France, specialization decreased in Portugal between 1983 and 1992, i.e., workers with different attributes have been working more and more together. Then, the author estimate a hierarchical model of the following form:

$$y_j = X_j\beta_j + e_j,$$

where y_j is a $(n_j \times 1)$ vector with n_j being the size of the employing firm, where X_j is a $(n_j \times K)$ matrix of workers observables, β_j is the $(K \times 1)$ vector of coefficients, and e_j is the $(n_j \times 1)$ statistical residual vector which is assumed to be distributed $N(0, \sigma^2 I_{n_j})$ with I_k being the identity matrix of size k. Furthermore, each coefficient β_j is modelled as the sum of a fixed component, β_0, and a random component, α_j, normally distributed with zero mean, Γ variance matrix, and independent of e_j.

The author estimated this model both in 1983 and in 1992. She displayed the distribution of these coefficients at both dates for the following variables: experience, tenure, tenure smaller than 1 year, sex, and schooling. She also tested the equality of the two distributions (using a Kolmogorov–Smirnov test). The main features that emerge from this statistical analysis are the following. Apart from experience, all other returns changed between 1983 and 1992. In particular, the gender gap increased, returns to schooling increased very strongly, while returns to tenure decreased. Such conclusions are directly related to the process of modernization that was taking place during this period, and still is, in the Portuguese economy.

Troske (1999) examines the employer size-wage premium using the WECD (see Table 1). As in most earnings function, the author starts by estimating the following equation

$$\ln w_i = X_i\beta + Z_{j(i)}\gamma + u_i,$$

where X_i is a vector of individual i's characteristics, $Z_{j(i)}$ is a vector of the employer j of individual i's characteristics, and u_i is a residual term. Among those employer characteristics, Troske uses the logarithm of the establishment employment as well as the logarithm of the firm employment. First, he shows that all results obtained using the WECD, without employers characteristics, are identical to those obtained using the 1990 SDF (Sample Detail File, from the Census, used to construct the WECD). Then, Troske shows that the size of the employing establishment or firm generates large returns (for instance, workers in plants with log employment one standard deviation above mean log employment receive 13% higher wages than workers in plants with log employment one standard deviation below mean log employment, the equivalent number for firms is 11%). After having established these basic facts, the author tries to find potential explanations for these large returns. First, to check if these returns come from the fact that large firms hire more skilled labor than other firms, he introduces measures of skills of the work force in the above regression. More specifically, the added variables are the mean years of potential experience of workers in the plant, the percentage of workers who are scientists, engineers, or technical workers, the percentage of workers who have some post-secondary education (but no college degree), and the percentage of workers with at least a college degree. The returns to size of the establishment fall from 13% (see above) to 11%, and from 11% to 9%. Second, the author examines the capital-skill complementarity hypothesis by introducing the capital–labor ratio of the plant. Once the skills of the work force variables are in the equation, the introduction of the capital–labor ratio reduces the coefficient of the firm-size variable (yielding a 6% premium). But, the introduction of this capital–labor ratio does not reduce the establishment size-wage premium. Then, the plant age is shown to be uncorrelated to the wage, once workers' characteristics are controlled for. To assess the rent-sharing hypothesis as an explanation of the firm-size wage premium, Troske uses the proportion of the total value of a seven-digit product produced by the plant and an Herfindahl index of concentration computed at the primary five-digit product of the plant. None of these variables affect the firm-size wage premium. The same diagnostic applies to measures of the managerial skills at the plant, the proportion of supervisors at the plant (as a measure of the cost of monitoring). Finally, the inclusion of the logarithm of total new investment in computers (in 1987) per employee adds no information as soon as the size and the labor–capital ratio are present in the regression.

Troske's conclusion is consistent with the results reported by Abowd et al. (1998b). Large employers appear to employ better workers. Most establishment or firm-level variables do not explain a large fraction of the firm-size wage premium.

Stephan (1998) uses a German dataset to examine similar questions to those we just analyzed. This dataset, the GLS (see Table 1), has matched employer–employee information for two cross-sections (1990 and 1995) of firms active in Lower Saxony, one of the largest German Länder. Each wave contains approximately 65,000 employees and 1500 firms. The sampling frame is such that for small firms all employees are included in the data while less than 10% of employees are in firms with 1000 employees or more. In addition to sex, tenure, age, contractual and effective working hours, shift or night work, and wages (with information on overtime, taxes and social security contributions) reported directly by the firm, schooling and occupation come from social insurance data, matched to this survey by the German statistical office. Indeed, the structure of the dataset is very similar to the French ESS. Stephan uses the hourly wage rate excluding overtime pay as the dependent variable. For blue-collar workers, between-plant dispersion accounts for a large fraction of the variance in wages (80% for females but less so for males) while this is the reverse for white-collar workers (60% for males and 40% for females). Then, Stephan notes that the inclusion of fixed-effects modifies the estimated coefficients in wage regressions. For firms with more than one sampled worker, the author computes establishment fixed-effects from the first-stage regressions for the above four groups of workers. Stephan finds that the dispersion of these fixed-effects is not different from those observed in other countries and that the standard deviation of these fixed-effects has increased between 1990 and 1995. Since Stephan estimates at most four effects per firm, it becomes possible to look at their correlation. He finds positive correlation between these effects. Finally, Stephan also performs firm by firm regressions and analyzes correlations between various estimated coefficients. His results show that returns to age and returns to tenure are negatively correlated. The author's results give the impression that pay determination in Germany does not differ widely from what is observed in other countries including the United States.

Leonard et al. (1999) use a Belgian dataset (see Table 1) to examine productivity in relation to firm compensation policy. To do that, they start their analysis by performing the same kind of regressions as done by Kramarz et al. (1996) and Stephan (1998)-firm by firm regressions of individual wages on observed characteristics in each year of their sample period. This results in 695 (number of firms) times 8 (years) of firm-specific estimates of a constant, age profiles, sex differentials, and white-collar/blue-collar differentials. They find that, as in all other countries, pay dispersion between and within-firms has increased over the period. By examining correlations across time of the estimated coefficients, as in Kramarz et al., they find evidence of large persistence of pay policies (see Section 5.2). These pay policies differ widely from firm to firm.

Chennouf et al. (1997) estimate the same type of equation using matched worker-firm data for a sample of establishments of the Algiers region (see Table 1). As in Cardoso (1997), the authors try to control for the group effects when estimating Mincer's model that includes both years of education and potential experience. Their results show that returns to education decrease when firm-effects are introduced.

Abowd et al. (1998b) compare the relative importance of employer and employee effects in compensation in France and in the United-States. For the US, they use the 1990 WECD while, for France, they use the ESS for 1986 and 1992. The basic statistical model used throughout the paper is identical to the one described in the statistical section in which the wage is regressed on worker's characteristics and a firm (or establishment) specific constant as follows:

$$\ln w_{it} = X_{it}\beta + \phi_{j(i,t)} + \varepsilon_{it}.$$

Then, the authors use the estimates to decompose the average wage at the firm into a part due to the average observed characteristics of the workers employed at the firm and a part due to the firm-effects, $\phi_{j(i,t)}$, to analyze the impact of compensation structure on firm's productivity and profitability.

The wage equations give the following results. First, coefficients are not very dissimilar across countries. A first noticeable difference is the shape of returns to experience which are steeper and never turn down in the US whereas the French profile peaks at 34 years of potential experience. A second interesting difference are the respective R^2 which are larger in France (around 0.80) than in the US (around 0.60). Therefore, firm fixed-effects obviously explain a larger fraction of wages in France than in the US.

Then, the authors present a table of correlation among the components of individual compensation. Strikingly, the correlation structure is very similar in the two countries. In particular, individual characteristics and firm fixed-effects are comparable in terms of their contribution to the variation of annual wages (approximately from 0.6 to 0.7) with an inter-correlation of 0.25 in the two countries. None of the above results are inconsistent with those of Abowd et al. (1999a,c) since the firm fixed-effect as estimated by Abowd et al. (1998b) are a mixture of individual and firm fixed-effects (see Eqs. (3.4) and (3.7)).

Finally, the authors estimate the impact of the compensation structure on firm's productivity and profits. They show that firms who employ workers with higher predicted wage rates (based on observed characteristics) are more productive (both in terms of log value-added per employee and of log sales per employee) in the two countries. The same is true for firms with higher fixed-effects. However, none of these two components have an impact on profitability.

This study confirms the above findings: the structure of compensation is very similar across countries. In addition, the effects of the compensation structure on firm outcomes appear roughly identical in two apparently different countries, France and the US.

Pelé (1997) also uses the French ESS to examine the effect on compensation of the coexistence of different methods of pay in the same firm for the same detailed occupation. This type of dataset, by matching firm and workers, enables him to compare within a firm and within an occupation (in a 4-digit classification) workers paid under time-rates or piece-rate (measured under different rules). There has been a great body of literature dealing with the choice of method of pay and the wage differentials due to various ways of payment. For example, Seiler (1984) showed that incentive workers receive more disperse and higher earnings than time workers. Brown (1992) found that piece-rate workers receive higher earnings than time-rate workers but when compensation is linked to merit (an evaluation by supervisors), it is lower than time-rate.

The data come from the Wage Structure Survey (Enquête Structure des Salaires) carried out by INSEE in 1986 (see above). For each worker, the age, seniority in the firm, sex, method of pay, conditions of work (especially work in shifts), occupation are used as control variables in the wage equation. Besides, for each firm or establishment, a the identification number is used to identify workers employed at the same establishment or firm. Four methods of pay are possible. The first one is time-rate, which consists in a salary. The three other ones are bonus payments. The bonus is based either on individual output, or on collective output or on both kinds of output. A worker can receive a bonus of exclusively one of those three types. Pelé used the total wage in October 1986, the amount and the type of the bonus (if it is the case) and the payment for overtime. He corrected for differences in hours worked in order to compare wages for a same duration (which is equivalent to use an hourly wage). To estimate the wage equation, he added indicator variables for each bonus method. To control for the exact occupation in the firm, Pelé introduced an indicator for each 4-digit occupation within each firm or establishment. Only those couples (occupation-establishment) with two workers employed in the same occupation within the same establishment under different methods of pay contribute to the identification of the coefficients of the bonus methods variables. Therefore, the occupation-establishment fixed-effects are nested within the pure establishment fixed-effects.

The results are the following. First, he found that bonus payments lead to higher compensation, result which is consistent with a selection of workers among the different payment schemes. It is profitable to give incentives through a bonus payment only to the best workers. But beyond this first conclusion, Pelé also showed that workers who get a bonus also receive a higher base wage, when comparing within homogeneous groups of workers of the same occupation in the same firm. High French minimum wages may partly explain such an observation, by preventing firms to set a low value to the base wage.

Other recent papers that include employer effects in wage equations and use matched data to study the resulting estimates include Bronars et al. (1999) and Vainiomäki (1999).

5.3. Models of the wage-seniority relation

How large are returns to seniority? This question has generated many important articles in the last 20 years. Some authors argue that returns to seniority are large and pervasive (on the order of 5% a year) while others find these returns to be small.

Although Topel and Ward's data were based on a matched employee–employer dataset, the LEED file constructed from the Social Security reports by employers, these authors did not use this aspect of their file. Indeed, they restricted their initial sample of over one million individuals to a final sample of 872 persons. Therefore, they lost the potential of looking at the inter-firm variability in the returns to seniority.

The possibility that returns to seniority might vary between firms has only been examined recently. Abowd et al. (1999a) allow for such variation in an exogenous mobility framework using French data. Margolis (1996) reexamines Topel's two stage estimator on the same French dataset. At least five other papers examine this issue of firm-specific compensation policies, two on US data (Bronars and Famulari, 1997; Finer, 1997), two on Norwegian data (Barth, 1997; Barth and Dale-Olsen, 1999), one on Italian data (Pacelli, 1997), and one on Portuguese data (Cardoso, 1997). Another paper (Dustmann and Meghir, 1997), based on German data, allows for the possibility of firm-specific returns to tenure even though the authors do not introduce firm fixed effects.

Abowd et al. (1999a) provide different estimation techniques for firm-specific returns to seniority. These different estimation methods (see above for brief description) provide estimates that differ in their levels but which are largely correlated across methods.

The consistent methodology, which uses first differences for workers who do not move between two consecutive years, gives the largest estimates and, indeed, the closest to the OLS results. Notice however that this methodology assumes that mobility is exogenous. All other techniques examined in this paper give returns to seniority that are close to zero. But all methodologies show that there is considerable between-firm variation in these returns. And, furthermore, all of these estimated firm-specific returns are strongly correlated across estimation techniques. Unfortunately, these authors do not examine the same question when mobility is endogenous.

Margolis (1996) tries to address this issue by allowing firm-specific compensation policies to vary by entry-cohort (the cohort of entry refers to entry in the firm). The data he uses are identical to Abowd et al. (1999a). Margolis compares OLS estimates other techniques. First, he examines on French data the results using Topel's two-stage techniques. He shows that based on French data, the returns to tenure are much lower than those estimated by Topel, 2% against 5% using US data. But, Margolis also notes that unobserved heterogeneity may well bias these results. Hence, he goes one step ahead of Abowd et al. (1999a) by introducing within-firm cohort-effects. Although the value of the mean value of the estimated seniority slopes is close to zero, Margolis (1996) finds even more variance in the returns to seniority than previously found in Abowd et al. (1999b).

Bronars and Famulari (1997) examine similar questions use a US dataset, the WCP (see Table 1), matching roughly 1700 workers and 241 firms. In addition, for 736 workers employed in 130 establishments, retrospective information on the starting pay is available. That allows the authors, first, to estimate returns to seniority based on a cross-section equation that includes firm or establishment fixed-effects. Then, they look at within-firm wage growth with, once more, establishment fixed-effects. Hence, the first estimates are directly comparable to the OLS with firm fixed-effects given in Abowd et al. (1999a) while the second are also directly comparable to the consistent estimates from the same authors.

At least for men, the authors find that estimated returns to tenure are roughly equal to 1% a year and approximately invariant across estimation techniques. This result does not hold for women. In addition, women's wage growth is larger than man's wage growth. This result, consistent with Abowd et al.'s (1999a) findings, demonstrates that exogenous mobility is not a very reasonable assumption and that all these estimated returns to seniority are likely to be biased upwards. Finally, Bronars and Famulari find important variation across firms in returns to tenure; standard deviation of the estimated firm-specific slopes is equal to 0.022, consistent with the French estimates giving a larger number based on a much greater number of firms.

Barth (1997) estimates related coefficients for Norway. He uses a cross-section of 2321 workers employed in 549 firms. His base equation is identical to the one estimated by Bronars and Famulari (1997). As found by Abowd et al. as well as Bronars and Famulari, the estimated coefficients are identical across estimation techniques, i.e., OLS, firm random effects, and firm fixed-effects. One additional year of seniority adds approximately 0.3–0.4% to the individual's wage, a much lower number than the one found in the US or in France. Interestingly, Barth finds no evidence of correlation between seniority and firm fixed-effects while there is a positive correlation between education or age and these fixed-effects. These final results are consistent with those given in Kramarz et al. (1996) (see Section 5.2). Barth and Dale-Olsen (1999) examine the relation between wage-seniority profiles and worker turnover using a heterogeneous firm effect model as in Eq. (3.2). They show that employee lower turnover is associated with having higher initial wages (ϕ_j) and higher slopes (γ_j).

Cardoso (1997) examines identical issues with the same type of dataset, i.e., cross-sections of matched employer–employee data (see Section 5.2 for other results using the same dataset). Using a multilevel model with the associated estimation method, the author confirms the heterogeneity of the returns to seniority across firms. Most estimated returns are inferior to 1%. She also estimates the distribution of firm-specific starting wages (hence, the firm-specific component of wage is either this latter part for the entrants or the former part for those with 1 year or more seniority), negative indeed for most of the distribution. Notice once more that these results are not widely different from those estimated in all other European countries.

The technique used in Dustmann and Meghir (1997) for Germany is completely different. Even though their data is based on Social Security reports of firms, these authors do not have full access to the matched employer–employee component of their data source. But, the principle of their technique – instrumental variables – is directly applicable and conceived for matched employer–employee data.[8] Their instruments are firm closure and information on the job held two jobs ago (available only for workers with at least three jobs in their dataset). Indeed, allowing for heterogeneous returns across firms yield surprisingly high estimates (which jump from 4.5% to 9%), casting some doubt on these exact

[8] That they should eventually obtain.

values based on approximate data, for instance sector-specific firm closures instead of firm-specific firm closure, on a selected sample of young apprentices, but showing the fruitfulness of the general approach.

Finer (1997) uses the same models as Abowd et al. (1999a). He finds that the average return to 1 year of additional seniority is 5% for the first 5 years and zero thereafter in the NLSY '79 data. The standard deviation of the estimated seniority coefficient is of the same order of magnitude, which indicates that in this younger sample there is still considerable heterogeneity in the return to seniority.

Pacelli (1997) examines identical issues using a longitudinal sample of 1737 young Italian workers under a period of 5 years with information on the employing firm constructed from the R&P dataset (see Table 1). The methodology adopted resembles Topel and Ward's. All estimates show that returns to seniority are, once more, smaller in Europe than in the US, even when controlling for firm-specific variables in a wage growth equation (see also Entorf and Kramarz, 1999, for similar results), but still significant.

6. New results with matched employer–employee datasets: wage and employment mobility

In this section we consider models of the changes in wage rates and employment that have been estimated using matched employer–employee data. All of the papers make use of large longitudinal administrative data sources that are dynamically representative of the target populations.

Jacobson et al. (1993) study the earnings losses of displaced workers in the State of Pennsylvania. A 5% random sample of the quarterly earnings reports from the State's unemployment insurance tax records for the period from 1974 to 1986 were matched to employer data from the State's ES-202 files, which are also administrative data from the unemployment insurance system. These authors define large involuntary worker displacements using the matched data. In particular, using the matched information about the employer, these authors are able to define a mass-layoff displacement, where there is a large reduction in the employment of the firm surrounding the displacement, and a non-mass-layoff displacement. They find that the earnings losses for mass-layoff displacements are very large: initially 25% of predisplacement earnings, rapid recovery during the first two post-displacement years to losses of around 15% of predisplacement earnings, followed by many years of stable earnings with no further recovery. The non-mass-layoff displacement sample has an initial loss of about 15% followed by a rapid recovery during the 2 years following displacement in which the full earnings loss is recovered. For all of the comparisons it is possible to use the earnings histories of workers who do not suffer displacements as the comparison group, including the possibility of comparing workers who were not displaced during the mass layoff, but who worked for the firm that incurred the mass layoff, with those who were displaced. Using this information, these authors find that individuals who are going to suffer a mass layoff also experience an earnings decline in the 3 years prior to the mass-layoff displacement. Workers who are going to be displaced in a non-mass-layoff displacements do not experience a predisplacement earnings decline.

Topel and Ward (1992) use the quarterly LEED data for 1957 to 1972 (see Table 1) to study the early career wage and employment mobility of young male workers. Although these authors use the employer identifying information only to identify the within-job wage growth, they are among the very few researchers to have used the LEED data in this manner. They find that the typical young male worker holds seven full-time job during his first 10 years in the labor force. Within job wage growth is one-third of total wage growth over this period and between job wage growth accounts for the remainder. Wages grow at an annual rate of 11%. Although the authors do not directly implement Eq. (3.1), their basic model is consistent with this formulation and their method for identifying within and between job wage growth.

Burgess et al. (1997, 1999) examine earnings dispersion using a decomposition similar to those described previously. They first decompose wages into a person fixed-effect, a firm fixed effect, a time trend, an unemployment effect, and a residual. Then, they examine the share of earnings dispersion that can be attributed to the different components. In particular, they focus on the share attributed to individual fixed-effects, the share attributed to establishment fixed-effects, the share due to the correlation between individual and firm fixed-effects, and, finally, the residual unexplained variance. Using a sample and a technique described above, they estimate individual and establishment fixed-effects for 2000 individuals and their 1432 employers based on a dataset with more than 2,700,000 quarterly observations. Their estimates show that 55% of the variance in log wages can be attributed to individual heterogeneity while firm-effects account for 35% and the correlation between person and firm effects is virtually nil. Then, they try to assess the share in the increased earnings dispersion over the 1980s that can be attributed to reshuffling of jobs between and within employers. Their first estimates seem to support the idea that, indeed, reallocation of jobs across firms was an important source of increase in the dispersion of wages under the sample period.

Abowd et al. (1998a) examine job and wage mobility in France and in the US using comparable matched employee–employer panel datasets for both countries. Most of their analysis focuses on employment durations and wage changes both between and within firms. The employment spells can be constructed because of the matched nature of the data; the employer identifier is a crucial component when constructing the individual careers across time and firms. Even though the analysis is still preliminary, the authors findings show that, in contrast with the usual view that the French labor market is inflexible i.e., little employment mobility and considerable real wage stability, in France there is substantial employment mobility, although the most mobile groups in France are not the same as those in the United States, and there is substantial real wage mobility on changes of employers.

Margolis (1998) uses several sources of matched employer–employee data (the DADS, BIC and BRN) to consider the effect of firm closure on workers in France. He find that a large share (almost 60%) of workers displaced by firm closure find new jobs without experiencing any interruption in their employment histories. In addition, falling into nonemployment appears to be a relatively transitory phenomenon for displaced workers, with over three-quarters finding a new job within the year following displacement and essentially all of them being reemployed 6 years after displacement. Workers who separate for reasons other than firm closure, on the other hand, have a much harder time, with 25% still without a job 6 years after separation. Wage changes for displaced workers in France reflect a major difference between those who find new jobs quickly and those who do not, with a wage penalty of over 20 percentage points for displaced workers who do not find new jobs in the year following their separation. The pre-separation pattern of wages shows a drop in the year preceding separation. Controlling for seniority differences causes wages for displaced workers to be consistently below those of continuously employed workers, even in the pre-separation period, and the penalty for finding a job slowly drops to under 10%.

7. New results with matched employer–employee datasets: firm outcomes and worker characteristics

7.1. Productivity

In this subsection we consider studies that relate individual characteristics of the employees and of their compensation to the productivity of the enterprise or establishment. The employer-level measures of productivity come either from direct measures of production or value-added per worker or from full production function specifications.

Hellerstein et al. (1996) use the WECD (see Table 1) to study the relative productivity of employee characteristics, estimated directly from a production function, that they compare to the relative pay earned by these different characteristics, estimated directly from a wage equation. They use a variety of production function specifications to capture the marginal productivity associated with employee characteristics like sex, race, marital status, age, and education. They use standard cross-sectional wage equations (estimated for the US census year 1990, which is the only year of individual data available in the WECD) to capture the market compensation associated with these factors. The find that workers who have ever been married are paid more than never-married workers and that there is a corresponding productivity difference of the same magnitude. On the other hand, prime-age workers (35–54) are as productive as younger workers but earn a wage premium. Wage premia for older workers (55–64) exceed all estimated productivity premia for this group. The same technique is used to conclude that wage differentials unfavorable to blacks are also associated with productivity differentials of the same magnitude. Wage differentials favoring men are not associated with productivity differentials of the same magnitude. Bayard et al. (1999) extend the analysis of Hellerstein et al.

(1996, 1997) to include non-manufacturing establishments using the NWECD (see Table 1). Their results are discussed below under sex segregation in the workplace.

Haegeland and Klette (1999) estimate similar productivity and wage models using Norwegian data. They find that education, except those with the lowest education, premia are directly and appropriately related to productivity differentials. Workers with highest experience have wage premia that exceed their productivity while the opposite is true for those in lower experience categories. The lower wages of females correspond to productivity differences of equal size.

Hayami and Abe (1998) use the Japanese matched data to estimate a full set of labor demand equations for age and sex categories for retail trade to study the deregulation of this labor market and the resulting effects on wages, employment and productivity.

Abowd et al. (1999a) develop a method for relating the firm effect, ψ_j, and the average value of the person effects (θ_j and its components $\bar{\alpha}_j$ and $\bar{u}_j\eta$) to firm-level productivity, profitability and factor use. The profitability results are discussed in Section 7.1. To measure productivity, they use firm-level value-added and sales per worker, averaged over the period 1978–1988. They find that all firm-level compensation components related to personal and enterprise heterogeneity are positively related to both productivity measures.

Abowd et al. (1998b), also take the firm effects from their analysis of compensation (see Section 5.2) and relate these to value-added and sales per worker for both the US and France firm effects inclusive of the average value of person effects within the firm, $\psi_j + \bar{\alpha}_j$, are positively related to the productivity measures.

Finer (1997) estimates an equation relating productivity, measured as ln(sales/employee), to the components of compensation structure. He finds that all compensation structure components are strongly related to sales/employee.

Using the estimated coefficients of firm by firm regressions across time, Leonard et al. (1999) examine the relation between firm-specific compensation policies and productivity (measured by value-added per worker). Since they have multiple estimates of the same coefficient for the same firm but different years, these authors are able to estimate this relation with fixed firm effects. They find that firms with high wage levels (i.e., the firm-specific constant of the firm by firm regression), returns to age, white-collar pay premium, or male pay premium also have high productivity.

7.2. Productivity and seniority

Kramarz and Roux (1998) use the DADS for the period 1976–1995 to examine the relationship between within-firm seniority structure and firm performance. Hence, these authors provide one of the first analysis of the impact of hiring and separations decisions on firm-specific outcomes such as productivity or profitability as well as employment or capital structures.

They first measure the seniority at the end of all job spells (either censored or non-censored). Then, these seniorities are aggregated at the firm-level for three subperiods

(1977–1982, 1983–1988, 1989–1994) in order to compute firm-specific descriptive statistics of the seniority structure. Using the Echantillon d'Entreprises (see Table 1) that gives information on balance-sheet, skill structure, and employment, Kramarz and Roux estimate various equations relating tenure structure and firm performance. The use of three subperiods allows these authors to perform instrumental variable techniques, in particular, they estimate their coefficients with equations in first difference (subperiod 3 minus subperiod 2) instrumented by the levels of subperiod 1. Their results show that a low turnover rate is associated with higher productivity but a high turnover rate slightly favors profitability. In addition, an increase in the within-firm variance of the seniority of the stayers (i.e., those workers who stay employed at the firm until the end of the subperiod, and, therefore, have censored spells) boosts profitability. Finally, capital, firm size, and the capital–labor ratio are all positively related to low turnover rates.

7.3. Profits

Four papers consider the relation between profits and firm-specific compensation or hiring and separation policies using matched employer–employee data. In all cases, the profit measure is operating income divided by total assets. Two papers focus on France (Kramarz and Roux, 1998; Abowd et al., 1999a), one uses American data (Finer, 1997), and one compares the French and the American case (Abowd et al., 1998b). The equation relating firm level profits, π_j, to firm level compensation policies, ψ_j and $\bar{\theta}_j$, and other firm level measures, z_j, is given by

$$\pi_j = z_j\gamma + \alpha\psi_j + \beta\bar{\theta}_j + \varepsilon_j. \tag{7.1}$$

This equation is estimated directly in Abowd et al. (1999a) and Finer (1997) while Abowd et al. (1998b) cannot separate person from firm-effects since they use cross-sections (WECD and ESS for the US and France, respectively). Kramarz and Roux replace ψ_j and θ_j by the within-firm seniority structure (see Section 5.3).

Abowd et al. find that those firms with higher wages because of observed characteristics, $\bar{x}_j\beta$ (in z_j), or with a larger firm effect, ψ_j, (thus, high-wage firms) are more profitable, while those employing high-wage workers (large values of $\bar{\theta}_j$) are not. Finer finds no effects on profits, although his data are from a sample representative of younger workers. Interestingly, Abowd et al. (1998b) find no effect for the same equation as soon as ψ_j and θ_j are confounded, a result which is fully consistent with the previous one. Finally, Kramarz and Roux find that a higher turnover rate as well as a larger variance of within-firm seniority tend to induce a larger profitability.

7.4. New technologies

We know that changes in the structure of wages were dramatic along the 1980s in the US while unemployment increased in Western Europe. Many analysts have blamed the same technical shocks that affected differentially the two continents. The role of computers has been central in the indictment, in particular after Krueger's seminal work (Krueger, 1993) that showed that computer users were better paid than non-users. On both sides of the Atlantic, researchers have tried to understand the nature of the computer wage premium. Two sets of studies, one for the US and one for France, are of particular interest for us since they use matched employee–employer datasets. They both demonstrate that new technology (NT) workers were better paid than non-users even before using NT (Entorf and Kramarz, 1997, 1999; Entorf et al., 1999) or that NT firms employed high-wage workers even before implementing NT (Doms et al., 1997).

Doms et al. (1997) use the WECD (see Table 1) in conjunction with the 1988 Survey of Manufacturing Technologies (SMT) among many data sources. The 1988 SMT contains plant-level information on NT use in American manufacturing plants. The techniques surveyed are production technologies such as robots, computer-aided design (CAD), lasers, networks, automatic systems, or computers used on the factory floor. To assess the technical development of the plant, the authors use the count of different techniques used at the plant. The SMT is matched to the WECD. This allows Doms et al. to examine the relation between the spread of techniques and education or the occupational mix of the work force. To perform this analysis, since they do not know if individual workers use a given technique, they create various plant-level measures of the educational or occupational structures. Results demonstrate that plants that use more advanced technologies employ a more educated or a more skilled work force. Using the same framework, they examine the relation between wages, once more averaged at the plant-level, and NT. The analysis is performed for different subgroups (production workers, managers and professionals, other non-production workers) and include average characteristics of the group under consideration together with plant-level employment and capital–labor ratio. These results show that, as in Krueger (1993), technology use is associated with a premium even after inclusion of workers characteristics. Then, using the LRD and the 1993 SMT, a longitudinal analysis demonstrates that the most technology advanced plants paid their workers higher wages prior to adoption of NT.

The same pattern emerges from the three studies performed on French matched employee–employer data. However, since the datasets used in these studies are built from the supplements on NT of the1987 and the 1993 waves of the French Labor Force Survey (LFS) in which workers can be followed at most three times (from 1985 to 1987 and from 1991 to 1993, respectively), it is possible to perform an individual-level longitudinal analysis while controlling for the employing firm.

The data used in Entorf and Kramarz (1997, 1999) come from four different INSEE sources. The basic sources are the French LFS, 1985–1987, a 3 year rotating panel, and the "Enquête sur la Technique et l'Organisation du Travail auprès des Travailleurs Occupés" (TOTTO) from 1987, an appendix to the labor force survey that asked questions about the diffusion of new technologies and the organization of the work place. Besides usual questions from labor force surveys (salary, tenure, age, education, etc.) the appendix contains information on the use (e.g., intensity, experience) of microcomputers, terminals, text processing, robots and other well specified groups of "New Technology" labor. The use of computers is described in more detail than in other surveys (see Krueger, 1993, for instance). The questionnaire provides explicit categories for using microcomputer for text processing only, data entry and use of listings. "Terminal" even covers a distinction between "reception only," "emission only" and both reception and emission while information on production techniques are also present.

In the first version of the TOTTO survey, only the 1987 employing firm is known (using the standardized Siren enterprise identification number). This feature of the French INSEE classification system enables the authors to employ information from corresponding firm-level surveys (BIC, which collects annual information on balance sheets and employment and ESE, which collects information on the employment structure).

In the cross-section, the approach is identical to Krueger's (1993). Entorf and Kramarz regressed the log of monthly wage on a vector of characteristics of the individual X_i and a vector of indicator variables for workers using one (or more) of the various NT groups. These variables were supplemented with firm-level characteristics $Z_{j(i)}$ (where $j(i)$ denotes the firm at which i is employed), some of which are available from the complement to the labor force survey (working time schedules, sector, size) and the others from the firm-level panel dataset (size, assets, profits, skill structure, export ratio). In all regressions, they control for the usual observable variables. Their results show that, in 1987, a worker receives a 16% bonus for using modern computer-related NT. This premium can be decomposed into two parts: for a worker with no NT-experience, a NT worker receives a premium of approximately 6%. Returns from experience with NT add 10% to the above premium (when estimated at the average level of experience in the population of modern computer-related NT users). When firm-level variables are introduced, some of the above results seem to be attenuated: the coefficient of the modern computer-related NT dummy is smaller (5%) and the standard error is larger. However, the role of experience with modern computer-related NT is increased. The firm-level variables that are used, even if they do not seem to be correlated with the individual NT variables, are important and increase significantly the explanatory power of the regression. Most important is the skill structure : the more skilled the structure is, in terms of larger shares of skilled workers and of managers, professionals and technicians, the larger is the influence on the wage. This effect is particularly strong for the latter category: a 1% increase in this share entails a 0.27% increase in the individual wage. The profits (profits/assets) also have a positive impact on wages. Finally, total employment has no significant influence on earnings. Finally, if firm fixed-effects are introduced, results are unchanged.

In the longitudinal dimension, all the above effects of NT almost completely disappear. The coefficients on the NT indicator variables are never significantly different from zero. However, even though NT use per se does not yield an immediate wage gain, coefficients of the experience with modern computer-related NT variables are significantly different from zero. In Entorf and Kramarz (1997), another version of the same equation in which a dummy for each year of experience (1,2,...,9 and more) is included is estimated and results are quite similar: returns increase until workers have 5–6 years of experience and then slightly decrease. The introduction of the firm-level variables do not change these results.[9]

In addition, these firm-level variables that represent the firm-specific policy have little impact on the individual wage once individual fixed effects are introduced. Coefficients are either not significantly different from zero or small (assets).

Most of the results that we have described for the 1985–1987 period also hold between 1991 and 1993. Most datasets are identical. A new feature of the LFS is the inclusion of the employing firm identifier in every year while only the 1987 employing firm was known previously (see above). In addition, the authors use a newly available dataset, the "Déclarations de Mouvements de Main d'Oeuvre (DMMO)," an establishment-based survey on hiring and separations. Entorf et al. are therefore able to follow the workers across firms in the 3 years of the panel.

Entorf et al. (1999) estimate wage equations with NT indicator variables without and with individual fixed-effects as well as without or with firm fixed-effects. Returns to computer use in 1993 are not different from those observed in 1987. The introduction of individual fixed-effects has the same impact as obtained in Entorf and Kramarz (1997, 1999). Returns are maximal, 2%, after 2 or 3 years experience with NT. The introduction of firm fixed-effects has no impact on the estimated coefficients, both in the cross-section dimension and in the longitudinal dimension.[10] This is consistent with the Abowd et al. (1999a) findings for France as well as those of Abowd et al. (1999c) for the US-firm compensation policies (as captured by the firm fixed-effects) are not highly correlated with individual observables and individual fixed-effects. To test other explanations of the results (in particular, to control for firm-level idiosyncratic shocks), the authors use the matched worker–establishment information on hiring, quits, and terminations coming from the DMMO. Results are identical to those described above. Finally, Entorf et al. use the quarterly LFS where workers are followed for three quarters after the TOTTO survey to test whether NT workers are protected from unemployment. Indeed, they find that in the short-run, NT users are protected from job losses. This result is stable, even when using the DMMO information on quits and terminations to measure the business conditions at the firm-level.

7.5. Creation and destruction of jobs

In this part, we do not intend to describe the whole "creation-destruction" vision of the labor market. Davis and Haltiwanger's chapter in this Handbook is fully devoted to this task. In this subsection we concentrate on the new types of results that matched worker-firm data have helped to bring to researchers' attention. Many of the papers that are discussed below use the basic definitions and analysis techniques that initiated by Leonard

[9] Since only the 1987 employing firm is known, the 1985 and 1986 firm is unknown for workers who changed firm at one of these dates. Entorf and Kramarz (1999) use the 1987 firm also for the movers.

[10] As indicated in Abowd et al. (1999a), in the longitudinal dimension, 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, the authors are able to identify 494 of the 1045 firm dummies.

(1987) and Davis and Haltiwanger (1992, 1996), neither of whom used matched employee–employer data.

The analysis of worker flows, in contrast to the study of job flows, has been made possible by the use of matched worker-firm data. The researchers who started this vein, which is flourishing now, were Anderson and Meyer (1994). Using the CWBH dataset for the years 1978–1984 (see Table 1), they compare worker turnover defined as the sum of total accessions – recalls plus new hires – and total separations – temporary layoffs plus permanent separations – to job creation and job destruction measures as promoted by Davis and Haltiwanger. In addition, they use firm-level measures computed from their individual-level data in relation with their measures of job turnover. They compute firm-size, quarterly payroll per worker, and tenure at the firm to create categories such as high- or low-paying firm, high- or low-tenure firm. Then, they present a tabulation of job creation, destruction, and turnover statistics for every of the above firm-level categories (Table 2, p. 191).[11] For instance, Anderson and Meyer (1994) show that high and low-tenure firms do not differ in their temporary separation rates but widely differ in their permanent separation rates. Indeed, the same pattern is exhibited for high and low-paying firms. The same type of analysis is pursued on the number of earnings weeks lost after separations followed by reemployment (Table 9, p. 212). They are able to show that the distribution of weeks lost is extremely skewed (the mean is roughly 13 weeks as the median is equal to 2 for total separations). In addition, they show that mean weeks lost after a temporary separation are a decreasing function of firm size while mean weeks lost after a permanent separation are an increasing function of firm size. Similar computations are provided for high- and low-paying firms. These statistics being computed from individual-level data, the authors also regress the above separations variables onto the (time-varying) firm-level variables and individual fixed-effects taking advantage of the structure of the dataset in which workers can be followed from firm to firm (Table 10, p. 214).

