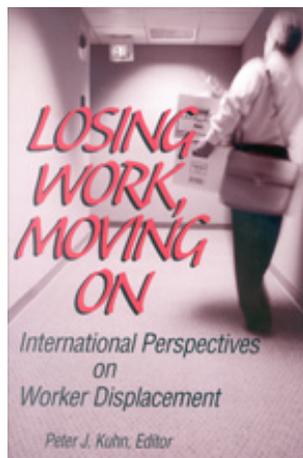

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Summary and Synthesis

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Summary and Synthesis

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Almost every day, we are told that innovation and change are the chief sources of prosperity in advanced industrial economies. Innovation and change, however, often involve abandoning the production of goods and services which are technologically superseded or relocating their production to lower-wage countries. All modern economies thus face the problem of how to move workers out of outmoded activities and into more productive ones.

What is the best way to achieve this? In some situations, a combination of voluntary practices, including workforce attrition, reductions in overtime and in regular weekly work hours, cuts in bonuses and wages, and adoption of new products and lines of business can be used to adjust to declines in business without permanent layoffs. Sometimes, however, worker displacement, i.e., the involuntary, permanent termination of long-term employees, is unavoidable. In the United States, displacement is fairly common, affecting about 5 percent of all employed workers each year, and 2.5 percent of those with more than 10 years of service.¹ Furthermore, at least for workers with high levels of tenure in the lost job, the lifetime consequences of displacement can be both severe and permanent (Ruhm 1991).

Do countries other than the United States rely more or less on worker displacement as a method of industrial adjustment? Are the consequences of displacement, when it occurs, more or less severe, and does this differ for employment versus wages? Do international differences in labor market policy, including employment-protection laws, unemployment insurance systems, and wage-setting regimes play a role in explaining differences in the incidence and effects of displacement? Are international differences in the experiences of displaced workers informative about important structural features of labor markets, such as the wage returns to tenure or the amount of firm-specific training?

To date, questions like these have been difficult to answer. Because displacement involves a change in individuals' labor market status, its statistical analysis requires panel data (in other words, data on individuals at more than one point in time). Such data have only recently become publicly available in many countries other than the United States, paving the way for a first examination of worker displacement there. The purpose of this volume is to conduct such an analysis, with an aim to answering as many of the above research questions as we can.

The insights gained from the research in this volume fall into four main categories. The first concerns the tremendous variation in the institutions that regulate and affect displacement in different countries. Institutions that affect displaced workers include employment-protection laws (such as advance notice, consultation requirements, and severance-pay requirements surrounding layoffs and plant closures), income support and retraining programs aimed at the unemployed (especially unemployment insurance), and the broader set of institutions involved in the setting of wages (which may have significant effects on the distribution of wage changes experienced by displaced workers).

The second set of lessons from this volume concern methodology. Because this volume has been a first foray into cross-national research on displacement, one of its most important outcomes is a list of methodological desiderata and pitfalls that future analyses of displaced workers, and future international labor market comparisons, would do well to take into account.

Third are substantive results concerning within-country patterns of displacement and its consequences: is displacement, for example, always more common among men than women? Are the consequences of displacement always more severe for older workers? In a search for "universal" patterns in displacement, we have uncovered a few probable universals, as well as some fascinating exceptions, which are worthy of study precisely because of their exceptionality.

Finally, the most difficult kind of results to generate, but potentially the most rewarding, are those which make cross-national comparisons in the levels of key variables (such as the frequency of displacement or the duration of postdisplacement unemployment) and which associate these with international differences in labor market

structure and policy. In this area, our conclusions are less firmly held than in other areas, but in equal measure more tantalizing.

In the remainder of this chapter, I discuss the four main categories of lessons learned from our analysis in turn.

INSTITUTIONS AFFECTING DISPLACED WORKERS

The main institutional differences among the 10 labor markets studied in this volume are summarized in Tables 1.1–1.3. These tables are derived from the much more detailed descriptions in Chapters 2 through 6, and they refer to the situation prevailing at the time for which the statistical analyses in those chapters was conducted. In four cases (Belgium, Denmark, France, and Germany) this is the mid to late 1980s. In the remaining six cases it is the mid 1990s. I begin this section by briefly summarizing the main features of employment protection laws, income support, and wage-setting policies in all these countries in turn. I then discuss a number of observations concerning the variation in institutions among countries and the ways in which this variation affects our analysis of displaced workers.

Employment Protection Laws

As Table 1.1 shows, there is dramatic variation among our 10 countries in the scope and stringency of their employment protection laws (EPLs). For example, suppose a firm wanted to lay some workers off permanently in response to a decline in sales. United States employers—like those in several other countries—would find the notion that they would need to explain and justify such layoffs to some external authority quite alien. Yet this is precisely the law in four of the countries under study here. In Japan, France, and Germany, economic necessity and/or social acceptability must be demonstrated to a point where the decision could be defended in a court of law. In Germany, for example, social acceptability is defined by a considerable body of case law and often involves detailed seniority rules for layoffs, consideration of economic need—such as family size and the number of dependents—in choosing whom to lay off, and extensive relocation

assistance. In the Netherlands, a firm needs to obtain a permit from a regional employment institution before it can lay off any workers.

Much more common than justification requirements for economic layoffs are mandatory notice laws covering layoffs of individual workers. Indeed, consistent with the emphasis on employment at will in the United States, individual notice requirements are present in all of the countries under study except the United States.² These requirements generally index the statutory amount of notice to a worker's tenure on the lost job (the Netherlands, in an interesting exception, ties it to the worker's age). In addition, several European countries (in particular Germany, Belgium, and Denmark) draw quite dramatic distinctions between blue-collar and white-collar workers, with the latter entitled to more notice. For example, in Belgium, a white-collar worker with 20 years' service is entitled to a year of notice, while an identical blue-collar worker is entitled to only two months; in Denmark, blue-collar workers are not entitled to any notice at all. Finally, comparing the level of individual notice at similar tenure levels reveals very large differences among countries, especially at high tenure levels. Inspection of the table reveals, for example, legislated notice levels for a white-collar worker with 20 years of service include zero notice in the United States, one week in Australia, one month in Japan, two months in Canada and France, three months in the United Kingdom, six months in Germany and Denmark, and—as already noted—one year in Belgium. This tendency of Belgium to stand out as an extreme case of EPL is discussed and analyzed much more thoroughly in Chapter 6 of this volume.

Many U.S. readers will be familiar with the United States' advance notice requirement for mass layoffs, passed into law in 1988. The Worker Adjustment and Retraining Notification Act (WARN) mandates two months of notice for workers involved in layoffs of a large fraction of an establishment's workforce. Are such group notice laws even more stringent in other countries (as we might expect from the previous discussion)? Perhaps surprisingly, the answer here is "no." While group notice levels can be higher for very large layoffs in the United Kingdom (up to three months) and Canada (up to four months), none of the remaining countries have group notice periods distinct from the individual requirements summarized above. One reason for this, of course, is the relatively high levels of individual notice already

required in those cases. Equally important, however, in three cases (Denmark, France, and Germany), the absence of extra legislated notice for mass layoffs is explained by the use of a different, consultative process for managing mass layoffs. This process involves mandated, case-by-case negotiations of notice, severance, and other downsizing procedures with the works council, union, and/or local government authorities. The resulting “social plan” can involve many forms of assistance to displaced workers, including job search assistance and direct outplacement with other firms.

Aside from mandated advance notice and in-kind job search/place-ment assistance, the other main form taken by EPLs involves cash payments to laid-off workers. In our sample of countries, mandated severance pay is totally absent in the United States, Japan, and Denmark. It is rare in Australia (applying to “model” awards only) and the Netherlands (applying only when a case goes to court). In some other countries it is limited in scope; for example, it applies only to a single (albeit populous) province in Canada, and only to mass layoffs in Belgium and Germany (in Germany’s case it is negotiated in the social plan). These qualifications aside, however, mandated severance payments can be very substantial in some countries and situations. In the United Kingdom, France, and in parts of Canada, they can amount to a half year or more of pay for individual terminations; in Belgium they can add up to several years’ wages when a plant is closed.

Finally, in a number of countries, the state’s involvement in layoffs takes forms that are harder to quantify but not without potentially great effect. Extensive procedural requirements such as those in Japan and France can lead to long, uncertain, and costly delays in implementing mass layoffs, but it is hard to specify their cost equivalent in terms of weeks of legislated notice or dollars of severance pay. Consultative requirements, including mandated negotiations with a union and/or works council, determine the main dimensions of assistance provided to mass-layoff victims in Germany and France. These can be very generous but will vary on a case-by-case basis depending on the outcomes of negotiations. Analyses that ignore these dimensions of EPL can seriously mismeasure the strength of EPL in some countries.

In sum, looking across countries, it is clear that restrictions on firms’ abilities to dismiss workers vary dramatically. Along almost all dimensions, the United States appears at one end of the spectrum with

the least-stringent regulations, and Belgium at the other end with the most stringent. Between these extremes, however, there is no consistent ranking: countries can be high on some dimensions of EPL and low on others. Canada, for example, has relatively high statutory notice and severance provisions, but relatively few consultative and other procedural impediments to layoffs. At the other extreme, Japan requires no severance pay and only a month of notice, but has extensive and complex procedural requirements for layoffs. Nor is “Europe” a high-EPL monolith: compare restrictive Belgium with *laissez-faire* Denmark. One important implication of this kind of heterogeneity is that any overall ranking of EPL among countries (including the several that are commonly used in cross-country regression studies) will not be invariant to the weights assigned to different EPL components.³ Another implication is that research opportunities clearly exist comparing European countries, a prime example of which is Chapter 6 of this volume.

Income Support Programs

As for employment protection laws, the countries in our sample differ substantially in their approaches to providing income support to unemployed workers (Table 1.2). For example, while most countries require some work history in the year preceding a claim, some—notably Australia and Belgium—do not. The most common qualifying periods amount to about six months of work in the year preceding the unemployment insurance (UI) claim (the United States, the Netherlands, Japan, and Denmark). Some are more restrictive (two years of continuous employment in the United Kingdom); others are less (as few as 10 weeks in high-unemployment regions of Canada). Benefits are unlimited in duration in Australia, Belgium, and Denmark; they can last up to five years in the Netherlands and almost three years in France and Germany. Maximum benefit durations in the United States, Japan, Canada, and the United Kingdom are all under one year.

Seven of the 10 countries under study set UI benefits as a fraction of predisplacement earnings; of these, 4 (the United States, Japan, Canada, and Denmark) have either a maximum benefit level or a sliding benefit scale that reduces the actual replacement rates well below the “statutory” rate for workers with higher levels of predisplacement

earnings. Three do not, offering *actual* replacement rates of 70 percent (the Netherlands), 57–75 percent (France) and 60–67 percent (Germany), which are much higher than U.S. levels. Three countries (the United Kingdom, Australia, and Belgium) do not index benefits to previous earnings at all, instead providing a fixed needs-based amount depending on family size and structure. Finally, the countries with limited UI duration all have some sort of fallback source of income support (such as “Unemployment Assistance” (UA) in the United Kingdom and *Arbeitslosenhilfe* (AH) in Germany). These programs can sometimes be quite generous (including a full rent and property-tax subsidy in the United Kingdom, and income support equal to 57 percent of the previous wage in Germany), but they are extremely limited in the United States.

Other noteworthy elements of the cross-country variation in income support systems include the fact that, in several cases (including the United States, the Netherlands, and the United Kingdom), disability or early-retirement benefits offer an attractive alternative to either unemployment insurance or general welfare. Also, with the exception of France, all countries impose some restrictions on UI payments to persons who voluntarily quit their jobs. Interestingly, however, France disentitles *seasonal* workers from UI, while in Canada they are among the system’s most politically influential beneficiaries. In Denmark, the trade unions rather than the state administer the unemployment insurance system(s). Finally, in addition to UI, some countries have income support and retraining programs targeted specifically at displaced workers or at workers who are displaced for specific reasons, such as changes in international trade. Best known here is the Trade Adjustment Assistance (TAA) program in the United States. Similar programs exist on a more ad hoc basis in Canada. The most formalized system of this kind, however, is Japan’s system of “employment maintenance and adjustment subsidies.” These programs make payments to laid-off workers in a (periodically updated) list of industries deemed to be in serious decline. They also subsidize retraining; and most interestingly, they pay wage subsidies to employers who hire workers displaced *from* the targeted industries.

Wage-Setting Institutions

Countries also differ dramatically in their regulation of the wage-setting process (Table 1.3). Union coverage rates vary from 14 percent in the United States to 90 percent in France, Germany, and Belgium. In many countries (especially the Netherlands, Australia, France, Germany, and Belgium) coverage is much higher than membership because of the mandatory extension of union contracts to nonmembers. Some nations, like Britain and Germany, have no minimum wage regulations at all; some (like the United States and Japan) set minima that are a very small fraction (30 to 40 percent) of the average manufacturing wage. This contrasts with France's minimum of over 80 percent of the mean industrial wage. Wage minima can be set on the national level (France), national and state levels (United States), province level only (Canada), industry and industry-by-prefecture level (Japan), and even nationally by occupation (Australia), with fascinating (but largely unexplored) implications for wage structure.

Some Observations on Institutions

In this subsection I discuss three main features of displacement-related institutions that are particularly relevant to the comparative study of the effects of labor market institutions on labor market outcomes, including the experiences of displaced workers.