The main disadvantage of the CWBH dataset lies in the absence of individual characteristics of the employed workers. Even though the states may have collected such information for the beneficiaries of unemployment insurance, these complementary datasets are inaccessible to the researchers. Of course, for each individual, it is always possible to compute a date of appearance and a firm-specific tenure, which is left-censored for all observations in the first-quarter of 1978. But no information on age, sex, education is used.

The same problem affects the recent analyses of Lane et al. (1997b) and Burgess et al. (1999). These articles have mostly focused on churning, the hires and separations in excess of total job reallocation using the Maryland quarterly employment and earnings information from the unemployment insurance dataset (see above). The period of analysis, 1985–1994, is the only difference between the data used in these papers and those used in Anderson and Meyer (1994). Lane et al. (1997b) provide an description of hiring and exit flows. The individual data on the characteristics of the movers have been aggregated

[11] Hamermesh et al. (1996) also document the importance of worker flows as compared to job creation and destruction for data from the Netherlands, although they do not use matched employer–employee data.

to the establishment-level and used as explanatory variables in the churning regression (Eq. (1) in their paper). The longitudinal component of the dataset allows the authors to include firm fixed-effects in this regression. One striking result, also found in Abowd et al. (1999b) for France, is that most of the changes in employment are accommodated through changes in the hiring rate.

Abowd et al. (1999b), who use an administrative dataset of all entries and exits in French establishments (see Table 1), perform most of their flow analysis at the establishment-level. Their empirical analyses distinguished between flows of workers, directly measured, and job creation and destruction, again, directly measured, using a representative sample of all French establishments for 1987–1990 (with more than 50 employees). The most important findings were that (a) annual job creation can be characterized as hiring three persons and separating two for each job created in a given year; (b) annual job destruction can be characterized as hiring one person and separating two for each job destroyed in a given year; (c) when an establishment is changing employment, the adjustment is made primarily by reducing entry and not by changing the separation rates; (d) for the highest skill groups, 10% of months with firm-initiated exits also have new hiring in the same skill group and, for the lowest skill groups, 25% of the months with firm-initiated separations also have new hiring in that skill group; (e) the rate of internal promotion into higher skilled positions is about three times the size of net employment changes inside the job category; (f) two-thirds of all hiring is on short term contracts and more than half of all separations are due to the end of these short term contracts; (g) approximately one-third of all short term employment contracts are converted to longterm contracts at their termination; (h) controlling for between-establishment heterogeneity and common trends, entry and exit of workers are both countercyclical.

Other studies that use matched employer–employee data to analyze these issues of job creation and job destruction are Norwegian (Salvanes and Forre, 1997), Austrian (Winter-Ebmer and Zweimüller, 1997), Danish (Belzil, 1997; Bingley and Westergård-Nielsen, 1998, Vejrup-Hansen, 1998; Albaek and Sørensen, 1999), Swedish (Persson, 1998), and Finnish (Laaksonen et al., 1998).

Albaek and Sørensen (1999) examine the relation between worker flows and job flows using the Danish IDA (see Table 1). In that respect, the type of analysis they perform is close to Hamermesh et al. (1996) and even closer to Abowd et al. (1998). These authors find that annual rates of hires and separations are much higher than the job creation or job destruction rates–28% and 12% respectively for Danish manufacturing. They also find that separations from existing jobs are dominated by quits. Another issue studied at length by these authors is cyclicality of the flows. They show that worker flows are strongly asymmetric over the business cycle.

Bingley and Westergård-Nielsen (1998) use the Danish IDA to show that the Danish labor market is dynamic and flexible. Among growing establishments two hires and one separation are required for each net job creation. Among shrinking establishments they find that one hire and two separations are required for each net job destroyed. Vejrup-

Hansen (1998) finds that workers separated from establishments with job destructions have unemployment incidence that is comparable to the general Danish population.

Salvanes and Forre (1997) use the individual information on the education-level of the employed workers from their registers (see above) to examine creation, destruction, entry, exit, and churning for three groups of education. They find an asymmetric and inverse U-shaped churning curve (the churning rate, i.e., the entries and exits in excess of job creation or job destruction, is larger for medium-education workers).

Winter-Ebmer and Zweimüller (1997) examine the relationship between firm-level measures of earnings dispersion and employment growth. To examine this issue they use a firm-based random sample of the Social Security files for the period 1975–1991 (see Table 1). With the resulting 130 firms, for which they have all employed workers (with their earnings – top-coded for 9% of them – and most other individual characteristics but education), they compute within firm measures of earnings dispersion as follows. Because of top-coding, they run a Tobit regression for each year and each firm. The resulting standard error of the residual of this regression is used as a first measure of dispersion due to all unobserved factors. They also perform the same regression using only male workers (they do not have hours worked, hence working with males reduces the part-time probability). Then, they use these variables in their firm-level employment growth regressions (1236 observations). These regressions are estimated without and with firm fixed-effects. While there is some evidence with OLS that an increased earnings variance reduces employment growth, the introduction of fixed-effects wipes out any such effects.

Interestingly, Belzil (1997) and Burgess et al. (1997) do the same type of analysis, but in the reverse direction. They both use measures of employment growth (creation, destruction, reallocation, or churning) as additional regressors for explaining wage structure of wage changes.[12] Belzil (1997) use a subsample of the IDA dataset to perform individual-level wage regressions. Since the dataset is longitudinal and contains both employee and employer identifier, it is possible to control for person fixed-effects as well as firm fixed-effects. Even though none of the reported regressions include firm fixed-effects, the author states that the introduction of these effects does not affect the coefficients of interest, a feature consistently found in France (Abowd et al., 1999a; Entorf et al., 1999), in the US (Abowd et al., 1999c), or in Denmark (Bingley and Westergard-Nielsen, 1996). Belzil also finds that employment creation, destruction, or reallocation affects wages, even though no systematic pattern seems to emerge across the different subsamples that he analyzes.

DiPrete et al. (1998) use matched employee–employer longitudinal data from France (LFS matched with firm-level information using the SIREN number, see Table 1) and Sweden (LFS matched with establishment registers, see Table 1) to examine the relation between the dynamics of employment of the employing establishment and job mobility. They model simultaneously unemployment, exit from an establishment, job mobility within an establishment, and entry into an establishment and estimate jointly five probit

[12] Other studies use employment growth as a regressor in their analysis of earnings (Kramarz et al., 1996; Entorf and Kramarz, 1997). But, they do not focus on the resulting estimates of employment growth coefficients.

equations. In particular, they try to examine the age of the mobile workers and how the selection process of such age category is determined in each country by the specific labor market institutions that prevail. Even though their results are only preliminary, the estimation methodology and the way the different datasets are matched constitute an excellent example of the potentialities of the use of matched employee–employer longitudinal data.

Hassink (1999) uses longitudinal matched Dutch data (see Table 1) to examine the effects of firm and employee characteristics on the probability of layoffs. He conducts parallel analyses using the firm's lay off rate and the individual's layoff event as the two dependent variables. Using a specification that includes firm effects, firm characteristics and individual characteristics. The effect of seniority is negative and essentially linear in both equations. The minimum layoff probability occurs at age 32, a result that is interpreted as supporting Lazear-style compensation models.

7.6. Training

The potential of matched employer–employee data to address issues surrounding training is enormous. Indeed, we believe that questions such as the identification of general versus specific knowledge can only be addressed with such longitudinal datasets. And movements of workers between firms, workers for which we measure most individual characteristics would help isolate those firms which provide firm-specific training on one side and those firms which provide general training on the other. Unfortunately, there are few datasets that provide information on training of individuals together with employer information. Even though some are being built now.

Bishop (1994) uses the EOPP which provides retrospective longitudinal data on training and productivity of two new hires at 659 firms. Using this pair, Bishop is able to estimate all equations of interest by doing within-firm difference, therefore eliminating all firm-specific unobserved heterogeneity. The dependent variables are respectively the logarithm of training time, the productivity at the end of first week, the starting wage, the current productivity, the current wage, and the profit in the first months. The results can be summarized as follows. New hires with relevant previous work experience, relevant employer-sponsored formal training, and relevant vocational education tend to require less training, to be more productive, and to receive higher starting wages and higher wages after 1 year of seniority.

Similar questions are examined in a group of papers based on a newly available dataset, the Multi-City Study of Urban Inequality for Holzer and Reaser (1996), the 1995 Survey of Employer-Provided Training for Frazis et al. (1997) (see Table 1). Unfortunately, the first of these two datasets only has one observation per person or per firm, while the paper which uses the second dataset does not use the full potentiality of matched employer–employee data which makes most estimated coefficients difficult to interpret since they are likely to be biased due to unobserved person or worker heterogeneity. Frazis et al. (1998) extend this analysis to show that those establishments that encourage long term relationships, using pension plans and other employee benefits, also provide more training.

Finally, Goux and Maurin (1997) use the French FQP dataset (see Table 1) to examine the impact of training on wages and mobility. Interestingly, they show that having been trained in the past years is associated with a higher wage (approximately 6%) in a simple OLS regression. However, the introduction of firm fixed-effects reduces this effect to less than 3%. Furthermore, when a correction for the selection bias induced by participation in a training program is introduced, all effects of training on wages disappear. Hence, they conclude that the higher wage associated to training is partly due to firm-specific compensation policies and partly due to unobserved worker heterogeneity.

7.7. Unions and collective bargaining

This section discusses the use of matched employer–employee data to study the behavior of unionized firms and negotiated wage rates. We consider, in sequence, Abowd and Allain (1996), Cahuc and Kramarz (1997), Lalonde et al. (1996), Hildreth (1996), Hildreth and Pudney (1997) and Margolis (1993).

Abowd and Allain (1996) use data from the French DADS and BIC (see Table 1) to model the division of the quasi-rent per worker in collectively bargained French wage rate.[13] They fit an equation of the form

$$w_j = x_j + \gamma_j q_j + \varepsilon_j, \tag{7.2}$$

where w_j is the negotiated wage rate, x_j is the opportunity cost of the worker's time, γ_j is the bargaining power of the union, and q_j is the expected quasi-rent per worker. The heterogeneity in γ_j is modeled using q_j and other variables z_j. Using the decomposition in Eq. (3.1), the opportunity cost of the workers is modeled as

$$x_j = \bar{\theta}_j + \psi_{10},$$

where ψ_{10} is the firm effect at the 10th percentile of the French labor force.[14] The quasi-rent per worker has two components: an expected part, which is related to international competition using export prices (from France or from the United States), and a measurement error, which is eliminated by the instrumental variable procedure (see Abowd and Lemieux, 1993). Two empirical measures of the quasi-rent per worker were used–one which eliminated only the opportunity cost of the workers' time and the other which also eliminated an estimate of the opportunity cost of capital. The interpretation of the coefficient on the quasi-rent per worker is, therefore, the average part of the expected quasi-rent per worker that goes to the workers. Abowd and Allain estimate that this coefficient is 0.4 in the French economy.

Hildreth (1996) investigates the same question as Abowd and Allain using the British PSME, a panel of manufacturing establishments (see Table 1) and the British Household

[13] In an earlier effort, Abowd and Kramarz (1993) use the firm data from the BIC combined with occupation data from the ESE and aggregated wage data from the DADS to fit models similar to those in Abowd and Allain. This earlier paper does not make direct use of matched employer–employee data.

[14] Approximately 90% of French jobs are covered by collective bargaining agreements.

Panel Study (BHPS). The basic wage equation is essentially the same as Eq. (7.2), except that Hildreth specifies the relation using log wage as the dependent variable, which means that the coefficient on q_j cannot be interpreted as the bargaining power of the union. The method of calculating the quasi-rent per worker is also different. Hildreth defines the opportunity cost of the worker's time using a table of the usual weekly earnings cross-classified by education, age and sex, with further refinements for the location and industry of the establishment. The appropriate value from this table was subtracted from the value-added per worker to get the quasi-rent per worker. There was no correction for the opportunity cost of capital. Hildreth gets estimates that are much smaller than those of Abowd and Allain, but of the same order of magnitude as those found in other studies using British data (e.g., Hildreth and Oswald, 1993). The main difference appears to be the interpretation of the bargaining power parameter. Abowd and Allain (following Abowd and Lemieux) interpret this parameter as applying to the expected quasi-rent per worker, that is, the part related to the price instruments, and not to the realized profit per worker, which is much more variable.

Cahuc and Kramarz (1997) use the French ESS of 1986 and 1992 (see Table 1) to examine the impact of the signature of a firm-level agreement on the stability of the work force. In some sense, they try to find an exchange of voice, as approximated by the existence of an agreement, against stability. Their analysis uses both the cross-sectional dimension of the ESS, i.e., individual information on multiple employees in each establishment, and the longitudinal dimension, i.e., the same establishments can be found in 1986 and 1992. Cahuc and Kramarz start by examining the probability of signature of an agreement at the firm-level. They show that this probability is positively affected by most variables that increase the cost of turnover, more particularly by training expenses and by the presence of workers with intermediary skills. Then, they examine the relation between workers' seniority and the impact of the signature of an agreement between 1986 and 1992. The relevant regressions have approximately 50,000 observations and more than 250 firm fixed-effects. Results show that the signature of an agreement induces an increase in the average seniority of the work force of roughly one month for every additional year of the agreement.

LaLonde et al. (1996) use an unusually well-conceived matched dataset containing longitudinal information on American manufacturing establishments and employee information on the conduct and results of union representation elections. The establishment data come from the Longitudinal Research Database while the union election data come from the National Labor Relations Board (see Table 1). A union representation election is necessary in the United States before the employees of an ongoing business can negotiated collectively over wages and working conditions. By following establishments over a period of 4 years prior and 9 years after the election, these authors were able to measure the effects of the newly formed union on total output, employment, other factor utilization, wage rates, and productivity. LaLonde et al. present both short and long term evidence comparing the profiles of establishments where the representation election was successful (union wins) with those which had an unsuccessful (union losses) representation election.

Following a successful representation election, establishments reduce their output, material purchases and employment levels permanently (an effect that lasts at least 9 years). The establishments do not, however, experience higher wage rates.

Margolis (1993) uses data from the French Enquête Structure des Salaires (see Table 1) matched with detailed information on the collective bargaining agreements supplied by the Ministry of Labor to study the consequences of mandatory extension of the collective agreements to firms and workers who did not participate in the negotiations, a common practice in France. He finds that the willingness of employers to join the negotiation is strongly affected by the probability that the agreement will be extended. He also finds that the possibility of non-compliance with the collective bargaining agreement influences the behavior of the firms during the negotiations.[15]

Hildreth and Pudney (1997) use the British Panel Study of Manufacturing Establishments (PSME, see Table 1), which includes information on two workers: the most recently hired and one randomly selected, cooperating, individual, to study the effects of union recognition on firm outcomes. In the United Kingdom, there is no statutory requirement that an employer recognize and bargain with a union. Employees can choose whether or not to join a union independent of the employers negotiating stance towards that union. Any collective agreement applies to all workers in the covered jobs regardless of the employees' union status. Hildreth and Pudney model the two-sided decision process that determines the union status of the employee and of the job using the matched data. Their statistical models correct for a variety of sampling and self-selection problems. They find that firms that recognize unions have lower quit rates and higher wage rates. Interestingly, the union wage premium is higher for individuals who are covered by the collective agreement but who do not join the union. The results also suggest that applicant workers do not find the jobs covered by a collective agreement more attractive than non-covered jobs, so there are not increased applicant rates for these jobs. These statistical results generally allow for firm effects in all equations.

7.8. *Other firm outcomes*

The new data sources matching workers and their firms have allowed American researchers to re-examine classical issues of American labor economics: race discrimination and sex segregation. All these new analyses have been based on the WECD (see Table 1 and Section 2.3). The matched data using state unemployment insurance records have also permitted the examination of the effects of changes in the tax system on layoffs and other employment decisions. We discuss these applications, as well as those that do not have an obvious place in other sections, in this subsection.

[15] Non-compliance with collective bargaining agreements in France is accomplished by reclassifying jobs into lower pay categories and gambling that the labor inspector will not force a higher classification.

7.8.1. Segregation of the work force

Carrington and Troske (1998b) examine the extent to which blacks and whites are integrated at work. In addition to the WECD (see Table 1), the authors use the Characteristics of Business Owners (CBO) database which give demographic information on owners, employees, and customers of small businesses (hence complementing the WECD which is particularly strong on large businesses). Then, the authors propose different measures of segregation and assess their adequacy in a multifirm context. First, they define the Gini coefficient as follows:

$$G = 1 - \sum_{i-1}^{T} s_{bi} \left(s_{wi} + 2 \sum_{j=i+1}^{T} s_{wj} \right),$$

where T is the number of firms, s_{bi} and s_{wi} are firm i's share of the black and white sample populations, respectively, and where firms are sorted in ascending order of s_{bi}/s_{wi}. Then, they define the Gini coefficient of random segregation in order to take into account the fact that random allocation of black and white workers will never generate a zero Gini coefficient. Based on the comparison of the two Gini indices, they create a Gini coefficient of systematic segregation.

Their results suggest that the national distribution of black and white employees across employers is far from even, as some employers have mostly white employees while others have mostly black. They also show that this segregation is due to black-white differences in MSA residence. Most of the remaining interfirm segregation come from racial differences in occupation, industry, or by simple random allocation which can almost never be rejected. Then, using more classical tools, they regress the black share of non-supervisory employment in the establishment on the share of black supervisors, the black sample share within each MSA, the log of establishment employment, the average age and education of non-supervisory employees, and indicator variables for industry and region. They show in particular that, using the WECD, black workers tend to be supervised by black managers (and vice-versa for whites). While, using the CBO, they show that black workers are more likely to work for firms with black owners and customers.

Finally, they decompose the black-white wage gap into a between and a within-plant component. In particular, they use the same type of techniques already described at many places in the preceding subsections, i.e., they introduce establishment fixed-effects. Carrington and Troske' s results demonstrate that the wage gap is mostly a within-plant phenomenon. Very little of the black-white wage gap comes from the allocation of black and white workers into firms that pay systematically different wages. Moreover, a large fraction of the within-plant gap is explained by the observable characteristics of the workers even though a significant fraction cannot be explained. In addition, when wages are regressed on the racial structure of the employing firm, it appears that black-majority plants pay their black employees less than black-minority plants. But these black-majority plants also pay their white employees more than their black employees.

The same authors use the same database, the WECD, and the same techniques to examine sex segregation (Carrington and Troske, 1998a). They find that the distribution of men and women across plants is far from even. But, they also find that much of this apparent segregation appears to be due to random allocation. Similarly to the race analysis, they examine the plant female share of non-supervisory employees and regress it on variables similar to those described above. Of interest are the following results that female managers tend to supervise female employees and that women have higher employment shares in large establishments. The analysis of the male-female wage gap proceeds along the same lines as those presented for the black-white wage gap. The authors show that there is an important, even dominant in the case of blue-collar workers, role played by between-plant segregation in explaining this wage gap. Therefore, men work in relatively high paying plants while women work in relatively low paying plants even after controlling for observable characteristics of the workers. In addition, they demonstrate that workers, either men or women, are paid less if they work in largely female plants.

Hellerstein et al. (1997) continue the analysis of sex discrimination. They match the WECD with information from the Longitudinal Research Database (LRD) to get information on the employing establishment or firm. They find that large firms or large establishments make more profits if they employ more women. No such relation exist for small firms. In the present version of the paper at least, the authors are not able to provide a definitive explanation of this phenomenon. Bayard et al. (1999) also study sex segregation and male-female wage differentials. In the current version of the paper, they find that a substantial portion of the male-female wage gap takes the form of wage differentials within narrowly-defined occupations within establishments, results that stand in marked contrast to Groshen (1991b), who found that sex-segregation into occupations within establishments explained most of the gap.

7.8.2. Unemployment insurance and layoffs

In Anderson and Meyer (1994), these authors begin a long series of papers that used state-level unemployment insurance system matched employer–employee data to study the effects of the unemployment insurance tax and benefit system on a variety of outcomes: layoffs, employment, wages, and UI benefit takeup. The 1994 paper is discussed in section 7.5. The data structure in the other papers is very similar and is not discussed again. Anderson and Meyer (1996a,b, 1997) use the state unemployment insurance data and the establishment employment information to establish a number of basic features of the UI system and its effects on labor market outcomes.

Anderson and Meyer (1996a) studies the effects of firm-level experience rating on layoff probabilities. Experience rating is the system of UI financing that increases a firm's UI tax payments as the firm imposes benefit liabilities on the system. The effect of such financing systems on a the firm's propensity to use layoffs and on its wage structure is an old an important question in the labor economics literature. They use the same States as the 1994 paper. They use a form of Eq. (3.1) in which the dependent variable is the event that a worker is laid off during the quarter. Both person and firm effects are included. The firm's UI tax rate is included in the model and instrumental variables are used to correct for the endogeneity that experience-rating induces. They find that the elasticity of the layoff rate with respect to the firm-specific tax cost is -0.3 and the corresponding fraction of temporary layoff unemployment that can be attributed to incomplete experience-rating is 20%.

Anderson and Meyer (1996b) studies the adoption of experience rating in the State of Washington UI system. Because the State of Washington adopted experience rating in 1985 to avoid a massive surplus in the system, these authors, who have data from 1979 to 1993 are able to provide direct evidence on the changes surrounding the adoption. They examine in detail the changes between the last half of 1984 (third and fourth quarters) and the last half of 1985 (again, third and fourth quarters). The change in the in the UI tax rates that was induced by the adoption of experience ratings was based on layoff rates over the period 1980:3–1984:2; however, the firm's only learned of the adoption of experience rating in 1984:3. Thus, this period represents an essentially exogenous change in the UI tax rates. The authors report that the full amount of the market-level change in tax rates is passed on to the workers in the form of lower earnings but that the firm-specific component is borne completely by the firm. They report mixed results of the effects of the experience rating on layoffs.

Anderson and Meyer (1997) use the CWBH data discussed in conjunction with their 1994 paper to study the effect of UI benefit levels and benefit tax treatment on the take-up rate for UI. They explain the late 1980s–early 1990s decline in UI receipts. According to these authors there is a strong positive relation between benefit levels and take-up rates. There are smaller, but still important effects arising from the tax treatment and potential duration of benefits. The inclusion of UI income in the US income tax base, therefore, accounts for most of the recent decline in UI receipts.

Abowd and Allain (1997) use the State of Washington UI data (see Table 1) to study the role played by workers and firms observable characteristics, as well as unobserved heterogeneity, in the probability that an individual participates in a short-time UI compensation (STC) program. Short-time compensation programs allow firms to pay UI benefits to workers whose hours have been reduced to avoid layoffs. These authors show that both types of unobserved heterogeneity are strongly correlated with this probability, with the individual effect having stronger correlation than the firm effect. In the context of Eq. (3.1), the dependent variable is the incidence of short-time compensation. A person-effect means that the individual has experienced short-time UI compensation and a firm effect means that the firm has used short-time compensation. Thus, the results are interpreted as meaning that some individuals have a greater propensity to be employed in short-time compensation jobs than others and some firms have a greater propensity to use this form of UI compensation. Firms with higher experience ratings were more likely to use short-time compensation.

Needels and Nicholson (1998) also study short time compensation systems using UI data from the states of California, Florida, Kansas, New York and Washington for the period 1991–1993 for 3300 establishments. They use a statistical matching algorithm to pair establishments with short-time compensation programs to those without. The statistical analyses are all conducted by differencing the paired establishments. Establishments with STC programs have higher layoff levels than those without, which they interpret as evidence of unmeasured heterogeneity among the establishments.

7.8.3. International trade and other topics

Kramarz (1997) examines the impact of international trade on wages and mobility of French workers using the matching of the French labor force survey with a unique dataset on all imports (to France) and exports (from France) of goods during the period 1986–1990. Origins of the imports and destination of the exports are known at the firm-level and are disaggregated into eight groups of countries. It comprises all movements of goods since it is an administrative data source from the customs administration. Matching is performed using the Siren number present in both files. This import-export dataset is also matched to the Echantillon d'Entreprises to measure total sales and total purchases. Hence, the independent variables on trade in the regressions are the ratio of imports to total purchases – a way to measure the reorientation of purchases from local markets to outside suppliers – and the ratio of exports to total sales. Then, Kramarz computes the change in these ratios between 1986 and 1990 for firm j and relates them to the probability of being unemployed at date $t + 1$ conditional on being employed in the same firm j at date t (t going from 1990 to 1993). He also examines the impact on the level of wage at date t in firm j. All these regressions include individual characteristics from the LFS that one expects to find in this type of analysis. In addition, to assess the impact of the competitive pressure, he also includes the change of the ratio of imports to purchases of all firms in the same 4-digit sector as well as in firms from the trade (retail or wholesale) industries. Results are the following. The unemployment probability is positively affected by increasing imports from the 4-digit competitors of the firm; a best response for the firm being to increase its imports. Hence, importing protects workers from unemployment when most other firms in the same sector increase their imports. The impact of imports and exports on wages have a similar structure. An increase of the share of firm-level purchases coming from outside France between 1986 and 1990 negatively affect the level of future wages while the opposite is true both for exports and imports from the firms of the same 4-digit sector. Origins of the imports appear to matter. For instance if these imports come from Germany, the impact on wage is positive, while if they come from developing countries or from the UK, the impact is large and negative. Similarly, workers employed at date t in firms that increase their share of exports to Japan have higher wages. Of course, these results could be seen as evidence of the impact of international trade on prices or, on the contrary, as showing nothing on international trade but capturing worker unobserved heterogeneity.

Abowd and Kramarz (1998b) analyze the costs of separating from French workers using the 1992 ESS (see Table 1). In this study, the authors used the individual-level variables: total annual compensation inclusive of all employee- and employer-paid benefits and bonuses but exclusive of non-wage-benefits, firm seniority, type of contract (permanent, CDI, or temporary, CDD), number of days of employment in the establishment in 1992, sex, age, nationality (French or non-French), skill-level (in 4 groups), bonuses for retirement and severance payments for workers that retired or were fired in 1992. They present estimates of the structure of retirement, termination, and hiring costs using, representative establishment-level data matched with individual-level information. These costs are directly reported by the sampled establishments. Both retirement and termination costs are increasing and mildly concave in the number of retired or terminated workers. The fixed costs are very large. Hence, these costs act as fixed adjustment costs, giving the firm an incentive to group exits instead of adjusting gradually. Termination costs are largest for collective terminations as opposed to individual ones. These costs are largest for highly skilled employees. Hiring costs also exhibit the same structure; concave adjustment costs with a strong fixed component. But these hiring costs do not have the same structure for all skill levels. Only hires of managers on longterm contracts (CDI) have an increasing and concave impact on the cost. For all other skill levels and types of contract, hiring costs do not depend upon the number of entries. Thus, for hiring costs, the firms have an incentive to group the managerial hiring but no adjustment costs for other hiring. The costs of hiring are much less important in France than the costs of separations (retirements and terminations).

Abowd et al. (1996) consider the following question: Are high-quality products produced by high-quality workers? To do this they use the decomposition of wages from Abowd et al. (1999a) and price measures of product quality. To measure the quality of a product, they use prices for very detailed products (8-digit classification) collected at the firm-level. Each basic product is allocated to a 6-digit basic commodity group. Therefore, quality can be measured as either the relative price of a 8-digit elementary product within a 6-digit basic commodity group or as the price change of each elementary product within the basic commodity group. Abowd et al. find little relation between worker quality and product quality within basic products. Hence, technological differences among firms, given basic products, seem very small. They conclude that, if worker quality and product quality are positively related, the effect is apparently more important for sorting workers among diverse and non-substitutable products than for explaining variation within groups of imperfectly substitutable detailed products.

7.9. Specialized applications

There are a variety of specialized uses of matched employer–employee data that we have not discussed in detail in this chapter because they figure prominently in other surveys. Pension data collected from the employers have been matched to several nationally representative cross-sectional and longitudinal databases in the United States including the Health and Retirement Survey, Mature Cohorts from the National Longitudinal Surveys, and the Survey of Consumer Finances. See the chapter on retirement issues by Mitchell and Lumsdaine for a discussion of these applications. Health researchers have also used administrative matched data to study productivity issues in the health service industry (see Dunn et al., 1998, and the chapter on health issues by Currie).

8. Conclusion

As the beginning of our chapter makes clear, new economic and statistical problems will emerge as new types of labor market questions are investigated with detailed data concerning both the worker and the firm. Even though the analysis of matched employer–employee data is relatively new, we are already confronted with some puzzling new results: the lack of correlation between person and firm effects in wage determination and the enormous employment flows associated with job creation and destruction, among others. To model such new facts, standard models of the allocation of workers among firms must be modified.

New statistical problems have also emerged: the analysis of duration models with correlated person and firm effects and the design of statistical models for non-random matches, for example. To estimate such statistical models, some solutions already exist, based on simulations, but they are extremely computer-intensive. Some simpler ones will surely be implemented in the near future. These statistical models could also be useful in other areas, such as health economics (doctors and hospitals–on one side–and their clients–on the other) or education economics (schools and professors–on one side–and their students–on the other) to resolve the same type of identification questions that the analysis of matched employer–employee data have helped resolve.

An area that will be even more demanding is the formulation and estimation of structural economic models. As we show in our discussion of the relation between different theoretical models and the simplest wage equation with correlated person and firm effects and firm-specific returns to seniority, an enormous amount of detail is required to assign the statistical effects to an economic model. Recovering the deep structural parameters from statistical models that include such effects will surely be difficult. In addition, one can argue that it is very unlikely that all firms follow the same model. Hence, the estimation of structural models will force the researcher to address structural heterogeneity problems, for instance, is rent-sharing more important than agency problems for a particular firm?.

Matched longitudinal employer–employee datasets should constitute the basis for further refinements of the theory of production and of the theory of the workplace organization. The possibility of evaluating the various combination of workers, jobs, and machines within a firm should allow labor economists to delve deeper into the internal organization of the firm. Indeed, data collected in the future should give information on each job in conjunction with each individual job holder in each individual firm. We are back to the "get more data" conclusion, so that we can play the role of Rosen and Willis for this volume of the *Handbook*.

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HIGH WAGE WORKERS AND HIGH WAGE FIRMS

BY JOHN M. ABOWD, FRANCIS KRAMARZ, AND DAVID N. MARGOLIS[1]

We study a longitudinal sample of over one million French workers from more than five hundred thousand employing firms. We decompose real total annual compensation per worker into components related to observable employee characteristics, personal heterogeneity, firm heterogeneity, and residual variation. Except for the residual, all components may be correlated in an arbitrary fashion. At the level of the individual, we find that person effects, especially those not related to observables like education, are a very important source of wage variation in France. Firm effects, while important, are not as important as person effects. At the level of firms, we find that enterprises that hire high-wage workers are more productive but not more profitable. They are also more capital and high-skilled employee intensive. Enterprises that pay higher wages, controlling for person effects, are more productive and more profitable. They are also more capital intensive but are not more high-skilled labor intensive. We find that person effects explain about 90% of inter-industry wage differentials and about 75% of the firm-size wage effect while firm effects explain relatively little of either differential.

KEYWORDS: Wage determination, person effects, firm effects, inter-industry wage differentials, heterogeneity.

1. INTRODUCTION

FOR DECADES LABOR ECONOMISTS have lamented the lack of microeconomic data relating characteristics of firms to characteristics of their workers (see, for example, Rosen (1986) and Willis (1986)) because such data would permit researchers to begin to disentangle the effects of firm-level decisions from the effects of choices made by workers. Why do high-paying firms provide more than the apparent going wage? Perhaps such a strategy delivers a gain in productivity or profitability that exceeds the incremental wage cost, as predicted by efficiency wage and agency models.[2] Perhaps high-paying firms select workers with higher external wage rates or better firm-specific matches, thus sorting the workers into

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[2] See Lazear (1979), Shapiro and Stiglitz (1984), Hart and Hölmstrom (1987), and Sappington (1991) for concise statements of the theories generating these predictions. Tests of these models have been performed by Abowd (1990), Abowd and Kramarz (1993), Cahuc and Dormont (1997), Gibbons and Murphy (1990, 1992), Hutchens (1987), Kahn and Sherer (1990), and Leonard (1990).

firms that have differential observed compensation programs.[3] Although broadly representative linked surveys of firms and workers are not available in the U.S., there have now been numerous studies that attempt to relate firm performance to the design of the compensation system.[4] Furthermore, many have analyzed the inter-industry wage differentials among individuals as if they were the manifestation of differences in firm level compensation policies.[5] In this paper we present the first extensive statistical analysis of simultaneous individual- and firm-level heterogeneity in compensation determination. We examine the variation in personal wage rates holding firm effects constant and variation in firm wage rates holding person effects constant. Due to the matched (person and firm) longitudinal nature of our data, we are able to control for both measured and unmeasured heterogeneity in the workers and their employing firms.

A high-wage worker is a person with total compensation higher than expected on the basis of observable characteristics like labor force experience, education, region, or sex. A high-wage firm is an employer with compensation higher than expected given these same observable characteristics. Until now all empirical analyses of personal and firm heterogeneity in compensation outcomes have relied upon data that were inadequate to identify separately the individual effect necessary to classify a worker as high-wage and the firm effect required to classify a firm as high-wage.

Using a unique longitudinal data set of firms and workers that is representative of private sector French employment, we are able to estimate both person and firm components of compensation determination, allowing for observable and unobservable factors in both dimensions and unrestricted correlation among the effects. Computational complexity prevents full least squares estimation of the models with unobserved heterogeneity in both the person and firm dimensions. After discussing these issues, we examine in detail several related statistical solutions, one of which is a consistent estimator of some of the parameters, and two others that are conditional methods. We also consider other simpler, more classical, techniques in order to assess the importance of person and firm heterogeneity. Although none of these techniques can be used to compute the full least squares solution to the statistical problem, which, for the moment, remains computationally infeasible, all of our methods approximate the full least squares solution and allow the components of person and firm heterogeneity to be intercorrelated. Our consistent method permits estimation of all time-varying coefficients, including those that are heterogeneous. One of our conditional methods, called "order independent," has the advantage that the estimated

[3] This view is espoused by Bulow and Summers (1976), Cain (1976), Jovanovic (1979), and Roy (1951). Weiss and Landau (1984) present a different theoretical version of this model. Some tests include Dickens and Lang (1985), Flinn (1986), Gibbons and Katz (1991), and Heckman and Sedlacek (1985).

[4] See Ehrenberg and Milkovich (1987), Ehrenberg (1990), Ichniowski and Shaw (1993).

[5] See Dickens and Katz (1987), Gibbons and Katz (1992), Groshen (1991), Krueger and Summers (1988), Thaler (1989).

person and firm effects do not depend upon which effect is estimated first and the disadvantage that it cannot impose orthogonality between the estimated residual and the model effects (a characteristic of the full least squares solution). The other conditional method, called "order dependent," has the advantage of imposing this orthogonality but the disadvantage of giving different results depending upon which order is used to estimate the person and firm effects. In particular, the outcome of "persons first and firms second" would differ from "firms first and persons second." In all our estimated models, we find that person effects are statistically more important than firm effects in explaining compensation and performance outcomes and that the two effects are not highly correlated. Using our consistent estimation method, we show empirically that any method in which persons effects are estimated first, whether firm effects are estimated at the same step or after the person effects, performs better than methods in which person effects are estimated after firm effects.

We use our statistical decomposition of wage rates into person and firm effects to address several classic questions in labor economics—the basis for inter-industry wage differentials, the source of the firm size-wage rate relation, the effect of seniority on wage rates, and the relation between pay structure, productivity, and profitability. Surprisingly, our French data give a clear answer to the first question. Virtually all of the inter-industry wage differential is explained by the variation in average individual heterogeneity across sectors. Person effects, and not firm effects, form the basis for most of the inter-industrial salary structure. A very large portion of the positive firm-size wage-rate relation is also due to person effects. The effect of seniority on wage rates is quite heterogeneous across firms; its estimated magnitude is very sensitive to the estimation technique. All our methods for estimating firm effects, including heterogeneous seniority effects, perform well for large firms.

To study pay structure models, we aggregate individual components of compensation to the firm level. Then, we show that firms that hire high-wage workers are more productive per worker, more capital intensive, more professional-employment intensive, and more likely to survive. These same firms are not more profitable, nor are they more skilled-labor intensive. Second, we show that high-wage firms are more profitable, more productive per worker, (possibly) more professional-employment intensive, and (possibly) more capital intensive. High wage firms are unskilled labor intensive and (possibly) less likely to survive.