First, although the various subdimensions of the institutional environment are correlated among countries, these correlations are highly imperfect. Still, it is probably a useful descriptive device to group the 10 countries examined here into two groups. In the United States, Canada, the United Kingdom, Australia, and to some extent Denmark, it strikes one that the primary focus of displacement-related institutions is a "palliative" one. At least in their public policy, these countries seem to place most of their emphasis on assisting impacted workers after the fact of displacement, via unemployment insurance and state-financed retraining programs. They seem much more reluctant to intervene in the displacement process itself than those European and Japanese governments, who—while also adopting some palliative measures—take what might be called a "preventative" stance. Preventative policies aim to prevent the layoff in the first place or, if that is

unavoidable, to prevent a spell of unemployment as a consequence of the layoff. The idea (presumably) is that unemployment can lead to a negative feedback cycle involving loss of skills, health, and self-esteem, which is best avoided completely.⁴ The challenge—and the source of much current debate in Europe and Japan—is how to do this without reducing firms’ incentives to hire new workers, especially in times or industries where demand uncertainty is high. One option, used in Japan, might be the payment of subsidies to firms which hire workers *formerly employed* in industries the government believes should be shut down.

Concerning the imperfection of the above correlations, a number of examples illustrate the point. First, within the palliative group, during our sample period Australia had a highly centralized wage-setting system, while Britain had no minimum wages at all. Canada and the United Kingdom had fairly stringent advance-notice requirements while the United States and Australia had almost none. Within the preventative group, Japan has very modest notice requirements compared to some countries (such as Canada) in the palliative group. (Japan’s preventative institutions take other forms.) While Denmark has generous unemployment insurance and centralized wage-setting, employment-protection laws differ so dramatically between Belgium and Denmark that Albæk, Van Audenrode, and Browning conclude in Chapter 6 that this is the most likely explanation for the large differences in displaced workers’ unemployment durations between these countries.

It is common in current public discussions to refer to the problem of high European unemployment or to attribute non-European differences in labor market performance to rigid European laws and institutions. As our discussion here makes clear, however, many differences exist within these two groups of countries. Furthermore, comparing broadly similar countries that differ substantially in only one or two relevant dimensions—such as the comparisons between Belgium and Denmark and between the United Kingdom and Australia in the current volume, and that between Canada and the United States in Card and Freeman (1993)—may be the most informative way to explore the effects of labor market institutions on labor market outcomes.

My second observation about institutions is simply that they are multidimensional. In particular, even legislation bearing on a very spe-

cific action (e.g., a firm's ability to initiate a layoff) can have several components, some of which are not easily quantified. Thus, as noted, in addition to severance pay and notice provisions, some countries (for example, Japan and Germany) prohibit layoffs unless they are "socially acceptable" or "economically necessary" and require them to be conducted according to appropriate procedures. In Italy, mandated notice periods are minimal but dismissals must be pre-approved by state officials with highly variable decision lags (Garibaldi and Brixiova 1997). The main implication of multidimensionality is that simple measures of a law's most-easily quantified aspects (such as weeks of notice) can be very poor proxies for its overall stringency. Japan has very lenient advance-notice laws, for example, but this does not mean it is easy to lay off a worker there. Both this and future studies would do well to pay close attention to procedural and other details when comparing legislation among countries.

My third observation is that not all institutions constraining individual firms and workers take the form of legislation. At least four main forms of nonlegislated institutions affect displaced workers, the most obvious of which are restrictions contained in union contracts. In many cases, collective bargaining agreements, especially those negotiated on national levels, and sometimes extended to unorganized workers, set binding constraints on minimum wages, dismissal procedures, and other elements of the employment contract. For example, in France, statutory advance-notice provisions are usually superseded by collectively bargained provisions, which cover 90 percent of the workforce.

A second set of nonlegislated institutions that are directly relevant to displaced workers are the mechanisms of exchange in industrial labor markets. An important case in point is the construction industry. In North America this industry has a very high separation and displacement rate because it is organized along craft lines: workers have long-run attachments to their craft, but not to any particular firm, and move with great frequency from one firm to another as projects are completed. Furthermore, while displacement in this industry is relatively inconsequential in its effect on wages, it is sufficiently frequent (and the industry is sufficiently large) to have a significant effect on the average *national* displacement rate. In Britain, construction labor markets are organized differently, and the construction industry has a

lower displacement rate than manufacturing. Whether or not construction is included in the statistics also affects comparisons of Canada's and Japan's displacement rates (see Chapter 3).

Just as the organization of labor markets can differ across industries, countries have different national mechanisms of worker termination. Among other things this necessitates great care in defining roughly comparable measures of displacement. In North American economies, for example, where temporary layoffs are common, the precise point at which any given worker makes a permanent break with his or her firm is often quite unclear. Workers on temporary layoff often look for other jobs and may or may not eventually be recalled to their former employer. They may even return after starting a job with another firm. The whole permanent separation process can thus be very drawn out, and results using *ex post* versus *ex ante* definitions of permanent layoffs can differ substantially. In some other countries (such as Germany or the Netherlands), displacement constitutes a sharper, more well-defined event. Still other countries have institutional mechanisms, such as the *shukko* system in Japan—involving outplacements with other firms within an association, or *keiretsu*—where workers can be involuntarily moved out of the firm without any spell of unemployment or any uncertainty regarding their new wage rate. As the next section illustrates, it matters whether we count such workers as displaced or not.

It is of course possible that these national termination styles are simply the *result* of long-standing differences in legislation among countries (see Burdett and Wright 1989; Van Audenrode 1994). The hypothesis that they are caused by legislation is, however, not always consistent with historical evidence, as Huberman (1997) argues. In several cases, differences in national adjustment practices predate the legislative differences that are supposed to have caused them. These long-standing practices may therefore be the more “primitive” of the institutions affecting displacement. Long traditions, as well as historical accident, thus play a role in any complete analysis of institutions' effects.

A final kind of nonlegislated institution is the generally accepted standard of relocation assistance that is “voluntarily” provided by firms to workers who are permanently laid off. In different countries and industries, certain amounts of help are simply considered normal and

fair treatment; sometimes (as in Canada and Belgium) these accepted practices are explicitly recognized in the common law regulating terminations. In some countries, like the United States, this may be quite minimal, but even there, significant amounts of advance notice were voluntarily provided by firms before any legislated standard existed.⁵ Another example is outplacement services, provided by many former employers, that directly secure jobs for workers at other firms. Such services, especially in some European countries, can make unemployment following plant closures the exception rather than the rule, unlike the case in North America. Voluntary relocation assistance is often ignored in existing analyses of displacement, to some extent surely because the U.S. Displaced Worker Survey contains no information on these activities. As some recent Canadian research (Riddell 1999) indicates, however, it can be substantial even when not required by any legislation.

Summary

In sum, the 10 countries studied in this volume can be roughly categorized into two groups. The first, consisting of the United States, Canada, the United Kingdom, Australia, and (to some extent) Denmark, confine their market interventions largely to palliative measures aimed at workers after the fact of displacement. The remainder (the Netherlands, Japan, France, Germany, and Belgium), while adopting (sometimes very generous) palliative measures, also take a preventative stance, adopting a number of policies designed to prevent layoffs in the event of plant closures or, if layoffs are inevitable, to prevent unemployment in the event of layoffs.

Within these very broad patterns, tremendous and often quite unexpected institutional heterogeneity remains. Policies affecting displacement are multidimensional, and some of the dimensions in which variation occurs (such as consultation requirements and approval procedures for layoff permits) can be hard to quantify. In some countries, collectively bargained provisions supersede legislated requirements as the main binding constraints on employers, not just in wage-setting decisions but in worker termination and plant shutdown procedures as well. Voluntarily provided assistance from firms in many cases constitutes the most important part of a worker's severance package. Some-

times the institution with the greatest effect on a statistic like the displacement rate is simply the organization of labor markets on the industry level: industries which are organized on a craft or hiring-hall basis, like construction in North America, will have very high displacement rates (and relatively small consequences of displacement) because of the methods by which labor is exchanged. National mechanisms of displacement differ too, ranging from the temporary-to-permanent transition common in North America, to the much sharper breaks which are traditional in some other countries. No analysis of international differences in displacement is complete without reference to these less formal (but no less fundamental) institutional differences between countries.

METHODOLOGICAL LESSONS FOR THE STUDY OF DISPLACED WORKERS AND FOR CROSS-NATIONAL LABOR MARKET RESEARCH

Because cross-national research on displacement—and on labor market dynamics more generally—is very new, one of the most useful things a volume like this can do is to alert future researchers to a number of key methodological issues that naturally arise in this context. That is the role of this section. Its main intended audience, therefore, is labor economists, especially those actively engaged in cross-national research and, most especially, that on labor market dynamics. Readers interested primarily in substantive results may wish to skip or skim this section and move right on to the next section of this chapter (p. 22).

One methodological lesson of this volume has already been discussed: the importance of getting the institutions right, i.e., taking care in measuring all aspects, including the nonlegislated ones, of the institutional environment in all countries. Aside from this, the main methodological lessons I believe we learned, as a group of 22 authors, are the following.

Defining Displaced Workers

Definitional issues arise in all comparative studies of labor markets. For example, studies of wage structure need to decide whether to define the annual bonuses paid to workers in countries like Japan and Germany as part of wages. These bonuses can be a large (and in Japan's case, a highly variable) fraction of compensation, and their treatment can change one's most basic conclusions.⁶ In the current volume, however, definitions matter in an even more fundamental way, because they concern membership in the population under study. At the very outset, we are confronted with the question of who *is* a displaced worker, in a group of countries with different institutional mechanisms of labor adjustment, each with its own nomenclature for worker-firm separations.

As noted already, two working definitions of displaced workers are used in this volume. One of these, used in administrative databases, consists of separations surrounding a firm or plant closure (or a large reduction in firm or plant size). This definition does not make use of the reported reason for a separation, either by the worker or the firm. The other definition consists of self-reported layoffs in surveys of individual workers.

Both the administrative-data-based and the survey-based definitions of displaced workers used here have their advantages and disadvantages. An advantage of the administrative definition is that it will include early leavers in the sample of displaced workers (*early leavers* are individuals who start searching for new jobs in response to information about an impending shutdown and who quit to take such jobs even before the plant closes). Presumably we would wish to include these very successful adapters in our count of the displaced. Another advantage is the notion, common in the displacement literature, that plant closure constitutes a better "natural experiment" with which to analyze broader labor market phenomena, because involvement in a shutdown is more likely to be orthogonal to an individual's unobserved ability than involvement in a person-specific layoff. A disadvantage is that a large majority of involuntary worker terminations occur on an individual basis; thus a study of plant closure victims alone would be unrepresentative of the whole population of displaced workers.⁷ Another disadvantage of the plant-closure-based definition, especially

if the “window” around the plant shutdown is relatively wide, is that such definitions can include a considerable amount of normal workforce turnover, i.e., workers who would have voluntarily left the firm even in the absence of an impending shutdown. A final disadvantage, the “false firm death” problem, is discussed in more detail below.

Consider now the survey-based definition of displaced workers, consisting of all separations labeled as involuntary (from the worker’s point of view). As argued above, this has the advantage of greater representativeness, but has the disadvantage—especially in an international context—of relying on workers’ (and/or firms’) self-reported reasons for why a separation occurred. Relying on reported separation reasons can give rise to three kinds of problems, especially in international studies. First, there may be simple, or “classical,” measurement error. Some evidence for this is available in the chapter on Canada and Japan, which presents data from a Canadian survey in which both firms and workers reported a separation reason. Interestingly, while the marginal totals in these cross-tabulations are roughly similar (for example, workers and firms both labeled about the same fraction of separations as firm-initiated), there are substantial off-diagonal elements (workers and firms often disagree on the cause of any given separation). Second, rather than reflecting fundamental differences in the source of the shock giving rise to the separation (see McLaughlin 1991), the labeling of separations as worker- versus firm-initiated may be an endogenous response to a nation’s labor market institutions. For example, the high share of layoffs relative to quits in the Canadian economy may reflect employers’ willingness, in the absence of experience rating, to label workers’ separations as layoffs in order to qualify them for employment insurance benefits (e.g., Kuhn and Sweetman 1988b). On the other hand, Bender, Dustmann, Margolis, and Meghir claim in Chapter 5 that in some European countries, “true” layoffs are relabeled as quits (presumably with some means of ensuring the worker’s cooperation) to avoid the many administrative complications involved in laying workers off. In Japan, a larger share of separations is labeled as voluntary than in any other OECD country (OECD 1997, Table 5.12). Abe, Higuchi, Kuhn, Nakamura, and Sweetman suggest in Chapter 3 that this may, in part, reflect a cultural reluctance to admit to a kind of “failure” on the worker’s part.