The paper is organized as follows. In Section 2, we present a detailed motivation of our statistical model in which we relate the different components of our statistical model to wage rate determination models used to study inter-industry wage differentials, firm-size wage effects, the measurement of opportunity wage rates, seniority-wage effects, and the economics of human resource management. In Section 3 we lay out the full details of our statistical methods. We discuss the institutional features of the French labor market and our data sources in Section 4. In Section 5 we discuss our results. Finally, we conclude in Section 6.

2. HETEROGENEITY AND LABOR MARKETS

That labor market outcomes are extremely heterogeneous—observably equivalent individuals earn markedly different compensation and have markedly different employment histories—is one of the enduring features of empirical analyses of labor markets in many countries. This heterogeneity has motivated an enormous literature that attempts to isolate its sources and to identify significant market factors that are statistically related to employment outcomes, particularly earnings or compensation.[6] One strand of this literature has focused on the extent to which wage heterogeneity is related to permanent unmeasured differences among the individuals, what we label a person effect. Another strain of this literature has focused on the extent to which wage heterogeneity is related to permanent differences among the employers, what we label a firm effect.

To put these different models in context, consider the following simple wage equation:

$$y_{it} = \mu_y + (x_{it} - \mu_x)\beta + \theta_i + \psi_{\mathbf{J}(i,t)} + \varepsilon_{it} \tag{2.1}$$

in which y_{it} is the logarithm of annual compensation of individual $i = 1, \dots, N$ at date $t = 1, \dots T$; x_{it} is a vector of P time-varying exogenous characteristics of individual i; θ_i is the pure person effect; $\psi_{\mathbf{J}(i,t)}$ is the pure firm effect for the firm at which worker i is employed at date t (denoted by $\mathbf{J}(i,t)$), μ_y is the grand mean of y_{it}, μ_x is the grand mean of x_{it}, and ε_{it} is the statistical residual. Assume that a simple random sample of N individuals is observed for T years.[7] Thus, ε_{it} has the following properties:

$$\mathbf{E}[\varepsilon_{it} | i, t, \mathbf{J}(i,t), x_{it}] = 0$$

and

$$\operatorname{cov}[\varepsilon_{it}, \varepsilon_{ns} | i, t, n, s, \mathbf{J}(i,t), \mathbf{J}(n,s), x_{it}, x_{ns}] = \begin{cases} \sigma_\varepsilon^2 & \text{for } i = n \text{ and } t = s, \\ 0 & \text{otherwise.}^8 \end{cases}$$

[6] See, for example, Rosen (1986), Willis (1986), Becker (1993), Juhn, Murphy, and Pierce (1993), Murphy and Welch (1992), and Blau and Kahn (1996).

[7] The actual data are in the form of an unbalanced panel. For notational simplicity, however, we describe the motivation in terms of a balanced panel. Our complete model is described in the next section and the proofs for the unbalanced case are given in the Statistical Appendix.

[8] One can always allow for a more complicated error structure for ε_{it}; however, as Abowd and Card (1989) show, except for measurement error, this residual exhibits trivial serial correlation in American longitudinal data. Measurement error in the data studied by Abowd and Card, which does exhibit significant serial correlation within individuals, is related to the structure of samples of individuals in which the individuals are the respondents. In this paper, we study data sampled at the level of the individual but reported by the employer; hence, respondent reporting error and other sources of measurement error in individual longitudinal data are not important problems. We will, therefore, maintain the covariance structure assumptions stated here for simplicity. When we consider consistent estimation of β below, we allow for a general covariance structure on ε_{it} for each i.

In matrix notation we have

$$(2.2) \qquad y = X\beta + D\theta + F\psi + \varepsilon,$$

where X is the $N^* \times P$ matrix of observable, time-varying characteristics (in deviations from the grand means), D is the $N^* \times N$ matrix of indicators for individual $i = 1, \ldots, N$, F is the $N^* \times mJ$ matrix of indicators for the firm effect at which i works at date t (J firms total),[9] y is the $N^* \times 1$ vector of annual compensation data (also in deviations from the grand mean), ε is the conformable vector of residuals, and $N^* = NT$. The parameters of equation (2.2) are β, the $P \times 1$ vector of coefficients on the time-varying personal characteristics; θ, the $N \times 1$ vector of individual effects;[10] ψ, the $mJ \times 1$ vector of firm effects; and the error variance, σ_ε^2.

Equations (2.1) and (2.2) are interpreted as the conditional expectation of annual compensation given information on the observable characteristics, the date of observation, the identity of the individual, and the identity of the employing firm. The discussion that follows clarifies the interpretation of classical least squares estimates of the parameters β, θ, and ψ when some of these effects are missing or are aggregated into linear combinations. The specification in equation (2.2) is a simplification of the model used in our full analysis below, which we adopt in this section to clarify the discussion. All of the results discussed in this section are general and our Statistical Appendix contains proofs for the general case implemented in our data analyses. As the assumptions on the error process make clear, equations (2.1) and (2.2) impose the assumption of exogenous mobility. In particular, the design matrix for the firm effects, F, is orthogonal to the error process ε. Although endogenous mobility is clearly an important problem, we maintain the assumption of exogenous mobility throughout this paper because we are interested in measuring and summarizing the role of personal and firm heterogeneity in the wage outcomes. The extent to which such heterogeneity arises from endogenous mobility, or other considerations, is the subject of future analyses.

Because many authors have estimated variations of (2.2), but not the full model, there is considerable ambiguity about the interpretation of various combinations of these parameters.[11] Leaving aside the distinction between the

[9] For simplicity in this section we treat the case $m = 1$, so that the firm effect is a constant for each firm. Later in the text we analyze more general firm effects.

[10] The parameter θ includes both the unobservable (to the statistician) individual effect and the coefficients of the non-time-varying personal characteristics.

[11] Since we began working on this paper; several working papers have appeared that use a specification similar to equation (2.2). In particular, see Goux and Maurin (forthcoming), who calculate the exact least squares solution for the modal in equation (2.2) using French data with a much smaller sample of firms and persons than we use; Entorf, Gollac, and Kramarz (forthcoming), who also compute the exact least squares solution using French data with fewer firms and persons than the present paper; and Belzil (1996) and Bingley and Westergård-Nielsen (1996), who use Danish data but do not compute the full least squares solution to equation (2.2); instead, they assume orthogonal firm and person effects. Leonard and Van Audenrode (1996b) use a specification similar to the present one on Belgian data.

conditional and structural interpretation of the parameters, about which we have nothing further to add, it is important to note that the omission or aggregation of one or more of the effects in equation (2.2) can change the meaning of the other effects significantly. Variations in the set of conditioning effects, which give rise to omitted-variable biases, are one source of confusion about the interpretation of the statistical parameters. The use of different linear combinations of the effects in equation (2.2), which gives rise to aggregation biases, is another source of differential interpretations for the parameters. We investigate each of these variations in the parameterization of equation (2.2) in the context of different problems in labor economics.

When the estimated version of equation (2.2) excludes the pure firm effects (ψ), the estimated person effects, θ^*, are the sum of the pure person effects, θ, and the employment-duration weighted average of the firm effects for the firms in which the worker was employed, conditional on the individual time-varying characteristics, X:

$$\theta^* = \theta + (D'M_X D)^{-1} D'M_X F\psi, \tag{2.3}$$

where the notation $M_A \equiv I - A(A'A)^{-1}A'$ for an arbitrary matrix A. Hence, if X were orthogonal to D and F, so that $D'M_X D = D'D$ and $D'M_X F = D'F$, then the difference between θ^* and θ, which is just an omitted variable bias, would be an $N \times 1$ vector consisting, for each individual i, of the employment-duration weighted average of the firm effects ψ_j for $j \in \{\mathbf{J}(i,1), \ldots, \mathbf{J}(i,T)\}$:

$$\theta_i^* - \theta_i = \sum_{t=1}^{T} \frac{\psi_{\mathbf{J}(i,t)}}{T}.$$

The estimated coefficients on the time-varying characteristics in the case of omitted firm effects, β^*, are the sum of the parameters of the full conditional expectation, β, and the omitted variable bias that depends upon the conditional covariance of X and F, given D:

$$\beta^* = \beta + (X'M_D X)^{-1} X'M_D F\psi.$$

Similarly, omitting the pure person effects (θ) from the estimated version of equation (2.2) gives estimates of the firm effects, ψ^{**}, that can be interpreted as the sum of the pure firm effects, ψ, and the employment-duration weighted average of the person effects of all of the firm's employees in the sample, conditional on the time-varying individual characteristics:

$$\psi^{**} = \psi + (F'M_X F)^{-1} F'M_X D\theta. \tag{2.4}$$

Hence, if X were orthogonal to D and F, so that $F'M_X F = F'F$ and $F'M_X D = F'D$, then the difference between ψ^{**} and ψ, again an omitted variable bias, would be a $J \times 1$ vector consisting, for each firm j, of the employment-duration weighted average of the person effects θ_i for $i \in \{\mathbf{J}(i,t)=j \text{ for some } t\}$:

$$\psi_j^{**} - \psi_j = \sum_{i=1}^{N} \sum_{t=1}^{T} \left[\frac{\theta_i 1(\mathbf{J}(i,t)=j)}{N_j} \right]$$

where

$$N_j \equiv \sum_{i=1}^{N} \sum_{t=1}^{T} 1(\mathbf{J}(i,t)=j)$$

and the function $1(A)$ takes the value 1 when A is true and 0 otherwise. The estimated coefficients on the time-varying characteristics in the case of omitted individual effects, β^{**}, are the sum of the parameters of the full conditional expectation, β, and the omitted variable bias that depends upon the covariance of X and D, given F:

$$\beta^{**} = \beta + (X' M_F X)^{-1} X' M_F D\theta. \tag{2.5}$$

Almost all existing analyses of equations like (2.2) produce estimated effects that confound pure person and pure firm effects in a manner similar to those presented above. The possibility of identifying both person and firm effects thus allows us to reexamine many important topics in labor economics using estimates that properly allocate the statistical effects associated with persons and firms.

2.1. *Inter-industry Wage Differentials*

Consider now the analysis of inter-industry wage differentials as done by Dickens and Katz (1987), Krueger and Summers (1987, 1988), Murphy and Topel (1987), Gibbons and Katz (1992), and many others. The principal finding of this literature has been that inter-industrial wage differentials cannot be explained by measured person or firm characteristics. There is continuing controversy regarding the extent to which these differentials are explained by unmeasured person effects, with Krueger and Summers claiming that they are not (Gibbons and Katz concurring), Murphy and Topel claiming that unmeasured person effects are the primary explanation, and Dickens and Katz not able to address the issue. As we make clear in this section, the ability to estimate both person- and firm-level heterogeneity will permit us to substantially resolve this question in our data analysis—in favor of the person-effect explanation, as it turns out.

To standardize notation and parameter interpretation, define the pure inter-industry wage differential, conditional on the same information as in equations (2.1) and (2.2), as κ_k for some industry classification $k = 1, \ldots, K$. Industry is a characteristic of the firm; thus, our definition of the pure industry effect is simply the correct aggregation of the pure firm effects within the industry. We select the definition of an industry effect as the one that corresponds to putting industry indicator variables in equation (2.2) and, then, defining what is left of the pure firm effect as a deviation from the industry effects. Hence, κ_k can be represented as an employment-duration weighted average of the firm effects within the industry classification k:

$$\kappa_k \equiv \sum_{i=1}^{N} \sum_{t=1}^{T} \left[\frac{1(\mathbf{K}(\mathbf{J}(i,t)) = k)\,\psi_{\mathbf{J}(i,t)}}{N_k} \right],$$

where

$$N_k \equiv \sum_{j=1}^{J} 1(\mathbf{K}(j) = k) N_j$$

and the function $\mathbf{K}(j)$ denotes the industry classification of firm j. If we insert this pure industry effect, the appropriate aggregate of the firm effects, into equation (2.1), then the equation becomes

$$y_{it} = x_{it}\beta + \theta_i + \kappa_{\mathbf{K}(\mathbf{J}(i,t))} + (\psi_{\mathbf{J}(i,t)} - \kappa_{\mathbf{K}(\mathbf{J}(i,t))}) + \varepsilon_{it}$$

or, in matrix notation as in equation (2.2),

$$(2.6) \qquad y = X\beta + D\theta + FA\kappa + (F\psi - FA\kappa) + \varepsilon$$

where the matrix A, $J \times K$, classifies each of the J firms into one of the K industries; that is, $a_{jk} = 1$ if, and only if, $\mathbf{K}(j) = k$. The parameter vector κ, $K \times 1$, may be interpreted as the following weighted average of the pure firm effects:

$$\kappa \equiv (A'F'FA)^{-1} A'F'F\psi,$$

and the effect $(F\psi - FA\kappa)$ may be re-expressed as $M_{FA}F\psi$. Thus, the aggregation of J firm effects into K industry effects, weighted so as to be representative of individuals, can be accomplished directly by estimation of equation (2.6). Only $\text{rank}(F'M_{FA}F)$ firm effects can be separately identified; however, there is neither an omitted variable nor an aggregation bias in the classical least squares estimates of (2.6). To be perfectly clear, equation (2.6) decomposes $F\psi$ into two orthogonal components: the industry effects $FA\kappa$, and what is left of the firm effects after removing the industry effect, $M_{FA}F\psi$.

Authors like Dickens and Katz (1987), Krueger and Summers (1987, 1988), Murphy and Topel (1987), and Gibbons and Katz (1992) do not have information identifying the employing firm, even when they do have longitudinal data.[12] Estimates of industry effects, κ^*, that are computed on the basis of an equation that excludes the remaining firm effects, $M_{FA}F\psi$, are equal to the pure industry effect, κ, plus an omitted variable bias that can be expressed as a function of the conditional variance of the industry effects, FA, given the time-varying charac-

[12] Krueger and Summers (1988, Table V), for example.

teristics, X, and the person effects, D,

$$\kappa^* = \kappa + (A'F'M_{[D\ X]}FA)^{-1}A'F'M_{[D\ X]}M_{FA}F\psi,$$

which simplifies to $\kappa^* = \kappa$ if, and only if, the industry effects, FA, are orthogonal to the subspace $M_{FA}F$, given D and X, which is generally not true even though FA and $M_{FA}F$ are orthogonal by construction.[13] Thus, it is not possible to estimate pure inter-industry wage differentials consistently, conditional on time-varying personal characteristics and unobservable non-time-varying personal characteristics, without identifying information on the underlying firms unless this conditional orthogonality condition holds. Similarly, estimates of the coefficients of the time-varying personal characteristics, β^*, are equal to the true coefficients of the conditional expectation, β, plus an omitted variable bias that depends upon the conditional covariance between these characteristics, X, and the residual subspace of the firm effects, $M_{FA}F$, given D:

$$\beta^* = \beta + (X'M_{[D\ FA]}X)^{-1}X'M_{[D\ FA]}M_{FA}F\psi,$$

which, once again, simplifies to $\beta^* = \beta$ if, and only if, the time-varying personal characteristics, X, are orthogonal to the subspace $M_{FA}F$, given D and FA, which is also not generally true. Thus, it is not possible to estimate the coefficients on time-varying personal characteristics consistently, conditional on industry effects and unobservable non-time-varying personal characteristics, without identifying information on the underlying firms unless this second conditional orthogonality condition holds.

When the estimation of equation (2.6) excludes both person and firm effects, the estimated industry effect, κ_k^{**}, equals the pure industry effect, κ, plus the employment-duration weighted average residual firm effect inside the industry, given X, and the employment-duration weighted average person effect inside the industry, given the time-varying personal characteristics X:

$$\kappa^{**} = \kappa + (A'F'M_X FA)^{-1}A'F'M_X(M_{FA}F\psi + D\theta),$$

which can be restated as

$$\kappa^{**} = (A'F'M_X FA)^{-1}A'F'M_X F\psi + (A'F'M_X FA)^{-1}A'F'M_X\, D\theta. \tag{2.7}$$

Hence, if industry effects, FA, were orthogonal to time-varying personal characteristics, X, and to non-time-varying personal heterogeneity, D, so that $A'FM_X FA = A'F'FA$, $A'F'M_X F = A'F'F$, and $A'F'M_X D = A'F'D$, the biased inter-industry wage differentials, κ^{**}, would simply equal the pure inter-industry wage differentials, κ, plus the employment-duration-weighted, industry-average

[13] $M_{[D\ X]}$ is the matrix M_Z with $Z \equiv [D \mid X]$ and is not equal to the matrix M_{DX}.

pure person effect, $(A'F'FA)^{-1}A'F'D\theta$, or

$$\kappa_k^{**} = \kappa_k + \sum_{i=1}^{N} \sum_{t=1}^{T} \frac{1[\mathbf{K}(\mathbf{J}(i,t)) = k]\theta_i}{N_k},$$

where $N_k \equiv \sum_{i,t} 1[\mathbf{K}(\mathbf{J}(i,t)) = k]$.

Thus, previous analyses that exclude person effects confound the pure inter-industry wage differential with an average of the person effects found in the industry, given the measured personal characteristics, X. To anticipate our results, we use equation (2.7) together with our estimated pure person effects, θ, and our estimated pure firm effects, ψ, to determine what proportion of the estimated inter-industry wage differentials κ^{**} is explained by person effects versus firm effects. We show that the pure inter-industry wage differential, κ, which we interpret, as in this section, as the part due to pure firm effects, is much less important than the contribution of the industry average person effect to κ^{**}.

2.2. *Firm Effects without Personal Heterogeneity*

There is a complementary line of research that attempts to explain heterogeneity in wage rates by using firm effects, for example Groshen (1991, 1996), Davis and Haltiwanger (1996), Entorf and Kramarz (1997, forthcoming) and Kramarz, Lollivier, and Pelé (1996). The principal finding in these studies has been that firm effects are substantially more important than measured personal characteristics in explaining wage variation, even when the measured personal characteristics include detailed occupational effects, which are typically interpreted as a proxy for our pure person effects, θ. An additional conclusion is that the effects of measured personal characteristics, β, are not very sensitive to the inclusion of firm effects. None of the studies in this strain of the wage-determination literature includes both pure person and pure firm effects, as defined in equation (2.1) or (2.2) above.

In our notation, studies like Groshen (1991) estimate ψ^{**}, from equation (2.4), and β^{**}, from equation (2.5). The size of the bias arising from the omission of person effects is, of course, an empirical matter; however, again to anticipate our results, it turns out to be substantial. Most of the estimated firm effect, ψ^{**}, in these studies is due to the employment-duration weighted average of the pure individual effects conditional on X, $(F'M_X F)^{-1}F'M_X D\theta$, and not to the pure firm effect, ψ. Furthermore, the bias in the estimated effects of time-varying personal characteristics, $\beta^{**} - \beta = (X'M_F X)^{-1}X'M_F D\theta$, due to the omission of pure individual effects, is also large.

2.3. *Firm-Size Wage Effects*

The repeated finding of a positive relation between the size of the employing firm and wage rates, even after controlling for a wealth of individual variables (see Brown and Medoff (1989)), has generated many alternative interpretations. Some explanations rely on efficiency wage considerations—monitoring being more difficult in larger firms—or, more generally, upon firm-specific compensation policies.[14] Others rely on the assumed existence of unobserved worker characteristics, compensated by the firms, that only larger firms would be able to spot because of better hiring practices.[15] The estimated firm-size effect on wage rates can be related to what we call pure firm effects as well as to the average person effect within the firm. Using our notation, a firm-size effect, δ, can be modeled using a matrix $S, J \times R$, that maps the size of firm j into R linearly independent functions of its size (polynomials in the logarithm or size intervals, for example). Following the same methods that we used to decompose the inter-industry wage differential, we express the wage equation (2.2) as:

$$(2.8) \qquad y = X\beta + D\theta + FS\delta + M_{FS}F\psi + \varepsilon;$$

so that the pure firm-size effects are related to the underlying pure firm effects by the equation

$$\delta \equiv (S'F'FS)^{-1}S'F'F\psi.$$

Once again, we stress that firm size is a characteristic of the employer; thus, a firm-size effect is simply an aggregation of the pure firm effects and can be analyzed using the same tools that we used for the inter-industry wage differential. Therefore, all of the bias formulas derived for the inter-industry wage differential apply to the problem of estimating the firm-size effects in the presence or absence of the various effects in equation (2.8). In particular, when the firm-size effects are estimated in the presence of measured time-varying personal characteristics, X, and person effects, D, but omitting the remaining firm effects, $M_{FS}F$, the resulting estimated firm-size effects, δ^*, as in Brown and Medoff (1989, Table 2) take the form

$$\delta^* = \delta + (S'F'M_{[D\ \ X]}FS)^{-1}S'F'M_{[D\ \ X]}M_{FS}F\psi$$

with a similar equation, which we do not state explicitly, for the bias in the estimation of the parameters β in equation (2.8). The firm-size effects estimated in the absence of firm effects, δ^*, are equal to the pure firm-size effects, δ, if, and only if, firm size, FS, is orthogonal to the residual subspace of firm effects, $M_{FS}F$, given time-varying personal characteristics, X, and person effects, D. As in the case of industry effects, we note that this conditional orthogonality does not follow from the fact that FS and $M_{FS}F$ are orthogonal by construction. Hence, the bias $\delta^* - \delta$ is not generally zero.

Most studies of the firm-size wage effect do not condition on person effects, D. Consequently, the estimated parameter vector associated with the firm-size

[14] See Bulow and Summers (1976), for example.
[15] See Weiss and Landau (1984), for example.

effect in those studies, δ^{**} (in our notation), can be represented as

$$\delta^{**} = (S'F'M_X FS)^{-1} S'F'M_X F\psi + (S'F'M_X FS)^{-1} S'F'M_X D\theta, \tag{2.9}$$

which we interpret as the sum of the firm-size, employment-weighted average firm effect and the similarly-weighted average person effect, conditional on personal characteristics, X. To anticipate our results, again, we use the decomposition displayed in equation (2.9) to explain the relation between firm size and the firm-size class average person and firm effects in our data, conditional on other firm-level and personal variables. The relation between firm size and these components of wage outcomes is, as Brown and Medoff hypothesized, importantly related to both pure firm heterogeneity in compensation, ψ, and pure individual heterogeneity, θ.

2.4. *Measurement of the Internal and External Wage*

Virtually all economic models of labor market outcomes require an estimate of the opportunity cost of the worker's time. In simple, classical equilibrium models without unmeasured person or firm heterogeneity, this generally corresponds to the measured wage rate. In models of wage determination such as quasi-rent splitting[16] or imperfect information (efficiency wage and agency models),[17] unmeasured statistical heterogeneity (person or firm) breaks the direct link between the observed wage rate and the opportunity cost of time. Moreover, such models usually make an explicit distinction between the compensation received and the wage rate available in the employee's next best alternative employment. The statistical model in equation (2.1), while not derived from an explicit labor market model, contains all the observable ele-

[16] In the collective bargaining and wage determination literature, this problem has a long theoretical history (see Leontief (1946), MacDonald and Solow (1981), and most recently Manning (1987)). Many empirical implementations, including Brown and Ashenfelter (1986), Card (1986), MaCurdy and Pencavel (1986), Abowd (1989), Christofides and Oswald (1991, 1992), Nickell and Wadhwani (1991), Abowd and Lemieux (1993), and Blanchflower, Oswald, and Sanfey (1996), use macro-economic wage series or sectoral wage series to represent the opportunity cost of time for the unionized workers. This technique fails to capture important variation in the average personal heterogeneity of the employees of different firms. See Abowd and Kramarz (1993) and Abowd and Allain (1996) for empirical models that permit unobserved heterogeneity in the opportunity cost of time.

[17] For agency models, the theory is summarized in Hart and Holmström (1987) and Sappington (1991). Some empirical implementations include Lazear (1979), Hutchens (1987), Abowd (1990), Gibbons and Murphy (1990, 1992), Jensen and Murphy (1990), Kahn and Sherer (1990), Leonard (1990), Cahuc and Dormont (1997), Cahuc and Kramarz (1997), Kramarz and Rey (1995), and Leonard and Van Audenrode (1996a), all of which require an empirical proxy for the external wage rate in order to identify a component of compensation that is related to performance. See also the summary in Ehrenberg and Milkovich (1987). For efficiency wage models, the theory is summarized in Shapiro and Stiglitz (1984), for the dual labor market version see Bulow and Summers (1976) and Cain (1976). Again, empirical models like Dickens and Lang (1985) require a measure of the opportunity cost in the low-wage sector. The measures used do not allow for unobserved personal heterogeneity between the low and high observed wage groups.

ments from which nonclassical labor market models derive their empirical content. Indeed, the simplest definition of the components of the external and internal wage rate based on a structural model leading to equation (2.1) is given by the following model:

$$y_{it} = x_{it}\zeta + \nu_{it} \tag{2.10}$$

where $\{x_{it}, \nu_{it}\}$ follows a general stochastic process for $i = 1, \ldots, N$ and $t = 1, \ldots T$ with

$$\mathrm{E}[\{x_{ii}, \nu_{it}\}\{x_{ns}, \nu_{ns}\} \,|\, i, n, s, t, \mathbf{J}(i,t), \mathbf{J}(n,s)] \neq 0 \qquad \text{iff} \quad i = n \quad \text{or} \quad \mathbf{J}(i,t) = \mathbf{J}(n,s). \tag{2.11}$$

Then,

$$\theta_i = \mathrm{E}[x_{it}\zeta + \nu_{it} \,|\, i] - \mathrm{E}[x_{it}\zeta + \nu_{it}]$$

and

$$\psi_j = \mathrm{E}[x_{it}\zeta + \nu_{it} \,|\, \mathbf{J}(i,t) = j] - \mathrm{E}[x_{it}\zeta + \nu_{it}].$$

The model in equation (2.10), together with the assumption (2.11), simply formalizes the conditions under which we can use our maintained assumption of exogenous mobility to apply a structural interpretation to equation (2.1).

2.5. *Analysis of the Seniority-Wage Rate Relation*

In the growing literature on the effects of seniority on wage rates, most authors assume that the relevant coefficient is homogeneous across firms.[18] Ironically, the first uses of the seniority-wage relation to test economic theories (Lazear (1979) and Hutchens (1987)) do not make this assumption. Furthermore, Margolis (1996) has shown, using estimated seniority effects related to those presented in the present paper, that heterogeneity in the returns to seniority is a significant empirical phenomenon and that one's interpretation of the average effect of seniority on wage rates is affected by whether or not the model allows for the heterogeneity. The seniority-wage relation is a firm-specific time-varying effect. Thus, the statistical techniques developed in this paper can be used to model and estimate this effect. We extend the analysis in Margolis (1996) by including a heterogeneous seniority effect in several statistical models. We provide consistent estimates of this effect within firms using assumptions that are comparable to Topel's (1991) assumptions. We compare these results with other estimation techniques that assume heterogeneous or homogeneous seniority effects. Furthermore, we provide direct evidence on the extent to which the between-firm variability in returns to seniority is related to the between-firm variability in initial pay. Several models of lifetime incentive contracts (Becker and Stigler (1974), Lazear (1979)) predict a negative relation, which our statistics support.

[18] See Abraham and Farber (1987), Altonji and Shakotko (1987), Brown (1989), and Topel (1991).

2.6. *Human Resource Management Policies*

In the emerging literature on the economics of human resource management policies (see Ehrenberg (1990) and Lazear (1998)), economists and other organization specialists have argued that a firm's personnel practices, particularly the design of its compensation policy, are directly related to the performance of the firm. These ideas, which we can consider formally in the context of statistical models like equation (2.1), take us back directly to the questions we posed in the introduction. We will measure the opportunity wage of our workers using our estimate of the person-specific heterogeneity in compensation. Thus, at the firm level, the presence of high-wage workers is measured by the average of the person-specific heterogeneity component of pay. The extent to which the firm, through its hiring practices, selects employees who are, on average, better or worse paid than observably-equivalent employees in other firms is, then, directly related to other firm-level outcomes. Again at the firm level, the presence of a high-wage policy will be measured by the firm-specific component of compensation. The extent to which a firm, through its compensation policy, attempts to pay above or below the prevailing market is, then, directly related to other firm-level outcomes. Firm outcomes of interest include the average productivity of labor, sales per employee (as measures of productivity), and the operating income per unit of capital (as a measure of profitability). Existing empirical studies have attempted to relate similar profitability or productivity measures to specific components of the firm's human resource management practices.[19] Because we have a large, representative sample of firms and easily-understood measures of the firms' compensation policies, we are able to supply very direct statistical evidence on the importance of these human resource management practices on the performance and the structure of the firm.

3. STATISTICAL MODEL

3.1. *Specification of the General Model*

Consider, again, our full model as described in equation (2.2). To make our analysis general enough for the data we use, we note that the rows of y, X, D, and F are arranged in the order $i = 1, \dots, N$ and, within each i, $t = n_{i1}, \dots, n_{iT_i}$, where T_i is the total number of years of data available for individual i and the indices $n_{i1}, \dots, n_{iT_i}$ indicate the year corresponding to the first observation on individual i through the last observation on that individual, respectively. Thus

[19] See almost all of the studies in Ehrenberg (1990) but, in particular, Abowd, Hannon, and Milkovich (1990), Kahn and Sherer (1990), and Leonard (1990). Other studies include Weiss and Landau (1984), Ehrenberg and Milkovich (1987), Cahuc and Dormont (1997), Ichniowski and Shaw (1993), Cahuc and Kramarz (1997), Abowd, Kramarz, and Moreau (1996), and Leonard and Van Audenrode (1996b).

the vector y is organized as

$$(3.1)\qquad y=\begin{bmatrix} y_{1,n_{11}} \\ \cdots \\ y_{1,n_{1T_1}} \\ \cdots \\ y_{N,n_{N1}} \\ \cdots \\ y_{N,n_{NT_N}} \end{bmatrix};$$

X, D, F, and ε are organized conformably; and ψ, the parameter vector associated with the firm effects, is $mJ \times 1$ with $m > 1$. To simplify the notation we will refer to a typical element of y as $y_{i,t}$ and a typical element of X, or any similarly organized matrix, as $x_{(i,t),j}$ where the pair (i,t) denotes the row index.

In all of our statistical models, we decompose the person effect, θ_i, into a part that is related to non-time-varying personal characteristics, u_i, and a part that is not observable to the statistician, α_i. We use the orthogonal decomposition of θ_i defined by

$$(3.2)\qquad \theta_i = \alpha_i + u_i\eta$$

where u_i is a vector of non-time-varying measurable personal characteristics, α_i is the person-specific intercept, and η is the vector of coefficients. We also use the following decompositions of ψ_j. The first of these defines a firm effect with $m=2$,

$$(3.3)\qquad \psi_j = \phi_j + \gamma_j s_{it},$$

where s_{it} denotes individual i's seniority in firm $j=\mathbf{J}(i,t)$ in year t, ϕ_j denotes the firm-specific intercept, and γ_j is the firm-specific seniority coefficient. The second decomposition of ψ_j defines a firm effect with $m=3$:

$$(3.4)\qquad \psi_j = \phi_j + \gamma_j s_{it} + \gamma_{2j} T_1(s_{it}-10),$$

where $T_k(x)=0$ when $x \le 0$ and $T_k(x)=x^k$ when $x>0$, and γ_{2j} measures the change in the firm-specific seniority coefficient that occurs after 10 years of seniority. In matrix form equation (3.3) decomposes $F\psi$ as

$$(3.5)\qquad F\psi = F_0\phi + F_1\gamma$$

where F_0 is the $N^* \times J$ design matrix associated with the vector of firm specific intercepts, F_1 is the $N^* \times J$ matrix whose columns consist of the direct product of the columns of F_0 and an $N^* \times 1$ vector whose elements are s_{it}, ϕ is the $J \times 1$ vector of firm-specific intercepts and γ is the $J \times 1$ vector of firm-specific seniority coefficients. In matrix notation, equation (3.4) decomposes the firm effect as

$$(3.6)\qquad F\psi = F_0\phi + F_1\gamma + F_2\gamma_2$$

where F_2 is the $N^* \times J$ matrix whose columns consist of the direct product of the columns of F_0 and an $N^* \times 1$ vector whose elements are $\mathbf{T}_1(s_{it}-10)$, and γ_2

is the $J \times 1$ vector of firm-specific changes in the seniority coefficient after 10 years of seniority.

For completeness, we also note that the derivation of some of our specification tests requires the assumption that $\varepsilon \sim N(0, \sigma_\varepsilon^2 I)$. This completes the notation used in the general specification of our statistical model.

3.2. *Identification of Parameters in the General Model*

We now consider basic issues in the identification of the parameters of our model. Although equation (2.2) is just a classical linear regression model, the full design matrix $[X\ D\ F]$ has high column dimension ($N \approx 1{,}000{,}000$ and $J \approx 50{,}000$, estimable). The cross-product matrix

$$\begin{bmatrix} X'X & X'D & X'F \\ D'X & D'D & D'F \\ F'X & F'D & F'F \end{bmatrix}$$

is patterned in the elements $D'D$ and $F'F$; however, projecting onto the columns D leaves a $100{,}000 \times 100{,}000$ unpatterned, nonsparse matrix to invert when $m = 2$ (the linear seniority effect case) because workers move between firms. Indeed, mobility is a necessary condition if one wants to separately identify person effects, θ, and firm effects, ψ, in the general model. Similarly, projecting onto the columns of F leaves a $1{,}000{,}000 \times 1{,}000{,}000$ unpatterned, nonsparse matrix to invert. Clearly, the usual computational methods for least squares estimation of the parameter vector $[\beta'\ \theta'\ \psi']'$ are not feasible. Hence, because one cannot compute the unconstrained least squares estimates for the model (2.2), we propose several different estimators that attempt to preserve as much of the general structure of the problem as is computationally possible.

Although we do not discuss the origin of our data until Section 4, one aspect of the data, inter-firm mobility, is so critical to the estimation and interpretation of our analyses that we present a summary now. Regardless of the computational approach used, between-employer mobility of the individuals is essential for the identification of our statistical model. Table I examines the pattern of inter-employer movements among all sample individuals. The rows of Table I correspond to the number of years a person is in the sample. The columns, with the exception of column (la), correspond to the number of employers the individual had. An individual contributes to only one cell (again, excepting column (la)). Notice that 59.4% of the individuals in the sample never change employers (column (1)).[20] Approximately one-fifth of the single employer indi-

[20] Notice that the cell (1, 1) contains 318,627 individuals who appear in the sample during a single year. Some of these individuals may represent coding errors in the person identifier; however, it is not possible to correct these errors.

TABLE I

STRUCTURE OF THE INDIVIDUAL DATA BY YEARS IN SAMPLE AND NUMBER OF EMPLOYERS
(Number of Individuals, Most Common Configuration of Employers)

Years in Sample	Number of Employers: 1	1a	2	3	4	5	6	7	8	9	10	Total	Percent
1	318,627	247,532										318,627	27.3%
	1	1											
2	75,299	57,411	51,066									126,365	10.8%
	2	2	11										
3	46,385	36,540	32,947	19,583								98,915	8.5%
	3	3	12	111									
4	43,019	34,922	26,631	17,191	8,330							95,171	8.2%
	4	4	13	112	1111								
5	41,130	34,596	26,408	15,291	8,685	3,610						95,124	8.2%
	5	5	14	113	1112	11111							
6	29,755	25,388	20,953	13,734	7,592	4,073	1,653					77,760	6.7%
	6	6	15	114	1113	11112	111111						
7	19,413	16,709	17,384	12,039	7,305	3,864	1,931	735				62,671	5.4%
	7	7	16	115	1114	11113	111112	1111111					
8	23,484	20,378	20,421	13,185	7,673	4,001	2,061	917	327			72,069	6.2%
	8	8	44	116	1115	11114	111113	1111112	11111111				
9	38,505	34,147	26,350	15,791	8,590	4,383	2,104	938	362	114		97,137	8.3%
	9	9	54	117	1116	11115	111114	1111113	11111112	111111111			
10	56,881	51,425	32,616	17,728	8,369	3,839	1,837	739	314	109	34	122,466	10.5%
	10[a]	10[a]	64	118	1117	11116	221113	1131112	11111113	111111112	1111111111		
Total	692,498	559,048	254,776	124,542	56,544	23,770	9,586	3,329	1,003	223	34	1,166,305	100.0%
Percent	59.4%	47.9%	21.8%	10.7%	4.8%	2.0%	0.8%	0.3%	0.1%	0.0%	0.0%	100.0%	

Notes: Employment configurations are described in terms of the number of consecutive years spent with each of the individual's employers, in order (e.g. configuration 124 means that the individual spent 1 year with his first employer, then 2 years with his second employer, and finally 4 years with his third employer). Column 1a refers to the subset of individuals with only one employer whose employing firm had at least one other individual who had changed firms at least once in his career (required for least squares identification of both firm and individual effects).
(a) This configuration corresponds to 10 years of data with the first (and only) employer.
Source: Authors' calculations based on the Déclarations Annuelles des Salaires (DAS).

viduals worked in firms with no movers while four-fifths (47.9% of the overall sample, column (1a)) worked in firms that, at one time or another, employed a person who changed employer. Thus, 88.5% of the sample individuals contribute to the estimation of firm-effects. It is also interesting to notice the pattern of employer spells among the movers (columns (2)–(10)). The second line of each cell shows the most frequent configuration of employer spells for individuals in that cell. In almost every case, short spells precede longer spells, indicating that mobility is greater earlier in the career (as Topel and Ward (1992) found for American men). It seems clear from Table I that the data should allow us to separate the individual effect from the firm effect.