Third, languages, survey instruments, and the actual mix of mechanisms via which labor moves between firms all differ among countries. At a mundane level, perhaps because layoffs often give rise to discomfort in conversation—at least when they affect us personally—they encourage both slang and euphemism. The variety of such expressions, even among English-speaking countries, is remarkable: consider “getting the sack,” “getting canned,” “downsized,” “right-sized,” “outplaced,” “fired,” “dismissed,” “discharged,” “dislocated,” getting the “pink slip,” “redundancy” (common in the United Kingdom), and “retrenchment” (common in Australia). Japan has “forced vacations” and “*kata tataki*” (literally, “shoulder-tapping,” referring to the common method by which employees are informed of their layoffs). More to the point, “firing” or “dismissal” in North America typically means a worker has not performed his or her job duties adequately; thus we would not typically count workers reporting these separation reasons as displaced. In Europe it is much more common to use “firing” and “dismissal” to refer to employment adjustments for purely economic reasons. For example, a worker with less than enough job tenure to qualify for statutory redundancy pay in Britain—typically two years—might very plausibly report that he or she had been dismissed if the firm reduced employment due to a shortage of demand. Analyses that do not take account of such semantic differences may not be comparing similar classes of workers. Furthermore, international comparisons need to decide how to compare certain methods of shedding labor which only exist in some countries (such as U.S.-style temporary layoffs or Japanese *shukko*) with other kinds of displacements used in other countries.

Aside from nomenclature, there are a number of other issues involved in the definition of displaced workers that, if not carefully accounted for, can change the main results of a one-country study or reverse the sign of international comparisons. One, already mentioned, is whether to condition on tenure: are workers who lose very short jobs really “displaced”? In this volume we usually focus on high-tenure workers, but for all countries we also provide some results—especially for displacement *rates*—that do not condition on tenure in the job lost.

Related to the issue of very short jobs, how should we treat separations due to the end of a seasonal job or the expiration of a fixed-term

contract? The treatment of contract expirations is particularly difficult in the United States, as its Displaced Worker Survey does not include this as a possible separation reason. Presumably some of these workers classify themselves as layoffs (i.e., separated due to a “shortage of work”), and some choose the “other” category. As Farber (1997) has shown, this can substantially affect estimates of displacement and its consequences in the United States. In Chapter 4, Borland, Gregg, Knight, and Wadsworth document very different wage-change patterns in the United Kingdom for temporary contract expirations versus other involuntary separations. Because contract expirations constitute a substantial fraction of involuntary separations in Japan, the statistical picture of displacement rates in that country (see Chapter 3) is quite different depending on whether these workers are classified as “displaced” or not. When contract expirations are not separately identified in the data, presumably restricting attention to jobs with longer tenures will eliminate most of them from the sample. To our knowledge, however, no hard evidence on this point exists.

One response to uncertainty regarding the usefulness of survey reports of separation reasons is to use *ex post* criteria to identify separations we truly believe are involuntary. One option, used for some countries in this volume (for example, the United Kingdom), is to ask how the results change when we restrict attention to individuals who experienced a positive unemployment spell. While this, to some extent, conflates initial conditions with outcomes, it can be a useful descriptive device and sensitivity check. A more extreme version of this strategy is adopted in the Canada/Japan chapter, which experiments with the idea of defining displacements by their wage consequences. This is of course tautological when measuring the effects of displacement on wages but can be useful when making international comparisons of displacement rates. Specifically, by defining *displacement* as a separation which leads to a large decline in hourly wages, one can compare displacement rates among countries in a manner which is unaffected by any labeling conventions—whether these are induced by survey instruments, “cultural” predispositions against admitting a separation was involuntary, opportunistic relabelling to avoid or take advantage of government regulations, or semantic differences. Future comparative studies of employment dynamics might do

well to document the resulting statistic—the frequency of “substantial separation-induced wage losses”—in a greater number of countries.

Other definitions

Clearly, how one defines the population of interest is a major consideration in any international study of displaced workers. A number of other definitions can also have pivotal impacts in such studies, however, and deserve close scrutiny. One of these is the distinction between *establishments* and *firms*: when the definition of displacement involves closure or substantial shrinkage of one’s workplace, it can matter a lot if the workplace is defined as an establishment or a firm. In two of the four longitudinal employer databases used in this volume, the unit of analysis is the firm; in the other two, it is the establishment. Because a common practice, especially in European plant closures, involves the relocation of large numbers of employees to other branches of the same firm, this can substantially affect estimates of the consequences of displacement in addition to the overall displacement rate. Close attention to the firm/establishment distinction is therefore required in any comparative study of displacement or of labor market dynamics more generally.

In analyzing the employment and wage consequences of displacement, three more definitions can have major impacts on the results. One is the distinction between *jobless durations* and *unemployment durations*. Because most data sets do not make careful distinctions between the unemployed and those who respond to displacement by leaving the labor force, most existing studies of displacement are careful to label their findings as applying to total jobless durations. Clearly, however, the mean jobless spell in a sample can substantially exceed the mean unemployment spell, especially among populations, such as women and older workers, for whom labor-force withdrawal is more common. A second key distinction is between the *mean unconditional jobless duration* and *mean duration conditional on experiencing a positive spell*. In the existing literature, both are commonly reported without further modifiers as “jobless durations.” Especially in some European countries, where fewer than half of displaced workers experience any unemployment at all, this distinction can be crucial, and all the chapters in this volume pay it close attention.

Finally, there is the distinction between *wages* and *earnings*. Due to data limitations, many studies of the financial implications of displacement limit their attention to total earnings in a month, quarter, or year. (For example, Jacobson, Lalonde and Sullivan's influential 1993 study confined its attention to quarterly earnings.) Even though the earnings declines in these studies consist of a mixture of unemployment, involuntary part-time work, and hourly wage declines, they are often discussed as if they are directly informative about hourly wage rates.⁸ Yet, as Stevens (1997) showed, making this distinction can change one's results considerably. The chapters in this volume are particularly careful in this regard, and future comparative work on displaced workers would do well to repeat that care.

Control Groups

A third methodological lesson concerns the use of control groups. While it is not obvious how relevant control groups would be to the analysis of displacement incidence or postdisplacement unemployment durations, they have been advocated and used by a number of authors for the study of displacement-induced wage and earnings losses. It has been argued, for example, that simple before-after wage changes experienced by displaced workers will understate the wage losses caused by displacement because they fail to account for any predisplacement wage losses,⁹ or for any foregone postdisplacement wage growth experienced by displaced workers, relative to their nondisplaced peers. The use of a control group of nondisplaced workers, it is argued, can address both these problems.

Some of the analyses in this volume have access to a nondisplaced control group; others do not. What does the experience of the authors who *can* use controls indicate for the rest? Our analysis shows that, in most cases, simple "difference" estimates do indeed underestimate wage losses, though exceptions exist. In particular, because real wages of Belgian nondisplaced workers fell during the sample period (Chapter 6), their "difference in difference" wage loss estimates are smaller than the simple difference estimates. That said, however, in most cases the use of a control group does not substantially change the results unless (as is the case for Australia in this volume) the population under study consists largely of young workers, who tend to be in a high-

wage-growth life-cycle stage.¹⁰ One reason for this is our focus in this volume on wage changes over a one- or two-year period bracketing the displacement date. During this relatively short period, wage or earnings growth among continuously employed workers is not sufficient to make a large difference in most of the estimates. Second, we cannot detect any predisplacement wage losses in countries outside the United States. We conjecture that this is largely due to the fact that wage-setting institutions in those countries are more centralized: as both Bertola and Rogerson (1997) and Teulings and Hartog (1998) have argued, there is simply less room for downward wage adjustments at the firm level in many non-U.S. economies.

There are also some drawbacks to using control groups. One is that, unless the data allow one to separate the two effects, the analysis may conflate temporary layoffs and/or hours cuts before the layoff with wage reductions, thus yielding a less accurate estimate of, say, the amount of firm-specific capital lost due to displacement. Finally, unless one has a very large sample of displaced workers, regression specifications—such as Jacobson, LaLonde, and Sullivan’s—which estimate completely separate earnings paths for displaced workers and controls, do not easily allow one to estimate a large number of interaction effects with observable characteristics, like age, tenure, or gender.

False Firm Deaths

False firm deaths is a methodological problem that is specific to administrative-data-based studies of displaced workers. In such databases, firm (or plant) closures are typically identified by the disappearance of a firm (or plant) identification number from the data. This could of course involve the shutdown of a plant or firm, but the possibility also exists that such changes result from simple reorganizations, in which a plant, together with its entire workforce, is simply subsumed into a new firm and continues producing as before.

The administrative-data-based chapters in this volume make diverse attempts to deal with the false-deaths problem, as discussed in detail in later chapters. A common approach is to exclude from the analysis large groups of workers who move together from one dying firm and into the same new firm.¹¹ This will, of course, eliminate simple takeovers, but it will also eliminate cases in which the displacing

firm has gone through extraordinary efforts to secure reemployment of its workers at other firms (or, in the case of plant closures, relocated them to another of its plants). While such efforts may be uncommon in North America, they are more frequent elsewhere, especially in those jurisdictions where a “social plan” must be implemented for all mass layoffs. In the research conducted for this volume, the results for some countries changed dramatically after corrections for false firm deaths were introduced. Future research on displacement using administrative data sources would be well advised to take great care in addressing this issue.

Alternative Destinations for Displaced Workers

Our collective experience in this volume strongly suggests that more attention should be given to several possible “destination” states for displaced workers besides reemployment. The possibility of labor-force withdrawal has already been mentioned, but further disaggregation of this state, as well as of the reemployment state, is needed. For example, in Chapter 2, Abbring, van den Berg, Gautier, van Lomwel, van Ours, and Ruhm show that both early retirement and official “disability” are important consequences of displacement. Farber (1999) recently pointed out the importance of nontraditional forms of employment as transitional states in the adjustment to displacement.

In addition to painting a more complete picture of displaced workers’ experiences, consideration of alternative destinations has several key implications for research on displaced workers. One is a better understanding of existing empirical regularities; for example, how much of the observed effects of age and gender on jobless durations can be explained by induced early retirement or by labor-force withdrawal? Another is a clarification of the welfare impacts of displacement; I argue below that the *direction* of the effect of displacement on retirement is informative about the *size* of its effects on lifetime utility. Research on these aspects of displacement, whether in the United States or elsewhere, has only just begun.

Comparability and Regression Design in Cross-National Research

Finally, an interesting consensus emerged among the authors around the broader issue of how to design regression-based studies in the context of comparative labor market research. A common temptation in the quest for comparability between countries is to choose a set of covariates that constitutes the lowest common denominator, in other words, the subset of control variables that is 1) available, and 2) measured in roughly the same way in both countries.¹² While most chapters in this volume provide this kind of comparison, they also make a point of providing less-comparable results based on fuller, more theoretically appropriate specifications of unemployment or earnings-loss regressions in each country. For example, just because Japan does not include a tenure variable in the relevant survey does not mean we should present no Canadian results that control for tenure. Sometimes, ancillary information from other data sources (such as age-tenure distributions in the Canada/Japan case) can be used to provide supplementary insights into what the results would be if the fuller set of covariates were indeed available. International studies of labor markets, in general, should not focus only on lowest-common-denominator regression specifications.

WITHIN-COUNTRY PATTERNS: EXCEPTIONS AND UNIVERSALS

Some economic phenomena are so deeply rooted in human tastes or technologies that they are observed in all cultures or countries that have been examined.¹³ Are there any such universal patterns in the incidence and effects of displacement? In this section I examine the variation in three displacement-related phenomena—the incidence of displacement, the unemployment effects of displacement, and the wage effects of displacement—across four basic demographic attributes (gender, age, predisplacement job tenure, and skill level) in all 10 countries studied in this volume. I identify some “universals,” as well as some fascinating and significant exceptions, and speculate on the meaning of both.

It is worth emphasizing at the outset that my focus in this section is on the patterns of covariation, within countries, between demographic attributes and economic outcomes. Unlike the following section, which tries to describe and explain differences in levels among countries, this exercise is not affected by international noncomparabilities in data, as long as these noncomparabilities affect only outcome *levels*. In that sense, the results—because they implicitly difference out a fixed effect for each country—are more robust to differences in data collection techniques among countries than the results in the following section.

The patterns of displacement-related outcomes across gender, age, tenure, and skill groups found in this volume are summarized in Tables 1.4–1.15. The tables display both raw correlations and regression-adjusted correlations, together with a list of all the characteristics held constant in each regression. To facilitate access to other details underlying the results, I also list the source table from which each result is drawn. The absence of a country from any one of these tables, or “n.d.” in a cell in any of them, means the corresponding results are not available for that country. For each table, I pose a question for which the majority answer—if any—is “yes.” If there is no majority answer, I choose what seems to me the most likely *ex ante* hypothesis. In the summaries of regression results, “insignif.” means the coefficient was not significant at the 5 percent level; “no” means the coefficient was significant but opposite in sign to the question posed.

Who Is Displaced?

Patterns in the incidence of displacement are summarized in Tables 1.4 through 1.7. Looking first at Tables 1.4 and 1.5, it is immediately clear that displacement disproportionately affects men, and unskilled workers, in essentially all countries.¹⁴ To some extent this is surely a consequence of the greater cyclicity of industries, such as construction and manufacturing, where unskilled men are overrepresented. As noted, the craft-based organization of the construction labor market also plays a role in some countries. Also related to industry effects, men are disproportionately employed in “old economy” sectors which are in long-term secular decline in all advanced economies. Not all of the disproportionate incidence of displacement among men is attribut-

able to industry mix, however: this result persists in the two countries (the Netherlands and the United Kingdom) for which the authors can add industry controls to displacement-rate regressions. The higher level of unskilled male displacement, to some extent, may thus also reflect the continuing erosion of men's traditional advantages in the labor market and the increase in demand for skill that appears to pervade all industrialized labor markets.