3.3. *Identification and Consistent Estimation of β and γ_j*

In this subsection we show how to obtain consistent estimates of β and γ_j using the within-individual-firm differences of the data. This method provides us with our most robust statistical method in the sense that we use no additional statistical assumptions beyond those specified in equation (2.1) and definition (3.3). Consider the first differences:

$$y_{in_{it}} - y_{in_{it-1}} = (x_{in_{it}} - x_{in_{it-1}})\beta + \gamma_{\mathbf{J}(i,n_{it})}(s_{in_{it}} - s_{in_{it-1}}) + \varepsilon_{in_{it}} - \varepsilon_{in_{it-1}} \tag{3.7}$$

for all observations for which $\mathbf{J}(i, n_{it}) = \mathbf{J}(i, n_{it-1})$, which we represent in matrix form as

$$\Delta y = \Delta X \beta + \tilde{F}\gamma + \Delta\varepsilon \tag{3.8}$$

where Δy is $\widetilde{N^*} \times 1$, ΔX is $\widetilde{N^*} \times P$, $\tilde{F}$ is $\widetilde{N^*} \times J$, $\Delta\varepsilon$ is $\widetilde{N^*} \times 1$, and $\widetilde{N^*}$ is equal to the number of (i,t) combinations in the sample that satisfy the condition $\mathbf{J}(i, n_{it}) = \mathbf{J}(i, n_{it-1})$. The matrix $\tilde{F}$ contains the rows of F_1 that correspond to the person-years (i,t) for which the condition $\mathbf{J}(i, n_{it}) = \mathbf{J}(i, n_{it-1})$ is satisfied minus the immediately preceding row. Then,

$$\tilde{\beta} = (\Delta X' M_{\tilde{F}}\, \Delta X)^{-1}\, \Delta X' M_{\tilde{F}}\, \Delta y \tag{3.9}$$

and

$$\tilde{\gamma} = (\tilde{F}'\tilde{F})^{-1}\tilde{F}'\left(\Delta y - \Delta X \tilde{\beta}\right). \tag{3.10}$$

A consistent estimate of $V[\tilde{\beta}]$ is given by

$$\widehat{\mathrm{V}[\tilde{\beta}]} = (\Delta X' M_{\tilde{F}}\, \Delta X)^{-1}\left(\Delta X' M_{\tilde{F}} \tilde{\Omega} M_{\tilde{F}}\, \Delta X\right)(\Delta X' M_{\tilde{F}}\, \Delta X)^{-1}$$

where

$$\tilde{\Omega} \equiv \begin{bmatrix} \tilde{\Omega}[\Delta\varepsilon_1] & 0 & \cdots & 0 \\ 0 & \tilde{\Omega}[\Delta\varepsilon_2] & \cdots & 0 \\ \cdots & \cdots & \cdots & \cdots \\ 0 & 0 & \cdots & \tilde{\Omega}[\Delta\varepsilon_N] \end{bmatrix}$$

and

$$\tilde{\Omega}[\Delta\varepsilon_i] \equiv \begin{bmatrix} \widetilde{\Delta\varepsilon}^2_{in_2} & \widetilde{\Delta\varepsilon}_{in_2}\widetilde{\Delta\varepsilon}_{in_3} & \cdots & \widetilde{\Delta\varepsilon}_{in_2}\widetilde{\Delta\varepsilon}_{in_{T_i}} \\ \widetilde{\Delta\varepsilon}_{in_3}\widetilde{\Delta\varepsilon}_{in_2} & \widetilde{\Delta\varepsilon}^2_{in_3} & \cdots & \widetilde{\Delta\varepsilon}_{in_3}\widetilde{\Delta\varepsilon}_{in_{T_i}} \\ \cdots & \cdots & \cdots & \cdots \\ \widetilde{\Delta\varepsilon}_{in_{T_i}}\widetilde{\Delta\varepsilon}_{in_2} & \widetilde{\Delta\varepsilon}_{in_{T_i}}\widetilde{\Delta\varepsilon}_{in_3} & \cdots & \widetilde{\Delta\varepsilon}^2_{in_{T_i}} \end{bmatrix}.$$

It is understood that only the rows of $\Delta\varepsilon$ that satisfy the condition $\mathbf{J}(i, n_{it}) = \mathbf{J}(i, n_{it-1})$ are used in the calculation of $\tilde{\Omega}$, which is therefore $\widetilde{N^*} \times \widetilde{N^*}$.[21]

Notice that, given our assumptions, the resulting estimators (3.9) and (3.10) are also unbiased. Our consistent method is not unique but, it has the advantage that the sample on which the estimation is performed includes both workers who remained in the same firm at all dates as well as workers who moved between firms at some point in time during our analysis period. Even for these mobile individuals, all first-differences for which the date t firm differs from the date $t-1$ firm are not included in the estimating sample. Hence, our consistent method is inefficient in the context of the specification of equation (2.2). In addition to this inefficiency, we also note that our consistent method cannot be used to identify separately the firm intercept, ϕ, and the person effect, θ. This results from the restriction of our analysis to a sample based on all observations for which $\mathbf{J}(i, n_{it}) = \mathbf{J}(i, n_{it-1})$. Any method that allows separate identification of the two effects must include in some form the remaining observations. Hence, we turn now to other methods more appropriate to this purpose.

3.4. *Conditional Estimation Methods*

In this section we provide statistical models for estimating all of the effects in equation (2.2) using a class of estimators we call conditional methods because of their relation to standard linear model computational techniques and because of their origins in the panel data literature on person-effect models.[22] Our purpose in developing these methods is to provide estimators that are as similar as possible to the full least squares solution but that are computationally tractable. The basic idea is also simple. Since we cannot compute the full least squares solution, we will have to impose some ancillary orthogonality assumptions in order to proceed. We use information in the data in the form of higher order interactions between observable characteristics, person identity and firm iden-

[21] The formula for the consistent estimator of $V[\tilde{\beta}]$ clearly allows for arbitrary correlation of the residuals ε_{it} over t for each i. Hence, our consistent estimator is unchanged if we permit an arbitrary time-series model for ε_{it}.

[22] The reader familiar with the analysis of variance as considered in, for example, Scheffé (1959) and Searle et al. (1992), will notice that our conditional methods can also be derived as analysis of covariance models in which the data are adjusted to remove certain effects, our conditioning variables, before the conventional analysis of covariance formulas are applied to the model.

tity, which are excluded by hypothesis from equation (2.2), to proxy for the correlation between X, D, and F. Then, we impose conditional orthogonality, given these higher order interactions. Since we have a consistent, but inefficient, estimator of some of the effects, we will use that estimator to assess the quality of our conditional estimation methods when we consider formal specification checks. We will, thus, have some formal and some informal methods for comparing a variety of estimators, none of which is the full least squares solution for estimating the parameters of equation (2.2).

Consider a matrix of variables $Z, N^* \times Q$, which depends upon Q functions of the information in X, D, and F. Using conditional methods we calculate the least squares estimates of equation (2.2) under different maintained hypotheses about the conditional orthogonality of X, D, and F, given Z. The first of these hypotheses imposes that the effects X and D be orthogonal to the projection of F onto the null space of Z. Under this hypothesis the basic equation can be restated as

$$y = X\beta + D\theta + Z\lambda + M_Z F\psi + \varepsilon \tag{3.11}$$

where the auxiliary parameter $\lambda \equiv (Z'Z)^{-1}Z'F\psi$. The assumption of conditional orthogonality between X and F, given Z, and between D and F, given Z, implies that

$$X'M_Z F = 0 \tag{3.12}$$

and

$$D'M_Z F = 0. \tag{3.13}$$

Hence, the conventional least squares formula for the estimator of the original parameters, $[\,\beta' \;\; \theta' \;\; \psi'\,]'$, and the auxiliary parameters, λ, is

$$\begin{bmatrix} \hat{\beta} \\ \hat{\theta} \\ \hat{\lambda} \\ \hat{\psi} \end{bmatrix} = \begin{bmatrix} X'X & X'D & X'Z & X'M_Z F \\ D'X & D'D & D'Z & D'M_Z F \\ Z'X & Z'D & Z'Z & Z'M_Z F \\ F'M_Z X & F'M_Z D & F'M_Z Z & F'M_Z F \end{bmatrix}^{-} \begin{bmatrix} X'y \\ D'y \\ Z'y \\ F'M_Z y \end{bmatrix} \tag{3.14}$$

where the notation $[\;\,]^-$ denotes a generalized inverse.[23] Since the elements $X'M_Z F$, $D'M_Z F$, and $Z'M_Z F$ are zero, either by hypotheses (3.12) and (3.13) or by construction, the formula (3.14) can be restated as

$$\begin{bmatrix} \hat{\beta} \\ \hat{\theta} \\ \hat{\lambda} \end{bmatrix} = \begin{bmatrix} X'X & X'D & X'Z \\ D'X & D'D & D'Z \\ Z'X & Z'D & Z'Z \end{bmatrix}^{-1} \begin{bmatrix} X'y \\ D'y \\ Z'y \end{bmatrix} \tag{3.15}$$

and

$$\hat{\psi} = (F'M_Z F)^{-} F'M_Z y. \tag{3.16}$$

[23] The use of a g-inverse is required because $(F'M_Z F)$ is rank $mJ - 1 - Q$.

As we demonstrated in Section 3.3, certain parameters in our model can be estimated consistently without the use of ancillary assumptions like equations (3.12) and (3.13). Consistent estimation of other parameters requires some extra hypotheses. If the conditional methods work well, then the conditional estimates of β and γ_j should not be too far from the estimates produced by the consistent method. This insight is the basis for the specification checks that we derive below.

3.4.1. *Order-independent estimation*

Our first method for the computation of the solution to equations (3.15) and (3.16) can be accomplished in two steps, which can be performed in either order, hence our designation of this method as "order independent." In the first step, called the within-*D* step, the parameters in equation (3.15) are estimated by conventional longitudinal methods in which X and Z are projected on D to produce the estimates of β and λ given by

$$(3.17)\qquad \begin{bmatrix} \hat{\beta} \\ \hat{\lambda} \end{bmatrix} = \begin{bmatrix} X'M_D X & X'M_D Z \\ Z'M_D X & Z'M_D Z \end{bmatrix}^{-1} \begin{bmatrix} X'M_D y \\ Z'M_D y \end{bmatrix},$$

which are usually called the "within-person" estimators of these parameters. The associated estimator of θ is

$$(3.18)\qquad \hat{\theta} = (D'D)^{-1} D'\left(y - X\hat{\beta} - Z\hat{\lambda}\right).$$

The second step in the computation of the complete set of order-independent, conditional least squares estimates for equation (3.11), called the within-*F* step, requires the solution of equation (3.16). This is accomplished by computing the least squares estimates of the parameters in the regression of y on F and Z jointly:

$$(3.19)\qquad y = F\psi + Z\pi + \upsilon,$$

where ψ is the same parameter vector that appears in equation (2.2), π is a $Q \times 1$ vector of auxiliary parameters, and $\upsilon \sim N(0, \sigma_\upsilon^2 I)$ because of the conditional orthogonality conditions imposed in equations (3.12) and (3.13). Computation of $\hat{\psi}$ is accomplished in two steps that are directly analogous to the method used in equations (3.17) and (3.18). First, compute $\hat{\pi}$ by the within-*F* estimator

$$(3.20)\qquad \hat{\pi} = (Z'M_F Z)^{-1} Z'M_F y.$$

Then, compute $\hat{\psi}$ with the estimator

$$(3.21)\qquad \hat{\psi} = (F'F)^{-1} F(y - Z\hat{\pi}).$$

The proof that the formulas (3.16) and (3.21) are equivalent follows. First, note that

$$\hat{\pi} = \left[(Z'Z)^{-1} + (Z'Z)^{-1}Z'F(F'M_ZF)^{-}F'Z(Z'Z)^{-1}\right]Z'y - (Z'Z)^{-1}Z'F(F'M_ZF)^{-}F'y$$

by direct application of the partitioned inverse formula to the full least squares solution to equation (3.19). Hence,

$$(3.22) \qquad y - Z\hat{\pi} = \left[M_Z - P_ZF(F'M_ZF)^{-}F'P_Z + P_ZF(F'M_ZF)^{-}F'\right]y$$

where $P_Z \equiv I - M_Z$. Substituting equation (3.22) into equation (3.21) yields

$$\begin{aligned}\hat{\psi} &= (F'F)^{-1}F'(y - Z\hat{\pi}) \\ &= (F'F)^{-1}F'M_Zy - (F'F)^{-1}F'P_ZF(F'M_ZF)^{-}F'P_Zy \\ &\quad + (F'F)^{-1}F'P_ZF(F'M_ZF)^{-}F'y \\ &= \left[(F'F)^{-1}(F'M_ZF) + (F'F)^{-1}F'P_ZF\right](F'M_ZF)^{-}F'M_Zy \\ &= (F'M_ZF)^{-}F'M_Zy. \qquad\qquad Q.E.D.\end{aligned}$$

In some applications, the matrix F is just F_0, the design matrix for a single firm-specific effect ($m = 1$), and the computation of equations (3.20) and (3.21) can be accomplished by conventional formulas in which the values of y and Z are deviated from within-firm means in order to compute $\hat{\pi}$. In our estimation using this conditional model we let $m = 2$ in order to capture a firm-specific intercept and seniority slope according to equation (3.5). The within-F step regression becomes

$$(3.23) \qquad y = F_0\phi + F_1\gamma + Z\pi + \nu.$$

In estimation of the firm effects by equation (3.23), the computation of $\hat{\pi}$, $\hat{\phi}$, and $\hat{\gamma}$ is more complex than for the case in which $F = F_0$. These complexities are described in the Statistical Appendix.

The estimation of the correct covariance matrix for the combined within-D and within-F estimator requires calculation of the correct residual for the full model in (2.2),

$$\hat{\varepsilon} = \left(y - X\hat{\beta} - D\hat{\theta} - Z\hat{\lambda} - M_ZF\hat{\psi}\right).$$

The computation of this residual is not straightforward. The first part of the residual is computed at the within-D step as

$$\hat{\varepsilon}^{[1]} = \left(y - X\hat{\beta} - D\hat{\theta} - Z\hat{\lambda}\right).$$

The second part of the residual is computed at the end of the within-F step as

$$\hat{\varepsilon}^{[2]} = M_ZF\hat{\psi} = F\hat{\psi} - Z\tilde{\lambda}$$

where $\tilde{\lambda} \equiv (Z'Z)^{-1}Z'F\hat{\psi}$. Finally,

$$\hat{\varepsilon} = \hat{\varepsilon}^{[1]} - \hat{\varepsilon}^{[2]}.$$

The standard analysis of variance estimator for the variance of the residual ε is given by

$$(3.24) \qquad \hat{\sigma}_{\varepsilon}^{2} = \frac{\left(y - X\hat{\beta} - D\hat{\theta} - Z\hat{\lambda} - M_Z F\hat{\psi}\right)'\left(y - X\hat{\beta} - D\hat{\theta} - Z\hat{\lambda} - M_Z F\hat{\psi}\right)}{N^* - P - N - Q - (mJ - 1 - Q)}$$

where we note explicitly that the estimation of the mJ firm effects uses only $mJ - Q - 1$ degrees of freedom and that the Q degrees of freedom missing from the firm effects have been used to estimate $\hat{\lambda}$. The proof follows:

$$\hat{\varepsilon} = \left[I - W(W'W)^{-1}W' - M_Z F(F'M_Z F)^{-1}F'M_Z\right]\varepsilon = M_{[W\ \ M_Z F]}\varepsilon$$

where $W \equiv [X\ \ D\ \ Z]$. Under the maintained orthogonality conditions in equations (3.12) and (3.13), the quadratic form

$$\frac{\varepsilon'\varepsilon}{\sigma_{\varepsilon}^{2}} \sim \chi^{2}_{N^*}.$$

and

$$(3.25) \qquad \frac{\varepsilon'\varepsilon}{\sigma_{\varepsilon}^{2}} = \frac{\hat{\varepsilon}'\hat{\varepsilon}}{\sigma_{\varepsilon}^{2}} + \frac{\varepsilon' W(W'W)^{-1}W'\varepsilon}{\sigma_{\varepsilon}^{2}} + \frac{\varepsilon' M_Z F(F'M_Z F)^{-1}F'M_Z\varepsilon}{\sigma_{\varepsilon}^{2}}$$

Since $W(W'W)^{-1}W'M_Z F(F'M_Z F)^{-1}F'M_Z = 0$, the last two quadratic forms on the right-hand side of equation (3.25) are independent χ^2 random variables with $\operatorname{rank}[W(W'W)^{-1}W'] = P + N + Q$ and $\operatorname{rank}[M_Z F(F'M_Z F)^{-1}F'M_Z] = mJ - Q - 1$ degrees of freedom, respectively. Thus,

$$\frac{\hat{\varepsilon}'\hat{\varepsilon}}{\sigma_{\varepsilon}^{2}} \sim \chi^{2}_{N^* - P - N - mJ + 1}.$$

The error degrees of freedom for the complete model is, thus, $N^* - P - N - mJ + 1$, so that the dimensionality of the auxiliary parameter vector λ does not affect the goodness of fit of the model in equation (3.11).

3.4.2. *Order-dependent estimation*

The order-dependent method, conditional on Z, means that the estimation of certain effects is performed before others; that is, that the residuals from the first step are used to compute the estimates of the second step. The result is order-dependent because estimating person effects before firm effects is not the same as estimating firm effects before person effects. We describe in detail the order-dependent: persons first method. We comment only briefly on the order-dependent: firms first method, because the analogous formulas are straightforward.

The first step of our order-dependent: persons first method uses the same conditional estimation equations that were described above for the order-independent method to estimate the coefficients of the time-varying observable variables, β, the person effects, θ, and the conditioning effects, λ, the coefficient of the variables Z. This is done according to equation (3.15). Hence, the estimated coefficients are given by equations (3.17) and (3.18).

In the second step of the order-dependent: persons first method, we estimate the firm effects using equation (3.4) and its matrix specification (3.6).[24] Define

$$\{j\} \equiv \{(i,t) \mid \mathbf{J}(i,t) = j\}, \quad \text{a set with } N_j \text{ elements.} \tag{3.26}$$

Now,

$$\hat{y}_{\{j\}} \equiv y_{\{j\}} - x_{\{j\}} \hat{\beta} - \hat{\theta}_{\{j\}}, \tag{3.27}$$

where

$$y_{\{j\}} \equiv \begin{bmatrix} \cdots \\ y_{ns} \\ \cdots \end{bmatrix}, \quad \forall (n,s) \in \{j\}, \tag{3.28}$$

and similarly for $x_{\{j\}}$ and $\hat{\theta}_{\{j\}}$. Equations (3.26) and (3.27) group all of the observations on individuals employed by the same firm into the vector $\hat{y}_{\{j\}}$, which is expressed as the deviation from the first-step estimated $x\hat{\beta}$ and $\hat{\theta}$. The firm-level equation is

$$\hat{y}_{\{j\}} = F_{\{j\}} \begin{bmatrix} \phi_j \\ \gamma_j \\ \gamma_{2j} \end{bmatrix} + \zeta_{\{j\}} \tag{3.29}$$

where

$$F_{\{j\}} \equiv \begin{bmatrix} & \cdots & \\ 1 & s_{ns} & \mathrm{T}_1(s_{ns} - 10) \\ & \cdots & \end{bmatrix}, \quad \forall (n,s) \in \{j\} \tag{3.30}$$

and

$$\zeta_{\{j\}} \equiv \varepsilon_{\{j\}} + x_{\{j\}} \left(\beta - \hat{\beta} \right) + \left(\theta_{\{j\}} - \hat{\theta}_{\{j\}} \right). \tag{3.31}$$

[24] Since this second method is much simpler to implement than the first one, we use a specification of the firm effect that is more complicated by including a linear spline after 10 years of seniority.

Least squares estimation of (3.29) yields the estimator

$$\begin{bmatrix} \hat{\phi}_j \\ \hat{\gamma}_j \\ \hat{\gamma}_{2j} \end{bmatrix} = (F'_{\{j\}}F_{\{j\}})^{-1}F'_{\{j\}}\hat{y}_{\{j\}}, \quad \text{for } j=1,\ldots J. \tag{3.32}$$

The asymptotic distribution of the estimator in equation (3.32) is

$$\begin{bmatrix} \hat{\phi}_j \\ \hat{\gamma}_j \\ \hat{\gamma}_{2j} \end{bmatrix} \to \mathrm{N}\left(\begin{bmatrix} \phi_j \\ \gamma_j \\ \gamma_{2j} \end{bmatrix}, \Omega_j\right) \quad \text{as} \quad N_j \to \infty \tag{3.33}$$

where

$$\begin{aligned} \Omega_j \equiv {} & \sigma_\varepsilon^2(F'_{\{j\}}F_{\{j\}})^{-1} + (F'_{\{j\}}F_{\{j\}})^{-1}F'_{\{j\}} \\ & \left(X_{\{j\}}\mathrm{var}\left[\hat{\beta}\right]X'_{\{j\}} + \mathrm{var}\left[\hat{\theta}_{\{j\}}\right] + 2X_{\{j\}}\mathrm{cov}\left[\hat{\beta}, \hat{\theta}_{\{j\}}\right]\right)F_{\{j\}}(F'_{\{j\}}F_{\{j\}})^{-1}. \end{aligned} \tag{3.34}$$

The first step of our order-dependent "firms first" method begins with the equations (3.16) or (3.21) defining the estimator $\hat{\psi}$ in the order-independent method. The order-dependent "firms first" estimator for β and θ is based on conventional computational formulas applied to equation

$$y - F\hat{\psi} = X\beta + D\theta + \xi$$

where $\xi = \varepsilon + F(\psi - \hat{\psi})$. The order-dependent "firms first" estimators are

$$\hat{\beta} = (X'M_D X)^{-1}X'M_D\left(y - F\hat{\psi}\right) \tag{3.35}$$

and

$$\hat{\theta} = (D'D)^{-1}D'\left(y - X\hat{\beta} - F\hat{\psi}\right). \tag{3.36}$$

The asymptotic covariance matrices of the estimators in equations (3.35) and (3.36) can be derived directly from the standard formulas and the asymptotic covariance matrix of the order-dependent "firms first" estimator of ψ, which is just $\sigma_\varepsilon^2(F'M_Z F)^-$.

3.4.3. *Relation between the order-independent and order-dependent estimates*

In the discussion of our empirical results we refer repeatedly to different forms of the conditional estimators. In this subsection we summarize the relations among the different conditional estimators. The order-independent estimator for β and θ, equations (3.17) and (3.18), is identical to the order-dependent "persons first" estimator for β and θ. The order-dependent "persons first" estimator for ψ is equation (3.32). The order-independent estimator for ψ, equation (3.16) or (3.21), is identical to the order-dependent "firms first" estimator for ψ. The order-dependent "firms first" estimators for β and θ are equations (3.35) and (3.36).

3.4.4. *Estimation of components of the individual effect*

Regardless of the estimator used for θ_i, we also decompose the individual effect into a component attributable to fixed individual characteristics, u_i (such as education), and an unobservable component, α_i, as shown in equation (3.2). To recover the α_i and $u_i\eta$ parts of the individual effect, we use the estimated individual effects, $\hat{\theta}_i$, and their associated estimated sampling variances to estimate the equation (3.2) by generalized least squares. We obtain $\hat{\eta}$, which satisfies

$$(3.37) \qquad \hat{\eta} \to \mathrm{N}\left(\eta, \left(U'\mathrm{diag}\left(\mathrm{var}\left[\hat{\theta}_i\right]\right)^{-1} U\right)^{-1}\right) \quad \text{as} \quad N \to \infty$$

where

$$(3.38) \qquad U \equiv \begin{bmatrix} u_1 \\ \cdots \\ u_N \end{bmatrix}$$

and diag(var$[\hat{\theta}_i]$) is a diagonal matrix $[\hat{\sigma}_\varepsilon^2/T_i]$, the asymptotic variances of $\hat{\theta}_i$ using the residual variance estimator from equation (3.24). The estimator of α_i is

$$(3.39) \qquad \hat{\alpha}_i = \hat{\theta}_i - u_i\hat{\eta}$$

and is unbiased and asymptotic in T_i (Chamberlain (1984)). We show below that statistics based upon aggregating $\hat{\theta}_i$ and $\hat{\alpha}_i$ to the level of the firm are consistent.

3.5. *Specification Checks*

Because of the result in equation (3.25), namely that the goodness of fit of the model does not depend upon the number of auxiliary parameters used in the within-D or within-F step, conventional specification tests and Bayesian model selection procedures are not applicable. Essentially, we must maintain the conditional orthogonality assumptions (3.12) and (3.13) in order to compute any estimates at all of equation (3.11). Although we cannot compute a classical specification test in the sense of Hausman (1978), we can use those principles to derive a specification test whose distribution is known under the null hypothesis

$$H_0 : \lambda = (Z'Z)^{-1} Z'F\psi,$$

which is the definition of the auxiliary parameter λ under the conditional orthogonality hypotheses of equations (3.12) and (3.13).

Consider the residual from equation (2.2) when $\hat{\beta}$, $\hat{\theta}$, and $\hat{\psi}$ are defined according to the order-independent estimation formulas (3.17), (3.18), and (3.16), respectively,

$$\begin{aligned}
\tilde{\varepsilon} &= \left(y - X\hat{\beta} - D\hat{\theta} - F\hat{\psi}\right) \\
&= \varepsilon - X\left(\hat{\beta} - \beta\right) - D(\hat{\theta} - \theta) - Z(\hat{\lambda} - \lambda) - M_Z F\left(\hat{\psi} - \psi\right) \\
&\quad + Z(\hat{\lambda} - \lambda) - P_Z F\left(\hat{\psi} - \psi\right) \\
&= \hat{\varepsilon} + Z(\hat{\lambda} - \lambda) - P_Z F\left(\hat{\psi} - \psi\right) \\
&= \hat{\varepsilon} + Z\left(\hat{\lambda} - (Z'Z)^{-1} Z'F\hat{\psi}\right) - Z\left(\lambda - (Z'Z)^{-1} Z'F\psi\right).
\end{aligned}$$

Hence, under the null hypothesis

$$\tilde{\varepsilon} - \hat{\varepsilon} = Z(\hat{\lambda} - \tilde{\lambda}).$$

The statistic $\hat{\lambda} - \tilde{\lambda}$ is very similar to a specification test statistic since it is the difference between the Cramer-Rao efficient estimator of λ, namely $\hat{\lambda}$, and an inefficient but unbiased estimator of the same auxiliary parameter, namely $\tilde{\lambda}$. By direct application of the Cramer-Rao lower bound implied by the efficiency of $\hat{\lambda}$ for the model given in equation (3.11), we have

$$\hat{\lambda} - \tilde{\lambda} \sim N(0, \sigma_\varepsilon^2 \Omega)$$

where

$$\sigma_\varepsilon^2 \Omega \equiv (Z'Z)^{-1} Z'F \operatorname{var}\left[\hat{\psi}\right] F'Z(Z'Z)^{-1} - \operatorname{var}[\hat{\lambda}],$$

and $\operatorname{var}[\hat{\lambda}]$ and $\operatorname{var}[\hat{\psi}]$ are the covariance matrices of the parameter estimators $\hat{\lambda}$ and $\hat{\psi}$, respectively, as computed in the solutions to equations (3.15) and (3.16). The variance of $\hat{\lambda} - \tilde{\lambda}$ is guaranteed to be positive semi-definite by the efficiency of $\hat{\lambda}$. Thus, a test of the specification of the model can be based upon the distribution of $\tilde{\varepsilon} - \hat{\varepsilon}$. The statistic

$$\frac{(\tilde{\varepsilon} - \hat{\varepsilon})'[Z\Omega Z']^{-}(\tilde{\varepsilon} - \hat{\varepsilon})}{\sigma_\varepsilon^2} \sim \chi^2_{Q^*},$$

where $Q^* = \operatorname{rank}[Z\Omega Z'] \leq Q$. An equivalent statistic that is easier to compute is based on the distribution of $Z'(\tilde{\varepsilon} - \hat{\varepsilon})$, a $Q \times 1$ random vector:

$$(3.40) \qquad \begin{aligned}
&\frac{(\tilde{\varepsilon} - \hat{\varepsilon})' Z(Z'Z)^{-1} \Omega^{-} (Z'Z)^{-1} Z'(\tilde{\varepsilon} - \hat{\varepsilon})}{\sigma_\varepsilon^2} \\
&\quad = \frac{(\hat{\lambda} - \tilde{\lambda})'(Z'Z)^{-1} \Omega^{-} (Z'Z)^{-1} (\hat{\lambda} - \tilde{\lambda})}{\sigma_\varepsilon^2} \sim \chi^2_{Q^*},
\end{aligned}$$

where $Q^* = \operatorname{rank}[\Omega] \leq Q$. This statistic is the only formal specification test that we derive for the reasonableness of the set of conditioning variables, Z.

To compare the different estimators of the β coefficients, we rely on our consistent estimate of β and use Hausman (1978) statistics. We derive conventional specification tests of the difference between the consistent estimates of β based on equation (3.9) and estimates from other methodologies, including our conditional methods.

3.6. *The Construction of Z*

The role of the conditioning variables, Z, is to proxy for the covariation among the effects represented by X, D, and F. The columns of Z should be chosen to preserve as many of the effects ψ as possible, recalling that each column of Z reduces the rank of $(F'M_Z F)$ by one, while capturing as much of the conditional covariance of X and D with F as possible. Since these are competing goals, we will rely on judgement and on the specification test in equation (3.40) to choose a reasonable set of Z variables. We begin by noting that every column of Z increases the computational complexity of solving the equation system (3.15) and (3.16) in proportion to N^*Q^2 in terms of both storage and calculations. It is therefore necessary to accept some a priori restrictions on this auxiliary design matrix Z. Second, we note that the within-F regression in our conditional estimation procedure will not be well-defined for Z variables that do not have within-F variation. In order to give all columns of Z some within-F variation while, at the same time, inducing correlation with X and D, we chose the Z variables as interactions between firm characteristics (functions of F) and personal characteristics (functions of X and D). Under the specification described by equation (3.11), none of these interactions enters the model directly.

The columns of Z are defined as follows. Let

$$\bar{x}_i \equiv \frac{\sum_{t=1}^{T_i} x_{in_{it}}}{T_i} = \text{the within-person mean of } x_{it},$$

and

$$\bar{f}_j \equiv \frac{\sum_{(i,t)\in\{\mathbf{J}(i,t)=j\}} f_{(i,t)j}}{N_j} = \text{firm average of characteristic } f_{(i,t)j},$$

where the firm characteristics are measured by taking functions of the columns of F. In particular, firm size can be measured as a fixed constant times the number of person-years observed in firm j over the life of the sample, N_j.[25] The industry of firm j can be determined by applying a classification matrix $A, J \times K$

[25] We can calculate firm size in our sample using the following method. In our data, the employee sampling rate is $1/25$th and the number of at risk years is 10; hence, the constant $= 2.5$. Thus, in matrix form, we convert F_0 into a vector of firm sizes, L, as

$$L = F_0[e'_{N^*}F_0 \cdot 2.5]'$$

where e_{N^*} is an $N^* \times 1$ vector of ones.

to F_0 so that the result $F_0 A$ classifies each row of F_0, thus all N^* persons-years, into one of K unique industries. The firm characteristics actually used in our analysis are firm size, its square, and a 10-industry classification. The personal characteristics actually used were labor force experience (time-varying) and age at the end of schooling (non-time-varying). The rows of Z were constructed as

$$\text{row}\,(i,t) = \begin{bmatrix} \bar{x}_i & u_i \end{bmatrix} \otimes \begin{bmatrix} \bar{f}_{\mathbf{J}(i,t)} \end{bmatrix} \otimes \begin{bmatrix} 1 & s_{it} \end{bmatrix}.$$

3.7. *Analysis of Firm-level Outcomes*

Our analysis of firm-level outcomes requires summary statistics, by firm, of the effects estimated from equation (2.2). Although we use several different estimators for these effects, we always use the same aggregation formulas; so, we have shown those formulas using generic estimators for the underlying parameters.

First consider firm-level averages of the person effects θ_i and α_i,

$$\text{(3.41)} \qquad \hat{\theta}_j \equiv \frac{1}{N_j} \sum_{(i,t)\in\{\mathbf{J}(i,t)=j\}} \hat{\theta}_i$$

and

$$\hat{\alpha}_j \equiv \frac{1}{N_j} \sum_{(i,t)\in\{\mathbf{J}(i,t)=j\}} \hat{\alpha}_i .$$

We use the asymptotic distribution for $\hat{\alpha}_j$:

$$\text{(3.42)} \qquad \hat{\alpha}_j \to \mathrm{N}\left(\alpha_j, \sigma^2_{\alpha_j}\right), \quad \text{as} \quad N_j \to \infty$$

where

$$\sigma^2_{\alpha_j} \equiv \frac{1}{N_j^2} \sum_{i=1}^{N_j} \frac{\sigma_\varepsilon^2}{T_i} \left[1 - \frac{T_i}{\sigma_\varepsilon^2} u_i' \left(U' \operatorname{diag}\left(\operatorname{var}\left[\hat{\theta}_i\right]\right)^{-1} U \right)^{-1} u_i \right]$$

and for $\hat{\theta}_j$ (not shown).[26] Similarly, the firm-level average education effect is given by

$$\text{(3.43)} \qquad \bar{u}_j \hat{\eta} \equiv \frac{1}{N_j} \sum_{(i,t)\in\{\mathbf{J}(i,t)=j} u_i \hat{\eta}$$

with asymptotic distribution based upon (3.37). In all our asymptotic results we hold constant the distribution of firm sizes. Thus as $N, N_j \to \infty$, we assume that their ratio N_j/N goes to a nonzero constant.

[26] The formula for the symptotic distribution of $\hat{\theta}_j$ is identical to the one for $\hat{\alpha}_j$ with the quadratic form in u_i removed.

We consider next the statistical relation between firm-level outcomes and our measures of firm-level compensation policy. Our basic model is

$$(3.44) \qquad p_j = \begin{bmatrix} \alpha_j & \bar{u}_j\eta & \phi_j & \gamma_j & \gamma_{2j} & q_j \end{bmatrix} \begin{bmatrix} \zeta \\ \rho \end{bmatrix} + \xi_j$$

where $j = 1, \ldots J$, p_j is any firm-level outcome, $[\alpha_j \;\; \bar{u}_j\eta \;\; \phi_j \;\; \gamma_j \;\; \gamma_{2j}]$ is a vector of firm-level compensation measures, ζ is a vector of parameters of interest, q_j is a vector of other firm-level variables, ρ is a vector of parameters associated with q_j, and ξ_j is a zero-mean homoscedastic statistical error.[27] In the regression analysis, firm-level outcomes and firm-level compensation variables were measured using data from two independently drawn samples. However, the firm-level compensation variables derived from our individual sample are estimated regressors. Consequently, we must allow for the estimation errors in $\hat{\alpha}_j$, $\bar{u}_j\hat{\eta}$, $\hat{\phi}_j$, $\hat{\gamma}_j$, and $\hat{\gamma}_{2j}$ in our assessment of the precision of the estimation of firm-level equations.[28] Equation (3.44) becomes

$$\begin{aligned} (3.45) \qquad p_j = {} & \begin{bmatrix} \hat{\alpha}_j & \bar{u}_j\hat{\eta} & \hat{\phi}_j & \hat{\gamma}_j & \hat{\gamma}_{2j} & q_j \end{bmatrix} \begin{bmatrix} \zeta \\ \rho \end{bmatrix} \\ & + \Big(\begin{bmatrix} \alpha_j & \bar{u}_j\eta & \phi_j & \gamma_j & \gamma_{2j} \end{bmatrix} \\ & - \begin{bmatrix} \hat{\alpha}_j & \bar{u}_j\hat{\eta} & \hat{\phi}_j & \hat{\gamma}_j & \hat{\gamma}_{2j} \end{bmatrix} \Big) \zeta + \xi_j \end{aligned}$$

where $([\alpha_j \;\; \bar{u}_j\eta \;\; \phi_j \;\; \gamma_j \;\; \gamma_{2j}] - [\hat{\alpha}_j \;\; \bar{u}_j\hat{\eta} \;\; \hat{\phi}_j \;\; \hat{\gamma}_j \;\; \hat{\gamma}_{2j}])\zeta$ is the error associated with the first-step estimation of the firm-level compensation measures.[29] In order to derive the error covariance matrix for equation (3.45), let

$$P_j'(\hat{\delta}_j) \equiv \begin{bmatrix} \hat{\alpha}_j & \bar{u}_j\hat{\eta} & \hat{\phi}_j & \hat{\gamma}_j & \hat{\gamma}_{2j} & q_j \end{bmatrix}$$

and

$$\hat{\delta}_j' \equiv \begin{bmatrix} \hat{\alpha}_j & \bar{u}_j\hat{\eta} & \hat{\phi}_j & \hat{\gamma}_j & \hat{\gamma}_{2j} \end{bmatrix}.$$

[27] This is the most general specification, corresponding to the parameterization of the firm effect ($m = 3$) used in our order-dependent "persons first" method. In some of our firm-level analyses the terms involving γ_{2j} do not appear because the underlying firm effects were of lower dimension ($m = 2$).

[28] The firm-level regressor $\bar{x}_j\hat{\beta}$ also contains some measurement error, in principle; however, the vector $\hat{\beta}$ is estimated with such precision that we do not carry along its estimated covariance matrix (including its estimated covariance matrix with $\hat{\alpha}_j$, $\bar{u}_j\hat{\eta}$, $\hat{\phi}_j$, $\hat{\gamma}_j$, and $\hat{\gamma}_{2j}$) in these calculations. Hence, we place $\bar{x}_j\hat{\beta}$ in the list of q_j.

[29] We adopt the model of Pagan (1984); namely, that the regression of interest relates a function of the individual-level data and several firm-level parameters to the other measured firm-level outcomes. We account for the estimation error $([\alpha_j \;\; \bar{u}_j\eta \;\; \phi_j \;\; \gamma_j \;\; \gamma_{2j}] - [\hat{\alpha}_j \;\; \bar{u}_j\hat{\eta} \;\; \hat{\phi}_j \;\; \hat{\gamma}_j \;\; \hat{\gamma}_{2j}])$ explicitly, but we do not add an additional measurement error. Thus, for example, we assert that the outcome p_j depends upon α_j and not upon $\alpha_j + \zeta_j$, where ζ_j is an independent measurement error.

Now, equation (3.45) can be re-expressed in a first order approximation around δ_j as

$$(3.46) \qquad p_j = P_j'(\delta_j)\begin{bmatrix} \zeta \\ \rho \end{bmatrix} + \omega_j$$

where

$$\omega_j \equiv \left(\hat{\delta}_j - \delta_j\right)' \frac{\partial P_j'(\delta_j)}{\partial \delta_j}\begin{bmatrix} \zeta \\ \rho \end{bmatrix} + \zeta_j .$$

The variance of the regression error term for equation (3.46) consists of the component due to the estimation error in $\hat{P}_j$ plus the component due to ξ_j:

$$(3.47) \qquad \text{var}[\omega_j] \equiv \begin{bmatrix} \zeta' & \rho' \end{bmatrix} \frac{\partial P_j}{\partial \delta_j'} \text{var}\left[\hat{\delta}_j\right] \frac{\partial P_j'}{\partial \delta_j}\begin{bmatrix} \zeta \\ \rho \end{bmatrix} + \text{var}[\xi_j]$$

where the components of $\text{var}[\hat{\delta}_j]$ are defined in the derivations above. We estimate equation (3.46) using generalized least squares based upon the error variance in equation (3.47).

4. DATA DESCRIPTION

In this section we describe the important institutions of the French labor market and compare some simple statistical models of wage determination in France and the United States. The wage regressions demonstrate that, even though French and American labor market institutions are quite different, there are strong similarities in the way compensation is related to labor market observables in the two countries. Next, we lay out the sample design of our French data and describe the process we used to create an analysis sample. Finally, we present all of the variable definitions. Summary statistics appear in the Data Appendix.

4.1. *The French Labor Market*

During the sample period (from the mid-seventies to the end of the eighties), the French labor market was characterized by stable employment, whereas over this period employment increased by 25% in the United States. GDP growth in both countries was more or less identical, implying faster productivity growth in France. In addition, the employment-population ratio in France shrank while it was growing in the U.S.; as a reference, employment-population ratios were the same in France and the United States in the mid-sixties. In particular, the employment-population ratio fell dramatically for young workers (below 25) as well as for older workers (above 55).[30] The prevailing view—challenged in Card,

[30] See Card, Lemieux, and Kramarz (1996), for a more detailed analysis of French labor market outcomes in comparison with those of the United States and Canada.

Kramarz, and Lemieux (1996)—is that wage rigidities, examples of which are presented in the following paragraphs, have destroyed jobs in France. Nevertheless, even though wage-setting institutions differ, wage setting outcomes in the two labor markets share many features.

French employment in the 1970s was characterized by centralized collective bargaining (convention collective de branche), in which different industrial sectors had collective agreements that were negotiated by groups of unions and employers associations, and these agreements were binding on the negotiating parties. The complete agreement was then typically extended to cover the entire industry (or region) by the Ministry of Labor and was thereby made binding on workers and firms that were not party to the original negotiation (see Margolis (1993)). More than 95% of the work force was covered by these collective bargaining agreements at the end of the 1980s, while union membership was approximately 10%. The collective agreements specified a set of minimum wages and wage progressions for the occupational categories covered by the negotiations (sometimes called a wage grid). Beginning in 1982, the "lois Auroux" (a set of revisions to the body of labor law named after the Minister of Labor at the time) required firms with at least 50 employees to negotiate firm-level collective agreements (accords d'entreprise). Although firms were explicitly *not* obligated to actually conclude an agreement, the percentage of the work force covered by firm-level agreements grew to over 30% by the mid-1980s (see Abowd and Kramarz (1993) and Cahuc and Kramarz (1997)). The law imposed that the firm-level agreements could only improve the conditions stated in the industrial agreement, a result being that, over time, the firm-level agreements have become more relevant for wage determination than the industry agreements.

Since 1951, French industry has also been subject to a national minimum wage (called the SMIC since the revisions to the relevant law in 1971) that is indexed to the rate of change in consumer prices and to the average blue-collar wage rate. Although more than 90% of French workers are covered by industrial agreements throughout our analysis period (1976–1987), the regular increases in the national minimum wage (in particular those driven by the indexation to the average blue-collar wage rate) outpaced contract renegotiations, and the lowest rungs on the pay scales in most industry contracts for most occupations ended up below the national minimum in 1985. When this occurs, it is the national minimum wage, and not the collectively bargained wage, that binds.

Even though the French institutional arrangements seem to differ widely from those prevailing in the United States, wage-setting outcomes in the two countries share many features. For instance, manufacturing operative wages, when measured in purchasing power parity, are not very different (see Abowd and Bognanno (1995)). However, the ratio of the minimum wage to the average wage fell sharply in the U.S. while it rose modestly in France during the eighties (see Card, Kramarz, and Lemieux (1996)). Roughly 7% of French employed young workers (30 years old and under), and 6% of American employed young workers are paid at the minimum during the same period (see Abowd, Kramarz, Lemieux, and Margolis (forthcoming)). Even though total labor costs at the minimum wage are higher in France than in the U.S. due to employee- and employer-paid payroll taxes and other nonwage compensation costs, a 1% increase in the minimum wage induces roughly a 2% decrease in employment of young people in both countries (Abowd, Kramarz, Lemieux, and Margolis (forthcoming)).

To further assess potential differences in wage setting, we ran two simple wage regressions using comparable household surveys (the Enquête Emploi for France and the Current Population Survey for the U.S.).[31] Table II presents our estimation results. Our models show that the same set of regressors has more or less the same explanatory power for wages in both the French and American data (roughly 37% for men in both countries, 32% for women in France and 24% in the U.S.). Returns to one additional year of education are 6.1% for men and 7.2% for women in the U.S. while they are 7.7% for men and 8.8% for women in France, with the difference between the sexes being identical. Returns to experience differ slightly, with the curvature of the quartic in experience implying a more hump-shaped profile in the U.S. Finally, the gender wage gap in the initial year is roughly equal in both countries, although it decreases over the sample period in the U.S. and is basically stable in France during the eighties.

Other examples of such similarities in wage-setting outcomes abound. Card, Kramarz, and Lemieux (1996) have shown that the fraction of workers using computers is roughly the same in the two countries. Furthermore, returns to new technologies, and in particular computer use, are identical in the two countries. Estimates in Krueger (1993), in Entorf and Kramarz (1997), or Entorf, Gollac, and Kramarz (forthcoming) show that computer users are better compensated than nonusers by the same amount (15%). Krueger and Summers (1987) also show that inter-industry wage differentials in France are highly correlated with American inter-industry wage differentials.

4.2. *Description of the DAS*

Our main data source is the "Déclarations Annuelles des Salaires" (DAS), a large-scale administrative database of matched employer-employee information collected by INSEE (Institut National de la Statistique et des Etudes Economiques) and maintained in the Division des Revenus. The data are based upon mandatory employer reports of the gross earnings of each employee subject to French payroll taxes. These taxes apply to all "declared" employees and to all self-employed persons, essentially all employed persons in the economy.

The Division des Revenus prepares an extract of the DAS for scientific analysis, covering all individuals employed in French enterprises who were born in October of even-numbered years, with civil servants excluded.[32] Our extract

[31] Similar results are also found using cross-sections of matched worker-firm data for the two countries (see Abowd, Kramarz, Margolis, and Troske (1998)).

[32] Meron (1988) shows that individuals employed in the civil service move almost exclusively to other positions within the civil service. Thus, the exclusion of civil servants should not affect our estimation of a worker's market wage equation. Employees of the state-owned firms are present in our sample, however.

TABLE II

Comparison of Least Squares Estimates of Wage Determination in France and the United States 1982–1987

	France				*United States*			
	Men		Women		Men		Women	
Variable	Mean	OLS Results	Mean	OLS Results	Mean	OLS Results	Mean	OLS Results
Intercept	1.000	1.365	1.000	1.163	1.000	0.534	1.000	0.380
	(0.000)	(6.746E-3)	(0.000)	(8.190E-3)	(0.000)	(5.614E-3)	(0.000)	(5.679E-3)
Years of	10.726	0.077	11.325	0.088	11.880	0.061	12.300	0.072
Education	(3.659)	(2.848E-4)	(3.267)	(3.998E-4)	(2.391)	(3.521E-4)	(2.149)	(3.712E-4)
Experience	20.722	0.058	19.048	0.060	15.894	0.112	16.036	0.082
	(12.222)	(1.228E-3)	(12.163)	(1.435E-3)	(12.311)	(9.219E-4)	(12.323)	(8.899E-4)
Experience^2/	5.788	−0.104	5.108	−0.188	4.042	−0.506	4.090	−0.436
100	(5.875)	(9.095E-3)	(5.533)	(1.133E-2)	(5.251)	(8.487E-3)	(5.077)	(8.482E-3)
Experience^3/	18.915	−0.007	16.207	0.018	12.611	0.102	12.544	0.093
1,000	(26.344)	(2.554E-3)	(23.730)	(3.355E-3)	(21.849)	(2.811E-3)	(20.514)	(2.898E-3)
Experience^4/	68.009	0.002	56.700	0.001	43.914	−0.007	42.506	−0.007
10,000	(120.682)	(2.397E-4)	(103.687)	(3.316E-4)	(92.920)	(3.037E-4)	(84.740)	(3.232E-4)
1982	0.175	0.036	0.167	0.027	0.163	0.072	0.160	0.019
	(0.380)	(3.001E-3)	(0.373)	(3.784E-3)	(0.370)	(2.715E-3)	(36.707)	(2.596E-3)
1983	0.170	0.018	0.166	0.006	0.162	0.049	0.160	0.015
	(0.375)	(3.020E-3)	(0.373)	(3.783E-3)	(0.369)	(2.707E-3)	(0.367)	(2.579E-3)
1984	0.166	0.019	0.164	0.012	0.164	0.032	0.162	0.000
	(0.372)	(3.033E-3)	(0.371)	(3.793E-3)	(0.370)	(2.679E-3)	(0.368)	(2.557E-3)
1985	0.165	0.005	0.165	−0.001	0.167	0.018	0.166	−0.002
	(0.371)	(3.040E-3)	(0.371)	(3.785E-3)	(0.373)	(2.658E-3)	(0.372)	(2.534E-3)
1986	0.162	0.023	0.168	0.018	0.174	0.015	0.175	0.004
	(0.369)	(3.051E-3)	(0.374)	(3.767E-3)	(0.379)	(2.601E-3)	(0.380)	(2.479E-3)
Paris Region	0.210	0.168	0.240	0.158	—	—	—	—
	(0.407)	(2.147E-3)	(0.427)	(2.567E-3)	—	—	—	—
Northeast U.S.	—	—	—	—	0.210	−0.046	0.217	−0.057
	—	—	—	—	(0.408)	(2.496E-3)	(0.412)	(2.393E-3)
Midwest U.S.	—	—	—	—	0.263	−0.039	0.273	−0.088
	—	—	—	—	(0.440)	(2.309E-3)	(0.446)	(2.222E-3)
Southern U.S.	—	—	—	—	0.296	−0.143	0.289	−0.128
	—	—	—	—	(0.457)	(2.206E-3)	(0.453)	(2.151E-3)
Observations	165,036	165,036	126,320	126,320	259,297	259,297	259,266	259,266
Adjusted R^2		0.3866		0.3254		0.3626		0.2428

Sources: Enquête Emploi (1982–1987) for France and NBER outgoing rotation group CPS extracts (1982–1987) for the United States. *Notes:* Standard Deviations/Errors in Parentheses. Both regressions included only individuals between 16 and 60 years old, inclusive. Both regressions used the sample weights. Experience is measured as (age) − (age at the end of schooling) in France and (age) − (years of schooling) − 5 in the United States.

runs from 1976 through 1987, with 1981 and 1983 excluded because the underlying administrative data were not sampled in those years. The initial data set contained 7,416,422 observations. Each observation corresponded to a unique individual-year-establishment combination. An observation in this initial DAS file includes an identifier that corresponds to the employee (called ID below), an identifier that corresponds to the establishment (SIRET) and an identifier that corresponds to the parent enterprise of the establishment (SIREN). For each observation, we have information on the number of days during the calendar year the individual worked in the establishment and the full-time/part-time status of the employee. For each observation corresponding to an individual-year-establishment, in addition to the variables listed above, we have information on the individual's sex, date and place of birth, occupation, total net nominal earnings during the year and annualized gross nominal earnings during the year for the individual, as well as the location and industry of the employing establishment.

4.3. *Observation Selection, Variable Creation and Missing Data Imputation*

4.3.1. *Aggregation across establishments*

The creation of the analysis data set involved the selection of desired individuals, the aggregation of establishment-level data to the enterprise level, and the construction of the variables of interest from the variables already in the data set. We selected only full-time employees (sample reduced to 5,966,620 observations). We then created a single observation for each ID-year-SIREN combination by aggregating within ID and year over SIRETs in the same SIREN. For each ID-year-SIREN, we summed total net nominal earnings and total days worked over all SIRETs. We assigned to the observation the occupation, location, and industry that corresponded to the establishment in which the individual worked the largest number of days during the year. This reduced the number of observations to 5,965,256. We then selected the enterprise at which the individual had worked the largest number of days during that year (sample reduced to 5,497,287 observations). The aggregation of total number of days worked across all establishments occasionally yielded observations for which the total number of days worked was greater than 360 (the maximum permitted). In these cases, we truncated days worked at 360. We then calculated an annualized net nominal earnings for the ID-year-SIREN combination. We eliminated all years of data for individuals who were younger than 15 years old or older than 65 years old at the date of their first appearance in the data set (sample reduced to 5,325,413 observations).

4.3.2. *Total compensation costs*

The dependent variable in our wage rate analysis is the logarithm of real annualized total compensation cost for the employee. To convert the annualized net nominal earnings to total compensation costs, we used the tax rules and computer programs provided by the Division des Revenus at INSEE (Lhéritier, internal, undated INSEE communication) to compute both the employee and employer share of all mandatory payroll taxes (cotisations et charges salariales: employé et employeur). Annualized total compensation cost is defined as the sum of annualized net nominal earnings, annualized employee payroll taxes, and annualized employer payroll taxes. Nominal values were deflated by the consumer price index to get real annualized net earnings, and real annualized total compensation cost. We eliminated 61 observations with zero values for annualized total compensation cost (remaining sample 5,325,352).

4.3.3. *Education and school-leaving age*

Our initial DAS file did not contain education information. We used supplementary information from the permanent demographic sample (Echantillon Démographique Permanent, EDP) available for 10% of the DAS, to impute the level of education for all remaining individuals in the DAS. The EDP includes information on the highest degree obtained. There were 38 possible responses, including "no known degree." These responses were grouped into eight degree-level categories as shown in Data Appendix Table B1. Using these eight categories as the dependent variable and data available in the DAS, we ran separate ordered logits for men and women. We used the data corresponding to the earliest date that an individual appeared in our sample to estimate these models. We used the same data and the estimated coefficients from these ordered logit models to impute the probability of obtaining each of the eight different aggregated degrees for the individuals in the DAS who were not part of the EDP. We used the actual value of the eight degree aggregates for the EDP sample members. Thus, a random 10% sample of the DAS individuals have true education and the remaining 90% have the probability of obtaining each of the eight degree aggregates. EDP sample statistics for the men are in Data Appendix Table B2, and those for the women are in Table B3. The estimated logit equations are in Table B4 for men and Table B5 for women.[33]

To calculate school-leaving age we used Table 14 in CEREQ-DEP-INSEE (1990), which provides the average age of termination for each French diploma separately for men and women in 1986. Using the probability of each degree category and the average school-leaving age for degrees in that category (the ages were fairly homogeneous within categories), we calculated expected school-leaving age.

4.3.4. *Total labor market experience*

For the first year in which individuals appear in the sample, we calculate potential labor market experience as age at the beginning of the year less our estimate of school-leaving age. In all subsequent years, total labor market experience is accumulated using the individual's realized labor force history. Our algorithm was the same for both labor force experience and seniority. It

[33] We considered, and rejected, the possibility of using a Rubin (1987) style multiple imputation algorithm for the missing schooling variable for the following reasons: (1) since schooling does not time-vary, and since our conclusions are completely unaffected by whether we remove a schooling effect from the person effect or not (θ as compared with α), we did not want to bear the computational burden associated with these methods for such a small return; (2) the schooling variable is substituting for occupational category, a more common control variable in French earnings equations because of the educational qualifications that define the occupational categories, in our models in order to make the analysis more comparable to the vast American literature that uses schooling rather than occupation and defines person effects with a schooling effect removed; (3) conditioning the imputation on the observed value of the compensation variable, as these methods require, would focus attention on the imputation procedure and detract from our main focus.

accounts for the holes caused by the fact that the administrative data were not available for 1981 and 1983. See the Data Appendix for details.

4.3.5. *Job seniority*

Individuals fell into two categories with respect to the calculation of job seniority (employer-specific experience): those for whom the first year of observation was 1976 and those who first appeared after 1976. For those individuals whose first observation was in 1976, we estimated the expected length of the in-progress employment spell by regression analysis using a supplementary survey, the 1978 Salary Structure Survey (Enquête sur la Structure des Salaires, ESS). In this survey, respondent establishments provided information on seniority (in 1978), occupation, date of birth, industry, and work location for a scientific sample of their employees. Using the ESS information, we estimated separate regressions for men and women to predict seniority for in-progress spells in 1976. The coefficients from these regressions were used to calculate expected job seniority in 1976 for DAS individuals whose first observation was in 1976. The dependent variable in the supplementary ESS regressions was current seniority with the employer and the explanatory variables were date of birth, occupation (1-digit), region of employment (metropolitan Paris), and industry (NAP 100, approximately 2-digit). Results of the seniority regressions are shown in equations (8.1) for men and (8.2) for women in the Data Appendix. We used the results of these seniority regressions to impute levels of job seniority in 1976 for the left-censored DAS individuals first observed in 1976. Details are provided in the Data Appendix.

4.3.6. *Elimination of outliers*

After calculating all of the individual-level variables, we eliminated observations for which the log of the annualized real total compensation cost was more than five standard deviations away from its predicted value based on a linear regression model with independent variables: sex, region of France, experience and education (see equation (8.4) in the Data Appendix). This gives us the analysis sample of 5,305,108 observations.

Table B7 in the Data Appendix shows the basic summary statistics, by sex, for the individual-level data. The usable sample consists of 3,434,530 observations on 711,518 men and 1,870,578 usable observations on 454,787 women. The basic individual-level variables are: sex, labor force experience, region of France, education, and seniority. Note that about 30% of the sample has no known educational attainment. For 74% of the individuals, there are enough observations in the sample to permit estimation of a distinct firm-effect.[34]

[34] The individuals from firms with fewer than 10 observations in the sample were pooled and a single firm-level regression was used to estimate their firm effects.

4.4. *Construction of the Firm-Level Data*

4.4.1. *DAS-based firm-level averages*

For our firm-level analyses we calculated the aggregates $\hat{\alpha}_j$, $\bar{u}_j\hat{\eta}$ and their respective sampling variances based on the $\hat{\alpha}_i$ and $u_i\hat{\eta}$ estimated according to the conditional estimation methods laid out in Section 3 above. The estimated parameters $\hat{\phi}_j$, $\hat{\gamma}_j$, and $\hat{\gamma}_{2j}$ have unique values for a given enterprise, by construction. In cases where any one of the following three conditions failed: $-3 \leq \hat{\phi}_j \leq 3$ or $-2 \leq \hat{\gamma}_j \leq 2$ or $-2 \leq \hat{\gamma}_j + \hat{\gamma}_{2j} \leq 2$, we set $\hat{\phi}_j$, $\hat{\gamma}_j$, and $\hat{\gamma}_{2j}$ equal to the values estimated in the pooled model for the firm effects for all firms with 10 or fewer observations.

4.4.2. *Other firm-level data sources*

The primary source of our firm-level data is the INSEE (1989, 1990a–c) enterprise sample (Echantillon d'Entreprises, EE), a probability sample of French firms (synonymous with enterprises for our purposes). The EE data set provides the sampling frame for the firm-based part of this paper. The universe for the sample is the annual report on profitability and employment by enterprises (Bénéfices Industriels et Commerciaux, BIC) and the annual survey of enterprises (Enquête Annuelle d'Entreprises, EAE). To construct the EE, firms with more than 500 employees were sampled from the BIC with probability 1; firms with 50 to 499 employees were sampled from the BIC with probabilities ranging from 1/4 to 1/2 depending upon the industry, and smaller firms were sampled from the BIC with probability 1/30. All firms responding to the BIC were at risk to be sampled exactly once. Hence, the EE is dynamically representative of French enterprises in all sectors except the public administration sector. We use the sampling weight (non-time-varying) and the variables described below, averaged over the period 1978 to 1988 for all available years, from the EE.

4.4.3. *Firm-level employment and capital stock*

The measure of employment, in thousands of workers, is full-time employment in an enterprise as of December 31 (prior to 1984) and the annual average full-time employment (1984 and later) as found in the BIC. We took the mean of this variable over all years that the firm appeared in the sample.

Total capital in the enterprise is defined as the sum of debt (dettes) and owners' equity (fonds propres d'entreprise). Our capital measure is equal to total assets (actif total) in French accounting systems. The information was taken directly from the BIC for every firm-year. We deflated the capital stock using an industry-specific, annual index of the price of capital from the INSEE macroeconomic time series data (Banque de Données Macroéconomiques, BDM). Our measure of real total capital is defined as total assets divided by the industry-specific price index of physical capital (in millions of 1980 FF), averaged over all available years for the firm. The capital labor ratio is defined as real total capital divided by total full-time employment. We also averaged this variable over all available years for the firm.

4.4.4. *Real operating income per unit of capital*

We used the BIC to obtain the operating income (excédent brut d'exploitation, EBE), for each firm in each year that it appeared in the firm sample. The formula used to calculate the EBE is shown in equation (8.6) in the Data Appendix. The EBE was deflated by the value added price index (prix de valeur ajoutée) also found in the BDM, to yield real operating income (in millions of 1980 FF). Real operating income was divided by real total capital to yield real operating income per unit of capital, stated as a proportion. We also took the mean of this variable over all available years for the firm.

4.4.5. *Real value added inclusive of labor costs*

To calculate the real value added inclusive of labor costs (valeur ajoutée réelle brute au coût des facteurs), we divided the employer's compensation costs (frais de personnel) in the BIC (thousands of FF) by the consumer price index (indice des prix à la consommation) from the BDM to yield the employer's real compensation cost (in millions of 1980 FF). The result was added to real operating income, as defined above in Section 4.4.4, to yield the real value added inclusive of labor costs (in millions of 1980 FF). Real value added inclusive of labor costs was divided by total employment to yield real value added inclusive of labor costs per worker (in thousands of 1980 FF). We then took the mean of this variable over all of the years that the firm appeared in the EE sample.

4.4.6. *Employment structure*

The variables concerning the occupational structure of employment (proportion of engineers, technicians and managers in the work force and proportion of skilled workers) were created from the employment structure survey (Enquête sur la Structure des Emplois, ESE), which is an annual administrative data base of the detailed (4-digit) occupational structure of all establishments with more than 20 employees. The occupational structure of the firm, measured in the ESE, was merged with the EE using the firm identifier and the survey year. Engineers, technicians and managers were coded using the simplified occupation classifications (1-digit equivalents) for individuals in categories 30 and 40. The proportion of skilled workers in the work force was calculated from the ESE using the simplified occupation classification for individuals in categories 50 and 61. Both variables were expressed as a ratio to total employment and averaged over all the available firm-years. The omitted category is unskilled workers, which would include all other codes.

5. ESTIMATION RESULTS

5.1. *Overview*

We present our statistical results in three main parts. Tables III–VI present detailed results from the analysis of the matched employer-employee microdata. Table III shows the regression coefficients for men and women from all of the estimation methods described above as well as results from standard specifications based upon incomplete parameterizations of equation (2.2). Table IV presents summary statistics for men and women for all of the components of compensation in our complete model and for the two estimation methods upon which we focus most of our subsequent attention. Table V is a diagnostic table of the correlations among the same components of compensation when we vary the method of estimation. Table VI is a table of correlations among all the components of compensation for our two chosen estimation methods. Tables VII and VIII present our statistical analysis of the inter-industry wage differential and the firm-size wage effect, respectively. Tables IX–XII present the results of analyses conducted at the firm level. Table IX shows summary statistics for the firm-level variables, including those we created from the matched microdata. Table X presents the results of our analysis of firm-level profitability and productivity. Table XI presents our analysis of firm-level factors of production and compensation components. Table XII presents the results of a survival analysis using the firm-level data.

5.2. *Results from the Estimation of the General Compensation Equation*

5.2.1. *Specification of the compensation equation*

Table III presents a summary of the estimates of β, the coefficients on the time-varying individual characteristics, for our consistent estimation method, our conditional estimation method with persons first, and ordinary least squares under a variety of different assumptions about the included person and firm effects, separately for men and women. The results labelled "Consistent Method Person & Firm Effects" were calculated according to equation (3.9). The results labelled "Conditional Method Persons First" were calculated using the formula found in equation (3.17). The β coefficients obtained by the order-dependent method with persons first, conditional on Z, and those obtained by the order-independent method, conditional on Z, are mathematically identical. The column labelled "Least Squares No Person/Firm Effects" presents the estimates obtained when we set all person effects, $D\theta$, firm effects, $F\psi$, and conditioning effects, $Z\lambda$, jointly to zero. Next, in the column labelled "Within Persons No Firm Effects," we present results obtained when we retain the person effects, $D\theta$, but set all firm effects, $F\psi$, and conditioning effects, $Z\lambda$, jointly to zero. In the column labelled "Within Persons Limited Firm Effects," we present β coefficients estimated when we retain all person effects, $D\theta$, choose a set of effects Z equal to the columns of F corresponding to the 115 largest employers

TABLE III

ESTIMATES OF THE EFFECTS OF LABOR FORCE EXPERIENCE, REGION, YEAR, EDUCATION, INDIVIDUALS, AND FIRMS ON THE LOG OF REAL TOTAL ANNUAL COMPENSATION COSTS INDIVIDUAL DATA BY SEX FOR 1976 TO 1987

Variable	*Consistent Method Person & Firm Effects* Parameter Estimate	Standard Error	*Conditional Method Persons First* Parameter Estimate	Standard Error	*Least Squares No Person/Firm Effects* Parameter Estimate	Standard Error	*Within Persons No Firm Effects* Parameter Estimate	Standard Error	*Within Persons Limited Firm Effects*[a] Parameter Estimate	Standard Error	*Within Firms No Person Effects* Parameter Estimate	Standard Error	*Within Industry No Person Effects* Parameter Estimate	Standard Error	*Within Firm Size No Person Effects* Parameter Estimate	Standard Error
Men																
Total Labor Force Experience	0.0586	(0.0015)	0.0687	(0.0004)	0.0542	(0.0003)	0.0695	(0.0004)	0.0685	(0.0004)	0.0448	(0.0003)	0.0521	(0.0003)	0.0507	(0.0003)
(Labor Force Experience)^2/100	−0.3432	(0.0072)	−0.4415	(0.0027)	−0.2280	(0.0030)	−0.4543	(0.0029)	−0.4446	(0.0030)	−0.1584	(0.0027)	−0.2115	(0.0030)	−0.2047	(0.0029)
(Labor Force Experience)^3/1,000	0.0734	(0.0025)	0.1053	(0.0010)	0.0503	(0.0010)	0.1100	(0.0010)	0.1074	(0.0010)	0.0298	(0.0009)	0.0452	(0.0010)	0.0441	(0.0010)
(Labor Force Experience)^4/10,000	−0.0057	(00003)	−0.0093	(0.0001)	−0.0046	(0.0001)	−0.0099	(0.0001)	−0.0096	(0.0001)	−0.0025	(0.0001)	−0.0041	(0.0001)	−0.0040	(0.0001)
Lives in Ile-de-France	0.0051	(0.0017)	0.0832	(0.0010)	0.1398	(0.0005)	0.0819	(0.0011)	0.0805	(0.0011)	0.1117	(0.0007)	0.1316	(0.0006)	0.1314	(0.0005)
Year 1977	0.0245	(0.0009)	0.0251	(0.0007)	0.0343	(0.0010)	0.0252	(0.0007)	0.0248	(0.0007)	0.0182	(0.0009)	0.0275	(0.0010)	0.0296	(0.0010)
Year 1978	0.0643	(0.0017)	0.0605	(0.0008)	0.0645	(0.0010)	0.0609	(0.0008)	0.0598	(0.0008)	0.0463	(0.0009)	0.0560	(0.0010)	0.0581	(0.0010)
Year 1979	0.0887	(0.0024)	0.0879	(0.0009)	0.0841	(0.0010)	0.0883	(0.0010)	0.0873	(0.0010)	0.0598	(0.0009)	0.0755	(0.0010)	0.0774	(0.0010)
Year 1980	0.1081	(0.0031)	0.1030	(0.0011)	0.0899	(0.0010)	0.1033	(0.0012)	0.1024	(0.0012)	0.0644	(0.0009)	0.0803	(0.0010)	0.0841	(0.0010)
Year 1982	0.1473	(0.0035)	0.1441	(0.0014)	0.1137	(0.0010)	0.1447	(0.0016)	0.1434	(0.0016)	0.0809	(0.0009)	0.1043	(0.0010)	0.1091	(0.0010)
Year 1984	0.1872	(0.0041)	0.1911	(0.0018)	0.1441	(0.0010)	0.1919	(0.0020)	0.1903	(0.0020)	0.1009	(0.0009)	0.1316	(0.0011)	0.1386	(0.0011)
Year 1985	0.2044	(0.0047)	0.2173	(0.0020)	0.1662	(0.0011)	0.2179	(0.0022)	0.2162	(0.0022)	0.1146	(0.0009)	0.1516	(0.0011)	0.1612	(0.0011)
Year 1986	0.2366	(0.0053)	0.2529	(0.0022)	0.1841	(0.0010)	0.2535	(0.0024)	0.2517	(0.0024)	0.1315	(0.0009)	0.1690	(0.0011)	0.1813	(0.0011)
Year 1987	0.2499	(0.0060)	0.2763	(0.0024)	0.1954	(0.0010)	0.2768	(0.0026)	0.2749	(0.0026)	0.1401	(0.0009)	0.1808	(0.0011)	0.1948	(0.0011)
Women																
Total Labor Force Experience	0.0144	(0.0016)	0.0290	(0.0005)	0.0326	(0.0004)	0.0308	(0.0006)	0.0298	(0.0005)	0.0224	(0.0004)	0.0603	(0.0004)	0.0286	(0.0004)
(Labor Force Experience)^2/100	−0.1063	(0.0091)	−0.1728	(0.0036)	−0.1117	(0.0038)	−0.1771	(0.0041)	−0.1729	(0.0040)	−0.0318	(0.0035)	−0.3525	(0.0039)	−0.0816	(0.0038)

(Labor Force Experience)^3/1,000	0.0184 (0.0032)	0.0379 (0.0013)	0.0183 (0.0013)	0.0391 (0.0014)	0.0381 (0.0014)	−0.0053 (0.0012)	0.0942 (0.0013)	0.0103 (0.0013)
(Labor Force Experience)^4/10,000	−0.0009 (0.0004)	−0.0031 (0.0001)	−0.0013 (0.0001)	−0.0031 (0.0002)	−0.0031 (0.0002)	0.0011 (0.0001)	−0.0091 (0.0001)	−0.0005 (0.0001)
Lives in Ile-de-France	0.0042 (0.0027)	0.0795 (0.0016)	0.1576 (0.0007)	0.0794 (0.0018)	0.0809 (0.0017)	0.1218 (0.0009)	0.1434 (0.0007)	0.1470 (0.0007)
Year 1977	0.0300 (0.0011)	0.0271 (0.0009)	0.0527 (0.0014)	0.0250 (0.0011)	0.0255 (0.0010)	0.0348 (0.0012)	0.1361 (0.0015)	0.0495 (0.0014)
Year 1978	0.0762 (0.0019)	0.0724 (0.0010)	0.1053 (0.0014)	0.0688 (0.0012)	0.0695 (0.0011)	0.0798 (0.0012)	0.1889 (0.0015)	0.1003 (0.0014)
Year 1979	0.1102 (0.0026)	0.1052 (0.0012)	0.1353 (0.0014)	0.1003 (0.0014)	0.1015 (0.0013)	0.1044 (0.0012)	0.2179 (0.0014)	0.1303 (0.0014)
Year 1980	0.1329 (0.0033)	0.1227 (0.0014)	0.1445 (0.0014)	0.1169 (0.0016)	0.1182 (0.0015)	0.1148 (0.0012)	0.2285 (0.0014)	0.1408 (0.0014)
Year 1982	0.1830 (0.0039)	0.1704 (0.0018)	0.1758 (0.0014)	0.1627 (0.0020)	0.1640 (0.0019)	0.1406 (0.0012)	0.2600 (0.0015)	0.1742 (0.0015)
Year 1984	0.2233 (0.0047)	0.2188 (0.0022)	0.2231 (0.0014)	0.2094 (0.0025)	0.2109 (0.0024)	0.1719 (0.0013)	0.3021 (0.0015)	0.2200 (0.0015)
Year 1985	0.2361 (0.0053)	0.2377 (0.0024)	0.2392 (0.0014)	0.2277 (0.0027)	0.2292 (0.0026)	0.1782 (0.0013)	0.3163 (0.0015)	0.2360 (0.0015)
Year 1986	0.2644 (0.0059)	0.2686 (0.0026)	0.2559 (0.0014)	0.2577 (0.0030)	0.2594 (0.0029)	0.1945 (0.0013)	0.3340 (0.0015)	0.2549 (0.0015)
Year 1987	0.2756 (0.0066)	0.2886 (0.0028)	0.2615 (0.0014)	0.2767 (0.0033)	0.2787 (0.0031)	0.1995 (0.0013)	0.3414 (0.0015)	0.2630 (0.0015)
Pooled								
Sample Size	5,305,108	5,305,108	5,305,108	5,305,108	5,305,108	5,305,108	5,109,008[b]	5,305,108
Coefficient Degrees of Freedom (β)	28	28	44	30	30	45	129	72
Coefficient Degrees of Freedom (λ)		48[c]						
Individual Degrees of Freedom (θ)	2,011,864	1,166,305		1,166,305	1,166,305			
Firm Degrees of Freedom (ψ)	521,182				229	521,182		
Error Degrees of Freedom (ε)	2,772,034	4,138,727	5,305,064	4,138,773	4,138,544	4,783,881	5,108,879	5,305,036
Root Mean Squared Error	0.2828	0.2732	0.4223	0.2737	0.2733	0.3577	0.4204	0.4179
R^2	0.8364	0.7720	0.3017	0.7711	0.7720	0.5482	0.3389	0.3164
Specification Test	na	3806.5	7543.6	3410.5	3400.1	6642.0	na	na

Notes: (a) Includes firm effects (intercept and seniority slope) for the 115 largest firms in the sample. (b) Total degrees of freedom differ from other columns because of missing industry data. (c) See Data Appendix Table B6 for summary statistics, coefficients and standard errors corresponding to these variables.