Tables 1.6 and 1.7 shed additional light on the incidence of displacement by focusing on age and tenure effects. With one important exception—Japan—the message of these tables is essentially the same for all countries. First, simply comparing means for different age categories, young workers are more likely to be displaced than older workers.¹⁵ As the rest of Table 1.6, and all of Table 1.7 make clear, however, this is *not* a genuine age effect: when tenure is held constant, displacement rates are no longer correlated with age. Tenure, however, continues to affect displacement when age is held constant. Thus, for all countries except Japan, the probability of being displaced from a job declines with the amount of time one has spent in it. Precise causes of this phenomenon are unclear—it could reflect institutions such as inverse-seniority (last-in, first-out, or LIFO) layoff rules, or simply the likelihood that high-tenure workers are, on average, better matched to their current jobs—but the phenomenon itself appears to be universal outside Japan.

And what of the Japanese exception? This phenomenon is analyzed in considerable detail in Chapter 3. All the evidence suggests that 1) it is largely confined to men, and 2) it reflects a practice, dominant among large Japanese employers, of offering essentially total job security to newly hired young men. Japanese workforce adjustment, when required, then takes a number of forms, all of which disproportionately affect senior workers: a variable age of mandatory retirement, an essentially mandatory form of outplacement called *shukko*, and (especially in the last few years) simple layoffs.

Thus, with one exception—Japan—displacement is most common among young, unskilled men. Furthermore, the greater prevalence of displacement among the young outside Japan is wholly explained by their lower tenure levels.

Joblessness Following Displacement

According to Table 1.8, women experience more joblessness than men after displacement. This phenomenon is essentially universal among developed countries, and—as is the case for the skill, age, and tenure patterns discussed below as well—holds irrespective of whether we measure joblessness as the occurrence of a positive spell, as duration conditional on a positive spell, as unconditional duration, or as non-employment at a particular (postdisplacement) survey date. Whatever causes women’s postdisplacement joblessness to be higher in all countries is very likely related to the factors that cause women’s overall labor force participation rate to be lower than men’s in all countries. Among these are greater participation in child-rearing (thus a higher opportunity cost of working) and—of particular relevance to displaced women—greater geographic constraints on married women’s job search.

According to Table 1.9, unskilled workers experience more joblessness than skilled workers, with one statistically significant exception (Germany). The most likely cause of this almost-universal phenomenon is unemployment insurance, which in most cases provides much higher benefit replacement rates to low-wage workers. (Absent something like unemployment insurance, one might expect skilled workers to have higher durations as they tend to operate in more specialized labor markets.) In this regard, it is intriguing to note that Germany is one of only three countries in our sample where UI benefits are a fixed fraction of the predisplacement wage with no maximum, and is the *only* country where the “second-tier” benefit system that takes over when UI expires (in Germany’s case, *Arbeitslosenhilfe*; see Table 1.2) has the same feature.

Although (as we have seen) older workers are less likely to be displaced, Table 1.10 shows clearly that they suffer longer jobless spells when displacement does occur. Further, Tables 1.10 and 1.11 together show that, unlike our incidence results, this is *not* simply a tenure effect. For one thing, tenure does not have a uniform effect on durations when age is held constant; the effect is significant in five of the seven countries where we can run these regressions, but the coefficient is negative in two of these five cases. Second, age remains significant when tenure is held constant. Thus there does appear to be a “pure”

age effect on postdisplacement unemployment that is essentially universal among countries with very different labor market institutions and conditions.

What explains the universal effect of age on postdisplacement joblessness? On one hand, this could reflect something as basic as the biology of aging, making older workers, on average, less adaptable to change than younger workers. On the other hand, it could reflect the fact that a greater fraction of older workers' skills are specific to an occupation and industry,¹⁶ thus exposing them to a much "thinner" labor market. An older worker's optimal response to what are presumably lower offer-arrival rates in such markets might well be to spend longer searching for a job.¹⁷ Alternatively, since these results are for jobless durations (rather than unemployment durations), they could simply reflect greater labor-force withdrawal for older workers. Indeed, Abbring et al. demonstrate in Chapter 2 that displacement hastens retirement in the United States. At the same time, however, a number of analyses find higher jobless durations among older workers even when workers who leave the labor market are dropped from the sample. Thus, labor-force withdrawal cannot be the only explanation for this phenomenon.¹⁸ Finally, longer unemployment durations could be caused by the greater average wealth (and therefore higher reservation wages) of older workers, though if this were the case one might expect older displaced workers to experience smaller wage losses (see below).

What explains the very different correlations between predisplacement job tenure and postdisplacement unemployment among countries as noted above? This correlation can be calculated for 7 of the 10 countries under study and is positive or insignificant in the United States, Canada, the United Kingdom, and Denmark—all countries with relatively weak employment-protection laws (EPL), by international standards. In contrast, the correlation is negative and significant in Germany, Belgium, and France¹⁹—all countries with strong employment-protection systems. Notably, this correlation is positive *even when age is not held constant*: even though older on average, high-tenure displaced workers in these high-EPL countries experience less unemployment than low-tenure displaced workers.

It is tempting to see the effects of employment-protection systems in these statistics: by requiring lengthy notice periods and detailed

adjustment plans, German-style employment-protection systems appear to continue to shield high-tenure “insiders” from market forces even if their firm or plant closes down. (Low-tenure workers in general qualify for less job protection than high-tenure workers.) This interpretation is supported by a comparison of the overall incidence of unemployment of displaced workers among countries: in high-EPL countries, displaced workers are much more likely to avoid a spell of unemployment altogether than, for example, in the United States or Canada. We discuss the effects of EPL on unemployment further in the section on “Patterns among Countries” (p. 34).

In sum, looking at postdisplacement jobless durations, a number of very consistent patterns emerge among all the countries in our sample. In particular, the demographic groups which are most likely to be displaced are not always those who suffer the least, or the most, from displacement. In particular, young, low-tenure men are more likely to be displaced, but they experience less joblessness if they are displaced. The one group that suffers disproportionately on both dimensions is the unskilled: unskilled workers are more likely to be displaced and take longer to become reemployed after displacement than other workers. Unlike with older workers, who also have longer durations, this seems unlikely to be caused by a disproportionate level of industry- and occupation-specific skill. More likely, this is caused—at least in part—by the greater relative attractiveness of income-support programs available to unemployed low-wage workers: as noted earlier, in most countries, the UI benefit replacement ratio declines precipitously with predisplacement earnings.²⁰ Another contributing factor may be the ongoing decline in demand for unskilled workers throughout the industrialized world, though it is unclear why—in the absence of a social safety net income “floor”—this would be reflected in unemployment durations, rather than simply in lower wages.

Displacement-Induced Wage Losses

In the United States, much discussion has centered around the widely observed positive correlation between tenure and displaced workers’ wage losses (see, for example, Kletzer 1989; Ruhm 1991; and Topel 1990). It is now broadly recognized that this phenomenon could reflect either a causal effect of tenure (such as specific human-capital

accumulation or seniority-related implicit contracts) or simply differences in average match quality between low- and high-tenure workers.²¹ Less well known, however, is the fact that in the simplest of dynamic matching models where workers move to better matches as they age, the expected correlation between average match quality and tenure (holding age constant) is negative.²² Thus, under very reasonable assumptions, a positive partial correlation between tenure and wages implies a positive causal effect of tenure on wages. As Topel (1991) argued, we can thus be fairly sure that firm-specific skills (or some similar causal mechanism such as seniority-based implicit contracts or industry-specific skills)²³ is an important feature of the U.S. labor market.

Is this also true in other countries? According to Table 1.12, the answer is a qualified “yes.” Reasonably strong and statistically significant tenure effects are found in the United States, Canada, and the United Kingdom, but tenure effects are generally insignificant in the Netherlands, Belgium, and Denmark—the only other countries where results are available.²⁴ In the Netherlands and Denmark, this can quite plausibly be attributed to the very small samples of displaced workers for whom reemployment wages are observed. In Belgium, however, sample size is not a plausible explanation: estimates of tenure effects on wage losses are tightly bounded near zero. As noted in Chapter 6, this may reflect two features of Belgian wage-setting institutions. One feature is the relatively high level of union coverage and the centralized nature of wage bargaining: most workers are covered by industry-level wage contracts that affect all Belgian firms in their industry. The second feature is the portability of seniority across firms in the same union bargaining unit, i.e., covered workers changing jobs within an industry retain their seniority in the new firm. Both of these factors make it harder for individual Belgian workers to accept a wage cut in order to become reemployed and may contribute to their very low reemployment rates.

In sum, the positive correlation between tenure and wage losses observed in the United States is also seen in two other countries (Canada and the United Kingdom) with similar wage-setting institutions. It is not seen in a country (Belgium) with very different, much more centralized wage-setting institutions, which also happens to be the only other country with enough data to estimate a tenure effect with reason-

able precision in this volume. This does not, of course, negate the possibility of firm-specific capital accumulation in countries like Belgium, but it does imply that the effects of firm-specific capital on wages are, in general, mediated by a country's system of wage-setting institutions. It also reinforces the notion, pursued further below, that a substantial fraction of the large wage losses observed among high-tenure displaced workers in the United States, may be directly associated with two features of its decentralized wage-setting institutions: 1) partial union coverage and 2) a high level of overall wage inequality in the labor market as a whole.²⁵ Both these features are less characteristic of most European countries.

The effect of age on displaced workers' wage losses is summarized in Table 1.13. Compared with the tenure effects just discussed, these are much more robust and uniform across countries: *in all countries for which we have data, older workers experience greater wage losses.* Furthermore, with one exception (Denmark), this effect persists when we hold tenure constant. Like the results for unemployment, these results suggest a pure effect of age, perhaps working through changes in workers' adaptability.²⁶ Given the strength and robustness of these results, it is in a way surprising that pure age effects on displaced workers' wage losses have not received more attention in the economic literature. If adaptability is a function of age, there may be important, but as yet largely unexplored, effects of an economy's (or firm's) age structure on its ability to adapt to change. An exceptionally old workforce might, for example, help explain Japan's current difficulties in restructuring its economy. Counterbalancing this, however, Japan's FIFO layoff system may promote the survival and recovery of ailing firms by keeping them relatively young during sustained periods of downsizing.

Do men or women experience larger wage losses when they are displaced? Outside Japan, Table 1.14 shows either no gender difference in wage losses, or a larger (percentage) fall for women. The latter result echoes a finding in Crossley, Jones, and Kuhn (1994), who found larger losses for displaced women (at all tenure levels) in Canada and who argued that this might be caused by tighter geographic constraints on women's job-search activities. This geographical mobility hypothesis is supported by Gladden's (1999) research, which quantified the effect of differential geographic mobility on the gender wage gap in the

United States. The exceptional Japanese result may reflect its peculiar institution of FIFO layoffs, which—being largely confined to men—may make the pool of displaced men and women very different. One simple way in which this might be true involves age: laid-off Japanese men might be, on average, considerably older than laid-off Japanese women, who do not appear to participate in the FIFO layoff system. Since we have no regression results that control for age and gender simultaneously in Japan, we cannot rule this out as an explanation of this particular Japanese exception.

Table 1.15 examines the association between education and wage losses among countries. Unlike the case of jobless durations, there is no consistent or significant association. The presence of an education/joblessness link, but the absence of an education/wage loss link, is consistent with the notion that income-support programs play an important role in the jobless durations of less-skilled workers: while lengthening jobless spells, these programs should not depress reemployment wages and may, in fact, raise them if more job offers are sampled during the jobless spell.

Summing up, our analysis of tenure patterns in displacement-induced wage losses cannot rule out the notion that substantial losses of firm- or industry-specific human capital occur among high-tenure displaced workers in all countries. However, the analysis strongly suggests that other factors, in particular labor market institutions, also play a role in determining the wage losses experienced by displaced workers. In particular, it appears that predisplacement tenure affects displaced workers' wage losses *only* in countries with decentralized wage bargaining and high overall wage inequality. In different institutional environments (such as Belgium's), the wage-loss pattern seen in the United States, Canada, and the United Kingdom is not present. Our analysis also suggests that gender differences in job search play a role in the gender differential in wage losses due to displacement, though this effect can also be overridden by institutional factors, as in Japan. Finally, we document a universal positive correlation between wage losses and age in all countries examined. Given its ubiquity, it is surprising that this relationship has not received more attention in the literature. Further attention to the potential causes of this pure "aging" effect would seem to be warranted in future analyses.

Summing Up: Who Loses Most?

Pulling together the three outcome measures (frequency of displacement, unemployment effects of displacement, and wage effects of displacement) and the four dimensions of demographic variation (gender, age, tenure, and skill level), is there any one demographic group that is hurt most on all dimensions in all countries examined? Our answer to this question is summarized in Table 1.16, which distills the results of Tables 1.4–1.15 into one 4×3 table. In almost all cases, the patterns reported in Table 1.16 apply both to zero-order correlations (not holding any other characteristics constant) and to correlations that hold the other characteristics constant in a regression framework. Cases where a distinction needs to be drawn are highlighted below.

Overall, none of the demographic groups examined in Table 1.16 fares unambiguously worse on all dimensions in all countries examined, though unskilled workers come close: they experience more frequent displacement, more postdisplacement joblessness, and about the same (percentage) wage loss from displacement as do skilled workers. Thus, as Farber (1997) has pointed out, while displacement among skilled workers is increasing and is attracting more public attention in the United States, displacement both in the United States and in all other developed countries where evidence exists remains a phenomenon that disproportionately hurts the unskilled.