TABLE IV

DESCRIPTIVE STATISTICS FOR COMPONENTS OF LOG REAL TOTAL ANNUAL COMPENSATION BY SEX FOR 1976 TO 1987

Variable Definition	*Men* Mean	*Men* Std. Dev.	*Women* Mean	*Women* Std. Dev.
Log (Real Annual Compensation, 1980 FF)	4.3442	0.5187	4.0984	0.4801
Order-Independent Method				
$x\beta$, Predicted Effect of x Variables	0.3890	0.1489	0.2849	0.1144
θ, Individual Effect Including Education	3.9552	0.4475	3.8135	0.3930
α, Individual Effect (Unobserved Factors)	0.0000	0.4051	0.0000	0.3771
$u\eta$, Individual Effect of Education and Sex	3.9552	0.1902	3.6893	0.1107
ψ, Firm Effect (Intercept and Slope)	−0.0363	0.4642	0.0665	0.5116
ϕ, Firm Effect Intercept	−0.1367	0.4532	−0.0235	0.4967
γ, Firm Effect Slope	0.0149	0.0503	0.0172	0.0531
Order-Dependent Method: *Persons First*				
$x\beta$, Predicted Effect of x Variables	0.4261	0.1383	0.3234	0.1120
θ, Individual Effect Including Education	3.9160	0.4387	3.7776	0.3843
α, Individual Effect (Unobserved Factors)	0.0000	0.3947	0.0000	0.3639
$u\eta$, Individual Effect of Education and Sex	3.9160	0.1915	3.7776	0.1238
ψ, Firm Effect (Intercept and Slope)	0.0028	0.0685	−0.0039	0.0566
ϕ, Firm Effect Intercept	0.0031	0.1044	−0.0072	0.0969
γ, Firm Effect Slope	−3.37*e*-05	0.0335	8.28*e*-04	0.0326
γ_2, Firm Effect Slope Change at 10 years	−5.36*e*-04	0.0542	−1.64*e*-03	0.0574
Other Estimation Methods				
Seniority coefficient, Least Squares (Standard Error)	0.0118	(0.0001)	0.0141	(0.0001)
Seniority coefficient, within Persons (Standard Error)	0.0033	(0.0001)	0.0024	(0.0001)
Seniority coefficient, within Firms (Standard Error)	0.0078	(0.0001)	0.0102	(0.0001)
Seniority coefficient, within Industry (Standard Error)	0.0090	(0.0001)	0.0121	(0.0001)
Seniority coefficient, within Size Class (Standard Error)	0.0097	(0.0001)	0.0126	(0.0001)
γ, Firm Effect Slope, 115 largest firms	0.0013	0.0065	0.0014	0.0076
γ, Firm Effect Slope, Consistent estimates	0.0116	0.0342	0.0138	0.0352

Notes: Seniority coefficients with standard errors were estimated in the same models reported in Table III. All other statistics are the means and standard deviations based upon the sample of 5,305,108 observations except for the Firm Effect Slope in the 115 largest firms, which are statistics based on 695,077 observations.

in our data (firm-specific intercepts and seniority slopes), and set all remaining firm effects, $F\psi$, to a single common effect. Thus, this column shows estimates of a model in which 695 thousand of our 5.3 million observations have separate, firm effects (firm-specific intercepts and seniority slopes) and all remaining observations are pooled into a single artificial "firm," which had its own

TABLE V

CORRELATIONS AMONG THE COMPONENTS OF PERSON AND FIRM HETEROGENEITY AS ESTIMATED BY THE ORDER-INDEPENDENT, ORDER-DEPENDENT, FULL LEAST SQUARES ON THE 115 LARGEST FIRMS, AND CONSISTENT METHODS

Source of Estimate of the Indicated Effect	Parameter Name	Simple Correlation with: Order-Independent Estimates		Order-Dependent Estimates			Full Least Squares Estimates on the 115 Largest Firms		Consistent Estimates
		Persons First							
Firm Effects		ϕ	γ	ϕ	γ	γ_2	ϕ	γ	γ
Order-Independent Estimates	ϕ	1.0000	−0.0718	0.1553	−0.0837	0.0188	0.0888	0.2800	0.0361
	γ	−0.0718	1.0000	−0.2202	0.5300	−0.0077	−0.3276	0.3126	0.0907
Order-Dependent Estimates (Persons First)	ϕ	0.1553	−0.2202	1.0000	−0.5625	0.2562	0.6659	−0.0231	−0.1810
	γ	−0.0837	0.5300	−0.5625	1.0000	−0.2094	−0.6580	0.2739	0.1358
	γ_2	0.0188	−0.0077	0.2562	−0.2094	1.0000	0.5492	0.0293	−0.0126
Full Least Squares Estimates Using the 115 Largest Firms	ϕ	0.0888	−0.3276	0.6659	−0.6580	0.5492	1.0000	−0.1841	−0.1964
	γ	0.2800	0.3126	−0.0231	0.2739	0.0293	−0.1841	1.0000	0.5106
Consistent Estimates	γ	0.0361	0.0907	−0.1810	0.1358	−0.0126	−0.1964	0.5106	1.0000
		Firms First							
Person Effects		α		α			α		
Order-Independent Estimates	α	1.0000		0.5833			0.9896		
Order-Dependent Estimates (Firms First)	α	0.5833		1.0000			0.5983		
Full Least Squares Estimates Using the 115 Largest Firms	α	0.9896		0.5983			1.0000		

Notes: $N = 5{,}305{,}108$, except for Full Least Squares Estimates Using the 115 Largest Firms where $N = 695{,}077$.
Source: Authors' calculations based on the DAS.

intercept and seniority slope. Table III also shows, in the column labelled "Within Firms No Person Effects," the results obtained when we estimate a model where we retain all firm effects, $F\psi$ (intercepts only), and set all person effects, $D\theta$, and all conditioning effects, $Z\lambda$, to zero. The last two columns present results obtained from estimating a model with person effects and firm effects jointly set to zero, and with the functions Z of the form $Z = FA$, where A generates 84 industry effects and is as defined in Section 2.1 ("Within Industry No Person Effects"), and of the form $Z = FS$, with the matrix S generating 25 firm-size classes based on the firm sizes constructed as described in Section 3.6 ("Within Firm Size No Person Effects").

All of the estimation methods that include person effects (consistent method, conditional method with persons first or order independent, within persons without firm effects, and within persons with limited firm effects) are able to explain a similar fraction of the variance—between 77% and 83%. In contrast, all of the results that exclude person effects (ordinary least squares, within firms, within industries, and within firm-size categories) give results similar to the

TABLE VI

SUMMARY STATISTICS FOR THE DECOMPOSITION OF VARIANCE USING THE ORDER-INDEPENDENT AND THE ORDER-DEPENDENT CONDITIONAL METHODS FOR INDIVIDUAL DATA, BOTH SEXES, 1976–1987

Order-Independent Estimation Variable Description	Mean	Std. Dev.	Simple Correlation with: y	$x\beta$	θ	α	$u\eta$	ψ	ϕ	$s\gamma$	γ
y, Log (Real Annual Compensation, 1980 FF)	4.2575	0.5189	1.0000	0.2614	0.8962	0.8015	0.4011	0.2604	0.1603	0.2729	0.0333
$x\beta$, Predicted Effect of x Variables	0.3523	0.1464	0.2614	1.0000	−0.0445	−0.1243	0.1509	0.0697	0.0824	−0.0279	0.0300
θ, Individual Effect Including Education[a]	3.9052	0.4335	0.8962	−0.0445	1.0000	0.8964	0.4433	0.2965	0.1717	0.3384	0.0387
α, Individual Effect (Unobserved Factors)[a]	0.0000	0.3955	0.8015	−0.1243	0.8964	1.0000	0.0000	0.2640	0.1465	0.3178	0.0372
$u\eta$, Individual Effect of Education	3.9052	0.1776	0.4011	0.1509	0.4433	0.0000	1.0000	0.1349	0.0910	0.1209	0.0122
ψ, Firm Effect (Intercept and Slope)	0.0000	0.4839	0.2604	0.0697	0.2965	0.2640	0.1349	1.0000	0.9259	0.2537	0.0860
ϕ, Firm Effect Intercept	−0.0968	0.4721	0.1603	0.0824	0.1717	0.1465	0.0910	0.9259	1.0000	−0.1305	−0.0718
$s\gamma$, Firm Effect of Seniority	0.0968	0.1844	0.2729	−0.0279	0.3384	0.3178	0.1209	0.2537	−0.1305	1.0000	0.4094
γ, Firm Effect Slope	0.0157	0.0513	0.0333	0.0300	0.0387	0.0372	0.0122	0.0860	−0.0718	0.4094	1.0000

Order-Dependent Estimation: Persons First Variable Description	Mean	Std. Dev.	Simple Correlation with: y	$x\beta$	θ	α	$u\eta$	ψ	ϕ	$s\gamma+T(s)\gamma_2$	γ	γ_2
y, Log (Real Annual Compensation, 1980 FF)	4.2575	0.5189	1.0000	0.3271	0.9310	0.7331	0.4143	0.2131	0.1303	0.0053	0.0293	0.0276
$x\beta$, Predicted Effect of x Variables	0.3899	0.1386	0.3271	1.0000	0.0787	−0.0290	0.2211	0.0325	0.0350	−0.0157	−0.0148	0.0077
θ, Individual Effect Including Education[a]	3.8672	0.4255	0.9310	0.0787	1.0000	0.8842	0.4769	0.1079	0.0889	−0.0223	−0.0190	0.0225
α, Individual Effect (Unobserved Factors)[a]	0.0000	0.3841	0.7331	−0.0290	0.8842	1.0000	0.0000	0.0926	0.0828	−0.0263	−0.0202	0.0202
$u\eta$, Individual Effect of Education	3.8672	0.1831	0.4143	0.2211	0.4769	0.0000	1.0000	0.0473	0.0263	0.0041	−0.0006	0.0081
ψ, Firm Effect (Intercept and Slope)	0.0000	0.0647	0.2131	0.0325	0.1079	0.0926	0.0473	1.0000	0.4428	0.2089	−0.0909	0.0717
ϕ, Firm Effect Intercept	−0.0009	0.1019	0.1303	0.0350	0.0889	0.0828	0.0263	0.4428	1.0000	−0.7844	−0.5625	0.2562
$s\gamma+T(s)\gamma_2$, Firm Effect of Seniority	0.0009	0.0935	0.0053	−0.0157	−0.0223	−0.0263	0.0041	0.2089	−0.7844	1.0000	0.5507	−0.2298
γ, Firm Effect Slope	0.0003	0.0332	−0.0293	−0.0148	−0.0190	−0.0202	−0.0006	−0.0909	−0.5625	0.5507	1.0000	−0.2094
γ_2, Change in Firm Effect Slope	−0.0009	0.0553	0.0276	0.0077	0.0225	0.0202	0.0081	0.0717	0.2562	−0.2298	−0.2094	1.0000

Notes: (a) Correlations have been corrected for the sampling variance of the estimated effect.

TABLE VII

GENERALIZED LEAST SQUARES ESTIMATION BETWEEN INDUSTRY WAGE EFFECTS AND INDUSTRY AVERAGES OF FIRM-SPECIFIC COMPENSATION POLICIES

Independent Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
Based on Order-Independent Estimates						
Industry Average α	1.0390	(0.0023)	1.0053	(0.0022)		
Industry Average ψ	−0.0220	(0.0006)			0.0683	(0.0005)
Intercept	3.3023	(0.0019)	3.3031	(0.0019)	3.0935	(0.0018)
R^2	0.8487		0.8425		0.0682	
Based on Order-Dependent Estimates: Persons First						
Industry Average α	0.8011	(0.0019)	0.8324	(0.0017)		
Industry Average ψ	0.2410	(0.0151)			−0.6659	(0.0150)
Intercept	3.1126	(0.0019)	3.1088	(0.0018)	3.0687	(0.0019)
R^2	0.9580		0.9213		0.2486	

Notes: The dependent variable is the 84 industry-effects estimated by least squares controlling for labor force experience (through quartic), seniority, region, year, education (eight categories) and sex (fully interacted). See Table III for the regression results. The independent variables are the industry averages for the indicated firm-specific compensation policy, adjusted for the same independent variables. The time period is 1976–1987.

TABLE VIII

GENERALIZED LEAST SQUARES ESTIMATES OF THE RELATION BETWEEN FIRM-SIZE WAGE EFFECTS AND FIRM-SIZE CATEGORY AVERAGES OF FIRM-SPECIFIC COMPENSATION POLICIES

Independent Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
Based on Order-Independent Estimates						
Firm-Size Category Average α	1.2222	(0.0043)	1.3245	(0.0041)		
Firm-Size Category Average ψ	0.2233	(0.0026)			0.4278	(0.0025)
Intercept	3.7397	(0.0022)	3.6737	(0.0021)	3.5215	(0.0021)
R^2	0.9604		0.8960		0.2559	
Based on Order-Dependent Estimates: Persons First						
Firm-Size Category Average α	1.1372	(0.0045)	1.3224	(0.0042)		
Firm-Size Category Average ψ	0.9217	(0.0085)			1.7395	(0.0079)
Intercept	3.6370	(0.0022)	3.6674	(0.0021)	3.3665	(0.0019)
R^2	0.9990		0.8950		0.4327	

Notes: The dependent variable is the 25 firm-size category effects estimated by least squares controlling for labor force experience (through quartic), seniority, region, year, education (eight categories) and sex (fully interacted). See Table III for full results. The independent variables are the firm-size category averages for the indicated firm-specific compensation policy, adjusted for the same independent variables. The time period is 1976–1987.

TABLE IX

SUMMARY STATISTICS FOR FIRMS
ANNUAL AVERAGES OVER ALL YEARS FOR WHICH THE FIRM DOES BUSINESS 1978–1988
(weighted by inverse sampling probability)

Variable Definition	Mean	Std Dev
Order-Independent Estimates		
Average Predicted Effect of x Variables ($x\beta$) at the Firm	0.3569	0.2586
Average Individual Effect, Unobserved Factors (α) at the Firm	−0.0575	0.6626
Average Education Effect ($u\eta$) of Employees at the Firm	3.8889	0.2757
ϕ, Firm Effect Intercept	−0.1791	1.0279
γ, Firm Effect Seniority Slope	0.0156	0.1167
Order-Dependent Estimates: Persons First		
Average Predicted Effect of x Variables ($x\beta$) at the Firm	0.3906	0.2420
Average Individual Effect, Unobserved Factors (α) at the Firm	−0.0549	0.6446
Average Education Effect ($u\eta$) of Employees at the Firm	3.8503	0.2836
ϕ, Firm Effect Intercept	−0.0196	0.2707
γ, Firm Effect Seniority Slope	0.0027	0.0775
γ_2, Firm Effect Change in Slope at 10 Years	−0.0031	0.1728
Other Firm Characteristics		
Number of Employees Sampled at Firm	34.2950	610.4800
Employment at December 31st (thousands)	0.1097	1.6789
Real Total Assets (millions FF 1980)	59.4769	3,938.9800
Operating Income/Total Assets	0.1254	0.4544
Value Added/Total Assets	1.0051	1.8889
Real Total Compensation (millions FF 1980)	1.3260	2.3570
Real Value Added/Employee (thou. FF 1980)	106.7672	936.5212
Real Total Assets/Employee (thou. FF 1980)	363.0707	21,067.5500
(Engineers, Professionals and Managers)/Employee	0.2362	0.4072
Skilled Workers/Employee	0.5414	0.5255
Log(Real Total Assets)	1.7711	3.3558
Log(Real Value Added/Employee)	4.5215	1.1050
Log(Real Sales/Employee)	5.5673	2.0139
Log(Total Employment at December 31)	−3.0262	2.1109
Log(Real Total Assets/Employee)	4.7972	2.2710
Age of Firm ($N = 7{,}385$)	19.5023	23.0331
Number of Firms	14,717	

Notes: Order-independent estimates are based upon $x\beta$, α, $u\eta$ estimated with persons first (conditional on Z) and ϕ and γ estimated with firms first (conditional on Z). Order dependent estimates are all based upon persons first ($x\beta$, α, and $u\eta$) and firms (ϕ, γ, and γ_2) second.

ordinary least squares analysis in that much less of the variance is explained (between 0.30 and 0.55). To assess the quality of the different methods in estimating the β coefficients of the time-varying observable personal characteristics, we used Hausman (1978) tests to compare the coefficients obtained from the different methods with those obtained using the consistent method. Once again, all methods that include a person effect—the conditional method with

TABLE X

GENERALIZED LEAST SQUARES ESTIMATES OF THE RELATION BETWEEN PRODUCTIVITY, PROFITABILITY AND COMPENSATION POLICIES

Dependent Variable:	Log (VAdded/Worker)		Log(Sales/Employee)		Operating Inc./Capital	
Independent Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
Based on Order-Independent Estimates						
Average Predicted Effect of x Variables ($x\beta$)	0.4937	(0.0270)	0.3050	(0.0393)	0.0670	(0.0151)
Average Individual Effect (α)	0.2234	(0.0108)	0.0809	(0.0156)	0.0081	(0.0060)
Average Education Effect ($u\eta$)	0.1338	(0.0254)	−0.0057	(0.0369)	−0.0107	(0.0143)
ϕ, Firm Effect Intercept	0.0371	(0.0084)	0.0054	(0.0122)	0.0138	(0.0047)
γ, Firm Effect Seniority Slope	−0.1210	(0.0582)	−0.1751	(0.0847)	−0.0028	(0.0328)
(Engineers, Professionals, Managers)/Employee	0.3428	(0.0238)	0.1773	(0.0346)	−0.1303	(0.0126)
(Skilled Workers)/Employee	0.1226	(0.0177)	0.3065	(0.0257)	0.0061	(0.0099)
Log(Capital/Employee)	0.2470	(0.0037)	0.5536	(0.0054)		
Intercept	3.0206	(0.1055)	0.1065	(0.1533)	0.1897	(0.0579)
Based on Order-Dependent Estimates: Persons First						
Average Predicted Effect of Variables ($x\beta$)	0.6057	(0.0310)	0.4833	(0.0494)	0.0569	(0.0161)
Average Individual Effect (α)	0.2617	(0.0118)	0.1623	(0.0188)	0.0102	(0.0061)
Average Education Effect ($u\eta$)	0.0725	(0.0275)	−0.0674	(0.0437)	−0.0036	(0.0143)
ϕ, Firm Effect Intercept	0.1240	(0.0343)	0.1128	(0.0546)	0.0415	(0.0179)
γ, Firm Effect Seniority Slope	0.1492	(0.1195)	0.2852	(0.1902)	0.0571	(0.0623)
γ_2, Firm Effect Change in Slope at 10 Years	−0.0485	(0.0428)	−0.1107	(0.0681)	−0.0264	(0.0223)
(Engineers, Tech., Managers)/ Employee	0.6815	(0.0247)	0.8989	(0.0394)	−0.1267	(0.0126)
(Skilled Workers)/Employee	0.2167	(0.0190)	0.4979	(0.0302)	0.0094	(0.0099)
Log(Capital/Employee)	0.1017	(0.0025)	0.2290	(0.0039)		
Intercept	4.3985	(0.1126)	2.9784	(0.1791)	0.1664	(0.0586)

Note: Models were estimated using 14,717 firms with complete data. All regressions include a set of 2-digit industry effects.

persons first or order independent, the within-persons method, and the within-persons with limited firm effects method—perform better than methods without person effects.[35] Both ordinary least squares and within-firm estimates yield much higher χ^2 statistics, indicating that our preferred model must include person effects that are not orthogonal to the time varying effects in the model, including the conditioning effects $Z\lambda$.

[35] All the χ^2 statistics in models with person effects are around 3,500. In all cases, the statistic has 28 degrees of freedom. Hence, none of these models pass the test according to classical criteria. The models are also rejected using a simple Bayes-Schwartz criterion. However, given the large number of observations, we are likely to reject any model using these criteria. Hence, we use this test statistic as a measure of proximity of the β estimates to the consistent ones.

TABLE XI

GENERALIZED LEAST SQUARES ESTIMATES OF THE RELATION BETWEEN FACTORS USE AND COMPENSATION POLICIES

	Dependent Variable					
Independent Variable	Log(Employees)	Log(Real Capital)	Log(Capital /Employee)	EPM /Employee	Skilled W /Employee	Unskilled W /Employee
Based on Order-Independent Estimates						
Average Predicted	0.2586	1.0369	0.7783	0.1420	0.0542	−0.1962
Effect of x ($x\beta$)	(0.0675)	(0.0971)	(0.0600)	(0.0110)	(0.0140)	(0.0132)
Average Individual	0.2967	0.7673	0.4705	0.1197	−0.0284	−0.0913
Effect (α)	(0.0267)	(0.0384)	(0.0237)	(0.0043)	(0.0054)	(0.0051)
Average Education	0.4380	0.5479	0.1100	0.2974	−0.1060	−0.1915
Effect ($u\eta$)	(0.0638)	(0.0918)	(0.0567)	(0.0102)	(0.0130)	(0.0123)
ϕ, Firm Effect Intercept	−0.2654	−0.2898	−0.0244	0.0315	0.0152	−0.0468
	(0.0212)	(0.0304)	(0.0188)	(0.0035)	(0.0044)	(0.0042)
γ, Firm Effect Seniority	0.4305	0.4149	−0.0156	0.0909	−0.0147	−0.0762
Slope	(0.1465)	(0.2106)	(0.1300)	(0.0241)	(0.0306)	(0.0290)
(Eng., Prof., Managers)/	−0.0479	2.0645	2.1123			
Employee	(0.0565)	(0.0812)	(0.0501)			
(Skilled Workers)/	−0.2505	0.1075	0.3580			
Employee	(0.0444)	(0.0638)	(0.0394)			
Intercept	−3.6868	2.6123	6.2991	−0.7097	0.8567	0.8530
	(0.2587)	(0.3719)	(0.2296)	(0.0420)	(0.0534)	(0.0506)
Based on Order-Dependent Estimates: Persons First						
Average Predicted	0.2541	1.0205	0.7665	0.1142	0.0628	−0.1770
Effect of x ($x\beta$)	(0.0724)	(0.1036)	(0.0638)	(0.0117)	(0.0150)	(0.0142)
Average Individual	0.2764	0.7454	0.4690	0.1231	−0.0316	−0.0914
Effect (α)	(0.0273)	(0.0391)	(0.0241)	(0.0043)	(0.0055)	(0.0052)
Average Education	0.3478	0.4076	0.0598	0.3307	−0.0964	−0.2343
Effect ($u\eta$)	(0.0643)	(0.0921)	(0.0567)	(0.0101)	(0.0129)	(0.0122)
ϕ, Firm Effect Intercept	0.3748	0.7618	0.3869	0.0057	−0.0052	−0.0005
	(0.0802)	(0.1148)	(0.0707)	(0.0131)	(0.0167)	(0.0158)
γ, Firm Effect Seniority	−0.0262	0.5277	0.5539	0.0835	−0.0303	−0.0532
Slope	(0.2798)	(0.4005)	(0.2467)	(0.0456)	(0.0582)	(0.0553)
γ_2, Firm Effect Change	0.0011	0.0497	0.0486	−0.0314	0.0140	0.0174
in Slope at 10 Years	(0.1002)	(0.1435)	(0.0884)	(0.0164)	(0.0209)	(0.0198)
(Engi., Prof., Managers)/	−0.1181	2.0038	2.1219			
Employee	(0.0568)	(0.0812)	(0.0500)			
(Skilled Workers)/	−0.2947	0.0707	0.3654			
Employee	(0.0445)	(0.0637)	(0.0392)			
Intercept	−3.4129	3.0371	6.4499	−0.8485	0.8309	1.0176
	(0.2630)	(0.3765)	(0.2319)	(0.0423)	(0.0539)	(0.0512)

Notes: The models were estimated using the 14,717 firms with complete data. All equations include a set of 2-digit industry effects. Standard errors in parentheses.

TABLE XII

PROPORTIONAL HAZARDS ESTIMATES OF THE RELATION BETWEEN FIRM SURVIVAL AND COMPENSATION POLICIES

Independent Variable	Parameter Estimate	Standard Error	Risk Ratio
Based on Order-Independent Estimates			
Average Predicted Effect of x ($x\beta$)	2.2163	(0.5821)	9.1730
Average Individual Effect (α)	−0.5874	(0.2100)	0.5560
Average Education Effect ($u\eta$)	−2.3441	(0.5327)	0.0960
ϕ, Firm Effect Intercept	0.3833	(0.1579)	1.4670
γ, Firm Effect Seniority Slope	1.2239	(1.0215)	3.4000
(Eng., Prof., Managers)/Employee	0.2328	(0.3689)	1.2620
(Skilled Workers)/Employee	0.2065	(0.2917)	1.2290
Based on Order-Dependent Estimates: Persons First			
Average Predicted Effect of x ($x\beta$)	2.0751	(0.6241)	7.9650
Average Individual Effect (α)	−0.5327	(0.2064)	0.5870
Average Education Effect ($u\eta$)	−1.8615	(0.5398)	0.1550
ϕ, Firm Effect Intercept	−0.5909	(0.5356)	0.5540
γ, Firm Effect Seniority Slope	1.6497	(2.4598)	5.2050
γ_2, Firm Effect Change in Slope at 10 Years	0.3592	(0.6677)	1.4320
(Eng., Prof., Managers)/Employee	0.4096	(0.3699)	1.5060
(Skilled Workers)/Employee	0.3372	(0.2926)	1.4010

Notes: Negative coefficients indicate a reduced probability of firm death. This model was estimated using the 7,382 firms with known birth dates. The model includes a set of 2-digit industry effects.

We also computed the specification test shown in equation (3.40), which tests the hypothesis that the conditioning variables Z used to compute the column labelled "Conditional Method Persons First" are adequate to represent the covariance between personal characteristics, both measured and unmeasured, and firm effects. The computed statistic is 21,000 with 48 degrees of freedom. Since the conditioning variables have the most effect on the results when firm effects are estimated using the order-independent method, we conclude that this large χ^2 statistic, in conjunction with the component correlation analysis we discuss below, is evidence that the order-independent estimated firm effects are less reliable than the firm effects from the order-dependent "persons first" method. Of course, with the large sample sizes in this analysis, it is also the case that the large value of this statistic can be interpreted as having enough data to reject (unsurprisingly) a low-dimensional simplification of the covariance between X, D, and F. In spite of the data evidence that one should permit all of the effects to be correlated and that one should estimate person effects first in the conditional method, we present all of our results using both the order-independent method and the order dependent "persons first" methods. None of our conclusions are affected by our choice of conditional estimator.

5.2.2. *Male-female wage differentials*

Comparing the results for men and women in Table III, we note that there is less variation in the $\hat{\beta}$ across estimation methods for women than for men.[36] We also note that the gender gap is decreasing over our period of analysis according to the least squares estimates of the time effects with no person or firm effects. However, changes in the composition of the work force must have been an important determinant of this trend because, when person effects are included, the estimates of the time effects are virtually identical for the two sexes. Thus, given the overall difference between men and women in the French labor market, once we control for personal heterogeneity, there is no evidence of declining or increasing male-female wage differentials. As usual, the experience profile for women is flatter than for men, regardless of the method of estimation, even though, for our data source, the measure of labor force experience excludes within-sample periods of nonemployment.

5.3. *Discussion of the Estimated Person and Firm Effects*

Table IV contains descriptive statistics for the components of real compensation implied by the estimated parameters from both of our conditional method specifications, estimated separately for each sex. Table V contains pooled summary statistics and corrected correlations for all of the components of real compensation and for two different conditional estimation methods (order independent and order dependent "persons first"). The table also contains the different estimates of the seniority effects based on the estimation techniques presented in Table III. For both males and females, the standard deviations of the individual-effect, θ, and its components α and $u\eta$, are the same order of magnitude as the firm effects, ψ, for the order-independent method and substantially larger than those of the firm effects for the order-dependent method with persons first. As noted in Table III, the complete parameterization explains about 80% of the variation in real annualized earnings; thus, the person-specific component of variance is clearly important. The firm-specific component of variance is less important but still a major source of variation in the compensation data.

5.3.1. *Specification checks based on correlations among the heterogeneity components*

To further compare the different estimation methodologies, Table V shows the correlations among the components of person and firm heterogeneity as estimated using order-independent, order-dependent (both ways), within persons

[36] This statement is based upon the average variation in the coefficients for men versus those for women from the estimates in columns labelled "Consistent Method Person and Firm Effects," "Conditional Method Persons First," "Within Persons No Firm Effects," "Within Persons Limited Firm Effects," and "Within Firms No Person Effects."

with firm specific intercepts and seniority slopes for the 115 largest firms, and consistent methods. This table is particularly complicated and some care is required to read it properly. The correlation coefficients reported in the table are all computed to be representative of persons; hence, we use the full sample of 5,305,108 observations for all methods except for correlations with the full least squares estimates with limited firm effects, where the number of observations is equal to the 695,077 person-years for which the firm coefficients are available.

The panel labelled "Firm Effects" contains correlations of the components of the firm effects, ϕ and γ, by method of estimation. In the "Firm Effects" panel the order-independent estimates (based on equation (3.16)) are conditional on Z but exclude person effects; hence, they are equivalent to order-dependent "firms first" estimates, conditional on Z. In this same panel the order-dependent estimates are persons first, conditional on Z. The full least squares estimates using the 115 largest firms show the firm effects from the appropriate equation reported in Table III. Finally, in the "Firm Effects" panel, the consistent estimates are based on equation (3.8). Note that consistent estimates of the ϕ component of the firm effect are not available.

In the panel labelled "Person Effects," we report correlation coefficients based upon the order-independent estimates in equation (3.15), which are equivalent to order-dependent estimates with persons first. In this same panel we report person effect estimates from the order-dependent method with firms first.[37] Finally, the person effects from the model labelled "Full Least Squares Estimates Using the 115 Largest Firms," are based on the estimates reported in Table III. Note that consistent estimates of the person effects are not available.

5.3.2. *Specification checks based on firm effects, including heterogeneous seniority slopes*

Consider first the firm-specific intercept, ϕ. The results in Table IV show that, for both sexes, the standard deviation of the estimated firm effects is very large. The data evidence that the complete firm effect, ψ, is heterogeneous is particularly compelling when we combine the results shown in Table IV with the formal specification analyses we showed in Table III. Furthermore, the results in Table V show that order-dependent and order-independent methods give very different results for the firm effect since the correlation between the two estimates of ϕ is only 0.16. For the subsample of persons employed in the largest firms, the full least squares solution for γ appears to be closest to the consistent method (see below). Thus, we assess the quality of the estimates of ϕ obtained by the order-dependent and order-independent methods by comparing them with the full least squares solution for this subsample. Using this criterion, ϕ as esti-

[37] Because, as the reader will see shortly, these estimates perform very poorly, we do not report any other estimates based on the order-dependent method with firms first.

mated by the order-independent method is weakly correlated with ϕ as estimated with limited firm effects. On the other hand, the ϕ estimated by the order-dependent "persons first" method is strongly correlated with the ϕ estimated with limited firm effects (correlation of 0.67). Thus, the evidence based on ϕ favors the order-dependent "persons first" conditional estimation method. For clarity we stress that both conditional methods imply very substantial firm effects. The conclusion from this specification discussion is that the similarity between the full least squares estimates of ϕ (for the 115 largest firms) and the order-dependent "persons first" estimates indicates that the order-independent estimates of ϕ confound the pure firm-specific intercept with the average person effect within the firm.

Considering next the seniority coefficients, γ, all methods in which we allow these returns to vary across firms show that the standard deviation of the estimated seniority slopes is large, at least three times the mean. Our results, therefore, strongly suggest that earnings equations should have a firm effect with at least a firm-specific intercept and seniority slope. The various estimation methods, however, also show considerable variability in γ across techniques. The average seniority coefficient is about 0.01 whenever the estimation method excludes person effects (the order-independent method,[38] ordinary least squares, within firms, within industry and within size class). The average seniority coefficient decreases to near zero when person effects are included (order-dependent persons first, within persons, and 115 largest firms). The consistent method, which includes person effects, gives results closer to the models that exclude person effects—around 0.01 for the average seniority slope.

To continue our discussion of the seniority effects, consider the correlation among our estimates of this component of firm heterogeneity. Because we have consistent estimates of the seniority coefficients, it is useful to examine the correlation of γ from the consistent method with the other estimates. First, the correlation of the consistent γ with the γ estimated in the order-independent method is quite low (0.09). The correlation of the consistent γ with the one estimated by the order-dependent "persons first" method is only slightly larger. However, on the restricted sample of individuals for which the full least squares solution has been implemented, the correlation of the consistent γ with the full least squares γ is quite high (0.51). In fact, the γ estimated using the full least squares solution with the 115 largest firms is well-correlated with all of the methods of recovering γ over the subsample for which this estimate is available. Hence, for the largest firms, the seniority slope coefficients seem to be reasonably estimated by any of our methods. However, for the other, smaller, firms, no estimation method appears to dominate in a clear-cut fashion if one relies only on γ to assess the methodology.

[38] In the order-independent method the firm effects are estimated without first eliminating person effects; thus, they exclude person effects.

5.3.3. *Implications of heterogenous seniority slopes*

As we noted in Section 2, by considering the possibility of differential returns to seniority as a part of the firm effect, we can provide some direct evidence on the debate surrounding the interpretation of the average seniority effect. Using our consistent estimates of the return to seniority, γ, we find that the average return to a year of seniority is just over 0.01 for both sexes. This estimate is lower than Topel's (1991) result but consistent with Brown's (1989) results when he includes person effects. The heterogeneity in our consistent estimates suggests that some of the difference between our results and Topel's may be due to correlation between the heterogenous firm effect and the person effects. The fact that our results are closer to Brown's supports this conclusion because Brown's seniority effect is heterogenous—the magnitude of the return to seniority depends upon characteristics of the job—and he permits correlation between this heterogeneity and his person effect. Brown, on the other hand, does not allow for the possibility of firm-specific intercepts, except as reflected in the job characteristics he used to model the heterogeneity in the return to seniority. Although we cannot use our consistent technique to address this question, we note that, for all the preferred estimates of γ, there is a negative correlation between γ and the associated estimate of ϕ. This negative correlation indicates that the firm-specific intercept and the firm-specific seniority slope are negatively correlated, a result predicted by Becker and Stigler (1974) and Lazear (1979).

5.3.4. *Specification checks based on person effects*

Consider now the correlation between the different estimates of α. An argument similar to the analysis we used for ϕ shows that the α's estimated with persons first are better than those estimated with firms first. In the estimation of α, the order-independent estimates are mathematically identical to the order-dependent "persons first" estimates, conditional on Z. The alternative method is to consider the order-dependent "firms first" estimation of α. We note that the correlation between the order-independent estimates and the full least squares solution for the 115 largest firms is 0.99, while the order-dependent "firms first" estimates are only correlated 0.58 with the order-independent estimates and 0.60 with the full least squares solution for the 115 largest firms. These correlations indicate that the order-dependent: "firms first" estimates of α are not capturing the pure person effect as reliably as either of the other two alternatives shown in the "Person Effects" panel of Table V.

5.3.5. *Implications of the correlations among compensation components*

Table VI shows the intercorrelations of the different components of compensation, first for the order-independent method, then for the order-dependent "persons first" method. Both methods indicate that α, the unobservable part of the individual effect, is the component of compensation that is most highly correlated with log real annual total compensation (0.80 or 0.73 depending on the method).[39] The firm components are much less important in the determination of total compensation (0.21 or 0.26 depending on the method). Using the order-independent estimates, the α component of the person effect and the ϕ component of the firm effect are positively correlated 0.15. The estimated correlation is 0.08 using the order-dependent "persons first" estimates. In either case, the estimated correlation between firm and personal heterogeneity is not large. Also notice that, although the firm-specific intercept, ϕ, and the α-component of the person effect are positively correlated, the firm-specific intercept is negatively correlated with the seniority slope (-0.07 order independent and -0.56 order dependent "persons first"). In both methods, the correlation between observables and compensation appears to be smaller than the correlation between unobservables and compensation. The correlation between compensation and education, $u_i\eta$, is around 0.4 and the correlation between compensation and the time-varying individual characteristics, $x_{it}\beta$, is around 0.3, for both methods shown in the table. Furthermore, $x_{it}\beta$ is only weakly negatively correlated with the unobservable α.[40]

5.3.6. *Summary of the evidence from the estimation results on person and firm heterogeneity*

After reviewing the evidence of the quality of the different estimation methods, the following conclusions can be drawn. First, person effects tend to be more important than firm effects in explaining compensation variability. For the parameters α and β, the estimation methods with persons first are preferred by the data. However, there is no definitive evidence in favor of one estimation method over another for the firm effect ψ. On one hand, the order-dependent "persons first" method tends to give results (on ϕ) that are more highly correlated with the consistent estimates. On the other hand, the order-independent method produces estimates of γ that are less correlated with those obtained by the consistent methodology. Hence, in what follows, we will examine the classical problems of labor economics that were mentioned in the motivation section using person effects that have been estimated first (i.e. person effects from the order-independent and order-dependent "persons first" methods) and firm effects from these same methods, in all cases conditional on Z. This provides us with results that reflect the widest range of possibilities regarding the appropriate estimate of the firm effect.