Older workers fare worse than younger workers after displacement in all countries, but they are less likely to be displaced in the first place in all countries except Japan. Thus, Japan—perhaps paradoxically a culture reputed to place exceptional value on respect for one's elders—is the only country in which older workers fare worse on all three displacement-related outcomes examined here.

Tenure effects are more complex than age effects. First, as for age, in all countries but Japan, high-tenure workers are less likely to be displaced than low-tenure workers. The effect of tenure on postdisplacement joblessness, however, appears to vary with the strength of the employment-protection legislation in a country: high-tenure displaced workers seem to have shorter durations in high-EPL countries and longer durations elsewhere. As expected, in the United States and other countries with broadly similar institutions, senior workers are less likely to be displaced but experience larger wage losses if they are.

In this volume, however, we have not been able to document seniority-related wage losses in countries with more centralized wage-setting institutions, and in one country (Belgium) we can rule out this phenomenon quite convincingly.

In all countries, men are more likely to be displaced than women, but (with the exception of Japan) women lose equal amounts or more when displaced. Overall, the most consistent patterns that emerge from Table 1.16 are that older workers, and unskilled workers, bear the main costs of displacement.

Other Within-Country Patterns

A careful reading of the chapters in this volume reveals four other cross-sectional patterns that, while not fitting neatly into the above framework of incidence, unemployment, and wages, nonetheless appear to be universal among countries.²⁷ One of these is negative duration dependence in the reemployment hazard. For all countries in which this statistic is available (the United States, the Netherlands, Canada, Japan, the United Kingdom, Australia, and France), workers who have been unemployed a longer time have a smaller chance of becoming reemployed than workers closer to the start of their unemployment spell. As is well known, this could be either a direct causal effect of unemployment (for example, skills may atrophy with time out of work, or workers may become depressed, discouraged, or ill) or a pure composition effect: workers who are most attractive to employers (on dimensions not measured by the econometrician) tend to be hired out of the pool of unemployed workers sooner than others. What the data firmly reject, however, is a model in which the predominant factor affecting durations is liquidity constraints: in such a model, unemployed workers become increasingly likely to accept jobs as their assets or UI benefit entitlements are used up during an unemployment spell. Increasing hazard rates are not seen in any country in any econometric specification in this volume.

Related to the declining-hazard phenomenon, the authors for four of the countries under study in this book (the Netherlands, France, Germany, and the United Kingdom) examined the correlation between (completed) unemployment durations and wages upon reemployment. In all four cases, this correlation is negative, even when we use

the predisplacement wage to control for unobserved heterogeneity in workers' abilities. While consistent with a story in which longer unemployment durations cause workers' skills to atrophy, this pattern could, of course, also be explained by selection effects. Indeed, as noted below, the low reemployment wages of German, French, Belgian, and Danish displaced workers who have been unemployed for more than a year appear to constitute the only observable gap in the safety net protecting displaced workers from adverse outcomes in those countries. The authors for the same four countries also compared the unemployment durations of displaced workers to those of workers experiencing other kinds of separations, such as voluntary quits. In a finding reminiscent of Gibbons and Katz's (1991) "layoffs and lemons" result (workers displaced *en masse* fare better than those displaced individually), it appears that—at least in the Netherlands, France and Germany—displaced workers fare *better* than other separators. This finding is particularly striking in the Netherlands, where displaced workers receive higher unemployment benefits than other separators and thus have a lower incentive to become reemployed. In the case of the Netherlands, the authors speculate that, once again, employment-protection laws may play a role; nondisplaced workers do not benefit from nearly as much relocation assistance as displaced workers. Further exploration of this distinction in countries such as the Netherlands with very generous EPL certainly seems warranted.

Finally, in only two countries did the authors pose the question, "Does displacement hasten retirement?" Before considering their answers, it is worth noting that, theoretically, the answer to this question is not obvious. While the wage loss associated with displacement among older workers creates a substitution effect away from continued work, an income effect could encourage later retirement: at a lower wage, people need to work longer to finance the same level of retirement income. Despite the possibility of these income effects, however, in both the Netherlands and the United States, displacement appears to hasten, not to delay, retirement. Either the income effects of displacement are unimportant for workers who are displaced late in life, or a combination of generous severance payments and social programs makes these income effects unimportant. The retirement-inducing effect of displacement thus offers some insight into the long-term effects of displacement-related wage losses on workers' well being. If

such effects were very severe, we would expect older displaced workers to delay, rather than to hasten, their retirement plans.²⁸

PATTERNS AMONG COUNTRIES

In this section I discuss what can be learned from patterns in the levels of various displacement-related phenomena among countries. As noted, these conclusions are more sensitive to differences in data collection methods among countries than those discussed in the previous section. Nonetheless, as I hope the discussion will show, attention to detail does allow some broad conclusions to be drawn.

Displacement Rates

Estimates of annual displacement rates taken from Chapters 2 through 6 are presented in Table 1.17. Because two alternative definitions of displacement are used in those chapters, these rates should be considered in two groups. In the first six countries (the United States, the Netherlands, Japan, Canada, the United Kingdom, and Australia) the statistics refer to total displacement rates—displacements of individual workers as well as mass layoffs and plant closures. In the remaining four countries (France, Germany, Belgium, and Denmark) they refer to persons displaced due to the closure of a firm or establishment, only.

Examination of the first six countries in Table 1.17 yields a result that some readers might find surprising: overall, total displacement rates are very similar among countries with very different labor market institutions. In fact, the annual rates for the United States, the Netherlands, Canada, the United Kingdom, and Australia are all between 4 and 5 percent per annum.²⁹ In Japan, if we restrict attention to “Western-style” layoffs only, its estimated displacement rate is much lower than all the other countries, at 1.2 percent. However, if we include in the count of Japanese displaced workers one source of job loss that is much more common in Japan than elsewhere (temporary contract expirations) plus another that is largely unique to Japan (mandatory retirements for which the timing is totally at the firm’s discretion), the

estimated displacement rate rises to 3.5 percent. This is not that different from the other five countries for which total displacement rates are available.

The remainder of Table 1.17 presents estimates of mass displacement rates for France, Germany, Belgium, and Denmark. Restricting attention to firm closures and to employees with more than three or four years of tenure yields annual displacement rates of 2.8 and 1.6 percent in France and Belgium, respectively.³⁰ Comparable estimates for plant closures are 1.0 and 0.6 percent for Germany and Denmark, respectively. In the United States, a little over a third of all displacements are due to plant closures.³¹ Given that high-tenure workers tend to have lower displacement rates (see Table 1.6), these figures seem roughly consistent with a 4- to 5-percent overall displacement rate as well (with the exception of France). While we remain unsure of the precise explanation for this French exception, we note that France is probably the country in which the false-firm-deaths problem is most severe. Individuals moving “together” into the same new firm can only be identified from sample information (rather than a census of the firm’s employees). Thus it is possible that the value of 2.8 percent per annum substantially overestimates France’s mass layoff rate.

How do we reconcile the rough similarity in displacement rates among countries with the popular notion that jobs are, on average, much less secure in the United States than in countries with strong employment-protection laws, like the Netherlands, or with a tradition of lifetime employment, like Japan? One point has already been made: at least in Japan, displacement is much more common than traditional statistics suggest if we account for the peculiar institutional features of involuntary workforce adjustment in that country. Two other considerations are also relevant to the Japanese case: first, displacement in Japan is concentrated among older workers, and women generally do not participate in the lifetime employment system. The former phenomenon makes the jobs of younger Japanese men much more secure than in the United States, and contributes to Japanese men’s very high age-specific mean tenure levels (see Chapter 3, Table 3.17), without necessarily reducing the total displacement rate. The latter raises Japan’s displacement rate substantially when women are included in the statistics.

Another point concerns the relationship between the displacement rates computed here and widely cited comparative estimates of unemployment inflows, such as those in Layard, Nickell and Jackman (1991, Chap. 5, Table 1) or OECD (1995, Table 1.9). These unemployment inflow rates are much higher in the United States than in most other OECD countries, but they differ from displacement rates for two very important reasons. First, unemployment inflow statistics generally include workers starting a temporary layoff spell. Temporary layoffs are much more common in the United States than most other countries and do not constitute displacements since the worker returns to his or her previous job. Second, as is shown below, when displacement occurs in the United States, it almost always results in an unemployment spell. The same is not true in a number of European countries, where a substantial majority of displaced workers never enter unemployment. As a result, similar European and U.S. displacement rates are quite consistent with a much lower unemployment inflow rate in Europe.³²

A final point regarding the perceived relative insecurity of U.S. jobs is that popular perceptions are influenced by the severity of displacement's effects, as well as by its frequency. As I shall argue below, the consequences of displacement do differ substantially among countries, and—at least for the case of wage losses—these are considerably more severe in the United States than most other countries.

What, if anything, does the international similarity in displacement rates imply about the relation between labor market institutions and outcomes? Perhaps, as noted, institutions can affect the precise *form* that displacement takes (for example, the distinction between “pure” layoffs and mandatory outplacements such as *shukko*). Perhaps, as I shall argue below, they can also have important effects on the *consequences* of displacement by providing generous outplacement assistance. But it may be that overall displacement rates are relatively immune to policy interventions. There could simply be a relatively fixed amount of labor reallocation that must occur in any dynamic capitalist economy.³³ At a minimum, we have yet to see convincing evidence of a modern capitalist economy with a total displacement rate very different from 4 to 5 percent per annum.

Incidence of Joblessness

International differences in the amount of joblessness experienced by displaced workers are summarized in Table 1.18. In sharp contrast to the evidence for displacement rates, there is wide variation among countries in the probability that a displaced worker experiences any joblessness. While U.S. analysts tend to assume (correctly, for their country) that involuntary termination almost always results in a jobless spell, this is not the case in some other countries. For example, Abbring et al. draw attention to the large difference in incidence of joblessness between U.S. and Dutch displaced workers. Using very similar samples and definitions, they calculate that only 30 percent of displaced workers in the Netherlands actually experience any joblessness, compared with 85 percent in the United States. A very low incidence of joblessness among displaced workers is also observed among workers involved in plant closures in Germany (39 percent) and in plant closures or shrinkages in Denmark (31 percent).³⁴

As is argued in several chapters of this volume, the most likely explanation for the low incidence of joblessness among displaced workers in some European countries is employment-protection legislation. This should not be surprising, since in many cases the intent of European EPL is to prevent displacement-induced joblessness. As Table 1.18 suggests, these apparent effects of EPL are most visible in the case of plant, rather than firm, closures (the French and Belgian statistics refer to firm closures and are not so low). It is in the case of plant closures that EPL is both strongest—involving all the provisions associated with mass layoffs such as a “social plan”—and most enforceable (enforcement problems naturally arise when the legal entity responsible for complying with the law ceases to exist). While further research is certainly warranted, the chapters in this volume strongly suggest that joblessness is *not* an inevitable consequence of displacement, and that—for better or worse—it is possible to design a system of employment-protection laws that makes joblessness the exception rather than the rule among workers displaced from dying plants.

Conditional Jobless Durations

Now suppose a displaced Dutch, German, or Japanese worker is unlucky enough to start a jobless spell. Is he or she likely to be jobless much longer than a U.S. worker in the same situation? Perhaps surprisingly, given the prevailing view of these labor markets as low-turnover and “sclerotic,” Table 1.18 provides only mixed support for this hypothesis. The conditional probability of long-term joblessness is substantially higher in Germany and France than in the United States. Given the much higher unemployment rates in these countries during the sample period, this is not surprising. More surprisingly, in the Netherlands, 28 percent of jobless spells experienced by displaced workers last more than a year, a fraction which is identical to that in the United States. Even more surprisingly, conditional durations in the United Kingdom are below those in the United States.

Why are U.S. displaced workers’ unemployment durations so unexpectedly high when viewed in an international context? To understand this phenomenon, at least three definitional and statistical points are relevant. First, recall again that the statistics in Table 1.18 exclude temporary layoff spells—which tend to be short—from the sample of jobless durations in all countries where they are a significant phenomenon (especially the United States and Canada, but also France). These short spells *are* included in most published estimates of comparative unemployment durations, which tend to show much shorter average spells in the United States. Second, note that Table 1.18 reports actual survivor rates (the fraction of displaced persons actually reemployed within 6 and 12 months of displacement) rather than, for example, estimated mean durations extrapolated from a sample of incomplete spells (as in Layard, Nickell, and Jackman 1991, Chapter 5, Table 1). As jobless durations tend to be very skewed, means tend to be much higher than the median; i.e., than the duration experienced by a typical individual, especially in European countries where the distribution has a long right tail. Even more importantly (in contrast to extrapolated means), our survivor function estimates do not depend on assumptions about the distribution of spells beyond the censoring point or on the assumption of a steady state.

Third, note that our numbers also differ from relatively well known statistics on the fraction of the stock of unemployed workers who have

been employed for over a year (e.g., OECD 1995, Table 1.8). These “stock” statistics vary much more among countries than ours do, but are unrepresentative of the experiences of a typical displaced worker in each country for a different reason. Stock statistics refer to the population of workers who are unemployed at a point in time. In contrast, our “flow” statistics refer to a random sample of new unemployment spells. Because long spells will (by definition) be overrepresented in a stock sample, such samples overstate the duration of unemployment a newly unemployed worker is likely to experience.

Aside from the above statistical issues, there may be a potentially important substantive reason why U.S. unemployment durations are so high. Consider a North American worker who is (*ex post*) permanently displaced. Compared to, say, a laid-off worker in Japan who has no prospect of returning to his or her original job, the North American worker may not search as intensively for a new job near the start of his or her spell because recall remains a possibility. Clearly, the effects of the North American temporary layoff system on the jobless durations of workers who are, ultimately, permanently displaced warrant further research.

Unconditional Jobless Durations

Combining incidence and duration, in which countries do displaced workers experience the most joblessness? This statistic is examined in the final three columns of Table 1.18 for the seven countries for which it is available. Three features stand out. First are the very low total jobless durations in Japan and the United Kingdom. Contrary to many popular discussions about the “thinness” of non-entry-level labor markets in Japan, by international standards displaced Japanese workers do *not* have long jobless durations, even when we exclude from the calculations those displacements taking place via mandatory outplacements (*shukko*) and even in 1995, when Japan was in a deep and prolonged recession. Furthermore, when we exclude temporary layoffs from the U.S. statistics, U.S. displaced workers in fact experience *more* joblessness than do the British. The second feature is the fact that total jobless durations in Canada, France, and Germany *do* exceed those in the United States. Among other factors, this could reflect a much more generous unemployment insurance system.

The third feature is the large gap between Belgian and Danish jobless durations, which is thoroughly documented and analyzed in Chapter 6 by Albæk et al. According to these authors, the only factor that can plausibly explain this differential between otherwise very similar countries is a negative effect of Belgium's very stringent employment-protection laws on the rate of new job creation. Certainly, Belgium has the most stringent advance-notice laws of the countries considered in this volume; it also has the highest total jobless durations among its displaced workers. Thus, despite their direct effect in reducing the incidence of jobless spells, it appears that very high EPLs can be counterproductive in combatting unemployment among displaced workers: their negative equilibrium effects on new job offer arrival rates can outweigh these direct effects.

In sum, rather than being at the low end of the scale, the United States is in the middle of the pack when it comes to the total amount of joblessness experienced by displaced workers. One reason for this is definitional: previous estimates of comparative unemployment durations underestimate the amount of unemployment experienced by U.S. displaced workers because they include the short durations of the many U.S. workers who are not displaced but are just on temporary layoff. Another may be a detrimental causal effect of the North American temporary layoff system on the jobless durations of workers who ultimately are permanently displaced: an expectation of recall might reduce search intensity. A third factor explaining the unexpectedly good unemployment "performance" of displaced workers in some European countries is related to the relatively large fractions of displaced workers who avoid unemployment altogether: strict EPLs prevent the inception of unemployment spells, raising the number of spells with an unconditional duration of zero. At the same time, however, EPLs—especially when they are very strict and legalistic, as in Belgium—may also play a detrimental role in the high conditional jobless durations experienced by displaced workers by reducing the equilibrium offer arrival rate.³⁵

Wage Changes

Table 1.19 presents estimates of mean percentage wage changes experienced by displaced workers, drawn from Chapters 2 through 6 of

this volume. In all cases these estimates are formulated to correspond as closely as possible to changes in the wage earned per hour of work (rather than total earnings during periods that could contain jobless spells) before and after displacement. Because—at least in some countries—displaced workers’ wage losses vary substantially with pre-displacement tenure, the table presents disaggregated results by individual years of tenure. (Where the only available results combine multiple years of tenure, I simply repeat the estimates in adjacent cells of the table.) Most estimates in the table consist of simple before/after mean wage changes for workers reemployed within a year or two of displacement. Where available, however, estimates of wage losses relative to a control group of continuously employed workers are also shown.

Even though Chapter 2 reports some estimates of wage losses for the Netherlands (see the discussion of Table 2.22), these are not included in Table 1.19. As the authors of that chapter indicate, their wage-change results are based on a very small sample, resulting in standard errors so high that no remotely plausible sizes of wage changes can be ruled out. Estimates from Australia (Table 4.17) are also excluded because they apply to a sample of very young workers and are thus not comparable with any of the other countries in Table 1.19. Because wage changes can differ between individual and mass layoffs, throughout the table I note whether the statistics refer to mass layoffs only or to the population of all displaced workers. To ensure that any conclusions involving wage changes in the United States are robust as to whether a sample of mass layoffs from administrative data or survey-based samples of individual layoffs are used, Table 1.19 also reports estimates from the well-known administrative-data-based study of U.S. displaced workers by Jacobson, LaLonde, and Sullivan (1993).

Among the broad patterns that emerge from an examination of Table 1.19, one has already been noted (see Table 1.14): real-wage changes associated with displacement become more negative (or less positive) with tenure on the lost job. Two other observations are primarily of methodological interest. First, in all countries but Belgium (and at all tenure levels within those countries), wage-change estimates that utilize a control group of continuously employed workers are more negative (or less positive) than estimates that do not use a control group. Evidently, in all those countries (even the United States, where

aggregate real wages had stagnated for decades) the typical nondisplaced worker experienced real wage growth during the sample period.³⁶ Displaced workers' own wage declines thus understate their losses relative to comparable workers who are not displaced. The exception is Belgium, where during our sample period continuously employed workers experienced real wage declines; here simple "difference" estimates *overstate* the wage losses "caused" by displacement. Second, in most cases—and especially in Germany, France, and Belgium—the disparity between the simple "difference" estimates and the "difference in differences" estimator is not large. Disparities of more than 5 percentage points are confined to the U.S. results of Jacobson, LaLonde, and Sullivan (1993)—where they are small relative to the size of the losses incurred—and the United Kingdom. Among the countries examined, the United Kingdom appears to be the only country where 1) continuously employed workers experience high rates of real wage growth during the sample period (of over 7 percent per year for continuously employed low-tenure workers and 3.5 percent for high-tenure workers), and 2) displaced workers experience measurable real wage declines. In general, however, because of real wage growth among continuously employed workers, simple "difference" estimates usually understate the amount workers lose as a result of displacement. However, because real wage growth in most economies during our sample period is not very large, they usually don't understate it by very much.

Turning to more substantive conclusions, consider first the wage changes experienced either by displaced workers with low levels of predisplacement tenure (say, under two years) or those experienced by displaced workers of all tenure levels combined. With one exception—again, the United Kingdom—the wage changes experienced by both these groups are either positive (as for short-tenure workers in the United States and Canada) or close to zero (as for the United States, Japan, and Canada overall). The small loss (or the gain) among short-tenure workers requires no explanation: most reasons displaced workers might experience substantial wage losses do not apply to very short-tenure workers. The small overall loss stems from a simple composition effect: because displacement rates decline sharply with job tenure (see Table 1.6), low-tenure workers will dominate in any representative sample of involuntary separations.

What of the British exception to this pattern? A closer examination of Table 1.19 shows that the British exception stems entirely from relatively large wage losses experienced by workers with under a year of tenure. Workers with between one and two years of tenure do fit the pattern noted above; furthermore, the inverted U-shaped relation between wage changes and predisplacement tenure observed for Britain is not found in any other country. Given this, I suspect that the real wage changes in the lowest tenure category in Britain may be related to the difficulties in measuring tenure in the British Household Panel Survey and to the resulting large number of missing observations on tenure there. I shall treat them as anomalous here and in what follows. If further research shows, instead, that they are genuine, it will be fascinating to try to understand what explains these high losses among a group for whom they are very rarely seen.

Next, it is hard not to notice the large number of positive entries in Table 1.19: U.S. displaced workers with tenure of under four years, Canadian displaced workers with tenure under one year, and apparently all German and French displaced workers experience a *mean* wage change that is positive. Apparently the large losses documented by Jacobson, LaLonde, and Sullivan (1993) for Pennsylvania workers are not universal; their focus on high-tenure workers (and, as I shall argue below, on workers in highly unionized industries in a relatively nonunionized, high-wage-inequality country, plus focus on quarterly earnings rather than on wage rates) explains much of their results.

Finally, consider high-tenure displaced workers. In contrast to the wage increases observed for many low-tenure workers, Table 1.19 indicates (as expected) that large mean wage losses *are* seen in some countries, in particular the United States, Canada, and—to a lesser extent—the United Kingdom. They are not observed in other countries, however.³⁷ What, then, is distinctive about the United States, the United Kingdom, and Canada? Recalling the discussion of labor market institutions earlier in this chapter, these are clearly the three countries with the most decentralized wage-setting institutions.³⁸ The likely effects of wage structure on displaced workers' wage losses are addressed most directly in Chapter 3 by Abe et al. Using very similar surveys and definitions, they document a much higher *variance* of displacement-induced wage changes in Canada than in Japan. This higher variance has dramatic implications for the amount of lifetime wage

security experienced by workers in both countries. For example, Table 3.21 indicates that an average 20- to 24-year-old employed Canadian man has a 4.7 percent chance of experiencing a separation that will cut his hourly wage rate by 30 percent or more. The comparable statistic in Japan is 0.8 percent. For men aged 35–39, the Canadian and Japanese probabilities are 1.7 percent and 0.2 percent, respectively. The much higher level of Japanese wage security cannot be primarily attributed to a lower permanent separation rate (Table 3.5). A substantial fraction of Japanese men's wage security thus derives from the much more compressed structure of wage changes they experience when they are displaced.

It is also well known that Germany, France, Belgium, and Denmark have much more compressed wage structures than the United States and Canada. This is in part due to higher collective bargaining coverage; in France's case the very high national minimum wage also plays a role. And according to Table 1.19, high-tenure displaced workers in these countries do not experience significant wage losses, either unconditionally or relative to continuously employed workers. Further, the fact that Jacobson, LaLonde, and Sullivan (1993) found large wage losses in the United States using administrative data on workers involved in mass layoffs implies that the low losses found in the above European countries are not just an artifact of a different data-collection scheme and displacement definition.³⁹

Blau and Kahn (1996b) convincingly demonstrated that international differences in overall wage inequality play a major role in explaining international differences in the gender pay gap. Overall, our findings in this section suggest that a similar mechanism may be at work for displaced workers: in countries with high wage inequality, senior displaced workers appear to experience larger wage losses. Relatedly—since partial union coverage in a nation contributes to high levels of wage inequality—both Jacobson, LaLonde, and Sullivan (1993, p. 703) and Kuhn and Sweetman (1988a) observed that a substantial portion of U.S. displaced workers' wage losses may in fact be attributable to the loss of union coverage upon displacement. Naturally, this is much less of a factor in countries where union coverage is almost universal.

Before leaving the subject of wage changes it is worth drawing attention to two exceptions to the phenomenon of small wage losses in

countries with compressed wage structures. In some sense, these are exceptions which, because of the special circumstances in which they occur, “prove” the rule. First, as Bender et al. point out, larger wage losses are observed in France and Germany when attention is restricted to the small minority of displaced workers who become unemployed and remain so for over a year. These workers appear to fall out of the protective net provided both by EPL and the compressed national wage structure. Second are Japanese men over the age of 50 or 55. These men essentially leave the “primary” labor market where jobs and wages are protected and take new “post-retirement” jobs in a very different sector of the labor market. They experience large wage losses because they switch from a primary to a secondary segment of the labor market, much like displaced Pennsylvania steelworkers who become reemployed in a nonunion, service-sector job.

Summary

In comparing the levels of displacement rates, postdisplacement unemployment, and displacement-induced wage losses among the countries under study in this volume, the following main conclusions emerge:

- 1) Although some difficulties remain in reconciling displacement counts from firm-based versus worker-based data, comparable worker-based data yields estimated displacement rates, which are surprisingly similar in all countries where they are available, of between 4 and 5 percent of the employed population each year. This occurs despite substantial differences in labor market institutions among countries. This phenomenon is *not* inconsistent with previous statistics showing very large differences in unemployment inflows among countries, and it is consistent with statistics showing similar rates of sectoral labor reallocation (Bertola and Rogerson 1997). Perhaps a certain rate of displacement is simply a necessary feature of a dynamic capitalist economy.
- 2) Given that a worker is displaced, the probability that he or she will experience any joblessness at all varies a great deal among countries. While over 80 percent of U.S. displaced workers

experience some joblessness immediately following displacement, experiencing a spell of joblessness is the exception rather than the rule in such countries as the Netherlands, Denmark, and Germany. The most likely explanations of this low incidence of unemployment are employment-protection laws and union negotiations of the terms surrounding layoffs, both of which employ a variety of means to forestall the inception of an unemployment spell.

Supporting evidence for the EPL explanation of low unemployment incidence among some European displaced workers comes from two sources. One is the effect of predisplacement tenure on postdisplacement unemployment, examined in Table 1.11: as noted, in France, Germany, and Belgium (no Dutch data on this question are available), high-tenure displaced workers actually experience less unemployment than low-tenure workers. In France and Germany this is true even if age is *not* held constant, even though older workers tend to have both higher tenures and longer unemployment spells otherwise. This is strongly suggestive of greater advance notice and reemployment assistance provided to high-tenure “insiders” in these economies. A second source of corroborating evidence is the observation that, in the Netherlands, displaced workers actually get reemployed faster than workers who voluntarily quit their jobs, even though quitters face unemployment insurance penalties. As Abbring et al. suggest, this may be attributable to extensive reemployment assistance required for displaced workers but not in the case of quits.