[39] As noted in the discussion of statistical methods, at the level of the individual the least squares estimate of the person effect is unbiased but inconsistent. Thus, the variance of $\hat{\alpha}_i$ as directly calculated from the summary measures consists of two components var$[\alpha_i]$ + var$[\hat{\alpha}_i - \alpha_i]$, and similarly for θ_i. The variances used to calculate all correlations with α_i and θ_i in Table VI have been corrected by subtracting an estimate of var$[\hat{\alpha}_i - \alpha_i]$, var$[\hat{\theta}_i - \theta_i]$, respectively.

[40] Recall that $\hat{\alpha}_i$ is orthogonal to $u_i\hat{\eta}$ by construction.

5.4. *Inter-Industry Wage Differentials*

In Table VII, we implement the equation (2.7) derived in Section 2, which allows us to decompose the industry effects, estimated as shown in Table III, into the component due to pure firm effects and the component due to person effects. Notice that the two right-hand side components must be adjusted for the observables as in equation (2.7). Table VII uses industry-level averages of the individual and firm-specific components of compensation to explain the industry effect found in our raw individual data (taken from the regression controlling for labor force experience, seniority, region, year, education, and sex reported in the column labelled "Within Industry No Person Effects" in Table III) in the spirit of Dickens and Katz (1987). Since the industry-average person and firm effects, also adjusted for the same set of factors as reported in the Table III regression, almost fully account for the industry effects in a statistical sense ($R^2 = 0.85$ using person and firm effects drawn from the order-independent method and $R^2 = 0.96$ using measures drawn from the order-dependent method with persons first), the interesting question concerns the relative importance of individual heterogeneity (the α-component of the person effect, in particular) and firm heterogeneity (the ψ-component) as components of the industry effects. For both estimation methods for the firm effects, the separate influence of person effects and firm effects in explaining the industry effects is shown. The separate analyses confirm the relative importance of person, as compared to firm, effects. The third through sixth columns of Table VII present separate industry-level regressions using, first, industry-average α alone (columns 3 and 4) and, then, using industry-average firm effects alone (columns 5 and 6). It is clear from the fact that industry-average α alone explains 84% (92% with the order-dependent estimates with persons first) of the inter-industry wage variation, whereas the industry-average ψ component explains only 7% (25% with the order-dependent estimates), that individual effects, as measured statistically by α, are more important than firm-components, as measured by ψ, for explaining French inter-industry wage differentials.[41]

Figures 1 and 2 show graphically the important difference in the strength of the relation between industry effects and industry-average person and firm effects. Figure 1 plots the industry effects from equation (2.7), the dependent variable in Table VII, against the industry-average person effects. The figure also shows the fitted regression line. Figure 2 plots the same industry effects against the industry-average firm effects, again showing the fitted regression line. Both figures are based on the order-independent estimates. The relation between the raw industry effects and the industry-average person effects is clearly much stronger than the one between raw industry effects and the industry-average firm effect. The graphical display for the order-dependent "persons first" estimates shows the same results.

[41] As shown in Table VI, these two components are not highly correlated, so little of the industry-average person effect is "explained by" the industry-average firm effect in a statistical sense.

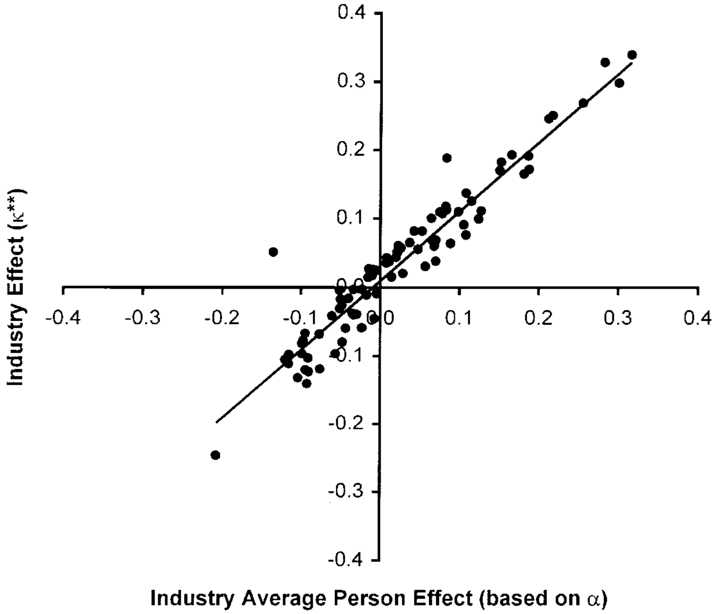

FIGURE 1.—Actual and predicted industry effects using industry-average person effects.

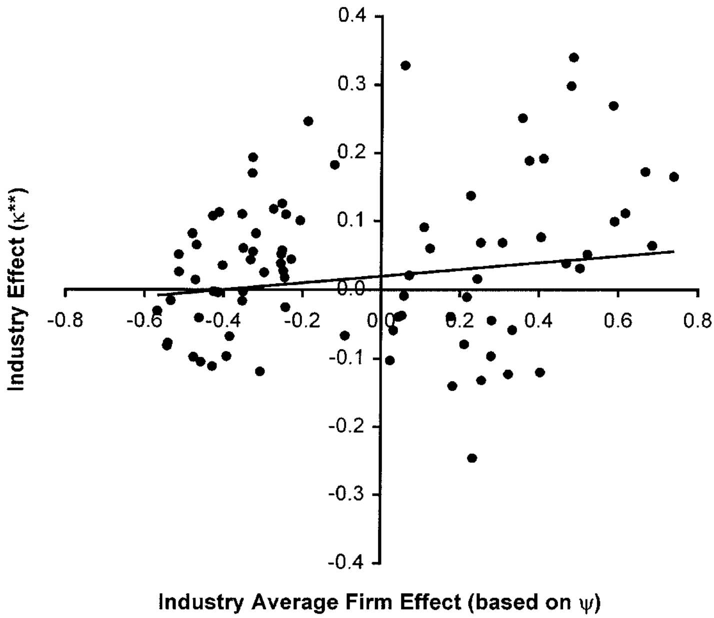

FIGURE 2.—Actual and predicted industry effects using industry-average firm effects.

5.5. *Firm-Size Wage Effects*

Table VIII presents a similar analysis for the firm-size effect based on equation (2.9). To implement this analysis, we constructed 25 firm-size categories. We then estimated the firm-size effects without controlling for person or firm effects, as in equation (2.9). Using calculations exactly parallel to those in Table VII, we constructed the appropriate weighted average person and firm effects within each firm-size category, conditional on the same X variables used in the other analyses. The complete set of X coefficients is shown in Table III in the column labelled "Within Firm Size No Person Effects." Table VIII shows that, for both methods of estimating the person and firm effects, the firm-size-average person effect is much better at explaining the firm-size wage effect than is the firm-size-average firm effect.[42] To more easily compare our results to others, Brown and Medoff (1989) in particular, we graph the raw firm-size wage effects against the log of firm size in Figure 3. The raw firm-size effects in our data strongly resemble the effects summarized by Brown and Medoff. The relation between firm size (log of employment at the firm) and compensation, controlling for the observable characteristics, follows a concave quadratic relation. Figure 3 also plots the average person effect (hollow boxes) within firm-size category. The average person effect can be seen to follow essentially the same quadratic function of log firm size and many of the average person effects are coincident with the solid dots representing the raw firm-size effect. Finally, Figure 3 shows the average firm effect (hollow triangles). The average firm effects do not follow the same concave quadratic function of log firm size as the other two effects. Indeed, the relation between the firm-size average firm effect and log firm size is slightly convex, with the largest positive average firm effect occurring in the largest firm-size category and the largest negative average firm effect occurring in the second largest firm size category. The effects plotted clearly show that average person effects are much more closely related to the firm size effects than average firm effects. The results shown are based on the order-independent estimates but are essentially the same for the order dependent estimates-persons first.

From these two analyses, we conclude that person effects are much more important in explaining inter-industry wage differentials and firm-size wage effects.

5.6. *The Economics of Human Resource Management*

We turn now to the analysis of the impact of the compensation structure on firm outcomes. To conduct this analysis we first computed the firm average of the different components of the compensation package as measured by our order-independent and order-dependent "persons first" methods. Hence, we

[42] As in the analysis of Table VII, the size effects used for the analysis in Table VIII come from the column in Table III labelled "Within Firm Size No Person Effects" and the size-class average person and firm effects have been adjusted for the same effects as found in the Table III regression.

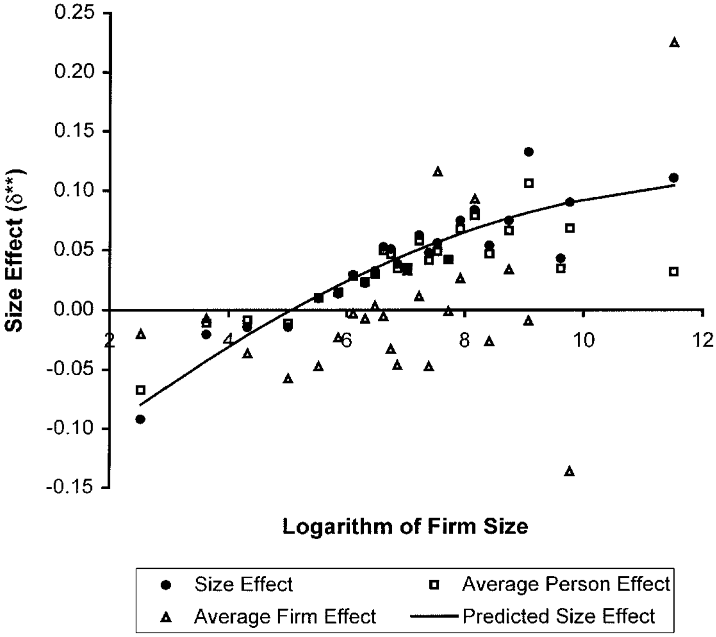

FIGURE 3.—Firm size effects related to firm-size average person and firm effects.

computed the average for each firm j of the part of compensation due to education $(u_i\eta)$, to time-varying observables $(x_{it}\beta)$, and to non-time-varying unobservables (α_i), using all observations (i,t) for which individual i was working in firm j at date t. The detailed formulas for this computation are described in the model section and the variables available for study are described in the data description.

Table IX presents summary statistics for the sample of firms (weighted to be representative of private industrial firms). Table X presents regression models of the logarithm of real value added per employee, real sales per employee (measures of productivity), and operating income as a proportion of total assets (a measure of performance). Results are reported for the order-independent and the order-dependent "persons first" methods. Using the firm-level compensation policy measures generated by our methods, we note that a larger value of the average component of the wage associated with time-varying characteristics $(x\beta)$ is associated with higher value-added and sales per worker and higher profitability for both estimation methods. A larger firm-average individual effect (α) is associated with a substantially larger value-added per employee and sales per employee but not with higher profitability. Once more, these results are consistent across estimation techniques. The part of the individual-effect related to education $(u\eta)$ is associated with higher value-added per worker but is not significant in the other two columns, irrespective of the estimation method. Higher firm-specific wages (ϕ) are associated with higher productivity (value-added per worker and sales per worker, albeit not with the order-independent method for this last variable) and with higher profitability.

The differences between the results based on the order-independent and order-dependent estimation methods, as shown in Table X, are most striking when looking at the impact of the seniority slope coefficient (γ). Using the order-dependent estimates, neither seniority slope is associated with higher (or lower) productivity or profitability. However, using the estimates from the order-independent method, it appears that there exists a negative association with firm productivity—firms that reward seniority the most tend to be the least productive.

The results in Table X can also be used to discuss the relation between firm level compensation policies and measurable outcomes in the context of hiring, rent-splitting, and efficiency wage models. Individuals with high opportunity wages, as captured by α, tend to work in firms with higher productivity per worker, as measured by either value-added per worker or sales per employee. Recall that the α-component of personal heterogeneity has been estimated using compensation as the dependent variable. Thus, it represents the market's valuation of this personal heterogeneity. It is thus not surprising that there is no profitability effect associated with α; however, for the same reason, the presence of an association between the observable characteristic component $(x\beta)$ of compensation and profitability is puzzling, especially since the education component of individual heterogeneity has no measured association with profitability. The firm-specific effect in compensation, as measured by the firm-specific intercept ϕ, is associated with both higher productivity (value-added for either method and sales for the order-dependent measure) and higher profitability. This result can be interpreted as evidence consistent with some efficiency wage or rent-splitting activity in the labor market.

Table XI presents the results for the relations among our compensation measures and a variety of firm-level factor utilization rates. Results are also reported for both conditional estimation techniques. Larger values of the firm-average, time-varying component of compensation, $x\beta$, are associated with higher employment, capital, capital-labor ratio, proportion professional employment, and proportion skilled employment and with lower unskilled employment. The unobservable component of the individual effect, α, is positively associated with employment, capital, the capital-labor ratio, and the proportion of engineers, technical workers, and managers in the work force; and is negatively related to the shares of both skilled and unskilled workers. Larger values of the average education effect (the observable component of the individual effect, $u\eta$) are associated with higher employment, capital, and proportion professionals but with lower values of the proportion skilled. All of these results hold regardless of the estimation method.

The estimation method for the compensation components matters when examining the impact of the firm effects on these outcomes. Based on the order-dependent "persons first" method, the firm-specific intercept, ϕ, is strongly positively associated with employment, capital, and the capital-labor ratio; but is not associated with any components of the skill structure of the work force. A high firm-specific seniority slope is positively associated with the capital-labor ratio and slightly positively associated with the proportion of professional employees. Based on the order-independent method, all of the associations with ϕ that were positive using the order-dependent method are now negative (significantly for employment and capital, marginally for the capital-labor ratio); but the firm-specific seniority slope, γ, plays the role that the firm-specific intercept, ϕ, played with the other estimation method—it is positively associated with employment and capital. Furthermore, managerial and skilled employment are both positively associated with firm-specific effects. It appears that our two estimation techniques both capture similar effects, but their allocation to the fixed part and to the seniority part of firm-specific heterogeneity differ. This is confirmed by a look at Table V in which we see that ϕ from the order-dependent method is highly negatively correlated with γ from the order-independent method and that the ϕ from the order-independent method is somewhat negatively correlated with γ from the order-dependent method.

Finally, Table XII presents a proportional hazards analysis of the relation between the survival of firms and our estimated compensation components at the firm level.[43] Both components of the individual effect, α and $u\eta$, are associated with an increase in survival probability in a statistically significant manner. The effects related to firm-specific compensation factors are large but very imprecise, even though a high ϕ tends to decrease survival when using order-independent estimates. The effect associated with the firm average of observable personal characteristics, $x\beta$, is also associated with a decreased survival probability. The results are interesting when combined with those found in Table X. High ϕ is related to high profitability with both estimation methods, but is linked to lower probabilities of firm survival. On the other hand, high α is related to increased survival probabilities, but has no significant relation to profitability.

6. CONCLUSIONS

In Section 2 we identified six broad areas of labor economics that could be advanced by the study of matched longitudinal employer-employee data:

• the role of individual and firm heterogeneity in the determination of wage rates;

• the sources of inter-industry wage differentials;

• the sources of firm-size wage effects;

• the role of seniority, and heterogeneous returns to seniority in determining wage rates;

• the measurement of internal and external wage rates;

• the study of the economics of human resource management policies.

[43] We estimate the Cox proportional hazards model using as independent variables the non-time-varying measures shown in Table XII. The nonparametric baseline hazard was not estimated.

We believe that our analysis of the French compensation data, linked to the economic performance data of the employing firms has, indeed, shed considerable new light on these questions. To summarize, we found:

- Personal heterogeneity and firm heterogeneity were both important determinants of compensation, although personal heterogeneity appears to be substantially more important in these French data.
- Across 84 industries, the industry-average person effect, adjusted for inter-industry differences in observable characteristics, is much more important than the industry-average firm effect, similarly adjusted, for explaining the inter-industry wage differential.
- Across 25 employment-size categories, the firm-size wage effect in France is increasing at a decreasing rate and this effect is more closely predicted by a similar pattern in the firm-size-average person effect than by the firm-size-average firm effect, which does not mirror the raw firm-size effects at all.
- There is considerable evidence for heterogenous returns to seniority but the method of estimating the return to seniority affected the conclusion regarding the average return to one year of additional seniority. Returns to seniority are negatively correlated with firm-specific intercepts in the compensation relation.
- If we associate the person effect with an individual's external wage rate and the firm effect with that person's internal wage rate, there is very little correlation between these two measures, suggesting that models that focus on explanations for the individual heterogeneity (human capital) and models that focus on explanations for the firm heterogeneity (compensation design, incentives, bargaining) are addressing features of the labor market that do not have large interactions.
- Firms that hire "high-wage workers," those with above average person effects, are observed to have more productive work forces but no higher profitability. "High-wage firms," those that pay above average firm effects, are observed to have both more productive work forces and higher profits.

Of course, our analysis of the separate effects of individual and firm heterogeneity on wage rates and on firm compensation policies has also raised many new questions:

- Do the results for France generalize to other labor markets?
- If person effects are much more important than firm effects in explaining variation in compensation, do these same effects also explain employment mobility?
- If pure firm effects are not very important in the explanation of inter-industry wage differences, then why do other analyses that control for personal heterogeneity but not for firm heterogeneity appear to suggest otherwise?
- Does the observed relation between hiring "high-wage" workers and having higher productivity per worker mean that employer's hiring and selection methods should be studied more closely?
- Is the observed relation between being a "high-wage" firm and being both more profitable and more productive per worker evidence that efficiency wage models play a role in explaining inter-firm differences in compensation policies?

Although we have provided considerable new evidence on these outstanding questions, we believe that our results also provide the statistical basis upon which to begin the process of testing the relevance of agency, efficiency wage, search/matching, rent sharing and endogeneous mobility models as potential explanations for compensation outcome heterogeneity.

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STATISTICAL APPENDIX

In this appendix we state and prove the basic statistical results relating our estimation techniques and our analysis of the aggregation and suppression of effects to the standard least squares analysis of individual and firm effects that have been estimated in other contexts. The model is stated in equation (2.2) and the definitions that follow. We use the same notation in this appendix.

There are a total of J firms indexed by $j = 1, \ldots, J$. The function $\mathbf{J}(i,t)$ gives the identity of the employer for individual i in period t. For each individual i and each year $t = n_{i1}, \ldots, n_{iT_i}$, a row of the matrix F_0 contains an indicator variable for which the jth column contains the value 1 and all other columns contain the value 0, where $j = \mathbf{J}(i,t)$. The matrix F_0 is, thus, $N^* \times J$ and the associated vector of firm effects, ϕ, is $J \times 1$. A row of the matrix F_1 contains, for each individual i and each year $t = n_{i1}, \ldots, n_{iT_i}$, in the jth column the value of the individual's seniority in the firm $j = \mathbf{J}(i,t)$, s_{it}, and 0 in all other columns. A row of the matrix F_2 contains, for each individual i and each year $t = n_{i1}, \ldots, n_{iT_i}$, in the jth column the value of the individual's seniority in the firm $j = \mathbf{J}(i,t)$ less 10 if this value is positive and 0 otherwise, $\mathbf{T}_1(s_{it} - 10)$, and 0 in all other columns. The complete firm effect can thus be represented as

$$F\psi = F_0\phi + F_1\gamma + F_2\gamma_2$$

where $F \equiv [F_0 \;\; F_1 \;\; F_2]$ and $\psi \equiv [\phi' \;\; \gamma' \;\; \gamma_2']'$.

The error vector, ε, is $N^* \times 1$ and has the following properties:

$$\mathrm{E}[\varepsilon | X, D, F] = 0,$$

$$\mathrm{var}[\varepsilon | X, D, F] = \sigma^2 I_{N^*}.$$

Hence, the full regression equation for the model in the main text of the paper is given by

$$y = X\beta + D\theta + F_0\phi + F_1\gamma_1 + F_2\gamma_2 + \varepsilon. \tag{7.1}$$

For completeness we note that the N individuals constitute a simple random sample of the population of persons ever employed (outside the government sector) between the years 1976 and 1987 (except for 1981 and 1983, for which the data were not made available in a computerized sample). In general, the individuals were sampled if their birth dates fell in October of an even year.

Once sampled, an individual's complete private-sector employment history between the years 1976 and 1987 is available, again except for the years 1981 and 1983.

At most $P+N+(3J-1)$ effects in the full model are identified. The least squares estimator of the complete set of effects is given by

$$\begin{bmatrix} \hat{\beta} \\ \hat{\theta} \\ \hat{\psi} \end{bmatrix} = \begin{bmatrix} X'X & X'D & X'F \\ D'X & D'D & D'F \\ F'X & F'D & F'F \end{bmatrix}^{-} \begin{bmatrix} X'y \\ D'y \\ F'y \end{bmatrix} \tag{7.2}$$

where the notation $[\ ]^{-}$ represents any generalized inverse. The standard method of calculating the least squares estimates of the effects is to take deviations from the within-person means of the variables. This operation is accomplished by premultiplying both sides of equation (7.1) by the matrix $M_D \equiv [I - D(D'D)^{-1}\ D']$. The least squares estimator of the identifiable effects can, then, be restated as

$$\begin{bmatrix} \hat{\beta} \\ \hat{\psi} \end{bmatrix} = \begin{bmatrix} X'M_D X & X'M_D F \\ F'M_D X & F'M_D F \end{bmatrix}^{-} \begin{bmatrix} X'M_D y \\ F'M_D y \end{bmatrix}. \tag{7.3}$$

It is because the off-diagonal submatrix $X'M_D F$ is neither null, patterned, nor sparse that we cannot directly compute the solution (7.3). Furthermore, even if we use the consistent estimators $\tilde{\beta}$ and $\tilde{\gamma}$ from equations (3.9) and (3.10) and set $\gamma_2 = 0$, because of the presence of the person effects, the consistent estimator for $\tilde{\phi}$ based upon equation (7.3) is

$$\tilde{\phi} = (F_0' M_D F_0)^{-} F_0' M_D (y - X\tilde{\beta} - F_1 \tilde{\gamma}),$$

which still requires the solution of a system of J equations that is neither diagonal, patterned, nor sparse.

Computation of the Conditional Estimates

The calculation of the solution to equations (3.17) and (3.18) for the order-independent and order-dependent (persons first) methods do not present any problems. The calculation of equation (3.21) for the order-independent and order-dependent "firms first" methods is, however, more complicated. For the order dependent method with firms first we leave $F_2\gamma_2$ in the model; however, we do not attempt the order independent calculations with $F_2\gamma_2$ in the model.

To calculate $\hat{\pi}$, we reorganize the columns of F so that the columns of F_0 and F_1 from the same firm are adjacent. Next, we sort the matrix F so that the observations are grouped by firm from $j = 1, \ldots, J$. Denote the reorganized F matrix by F^* and denote the conformably reorganized y and Z matrices by y^* and Z^*, respectively. Note that the cross-product matrix $F^{*\prime}F^*$ is block diagonal with J blocks, each one 2×2, and a typical block is given by

$$\begin{bmatrix} N_j & \sum\limits_{\mathbf{J}(i,t)=j} s_{it} \\ \sum\limits_{\mathbf{J}(i,t)=j} s_{it} & \sum\limits_{\mathbf{J}(i,t)=j} s_{it}^2 \end{bmatrix}$$

where

$$N_j \equiv \sum_{\forall(i,t)} 1[\mathbf{J}(i,t) = j],$$

the notation $\Sigma_{\mathbf{J}(i,t)=j}$ means to sum over all (i,t) such that $\mathbf{J}(i,t) = j$, and the function $1[A] = 1$ if A is true and 0, otherwise. Similarly, the cross-product matrix $F^{*\prime}Z^*$, which is $2J \times Q$, has the structure

$$\begin{bmatrix} \sum_{\mathbf{J}(i,t)=j} z_{(i,t)1} & \cdots & \sum_{\mathbf{J}(i,t)=j} z_{(i,j)Q} \\ \sum_{\mathbf{J}(i,t)=j} s_{it} z_{(i,t)1} & \cdots & \sum_{\mathbf{J}(i,t)=j} s_{it} z_{(i,t)Q} \\ \cdots & \cdots & \cdots \\ \sum_{\mathbf{J}(i,t)=j} z_{(i,t)1} & \cdots & \sum_{\mathbf{J}(i,t)=j} z_{(i,t)Q} \\ \sum_{\mathbf{J}(i,t)=j} s_{it} z_{(i,t)1} & \cdots & \sum_{\mathbf{J}(i,t)=j} s_{it} z_{(i,t)Q} \end{bmatrix}.$$

The product $(F^{*\prime}F^*)^{-1}F^{*\prime}Z^*$ is, therefore, a $2J \times Q$ matrix of firm-specific regression coefficients. A similar argument can be made for the coefficients associated with the regression of y^* on Z^*. Hence, the adjustment of Z^* with respect to F^* can be accomplished performing firm by firm regression of the appropriate rows of each column of Z^* on the appropriate columns of F^* and retaining the residuals to cumulate in the cross-product matrices $(Z^{*\prime}M_{F^*}Z^*)$ and $(Z^{*\prime}M_{F^*}y^*)$. Thus

$$\hat{\pi} = (Z^{*\prime}M_{F^*}Z^*)^{-1}(Z^{*\prime}M_{F^*}y^*),$$

where we note that it is not necessary to adjust y^* with respect to F^* as long as each column of Z^* has been adjusted (i.e. the matrix M_{F^*} is idempotent).

A computationally identical approach to the estimation of π may be obtained by directly solving the least squares equations associated with the solution of (3.20). To begin, notice that the least squares solution to this equation has the property

$$\sum_{\mathbf{J}(i,t)=j} [y_{it} - \phi_j - \gamma_j s_{it} - z_{it}\pi] = 0$$

for $j = 1, \ldots, J$, where the variable z_{it} is a row of the matrix Z. These J conditions imply

$$\hat{\phi}_j = \bar{y}_j - \hat{\gamma}_j \bar{s}_j - \bar{z}_j \hat{\pi} \tag{7.4}$$

where the notation

$$\bar{a}_j \equiv \frac{\sum_{\mathbf{J}(i,t)=j} a_{it}}{\sum_{\forall(i,t)} 1[\mathbf{J}(i,t)=j]}.$$

Next, consider the J orthogonality conditions associated with the variable s_{it}, which imply

$$\sum_{\mathbf{J}(i,t)=j} \left(y_{it}s_{it} - \hat{\phi}_j s_{it} - \hat{\gamma}_j s_{it}^2 - z_{it}s_{it}\hat{\pi} \right) = 0$$

for $j = 1, \ldots, J$. Hence,

$$\hat{\gamma}_j = \frac{\sum_{\mathbf{J}(i,t)=j}[(y_{it} - \bar{y}_j)s_{it} - (z_{it} - \bar{z}_j)s_{it}\hat{\pi}]}{\sum_{\mathbf{J}(i,t)=j}(s_{it} - \bar{s}_j)s_{it}}. \tag{7.5}$$

Substituting equations (7.4) and (7.5) into (3.19) yields

$$\begin{aligned} &y_{it} - \bar{y}_j - \frac{[\sum_{\mathbf{J}(i,t)=j}(y_{it} - \bar{y}_j)](s_{it} - \bar{s}_j)1(\mathbf{J}(i,t)=j)}{\sum_{\mathbf{J}(i,t)=j}(s_{it} - \bar{s}_j)s_{it}} \\ &\quad = \left(z_{it} - \bar{z}_j - \frac{[\sum_{\mathbf{J}(i,t)=j}(z_{it} - \bar{z}_j)s_{it}]((s_{it} - \bar{s}_j)1(\mathbf{J}(i,t)=j))}{\sum_{\mathbf{J}(i,t)=j}(s_{it} - \bar{s}_j)s_{it}} \right)\pi. \end{aligned} \tag{7.6}$$

Thus, the least squares estimator of the $Q \times 1$ vector π is given by

$$\hat{\pi} = (\tilde{Z}'\tilde{Z})^{-1}\tilde{Z}'\tilde{y}$$

where the $1 \times Q$ vector

$$\tilde{z}_{it} \equiv z_{it} - \bar{z}_j - \frac{[\Sigma_{\mathbf{J}(i,t)=j}(z_{it} - \bar{z}_j)s_{it}]((s_{it} - \bar{s}_j)1(\mathbf{J}(i,t) = j))}{\Sigma_{\mathbf{J}(i,t)=j}(s_{it} - \bar{s}_j)s_{it}}$$

and the scalar

$$\tilde{y}_{it} \equiv y_{it} - \bar{y}_j - \frac{[\Sigma_{\mathbf{J}(i,t)=j}(y_{it} - \bar{y}_j)s_{it}](s_{it} - \bar{s}_j)1(\mathbf{J}(i,t) = j)}{\Sigma_{\mathbf{J}(i,t)=j}(s_{it} - \bar{s}_j)s_{it}}.$$

Finally, using either computational formula for the estimator $\hat{\pi}$, we have

$$\begin{bmatrix} \hat{\phi}_1 \\ \hat{\gamma}_1 \\ \cdots \\ \hat{\phi}_J \\ \hat{\gamma}_J \end{bmatrix} = (F^{*\prime}F^*)^{-1}F^{*\prime}(y^* - Z^*\hat{\pi}), \tag{7.7}$$

which, once again, can be computed firm-by-firm using the appropriate columns of F^* and the appropriate rows of $(y^* - Z^*\hat{\pi})$, so that

$$\begin{bmatrix} \hat{\phi}_j \\ \hat{\gamma}_j \end{bmatrix} = \begin{bmatrix} N_j & \sum\limits_{\mathbf{J}(i,t)=j} s_{(i,t)} \\ \sum\limits_{\mathbf{J}(i,t)=j} s_{(i,t)} & \sum\limits_{\mathbf{J}(i,t)=j} s_{(i,t)}^2 \end{bmatrix}^{-1} \begin{bmatrix} \sum\limits_{\mathbf{J}(i,t)=j} \left(y_{(i,t)}^* - \sum\limits_{q=1}^{Q} z_{(i,t)q}^* \hat{\pi}_q \right) \\ \sum\limits_{\mathbf{J}(i,t)=j} \left(y_{(i,t)}^* - \sum\limits_{q=1}^{Q} z_{(i,t)q}^* \hat{\pi}_q \right) s_{(i,t)} \end{bmatrix}.$$

Least Squares and Our Conditional Methods

Consider next the relation between our conditional method estimators and the conventional least squares estimator. Because this appendix contains the proofs of the claims in the paper, we use the full model in equation (7.1). The conditional method matrix Z can be expressed as

$$\left[\begin{array}{c|c|c} \bar{x}_1 f_{\mathbf{J}(1,n_{11})} & \bar{x}_1 s_{1n_{11}} f_{\mathbf{J}(1,n_{11})} & \bar{x}_1 \mathbf{T}_1(s_{1n_{11}} - 10) f_{\mathbf{J}(1,n_{11})} \\ \cdots & \cdots & \cdots \\ \bar{x}_1 f_{\mathbf{J}(1,n_{1T_1})} & \bar{x}_1 s_{1n_{1T_1}} f_{\mathbf{J}(1,n_{1T_1})} & \bar{x}_1 \mathbf{T}_1(s_{1n_{1T_1}} - 10) f_{\mathbf{J}(1,n_{1T_1})} \\ \cdots & \cdots & \cdots \\ \bar{x}_N f_{\mathbf{J}(N,n_{N1})} & \bar{x}_N s_{Nn_{N1}} f_{\mathbf{J}(1,n_{N1})} & \bar{x}_N \mathbf{T}_1(s_{Nn_{N1}} - 10) f_{\mathbf{J}(1,n_{N1})} \\ \cdots & \cdots & \cdots \\ \bar{x}_N f_{\mathbf{J}(N,n_{NT_N})} & \bar{x}_N s_{Nn_{NT_N}} f_{\mathbf{J}(1,n_{NT_N})} & \bar{x}_N \mathbf{T}_1(s_{Nn_{NT_N}} - 10) f_{\mathbf{J}(1,n_{NT_N})} \end{array}\right]$$

where $\bar{x}_i$ are the rows of $(D'D)^{-1}D'XC$, C is a $P \times (Q/3)$ matrix that selects $Q/3$ columns of X to place in the Z matrix, $\mathbf{T}_1(z)$ is the first order spline basis function defined in the text of the article, and all other variables are defined above. Hence, Z is $N^* \times Q$.

We express the projection of $F\psi$ on Z as

$$F_0\phi + F_1\gamma_1 + F_2\gamma_2 = Z\lambda + \nu$$

where the vector λ is $Q \times 1$ and the error process ν is defined as the component of the firm effect that is orthogonal to $Z\lambda$. The statistical equation substituting the projection of the firm effects for the actual firm effects is given by

$$y = X\beta + D\theta + Z\gamma + \varepsilon + \nu$$

with an error process $\varepsilon + \nu$ with the following properties:

$$\mathrm{E}[\varepsilon + \nu | X, D, Z] = 0,$$

$$\mathrm{var}[\varepsilon + \nu | X, D, Z] = \sigma_1^2 I_{N*},$$

where σ_1^2 is the variance of $\varepsilon_{it} + \nu_{it}$ for all (i, t) that are part of N^*. As a direct consequence of this statistical model we are assuming the following orthogonality condition:

$$\text{(7.8)} \qquad \begin{bmatrix} X'(F_0\phi + F_1\gamma_1 + F_2\gamma_2 - Z\lambda) \\ D'(F_0\phi + F_1\gamma_1 + F_2\gamma_2 - Z\lambda) \end{bmatrix} = 0,$$

which means that the columns of Z should be chosen to maximize the correlation of X and D with $F_0\phi + F_1\gamma_1 + F_2\gamma_2$. We calculate the within-person least squares estimator for β and λ using the formula

$$\begin{bmatrix} \hat{\beta} \\ \hat{\lambda} \end{bmatrix} = \begin{bmatrix} X'M_D X & X'M_D Z \\ Z'M_D X & Z'M_D Z \end{bmatrix}^{-} \begin{bmatrix} X'M_D y \\ Z'M_D y \end{bmatrix}.$$

The proof of the consistency of this estimator follows directly from the condition (7.8) so that the asymptotic distribution of $[\,\hat{\beta}' \;\; \hat{\lambda}'\,]'$ is given by the usual least squares formulas.

Aggregation of Effects

We consider next the consequences of various aggregations and substitutions on the least squares estimators of the various effects in the model 2.2. The algebra for all of the aggregations considered in Section 2 is identical so we will discuss only the generic case in this appendix. An aggregation of the firm effect can be defined as an orthogonal decomposition of the firm effect into a part related to the aggregation and a part that represents the residual from this aggregation. We consider the industry aggregation given by the matrix A and the parameters κ, defined in Section 2.

The model (2.2) can be restated as

$$\text{(7.9)} \qquad y = X\beta + D\theta + FA\kappa + (I_{N*} - FA(A'F'FA)^{-}A'F')F\psi + \varepsilon.$$

If the firm effects are omitted from the model, then the statistical error becomes

$$\text{(7.10)} \qquad \zeta \equiv (I_{N*} - FA(A'F'FA)^{-}A'F')F\psi + \varepsilon.$$

By construction, the design matrices FA and $(I_{N*} - FA(A'F'FA)^{-}A'F')F$ are orthogonal. However, neither design matrix is orthogonal to X or D. Thus, the least squares estimates of the pure class effects, κ, suffer from an excluded variable bias when they are estimated in the absence of firm effects. Specifically, the within-person least squares estimator of the effects β^* and κ^* from equation (7.9) with the error term defined by equation (7.10) is

$$\begin{bmatrix} \hat{\beta}^* \\ \hat{\kappa}^* \end{bmatrix} = \begin{bmatrix} X'M_D X & X'M_D FA \\ A'F'M_D X & A'F'M_D FA \end{bmatrix}^{-} \begin{bmatrix} X'M_D X\beta + X'M_D FA\kappa + X'M_D\zeta \\ A'F'M_D X\beta + A'F'M_D FA\kappa + A'F'M_D\zeta \end{bmatrix}.$$

By direct calculation of the partitioned G-inverse we have

$$\operatorname*{plim}_{N\to\infty} \hat{\kappa}^* = \kappa - Q^{-}(A'F'M_D X(X'M_D X)^{-}X'M_D)(I_{N*} - FA(A'F'FA)^{-}A'F')F\psi$$

where

$$Q \equiv (A'F'M_D FA - A'F'M_D X(X'M_D X)^{-} X'M_D FA).$$

By inspection we note that the source of the inconsistency in the within-persons least squares estimator of the class effects κ is the covariance between the observed characteristics, X, and the part of the firm effects that is not correlated with the industry effects, $(I_{N*} - FA(A'F'FA)^{-}A'F')F$, conditional on the person effects D.