- 3) Given that a spell of joblessness has begun, its expected length also varies substantially across countries. Perhaps unexpectedly, these conditional durations are not lowest in the United States: the United States is in the middle of the pack. One reason for the unexpectedly high unemployment durations of U.S. workers is that previous comparisons may have been contaminated by the inclusion of temporary layoffs, which tend to have short durations, in the U.S. statistics. Also, the fact that many (*ex post*) displaced workers in the United States have a prospect of being recalled to their former firm may reduce their search intensity

relative to workers in countries where displacement is a more discrete phenomenon.

- 4) In all countries, the mean wage change experienced by a low-tenure displaced worker is close to zero or positive. Small losses are also observed when we consider all displaced workers as a group, not conditioning on tenure. In essence, this reflects the fact that all the countries under study have a casual labor market, in which displacement is frequent but relatively inconsequential for current wages. Displacements from such jobs tend to dominate flow samples of involuntary separations everywhere.
- 5) Large percentage wage losses are observed only for workers with high tenure levels, and only in countries (the United States, Canada, and the United Kingdom.) with relatively high levels of wage inequality and low rates of union coverage. Just as a compressed wage structure may reduce the size of gender-wage differentials (Blau and Kahn 1996b), it may also reduce the magnitude of the wage changes experienced by displaced workers. Relatedly, displaced workers in a partially unionized economy such as these may be more likely to experience wage losses due to the loss of union coverage upon displacement.

Overall, the cross-national experience suggests that—with the possible exception of Belgium and its especially inflexible employment-protection system—it is hard to pinpoint any large negative effects of the highly regulated labor markets of Europe and Japan on displaced workers. Instead, employment-protection laws appear to dramatically reduce the incidence of an unemployment spell among workers who lose their jobs involuntarily. Compressed national wage structures also appear to reduce the frequency of large, displacement-induced wage losses experienced by a country's labor force. And while jobless durations, conditional on starting a spell, do tend to be higher outside the United States, they are not dramatically so, especially when temporary layoffs are removed from the statistics.

Of course, this does not necessarily mean that strong EPLs, for example, are good for any country. It does mean, however—and again with the probable exception of Belgium—that researchers looking for evidence of major EPL-induced costs need to look somewhere other

than at displaced workers, perhaps at the labor market for new entrants, including immigrants, women, and students.⁴⁰ There is a sense in which, in some European labor markets, once one becomes an insider, one is always an insider. Even permanent job loss and the closure of one's workplace do not undermine the strong employment rights given to incumbents in these labor markets. This may harm new entrants, but that is one subject that is beyond the scope of this volume.

FUTURE DIRECTIONS

Clearly, much has been learned from the research in this volume, both about displaced workers and about comparative labor markets. Just as clearly, however, much more remains to be learned. Indeed, as was noted, and as is to be expected in a first attempt at international research on displacement, some of the key lessons from our work are methodological ones, which we hope will speed up and improve the work of others on these topics. Just what should future researchers in this area focus on, and how should they approach this subject matter? I consider these two questions in turn below.

One very worthwhile goal for future research would be to understand the "universals" uncovered in this volume. For example, pure age effects on wage losses and on unemployment following displacement are observed in every country under study. What is it about older workers that causes these larger losses? Can one disentangle "thinner" labor markets from shorter time horizons, higher wealth, or declining adaptability to change? Why are women's jobless durations almost always longer? Is it simply greater labor-force withdrawal connected with family responsibilities or are other factors (like geographical search constraints) at work? Careful studies, which pay close attention to empirical implications that distinguish these simple hypotheses from each other, can add a lot to our understanding of labor markets worldwide.

Another universal that could benefit from greater scrutiny is the finding of very small, or zero wage losses for the entire population of displaced workers in all countries. The contrast between this result and Jacobson, LaLonde, and Sullivan's could not be more stark, emphasize-

ing the point that their workers may be a very special case: they are high-tenure workers, in distressed, high-rent, highly unionized industries in a relatively nonunionized country, displaced during a major recession. Further research needs to carefully distinguish these special cases from the experiences of typical job losers, whose situations are not nearly as severe. Just when and where do large wage losses occur, and when do they not?

Second are the exceptions, especially Japan. In particular, what drives the apparent Japanese exception to the age profile of displacement? Can a FIFO layoff system be directly observed in Japanese firms? In what industries is it the strongest? How does it affect declining firms (does it keep them younger, thus helping to forestall their decline)? As the relative displacement rates of older workers in North America are beginning to increase, an examination of this Japanese practice might yield insights of relevance to North America as well.

Third is a deeper understanding of displaced workers' flows into labor market states other than reemployment: what is the role of early retirement, discouragement, disability, retraining, and other forms of nonparticipation? Not only are these flows interesting in their own right, they also yield insights into the welfare effects of displacement. Retirement behavior, for example, like consumption behavior, provides clues to the severity of displacement's effects on workers' permanent incomes.

Fourth is the need to draw closer links between labor market institutions and outcomes, links which, while highly suggestive, are of necessity drawn on a relatively preliminary and impressionistic basis in this volume. One key institution that deserves more comparative attention is the system of temporary layoffs in the United States and Canada. Does the prospect of recall reduce search? During a layoff spell with some (*ex ante*) probability of recall, how do workers update their priors about recall probabilities and adjust their search strategies? How does this system compare with one in which the break with the predisplacement employer is a sharper, more well-defined event? Other key institutions appear to be the level of union coverage and the degree of centralization in wage bargaining. This volume provides highly suggestive, though not yet conclusive, evidence that loss of union coverage and a decentralized wage-setting system explain much

of the large wage losses experienced by senior workers in the United States.

Fifth, more needs to be known about voluntary reemployment assistance provided by firms even in the absence of, or in excess of legislated benefits. Such help can take the form of arranging employee transfers, setting up interviews, providing outplacement consultants, and so forth. Is this more, or less common in jurisdictions with strong EPLs? If less common, then EPLs could simply be displacing voluntary assistance with little net effect. If more common, EPLs effects may be magnified by changes in assistance voluntarily provided by firms.

Most important, however, is the effect of employment-protection laws and compressed national wage structures on workers who are not displaced—who, after all, at least in any particular year constitute the vast majority of a nation's labor force. Of particular interest here are new entrants to the labor force, including young school-leavers, women reentering after childbirth or child-rearing, and immigrants. We have already shown, I believe quite convincingly, that for the most part, displaced workers benefit from strong EPLs and compressed wage structures. Demonstrating whether or not new labor market entrants are harmed by these practices—which may also make it more difficult to break into the labor market and become a protected “insider”—is a much harder question to answer. This is because the effects are indirect, working through changes in market prices and search frictions rather than directly on the groups specifically targeted by these laws. It is, however, the most important question left unanswered by this volume.

How should future studies conduct the above analyses? As our collective experience makes clear, such studies will need to pay close attention to institutional details, and not just those embodied in legislation and regulations. These nonlegislated institutions include labor unions, the organization of industrial labor markets, and accepted mechanisms of worker termination that differ among countries for apparently historical reasons. Future studies also need to pay excruciatingly close attention to definitional and, perhaps less expected, to linguistic issues. This is hard work, but, as this volume shows, it can be done. The results not only test existing hypotheses and preconceptions about how labor markets work, but they also yield new hypotheses to

be assessed in future work. It is my sincere hope that this volume will encourage others to embark on the difficult but rewarding path of comparative labor market research.

Notes

1. In this volume, Chapter 2, Table 2.3.
2. The United States imposes notice requirements only for *mass* layoffs, as defined in the Worker Adjustment and Retraining Notification Act (WARN).
3. For a recent example of such a study and a summary of various recent attempts to derive one-dimensional measures of the overall “strictness” of EPL across countries, see Heckman and Pages (2000).
4. For a recent example of the effects of displacement on health, see Gallo et al. (2000).
5. See, for example, Addison and Blackburn (1994).
6. See, for example, Tachibanaki (1996).
7. One might think a plant-closure-based sample would overestimate the severity of the consequences of displacement, as congestion effects in local labor markets might make it harder for each individual worker to become reemployed. As is now well known, however, Gibbons and Katz (1991) found the opposite: persons displaced individually fare worse. They attribute this to a “lemons” phenomenon in which individual layoffs serve as adverse signals about an employee’s productivity.
8. Any discussion of losses in firm- or industry-specific skills as a possible cause of displacement-induced wage losses implicitly makes this assumption.
9. See de la Rica (1995).
10. See also Kletzer and Fairlie’s 1999 study of displacement among young U.S. workers.
11. This is typically only possible in data sets where one has access to the full population of workers at each plant before it disappears. Identifying the size of a group of workers who move together when one only has a sample of workers raises some difficult sampling issues. This is the case with the data from France and is discussed in detail in Chapter 5.
12. A similar comment applies to the way in which variables are measured themselves; e.g., the number of occupational categories used or the time units in which wages are measured.
13. One example appears to be the effect of children on women’s labor-force participation rates. See, for example, Duleep and Sanders (1994).
14. The only exception to the gender pattern in incidence is in the simple means for Belgium; this applies to mass displacements only, and reverses sign when standard covariates are added to the regression. Some minor exceptions affect the education patterns (a U-shaped effect in France and a positive effect of job com-

- plexity in the Netherlands, when both education and the predisplacement wage are held constant).
15. There is a tendency for displacement rates to rise a small amount after age 50 or 55 in some countries, but no other country exhibits anything like the monotonic increase in age, starting at age 20, that is observed very clearly in Japan.
 16. Skills that are specific to a firm should be captured by the tenure coefficient.
 17. See van den Berg (1994).
 18. For example, these results hold for Dutch reemployment hazards in Chapter 2, where the sample consists only of workers engaged in active search; they also hold in Crossley, Jones, and Kuhn (1994), who remove workers who leave the labor market from their sample.
 19. In France, this is only the case for the probability of experiencing a positive spell of unemployment.
 20. Evidence in favor of this interpretation comes from a number of less-developed countries without a meaningful social safety net. Unlike the United States and Western Europe, unemployment rates are higher in these countries among skilled workers, perhaps because only they can afford to spend time in this activity. For further discussion, see Dickens and Lang (1995).
 21. Any differences in general ability that might exist between high- and low-tenure workers in a cross-section sample are “differenced out” when looking at wage changes experienced by displaced workers.
 22. See Topel (1991, p. 152). The intuition is that, while good matches last, it is also the case that (especially among more experienced workers) new matches will not be consummated unless they are especially good. Whenever the true causal effect of tenure is positive, the latter effect outweighs the former.
 23. Neal (1995) has noted that the tenure effect also captures industry-specific skills when (as is usually the case) tenure in the industry is not in the list of regressors.
 24. Hashimoto and Raisian (1985) presented estimates of stronger tenure effects on wages in Japan than in the United States. Teulings and Hartog (1998, chap. 1) presented estimates for 11 countries that are consistent with the patterns noted below (higher tenure effects in “noncorporatist” countries). Unlike the displacement-based estimates in this volume, however, all these estimates are based on cross-section data only, and thus do not correct for unobserved ability differences between workers of different tenure levels.
 25. See Teulings and Hartog (1998), Table 1 of the synopsis, and Chapter 5.
 26. If anything, human-capital theory predicts the opposite: older workers should invest less in new skills; to the extent they pay their own training costs, this should *raise* their starting wages on their postdisplacement jobs. As discussed for unemployment durations, however, older workers may have more specialized industry- or occupation-specific skills that are not completely captured by the tenure variables used here. Another explanation might be a composition effect related to induced retirement. For this to explain the age effect, however, it would have to be the case that among older workers, those with *low* reemployment wage

- prospects (relative to their previous job) are more likely to return to work. This seems unlikely.
27. At least this is true in the sense that, for every country in which the given correlation is reported in this volume, it has the same sign.
 28. This question has recently been addressed using consumption data by Stephens (1999) and by Browning and Crossley (2000).
 29. This assumes the Australian average for men (3.9) and women (5.2) together is between 4 and 5 percent.
 30. I focus on closures rather than substantial shrinkage of a firm's or plant's workforce for two reasons. One is comparability among all four countries: in France and Germany, the statistics on closures are the only ones reported. The more important reason is conceptual: estimated displacement rates due to firm or plant shrinkage in Belgium and Denmark may be substantially inflated by voluntary turnover during the window period, especially in small firms and plants.
 31. See, for example, Kuhn and Sweetman (1999, Table 1).
 32. Consistent with our interpretation, Bertola and Rogerson (1997) also noted a broad uniformity among countries in job turnover rates, despite large differences in unemployment inflows. They also made reference to employment-protection laws as an explanation for this pattern because they allow worker reallocation to occur without an intervening unemployment spell.
 33. One could counter, of course, that even if the total amount of labor reallocation is fixed, the share of such reallocation that is involuntary (from the worker's point of view) may not be. Bertola and Rogerson (1997), however, reported similar rates of labor reallocation (as measured by job turnover) among countries, and present theoretical arguments—related to wage compression—for why most reallocation is involuntary from the worker's point of view.
 34. It may be worth recalling (see Note 30) that the Danish data could be contaminated by normal turnover during the “window” period, especially in small plants. This is of much less concern in the German data, which restrict attention to plant closures only.
 35. Other European countries tend to have more flexible EPL provisions, which may be more sensitive to the circumstances surrounding each mass layoff or plant closure, because they involve case-by-case negotiations in the construction of a “social plan.”
 36. Recall that the control group wage-growth rates that are relevant here occur within cohorts, while most aggregate wage-growth statistics make comparisons among cohorts.
 37. The six percent loss (relative to controls) among Danish workers with three or more years of tenure is the most negative point estimate, but—due in part to the small sample size—the 95 percent confidence interval stretches from about –2 to –10 percent (see Chapter 6, Table 6.10B).
 38. These countries also tend to have the highest measured earnings inequality, especially at the low end of the earnings distribution—which is most relevant to the losses experienced by displaced workers. See, for example, the statistics on the

ratio between the 50th and 10th percentiles of the earnings distribution in OECD 1993, Table 5.2.