For completeness we note that if the pure class effect, say κ^{***}, is defined to be representative of firms, and not of individuals, then

$$\kappa^{***} \equiv (A'A)^{-} A'\psi.$$

Using this definition of the pure class effect, there will be an additional term in the probability limit of $\hat{\kappa}^{***}$ that gives the aggregation bias associated with estimating this pure class effect using the firm design matrix F and a sampling plan that is representative of persons. To our knowledge, none of the articles cited in this paper that estimate industry or size effects from samples that are representative of the population of employed individuals use a definition of a class effect that is representative of the population of firms.

Firm Effects That Depend on Firm-level Data

Suppose next that the firm effect, ψ, depends upon a non-time-varying characteristic of the firm over the sample period. Let the $J \times 1$ vector f contain the characteristic of firm j, less the grand mean, in each row. The grand mean should be calculated over the population of employed individuals so that the average firm effect in the population of persons remains zero. Because the parameters of our firm effects are constant over time, we cannot nest a model of time-varying firm characteristics in equation (2.2). The pure firm effects can be decomposed into the part that is linearly related to f and a residual from this linear relation:

$$\psi = f\delta + v$$

where δ is a scalar parameter relating the firm's characteristic to its firm effect and the $J \times 1$ vector v gives the residual from this projection. By an argument completely analogous to the one we used for pure classification effects, it can be shown that the within-person least squares estimator of δ is also inconsistent because X and v are not orthogonal, conditional on D. Specifically,

$$\operatorname*{plim}_{N \to \infty} \hat{\delta} = \delta + \frac{1}{q} f'FM_D(I_{N*} - X(X'M_D X)^{-} X')M_D F(\psi - f\delta)$$

where $q = f'F'M_D Ff - f'F'M_D X(X'M_D X)^{-} X'M_D Ff$.

DATA APPENDIX

This Appendix contains details of the definitions of variables, missing data imputation, and statistical calculations not reported in the text.

Education and School-Leaving Age

Our initial DAS file did not contain education information. We used supplementary information available for a strict subsample of the DAS (called the EDP, Echantillon Démographique Permanent) to impute the level of education of all other individuals in the DAS as described in Section 4. The education responses were grouped into 8 degree-level categories as shown in Data Appendix Table B1. EDP sample statistics for the men are in Data Appendix Table B2, and those for the women are in Data Appendix Table B3. The estimated logit equations are in Data Appendix Table B4 for men and Data Appendix Table B5 for women.

DATA APPENDIX TABLE B1

CLASSIFICATION OF FRENCH DEGREES AND U.S. EQUIVALENTS

Category	Degree	U.S. Equivalent
1	Sans Aucun Diplôme	No Terminal Degree
2	CEP	Elementary School
	DFEO	
3	BEPC	Junior High School
	BE	
	BEPS	
4	BAC (not F, G or H)	High School
	Brevet supérieur	
	CFES	
5	CAP	Vocational-Technical School (Basic)
	BEP	
	EFAA	
	BAA	
	BPA	
	FPA 1er	
6	BP	Vocational-Technical School (Advanced)
	BEA	
	BEC	
	BEH	
	BEI	
	BES	
	BATA	
	BAC F	
	BAC G	
	BAC H	
7	Santé	Technical College and
	BTS	Undergraduate University
	DUT	
	DEST	
	DEUL	
	DEUS	
	DEUG	
8	2ème cycle	Graduate School and Other
	3ème cycle	Post-Secondary Education
	Grande école	
	CAPES	
	CAPET	

Notes: Authors' adaptation of French degree codes appearing on the EDP (Echantillon démographique permanent).

DATA APPENDIX TABLE B2

EDP SAMPLE STATISTICS—MEN
(Std. Deviations in Parentheses)

Variable Name	Overall	Degree Category 1	2	3	4	5	6	7	8
$DOB_i < 1925$	0.188	0.254	0.295	0.160	0.136	0.055	0.098	0.063	0.186
	(0.391)	(0.435)	(0.456)	(0.367)	(0.343)	(0.228)	(0.297)	(0.243)	(0.389)
$1924 < DOB_i < 1930$	0.056	0.062	0.085	0.042	0.049	0.034	0.048	0.026	0.065
	(0.230)	(0.242)	(0.279)	(0.200)	(0.215)	(0.180)	(0.214)	(0.158)	(0.247)
$1929 < DOB_i < 1935$	0.097	0.109	0.120	0.067	0.068	0.081	0.095	0.054	0.101
	(0.296)	(0.311)	(0.325)	(0.250)	(0.252)	(0.273)	(0.293)	(0.226)	(0.301)
$1934 < DOB_i < 1940$	0.061	0.056	0.070	0.048	0.048	0.063	0.079	0.047	0.078
	(0.240)	(0.229)	(0.255)	(0.214)	(0.215)	(0.244)	(0.270)	(0.212)	(0.268)
$1939 < DOB_i < 1945$	0.094	0.070	0.091	0.075	0.098	0.117	0.133	0.118	0.149
	(0.292)	(0.256)	(0.287)	(0.264)	(0.298)	(0.322)	(0.340)	(0.323)	(0.356)
$1944 < DOB_i < 1950$	0.102	0.064	0.097	0.099	0.130	0.130	0.152	0.175	0.164
	(0.302)	(0.244)	(0.296)	(0.299)	(0.336)	(0.336)	(0.359)	(0.380)	(0.370)
$1949 < DOB_i < 1955$	0.159	0.095	0.132	0.166	0.245	0.224	0.217	0.288	0.201
	(0.365)	(0.293)	(0.339)	(0.372)	(0.430)	(0.417)	(0.412)	(0.453)	(0.401)
$1954 < DOB_i < 1960$	0.101	0.072	0.060	0.182	0.157	0.145	0.110	0.176	0.054
	(0.302)	(0.259)	(0.238)	(0.386)	(0.364)	(0.352)	(0.313)	(0.381)	(0.226)
$1959 < DOB_i < 1977$	0.141	0.218	0.050	0.160	0.069	0.151	0.068	0.052	0.003
	(0.348)	(0.413)	(0.218)	(0.367)	(0.253)	(0.358)	(0.251)	(0.224)	(0.056)
Works in	0.232	0.204	0.226	0.288	0.352	0.187	0.284	0.309	0.457
Ile de France	(0.422)	(0.403)	(0.418)	(0.453)	(0.478)	(0.390)	(0.451)	(0.462)	(0.498)
CSP62	0.263	0.357	0.282	0.188	0.157	0.199	0.145	0.184	0.105
	(0.440)	(0.479)	(0.450)	(0.391)	(0.364)	(0.399)	(0.352)	(0.387)	(0.307)
CSP61	0.225	0.231	0.255	0.117	0.071	0.299	0.186	0.096	0.058
	(0.418)	(0.422)	(0.436)	(0.321)	(0.266)	(0.458)	(0.390)	(0.295)	(0.233)
CSP50	0.151	0.118	0.166	0.279	0.279	0.108	0.203	0.235	0.203
	(0.358)	(0.322)	(0.372)	(0.448)	(0.448)	(0.310)	(0.402)	(0.424)	(0.402)
CSP40	0.112	0.061	0.110	0.173	0.233	0.080	0.258	0.275	0.225
	(0.315)	(0.240)	(0.314)	(0.379)	(0.423)	(0.272)	(0.438)	(0.447)	(0.418)
CSP30	0.043	0.020	0.025	0.053	0.147	0.015	0.057	0.080	0.359
	(0.203)	(0.142)	(0.157)	(0.224)	(0.354)	(0.121)	(0.232)	(0.271)	(0.480)
Number of Observations	71229	26236	12825	3847	3036	16489	3878	2387	2531

DATA APPENDIX TABLE B3

EDP SAMPLE STATISTICS—WOMEN
(Std. Deviations in Parentheses)

Variable Name	Overall	Degree Category 1	2	3	4	5	6	7	8
$DOB_i < 1925$	0.152	0.235	0.206	0.129	0.055	0.034	0.042	0.055	0.056
	(0.359)	(0.424)	(0.405)	(0.336)	(0.229)	(0.181)	(0.202)	(0.228)	(0.230)
$1924 < DOB_i < 1930$	0.047	0.053	0.078	0.045	0.025	0.024	0.017	0.022	0.023
	(0.212)	(0.224)	(0.268)	(0.206)	(0.156)	(0.153)	(0.130)	(0.146)	(0.148)
$1929 < DOB_i < 1935$	0.084	0.096	0.118	0.070	0.043	0.061	0.054	0.049	0.052
	(0.278)	(0.294)	(0.322)	(0.255)	(0.203)	(0.239)	(0.226)	(0.216)	(0.222)
$1934 < DOB_i < 1940$	0.054	0.056	0.069	0.047	0.036	0.050	0.045	0.038	0.047
	(0.226)	(0.229)	(0.254)	(0.211)	(0.185)	(0.218)	(0.208)	(0.190)	(0.212)
$1939 < DOB_i < 1945$	0.093	0.070	0.113	0.086	0.090	0.103	0.108	0.101	0.127
	(0.290)	(0.255)	(0.317)	(0.281)	(0.287)	(0.304)	(0.311)	(0.301)	(0.334)
$1944 < DOB_i < 1950$	0.114	0.077	0.125	0.109	0.116	0.135	0.164	0.156	0.209
	(0.317)	(0.267)	(0.331)	(0.311)	(0.321)	(0.341)	(0.371)	(0.363)	(0.407)
$1949 < DOB_i < 1955$	0.186	0.112	0.180	0.167	0.285	0.247	0.252	0.298	0.354
	(0.389)	(0.315)	(0.384)	(0.373)	(0.451)	(0.431)	(0.434)	(0.457)	(0.478)
$1954 < DOB_i < 1960$	0.120	0.078	0.067	0.178	0.217	0.166	0.169	0.223	0.125
	(0.325)	(0.267)	(0.251)	(0.383)	(0.412)	(0.372)	(0.375)	(0.416)	(0.331)
$1959 < DOB_i < 1977$	0.150	0.224	0.043	0.170	0.133	0.180	0.147	0.059	0.008
	(0.357)	(0.417)	(0.202)	(0.375)	(0.339)	(0.384)	(0.355)	(0.236)	(0.088)
Works in	0.254	0.237	0.239	0.286	0.333	0.221	0.316	0.283	0.466
Ile de France	(0.435)	(0.425)	(0.426)	(0.452)	(0.471)	(0.415)	(0.465)	(0.451)	(0.499)
CSP62	0.227	0.343	0.296	0.108	0.079	0.126	0.073	0.061	0.053
	(0.419)	(0.475)	(0.456)	(0.310)	(0.270)	(0.331)	(0.259)	(0.240)	(0.224)
CSP61	0.050	0.061	0.067	0.027	0.023	0.044	0.027	0.029	0.015
	(0.218)	(0.239)	(0.249)	(0.163)	(0.150)	(0.205)	(0.161)	(0.168)	(0.120)
CSP50	0.458	0.365	0.427	0.596	0.570	0.539	0.630	0.420	0.511
	(0.498)	(0.482)	(0.495)	(0.491)	(0.495)	(0.498)	(0.483)	(0.494)	(0.500)
CSP40	0.073	0.040	0.035	0.090	0.165	0.045	0.097	0.350	0.214
	(0.261)	(0.195)	(0.185)	(0.286)	(0.371)	(0.208)	(0.296)	(0.477)	(0.410)
CSP30	0.013	0.008	0.005	0.016	0.048	0.005	0.009	0.032	0.150
	(0.115)	(0.090)	(0.068)	(0.125)	(0.214)	(0.071)	(0.093)	(0.176)	(0.357)
Number of Observations	57677	19822	12768	4760	3112	10388	2633	3173	1021

DATA APPENDIX TABLE B4

MULTINOMIAL LOGIT ON DEGREE CATEGORIES–MEN
(Std. Errors in Parentheses)

Variable Name	1	2	3	4	5	6	7
Intercept	6.254	5.828	2.465	0.803	3.985	1.714	−0.141
	(0.122)	(0.125)	(0.134)	(0.142)	(0.125)	(0.139)	(0.158)
$1924 < DOB_i < 1930$	−0.496	−0.320	−0.333	0.005	0.392	0.266	0.102
	(0.105)	(0.106)	(0.131)	(0.133)	(0.113)	(0.132)	(0.179)
$1929 < DOB_i < 1935$	−0.493	−0.518	−0.344	−0.109	0.734	0.471	0.407
	(0.090)	(0.091)	(0.112)	(0.117)	(0.096)	(0.111)	(0.145)
$1934 < DOB_i < 1940$	−1.234	−1.117	−0.667	−0.325	0.446	0.318	0.349
	(0.100)	(0.102)	(0.124)	(0.130)	(0.105)	(0.119)	(0.154)
$1939 < DOB_i < 1945$	−2.031	−1.863	−1.120	−0.381	0.090	0.000	0.519
	(0.085)	(0.087)	(0.105)	(0.106)	(0.089)	(0.102)	(0.126)
$1944 < DOB_i < 1950$	−2.818	−2.430	−1.307	−0.379	−0.336	−0.216	0.653
	(0.085)	(0.087)	(0.102)	(0.104)	(0.089)	(0.102)	(0.123)
$1949 < DOB_i < 1955$	−3.388	−3.248	−1.373	−0.069	0.700	−0.363	0.843
	(0.086)	(0.089)	(0.100)	(0.101)	(0.090)	(0.103)	(0.121)
$1954 < DOB_i < 1960$	−2.289	−2.649	0.074	0.830	0.230	0.312	1.704
	(0.113)	(0.119)	(0.123)	(0.127)	(0.116)	(0.130)	(0.145)
$1959 < DOB_i < 1977$	1.897	0.246	2.891	2.855	3.319	2.742	3.339
	(0.360)	(0.363)	(0.364)	(0.369)	(0.362)	(0.368)	(0.379)
Unskilled Blue-Collar at Date t in Firm $\mathbf{J}(t,t)$	−0.850	−1.311	−0.681	−0.193	−1.306	−0.849	−0.155
	(0.116)	(0.119)	(0.126)	(0.134)	(0.116)	(0.129)	(0.136)
Skilled Blue-Collar at Date t in Firm $\mathbf{J}(i,t)$	−0.904	−1.074	−0.557	−0.294	−0.340	−0.006	−0.055
	(0.132)	(0.135)	(0.144)	(0.156)	(0.131)	(0.142)	(0.157)
Unskilled White-Collar at Date t in Firm $\mathbf{J}(i,t)$	−2.758	−2.635	−0.944	−0.217	−2.494	−1.100	−0.437
	(0.111)	(0.114)	(0.118)	(0.125)	(0.110)	(0.121)	(0.129)
Skilled White-Collar at Date t in Firm $\mathbf{J}(i,t)$	−4.028	−3.740	−1.610	−0.377	−3.011	−1.030	−0.100
	(0.117)	(0.121)	(0.127)	(0.132)	(0.117)	(0.126)	(0.134)
Manager at Date t in Firm $\mathbf{J}(i,t)$	−5.892	−5.996	−3.400	−1.311	−5.195	−3.036	−1.648
	(0.124)	(0.132)	(0.142)	(0.136)	(0.131)	(0.141)	(0.148)
Works in Ile de France	−0.627	−0.629	−0.410	−0.265	−0.766	−0.510	−0.399
	(0.048)	(0.050)	(0.057)	(0.057)	(0.049)	(0.056)	(0.062)

DATA APPENDIX TABLE B5

MULTINOMIAL LOGIT ON DEGREE CATEGORIES−WOMEN
(Std. Errors in Parentheses)

Variable Name	1	2	3	4	5	6	7
Intercept	7.296	7.148	4.645	2.263	4.555	2.693	2.278
	(0.205)	(0.206)	(0.211)	(0.223)	(0.211)	(0.231)	(0.223)
$1924 < \mathrm{DOB}_i < 1930$	−0.723	−0.224	−0.307	0.023	0.391	−0.148	−0.137
	(0.257)	(0.257)	(0.265)	(0.285)	(0.267)	(0.309)	(0.289)
$1929 < \mathrm{DOB}_i < 1935$	−0.999	−0.683	−0.742	−0.314	0.441	0.111	−0.201
	(0.199)	(0.200)	(0.207)	(0.225)	(0.208)	(0.233)	(0.224)
$1934 < \mathrm{DOB}_i < 1940$	−1.393	−1.073	−1.021	−0.383	0.371	0.054	−0.361
	(0.206)	(0.207)	(0.217)	(0.233)	(0.214)	(0.241)	(0.233)
$1939 < \mathrm{DOB}_i < 1945$	−2.328	−1.743	−1.550	−0.542	−0.057	−0.210	−0.439
	(0.169)	(0.169)	(0.177)	(0.189)	(0.177)	(0.199)	(0.189)
$1944 < \mathrm{DOB}_i < 1950$	−3.023	−2.429	−2.011	−0.894	−0.529	−0.461	−0.552
	(0.161)	(0.161)	(0.167)	(0.180)	(0.168)	(0.189)	(0.178)
$1949 < \mathrm{DOB}_i < 1955$	−3.791	−3.433	−2.537	−0.694	−1.022	−0.927	−0.601
	(0.156)	(0.157)	(0.162)	(0.172)	(0.163)	(0.184)	(0.173)
$1954 < \mathrm{DOB}_i < 1960$	−3.082	−3.323	−1.409	0.075	−0.342	−0.264	0.153
	(0.172)	(0.175)	(0.176)	(0.187)	(0.178)	(0.199)	(0.187)
$1959 < \mathrm{DOB}_i < 1977$	1.070	−0.673	1.506	2.448	2.753	2.531	1.638
	(0.382)	(0.384)	(0.385)	(0.390)	(0.385)	(0.396)	(0.395)
Unskilled Blue-Collar	−0.205	−0.787	−0.778	−0.248	−0.898	−0.969	−0.511
at Date t in Firm $\mathbf{J}(i,t)$	(0.195)	(0.196)	(0.202)	(0.210)	(0.196)	(0.212)	(0.213)
Skilled Blue-Collar	−0.634	−0.977	−0.840	−0.167	−0.645	−0.675	0.064
at Date t in Firm $\mathbf{J}(i,t)$	(0.295)	(0.296)	(0.308)	(0.320)	(0.297)	(0.320)	(0.315)
Unskilled White-Collar	−2.250	−2.466	−1.218	−0.502	−1.593	−1.008	−0.749
at Date t in Firm $\mathbf{J}(i,t)$	(0.144)	(0.146)	(0.149)	(0.154)	(0.144)	(0.153)	(0.155)
Skilled White-Collar	−3.853	−4.352	−2.379	−0.880	−3.272	−2.062	−0.047
at Date t in Firm $\mathbf{J}(i,t)$	(0.161)	(0.165)	(0.166)	(0.169)	(0.162)	(0.174)	(0.166)
Manager at Date t	−5.449	−6.431	−3.977	−1.725	−5.147	−4.133	−2.052
in Firm $\mathbf{J}(i,t)$	(0.191)	(0.216)	(0.209)	(0.193)	(0.218)	(0.272)	(0.201)
Works in	−0.925	−0.983	−0.738	−0.462	−0.967	−0.541	−0.738
Ile de France	(0.069)	(0.070)	(0.074)	(0.076)	(0.070)	(0.078)	(0.077)

Seniority and Labor Force Experience

In order to impute a level of seniority for left-censored employment spells, we ran regressions (separately for men and women) of seniority on a set of demographic and occupational characteristics using data from the 1978 Salary Structure Survey (ESS, Enquête sur la Structure des Salaires). The results for men are shown in equation (8.1) and the results for women are in (8.2). All regressions included controls for 84 industries.

$$
\begin{aligned}
\text{(8.1)} \quad \text{seniority}_{it} = {} & \underset{(0.081)}{2.513} \\
& + \underset{(0.067)}{14.151}\,[DOB_i \le 1924] + \underset{(0.067)}{12.820}\,[1925 \le DOB_i \le 1929] \\
& + \underset{(0.066)}{10.299}\,[1930 \le DOB_i \le 1934] + \underset{(0.067)}{7.445}\,[1935 \le DOB_i \le 1939] \\
& + \underset{(0.067)}{4.748}\,[1940 \le DOB_i \le 1944] + \underset{(0.065)}{2.569}\,[1945 \le DOBs \le 1949] \\
& + \underset{(0.065)}{0.612}\,[1950 \le DOB_i \le 1954] - \underset{(0.067)}{0.642}\,[1955 \le DOB_i \le 1959] \\
& + \underset{(0.038)}{4.039}\,CSP30_{it} + \underset{(0.031)}{4.939}\,CSP40_{it} \\
& + \underset{(0.037)}{1.885}\,CSP50_{it} + \underset{(0.027)}{2.898}\,CSP61_{it} \\
& - \underset{(0.026)}{0.958}\,\text{Ile de France}_{it},
\end{aligned}
$$

$$N = 547{,}746, \qquad R^2 = 0.461,$$

$$
\begin{aligned}
\text{(8.2)} \quad \text{seniority}_{it} = {} & \underset{(0.084)}{2.114} \\
& + \underset{(0.074)}{12.669}\,[DOB_i \le 1924] + \underset{(0.075)}{11.014}\,[1925 \le DOB_i \le 1929] \\
& + \underset{(0.073)}{8.979}\,[1930 \le DOB_i \le 1934] + \underset{(0.074)}{7.278}\,[1935 \le DOB_i \le 1939] \\
& + \underset{(0.075)}{5.989}\,[1940 \le DOB_i \le 1944] + \underset{(0.070)}{4.604}\,[1945 \le DOB_i \le 1949] \\
& + \underset{(0.068)}{2.822}\,[1950 \le DOB_i \le 1954] + \underset{(0.068)}{0.641}\,[1955 \le DOB_i \le 1959] \\
& + \underset{(0.082)}{5.116}\,CSP30_{it} + \underset{(0.057)}{5.789}\,CSP40_{it} \\
& + \underset{(0.037)}{1.442}\,CSP50_{it} + \underset{(0.054)}{2.429}\,CSP61_{it} \\
& - \underset{(0.031)}{0.988}\,\text{Ile de France}_{it},
\end{aligned}
$$

$$N = 260{,}580, \qquad R^2 = 0.373,$$

where

$$
\begin{aligned}
&DOB_i = \text{Date of birth of individual } i,\\
&CSP30_{it} = 1 \text{ if } i \text{ is an engineer, professional, or manager},\\
&CSP40_{it} = 1 \text{ if } i \text{ is technician or technical white-collar},\\
&CSP50_{it} = 1 \text{ if } i \text{ is any other white-collar},\\
&CSP61_{it} = 1 \text{ if } i \text{ is a skilled blue-collar},\\
&CSP62_{it} = 1 \text{ if } i \text{ is an unskilled blue-collar (omitted)},\\
&\text{Ile de France}_{it} = 1 \text{ if the establishment is in Ile-de-France}.
\end{aligned}
\tag{8.3}
$$

The excluded date of birth category was $1960 \le DOB_i$. The coefficients on the industry indicators are not shown.

To compute the values of seniority and labor force experience, we used the following algorithms. If the individual was left-censored and the imputed job seniority was negative, we set job seniority prior to 1976 to zero. If the individual was first observed after 1976, we assumed that job seniority on that job prior to the date of the first DAS observation for the individual was zero. If the age at the date of any observation (1976 or otherwise) was less than the expected school-leaving age, both total labor force experience and prior job seniority were set to zero. In all other cases (when the age was greater than the expected school-leaving age), we calculated total labor market experience and job seniority as follows. If the observation was the earliest appearance of the individual in our data, we set job seniority equal to job seniority up to the date of the first observation plus the number of days worked for that enterprise in the year of the first observation, divided by 360 and we set total labor market experience to the current age less the school-leaving age. If the observation was not the first for the individual but there was an observation in the previous year for the person,[44] we added 1 to total labor market experience. If the individual was employed for the majority of the current year by the same enterprise that employed him or her for the majority of the previous year, i.e. $SIREN_t = SIREN_{t-1}$, we added 1 to the level of seniority at $t-1$. If $SIREN_t \ne SIREN_{t-1}$, we set seniority equal to the number of days worked divided by 360.

If, on the other hand, there was no observation in the previous year, we distinguished between $t = 1982$ or $t = 1984$ and other years. When $t \ne 1982$ or 1984, total labor market experience was increased by 1 (reflecting experience gained in the year of the observation). If the current *SIREN* and the most recent previous *SIREN* were the same, we added the number of days worked divided by 360 to the most recent previous level of seniority. This is similar to assuming that the worker was temporarily laid off but retained his or her seniority in the firm when recalled. Otherwise, we set seniority to the number of days worked divided by 360.

In the case where $t = 1982$ or $t = 1984$, if the preceding observation was 2 years earlier (i.e. the missing data only occurred over a period when no data were available for any individual), we increased total labor market experience by 2. If $SIREN_{t-2} = SIREN_t$, seniority was increased by 2. If $SIREN_{t-2} \ne SIREN_t$, seniority was increased by 0.5 plus the number of days worked divided by 360.[45]

[44] The structure of our database is such that this condition (observations for individual i at both t and $t-1$) could only fail to be satisfied under 3 conditions. The first is that the individual was employed in the civil service in the intervening years. The second is that the individual was unemployed for an entire calendar year. The third is that $t = 1982$ or $t = 1984$, since we were not given access to the data for 1981 or 1983. We largely discount the first possibility for the reasons mentioned in the text. The other two possibilities are treated explicitly.

[45] We assumed that the probability the individual was reemployed in the missing year was equal to the probability that the individual was reemployed in the observation year. Thus, the expected increment to job seniority is the share of the year worked in the observation year plus $(\frac{1}{2} \cdot 0) + (\frac{1}{2} \cdot 1) = 0.5$.

If the preceding observation was more than 2 years earlier, we increased total labor market experience by 1.5.[46] If the current *SIREN* and the most recent previous *SIREN* were the same, we added the number of days worked divided by 360 plus 0.5 to the most recent previous level of seniority. This is similar to assuming that the worker was recalled from temporary layoff with equal probability in the observation year and in the missing year. If the two *SIREN*s were different, we set seniority to 0.5 plus the number of days worked divided by 360.

Elimination of Outliers

We ran a standard log earnings regression (the dependent variable was the logarithm of real annualized compensation cost, *LFRAISRE*, the same one used in the analyses reported in Tables II–XII) on our DAS data and considered all observations that were more than 5 standard deviations away from their predicted values as outliers. These observations were discarded. The estimated coefficients of this earnings regression are shown in equation (8.4).

$$
\begin{aligned}
(8.4)\qquad LFRAISRE_{it} = &-\underset{(0.005)}{3.250} \\
&+\underset{(0.000)}{0.210}\ \text{Male}_i + \underset{(0.000)}{0.123}\ \text{Ile de France}_{it} \\
&+\underset{(0.000)}{0.082}\ \text{Year}_{\text{it}} + \underset{(0.002)}{0.056}\ \text{Degree Category } 2_i \\
&+\underset{(0.002)}{0.415}\ \text{Degree Category } 3_i + \underset{(0.003)}{0.627}\ \text{Degree Category } 4_i \\
&+\underset{(0.001)}{0.266}\ \text{Degree Category } 5_i + \underset{(0.003)}{0.642}\ \text{Degree Category } 6_i \\
&+\underset{(0.002)}{0.648}\ \text{Degree Category } 7_i + \underset{(0.003)}{1.421}\ \text{Degree Category } 8_i \\
&+\underset{(0.000)}{0.055}\ \text{Experience}_{it} - \underset{(0.003)}{0.222}\ \text{Experience}^2_{it} \\
&+\underset{(0.001)}{0.052}\ \text{Experience}^3_{it} - \underset{(0.000)}{0.005}\ \text{Experience}^4_{it},
\end{aligned}
$$

$$N = 5{,}325{,}352,\quad R^2 = 0.437,\quad \sigma = 0.477.$$

Definition of Z Variables and Coefficients in the Conditional Method

Data Appendix Table B6 contains the definitions, regression coefficients, and coefficient standard errors for the Z variables used in estimating the statistical model (3.17) as reported in Table III in the column labelled "Conditional Method Persons First."

Pooled Regression for Order-Dependent Persons-First Estimation

Recovery of the firm effects was done in the conditional methods on a firm-by-firm basis. All observations corresponding to firms for which there were fewer than 10 observations were grouped together and included in a single, pooled regression. The results of this regression for the pooled "firm" in the order-dependent, persons-first case are shown in equation (8.5). The results for the order-independent pooled "firm" are not shown.

$$(8.5)\qquad DLFRAISR_{it} = -\underset{(3.375e\text{-}4)}{0.028} + \underset{(8.476e\text{-}5)}{0.003}\ s_{it} - \underset{(1.772e\text{-}4)}{0.005}\ \mathbf{T}_1(s_{it}-10),$$

$$N = 1{,}353{,}794,\quad R^2 = 0.0013.$$

[46] We assumed that the probability the individual was reemployed in the missing year was equal to the probability that the individual was reemployed in the observation year. Thus, the expected increment to total labor market experience is $(\frac{1}{2}\cdot 1) + (\frac{1}{2}\cdot 2) = 1.5$.

DATA APPENDIX TABLE B6

SUMMARY STATISTICS, COEFFICIENTS AND STANDARD ERRORS FOR Z VARIABLES IN THE CONDITIONAL METHOD ORDER INDEPENDENT ESTIMATION

Variable Definition	Mean	Standard Deviation	Coefficient	Standard Error
Firm size × average experience	2.54E-05	3.16E-06	1.11E-05	3.82E-06
Firm size × age at end of school	1.79E-04	2.91E-06	1.77E-05	3.58E-06
Firm size squared × average experience	− 7.57E-08	2.00E-08	6.38E-08	2.00E-08
Firm size squared × age at end of school	− 5.28E-07	1.00E-08	− 3.06E-08	2.00E-08
Firm size × seniority × average experience	3.23E-06	3.30E-07	2.76E-06	3.50E-07
Firm size × seniority × age at end of school	− 1.43E-05	4.50E-07	− 6.95E-06	4.30E-07
Firm size squared × seniority × average experience	− 5.76E-09	1.60E-07	− 1.12E-09	7.19E-10
Firm size squared × seniority × age at end of school	4.47E-08	1.00E-08	2.01E-08	1.91E-07
Industry 1 × average experience	− 3.92E-04	1.28E-04	− 2.06E-03	3.19E-03
Industry 1 × age at end of school	− 2.22E-02	1.48E-04	1.04E-02	3.49E-03
Industry 1 × seniority × average experience	3.95E-04	1.61E-05	1.50E-04	1.53E-05
Industry 1 × seniority × age at end of school	− 2.98E-04	2.51E-05	− 1.12E-04	2.17E-05
Industry 2 × average experience	2.10E-03	2.17E-04	− 4.71E-03	3.20E-03
Industry 2 × age at end of school	1.62E-02	2.20E-04	1.39E-02	3.50E-03
Industry 2 × seniority × average experience	− 1.25E-04	2.18E-05	− 1.41E-04	2.27E-05
Industry 2 × seniority × age at end of school	6.14E-04	3.14E-05	3.32E-04	3.04E-05
Industry 3 × average experience	3.82E-04	7.75E-05	− 1.93E-03	3.19E-03
Industry 3 × age at end of school	− 3.61E-02	8.33E-05	1.03E-02	3.49E-03
Industry 3 × seniority × average experience	2.07E-04	8.98E-06	8.41E-05	8.00E-06
Industry 3 × seniority × age at end of school	− 4.80E-05	1.36E-05	− 1.52E-05	1.14E-05
Industry 4 × average experience	− 2.52E-04	7.46E-05	− 2.15E-03	3.19E-03
Industry 4 × age at end of school	− 1.76E-02	7.08E-05	1.08E-02	3.49E-03
Industry 4 × seniority × average experience	4.09E-05	8.03E-06	8.92E-05	7.62E-06
Industry 4 × seniority × age at end of school	3.66E-04	1.12E-05	− 1.38E-05	9.93E-06
Industry 5 × average experience	2.16E-03	8.12E-05	− 1.95E-03	3.19E-03
Industry 5 × age at end of school	− 3.59E-02	8.61E-05	9.18E-03	3.49E-03
Industry 5 × seniority × average experience	2.92E-04	9.78E-06	1.14E-04	9.37E-06
Industry 5 × seniority × age at end of school	− 4.70E-04	1.48E-05	7.38E-06	1.29E-05
Industry 6 × average experience	1.02E-03	8.46E-05	1.67E-04	3.19E-03
Industry 6 × age at end of school	− 2.94E-02	1.05E-04	4.62E-03	3.49E-03
Industry 6 × seniority × average experience	7.20E-04	1.20E-05	1.07E-04	1.07E-05
Industry 6 × seniority × age at end of school	− 1.41E-03	1.85E-05	− 1.00E-04	1.50E-05
Industry 7 × average experience	− 3.46E-04	6.93E-05	− 2.16E-03	3.19E-03
Industry 7 × age at end of school	6.53E-03	8.00E-05	8.91E-03	3.49E-03
Industry 7 × seniority × average experience	− 4.73E-05	9.20E-06	− 4.40E-05	8.55E-06
Industry 7 × seniority × age at end of school	9.89E-04	1.38E-05	2.34E-04	1.17E-05
Industry 8 × average experience	− 3.60E-04	1.32E-04	− 2.88E-03	3.19E-03
Industry 8 × age at end of school	2.35E-02	1.39E-04	9.77E-03	3.49E-03
Industry 8 × seniority × average experience	− 3.22E-04	1.53E-05	7.68E-05	1.49E-05
Industry 8 × seniority × age at end of school	1.70E-03	2.25E-05	1.02E-04	2.06E-05
Industry 9 × average experience	5.22E-04	5.53E-05	− 2.81E-03	3.19E-03
Industry 9 × age at end of school	3.57E-02	5.89E-05	8.25E-03	3.49E-03
Industry 9 × seniority × average experience	− 3.87E-04	6.81E-06	− 2.85E-05	6.40E-06
Industry 9 × seniority × age at end of school	1.89E-03	9.63E-06	1.79E-04	8.36E-06
Industry 10 × average experience	− 1.98E-03	8.29E-05	− 3.20E-03	3.19E-03
Industry 10 × age at end of school	3.43E-02	7.92E-05	8.87E-03	3.49E-03
Industry 10 × seniority × average experience	− 1.10E-04	9.56E-06	− 1.97E-05	1.01E-05
Industry 10 × seniority × age at end of school	0.001673	0.00001264	0.000238	0.00001243

Notes: These coefficients supplement the coefficients reported in Table III, Column "Conditional Method Persons First."

DATA APPENDIX TABLE B7

DESCRIPTIVE STATISTICS FOR BASIC INDIVIDUAL LEVEL VARIABLES BY SEX FOR 1976 TO 1987

	Men		*Women*	
Variable Definition	Mean	Standard Deviation	Mean	Standard Deviation
Real Total Annual Compensation Cost, 1,000FF 1980	89.0967	61.6302	67.3646	37.4208
Log (Real Annual Compensation Cost, 1980 FF)	4.3442	0.5187	4.0984	0.4801
Total Labor Force Experience	17.2531	11.8258	15.4301	12.0089
(Total Labor Force Experience)^2/100	4.3752	4.9197	3.8230	4.9440
(Total Labor Force Experience)^3/1,000	13.1530	19.4305	11.6079	19.6863
(Total Labor Force Experience)^4/10,000	43.3453	77.9542	39.0589	80.3251
Seniority	7.7067	7.5510	6.5437	6.5268
Lives in Ile-de-France (Paris Metropolitan Region)	0.2561		0.2910	
No Known Degree	0.3064	0.2190	0.2971	0.2124
Completed Elementary School	0.1556	0.1458	0.1893	0.1739
Completed Junior High School	0.0565	0.0792	0.0869	0.1008
Completed High School (Baccalauréat)	0.0528	0.0804	0.0711	0.0881
Basic Vocational-Technical Degree	0.2652	0.1849	0.1926	0.1545
Advanced Vocational-Technical Degree	0.0701	0.0893	0.0532	0.0802
Technical College or University Diploma	0.0469	0.0754	0.0838	0.1247
Graduate School Diploma	0.0465	0.0964	0.0259	0.0551
Year of data	81.3106	3.7250	81.4730	3.7180
Number of Observations for the Firm in Sample	4,402.3800	16,164.6200	1,605.3100	7,797.1300
Observations	3,434,530		1,870,578	
Persons	711,518		454,787	
Proportion with Identified Least Squares Estimate of Individual and Firm Effect	0.7425		0.7448	

Source: Authors' calculations based on the Déclarations annuelles des salaires (DAS).

Construction of the Operating Income Variable

The operating income variable (excedent brut d'exploitation) was constructed as in the following equation:

(8.6) *EBE* = ventes de marchandises (merchandise sold)
− achat de marchandises (merchandise purchased)
− variation de stock de marchandises (variation in merchandise inventory)
+ ventes de biens (goods sold)
+ ventes de services (services sold)
+ production stockée (inventoried production)
+ production immobilisée (unfinished production)
− achats de matières premières (primary materials purchased)
− variation de stocks sur matières premières (variation of primary materials inventories)

− autres achats et charges externes

(other purchases and outside charges)

+ subventions d'exploitation (incentives for production)

− impôts, taxes et versements assimilés

(value added tax and other accrued taxes on

or credits for production)

− salaires et traitements (salaries and benefits)

− charges sociales (payroll taxes).

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