39. Another possible manifestation of the relation between wage-setting institutions and displaced workers' wage losses is alluded to in Chapter 4 by Borland et al. In many respects, the United Kingdom and Australia have similar labor markets, with low levels of employment protection and moderate levels of union membership. One big difference, however, is the very centralized system of wage-setting institutions embodied in Australia's awards system. Noting that they found substantial wage losses in Britain but not Australia, these authors speculated that wage-setting institutions may play a role. As they pointed out, however, the fact that their Australian sample was much younger could also have accounted for some, or all, of this difference.
40. These issues have recently been addressed for a sample of Latin American countries by Heckman and Pages (2000).

Table 1.1 Institutions Affecting Displacement: Employment-Protection Laws

Country	Justification for “economic” layoffs needed?	Mandatory advance notice: individual terminations	Mandatory advance notice: mass terminations	Mandatory severance pay	Consultation and other requirements
U.S.A.	No	None	2 Months	None	Inform local government of mass layoffs
Netherlands	Yes ^a —layoffs are by permit only	Workers under age 45: 0.25 month per year service Over age 45: 0.5 months per year service, up to 6 months Notice begins after permit issued	No special provision	Only in court cases	No special provisions
Japan	Yes—firms must demonstrate economic necessity and correct procedure	One month	No special provision	None	Extensive procedural requirements, including use of all reasonable alternatives (early retirements, cutting temporary and part-time employment) before layoffs of “regular” employees
Canada ^b	No	0.25 Month per year service, up to 2 months	Up to 4 months for layoffs of over 500 workers	0.25 Months per year of service, up to 6 months ^c	Notify local authorities of mass layoffs

U.K.	No	0.25 Month per year service, up to 3 months.	1 Month for layoffs of 20–100 workers 3 Months for layoffs of over 100 workers	0.25 Month per year of service between ages 22–40 0.375 Months per year of service between ages 41–60 ^d	No special provisions
Australia	No	0.25 Month in “typical” pre-1984 award “As soon as practicable” in TCR test-case award ^e	No special provision	0.5 Months per year service, up to 2 mo. (in TCR-test case awards only)	Restrictions on terminations specified in award settlements, which vary by occupation, industry, and state.
France	Yes—must demonstrate economic necessity to works council	0.5–2 Years service: 1 month >2 Years service: 2 months However, legal procedures <i>before</i> notice can be issued take from 1 to 2.5 months.	Same as for individual terminations	>2 Years service: 0.1 month salary per year of service >10 Years service: 0.17 month salary per year of service	Employer must offer a retraining option Workers have priority in future hiring for one year Must share accounting information with works council In larger layoffs, a “social plan” must be devised

(continued)

Table 1.1 (continued)

Country	Justification for “economic” layoffs needed?	Mandatory advance notice: individual terminations	Mandatory advance notice: mass terminations	Mandatory severance pay	Consultation and other requirements
Germany	Yes—layoffs of workers with > 6 months tenure prohibited unless shown to be “socially acceptable” ^f	Blue-collar workers: 1 month after 5 years 3 months after 10 years White collar workers: 3 months after 5 years 6 months after 10 years	No special legislation; may be negotiated in social plan	Generally negotiated in social plan	Works council must be consulted for all layoffs For large layoffs, council can demand a “social plan” For large layoffs, must inform local employment office
Belgium	No	Blue-collar workers: <20 years service: 1 month >20 years service: 2 months White-collar workers: <5 years service: 3 months 5-9 years: 6 months 10-15 years: 9 months, etc. (courts often award even longer notice periods for white-collar workers)	None	One month’s pay per year service in plant closures (less in mass layoffs)	In addition to statutory minima, notice periods for white-collar workers are affected by an extensive body of case law. Typical periods depend on age, specialization, tenure and wage, ranging up to 36 months

Denmark	No	White-collar workers only: increases with tenure to a maximum of 6 months	None	None	For mass layoffs: required to notify regional labor market board and negotiate with union
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NOTE: For comparability, any notice or severance requirements legislated in weeks have been converted to monthly amounts, at 0.25 months per week (exception: 26 weeks is converted as 6 months).

^a Employees on fixed-term contracts are exempt from these requirements.

^b Canadian legislation varies by province. Figures are for Ontario, which is the largest province and fairly typical (except for severance pay).

^c Only applies to Ontario (Canada's most populous province). Ontario has about 40% of the national population.

^d The maximum period of service for which severance is paid is 20 years. Statutory redundancy payments are free of income tax.

^e The TCR (Termination, Change, and Redundancy) test case award incorporated stronger restrictions on dismissals for the first time; its provisions have been adopted by only a minority of awards since 1984.

^f Since the 1985 Employment Promotion Act, nonrenewable limited-duration contracts of up to 18 months have been exempt from these requirements.

Table 1.2 Institutions Affecting Displacement: Unemployment Insurance Systems

Country	Qualifying period	Duration of benefits	Replacement rate	“Fallback” program	Comments
U.S.A. ^a	26 Weeks of work in past year	26 Weeks (plus 13 weeks extended benefits in years or states with high unemployment)	50–70% to a maximum Actual average replacement rate 30–40%	Means-tested welfare benefits available to single parents only Lifetime limit of 5 years	Very low takeup rate Quitters disqualified Benefits taxed as income
Netherlands	26 Weeks of work in past year	6 Months to 5 years, depending on employment history	70%	Universal social assistance; unlimited duration Disability insurance an attractive alternative to UI	Quitters disqualified High takeup Long-term unemployed must accept jobs below their previous “skill” level
Japan	26 Weeks of work in past year	90–300 Days; increases with age, years worked, and full time status	50–80%, depending on age and rate of pay, to a maximum	Universal welfare, unlimited duration	Some restrictions on quitters
Canada	10–20 Weeks of work in past year (decreases with local unemployment rate)	14–50 Weeks, increases with weeks worked and local unemployment rate	55%, to a maximum	Universal welfare, unlimited duration	Quitters disqualified Easy to requalify in successive years based on seasonal work. Higher takeup than U.S.A.

U.K.	2 Years continuous employment ^b	6 Months	Flat rate cash benefit (£48.25 in 1996)	<p>UA (means-tested unemployment assistance), based on household income; unlimited duration</p> <p>Long-term sickness benefit an attractive alternative to both UA and UI</p>	UA is often more generous than UI, especially if no other earners in the household. UA can include full rent and property tax subsidy.
Australia	None	Unlimited	Flat amounts based on family income, family size, and home ownership	Unemployment benefit itself acts as “the” welfare system.	Benefits low relative to average earnings
France	5 Alternative ways to qualify for different benefit durations, depending on work history in last 3 years	4 Months to 33 months, depending on employment history and age	57–75% of previous earnings (no maximum); benefit rate falls after an initial jobless period	RMI (Minimum Insertion Allowance): means-tested program	Seasonal workers disqualified

(continued)

Table 1.2 (continued)

Country	Qualifying period	Duration of benefits	Replacement rate	“Fallback” program	Comments
Germany	12 months in last 3 years (for AG or <i>Arbeitslosengeld</i>)	156–832 Days, depending on age and employment history	67% of previous net wage (60% for workers without children)	AH (<i>Arbeitslosenhilfe</i>) 57% of previous net wage (50% without children) Means-tested; unlimited duration.	12-Week waiting period for quitters
Belgium	None	Unlimited, though benefits reduced after one year of unemployment	Effectively, benefits are flat amounts based on family status and current income ^c	UI functions as the main social safety net	Search requirement rarely enforced
Denmark	6 Months work in previous year ^d	Essentially unlimited ^c	Up to 90% Declines with previous wage; Average replacement rate is 65%	Means-tested “social assistance” program (<i>bistandsydelse</i>) provides unlimited-duration benefits	UI funds administered by trade union; very wide coverage

^a Provisions vary by state. Typical parameters are presented.

^b Before 1988, insurance contributions could be credited during unemployment spells to maintain eligibility.

^c Official replacement rates are 60% in the first year of unemployment and 40% after that. In most cases these are made irrelevant by a higher flat benefit amount based on the family's "needs."

^d Some new entrants can qualify for UI based on apprenticeship training periods.

^e Throughout the sample period in this volume (until the early 1990s) unemployed workers could requalify for UI by participating in a public employment scheme.

Table 1.3 Institutions Affecting Displacement: Unions and Minimum Wages

Country	Union density and coverage	Bargaining level(s), contract extension	Minimum wages: jurisdic. and coverage	Minimum wages: levels
U.S.A.	Density 14.5% (1996) (10% in private sector) Coverage rates similar to membership	Firm-level bargaining No extension	State and federal; federal supersedes state if higher	\$5.15/hr (1997) (40% of average production worker wage)
Netherlands	Membership 26% (1990) ^a Coverage 75% (1998)	Industry-level bargaining, extensions common	National	14.01 Guilders (US\$7)/hr
Japan	Membership 24% (1998) Coverage similar	Firm-level bargaining, no extension	Prefectural minima, plus some industry minima within prefectures	4868 Yen/day (weighted regional minimum, 1995); 36% of mean contract wage
Canada	Membership 31% Coverage 34% (1997)	Firm-level bargaining, no extension	Provincial (with a small federal sector)	38% of mean manufacturing wage, 1994
U.K.	Membership about 30% Coverage 37% (1996)	Firm-level bargaining, no extension	None: wages councils were abolished in 1993	None
Australia	Membership 31% (1996) Coverage 80% ((1990) ^a	Industrial tribunals, with union and firm representation; set wage "awards" by occup. and industry	Awards pervasive; no other wage minima	Set by awards
France	Membership 10% (1996) Coverage 90% (1985) ^a	Industry-level bargaining, extension pervasive	National minimum wage	84% of mean industrial wage (1995)

Germany	Membership 32% Coverage 90% (1992) ^a	Industry- and regional-level bargaining, extension is pervasive	No minimum wage apart from (extended) union contracts	None
Belgium	Membership 51% Coverage 90% (1990) ^a	National-, industry-, and firm-level bargaining (pyramidal)	National minimum wage	Low national minimum, generally superseded by extended union contracts
Denmark	Membership 80–90% Coverage 75%	National bargaining, supplemented by firm-level agreements	No minimum wage apart from (extended) union contracts	None

^a Figures from OECD 1994, chart 5.1.

Table 1.4 Displacement and Gender: Are Men More Likely to Be Displaced?

Country	Comparing means		Using regressions		
	Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Yes	2.2	n.d. ^a	—	—
Netherlands	Yes	2.7	Yes	2.9	Age, tenure, educ., part-time, industry, firm size, wage, occup., job complexity
Japan	Yes, but small difference	3.8	n.d.	—	—
Canada	Yes, very much	3.8	n.d.	—	—
U.K.	yes	4.2	Yes ^b	4.3	Age, tenure, educ., part-time, industry, firm size, marital status, children
Australia	Yes	4.11	n.d.	—	—
Belgium	No, women more likely	6.4	Yes	6.5	Age, tenure, blue- collar, wage
Denmark	No difference	6.4	Yes	6.2	Age, tenure, blue- collar, wage

NOTE: The chapter on France and Germany presents results for men only and thus is excluded from this table.

^a n.d. = no data available.

^b But significant only for temporary contract expirations.

Table 1.5 Displacement and Skills: Are Unskilled Workers More Likely to Be Displaced?

Country	Skill measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Netherlands	Job complexity	Yes	2.7	No, ^a lower rates in less complex jobs	2.8, 2.9	Gender, age, tenure, educ., part- time, industry, firm size, wage, occup.
	Wage	n.d. ^b	—	Yes	2.8, 2.9	Gender, age, tenure, educ., part- time, industry, firm size, occup., job complexity
	Education	Yes (weak effect)	2.7	Insignif. ^c	2.8, 2.9	Gender, age, tenure, part-time, industry, firm size, wage, occup., job complexity
U.K.	Education	Nonmonotonic: lowest disp. in middle groups	4.2	Yes	4.3	Gender, age, tenure, part-time, industry, firm size, marital status, children

(continued)

Table 1.5 (continued)

Country	Skill measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
France	Education	n.d.	—	Lowest disp. in middle groups	5.3	Gender, ^d age, tenure
Belgium	B/W collar	Yes	6.4	Yes	6.5	Gender, age, tenure, wage
	Wage	Yes	6.4	Yes	6.5	Gender, age, tenure, blue-collar
Denmark	B/W collar	Yes	6.4	Yes	6.5	Gender, age, tenure, wage
	Wage	Yes	6.4	Yes	6.5	Gender, age, tenure, blue-collar

NOTE: Recall that France, Germany, Belgium, and Denmark focus on mass displacements only; others combine individual and mass displacements. No results on skills and displacement rates are available in this volume for the U.S.A., Australia, Japan, Canada, or Germany. For the United States, Farber (1997) showed that educated workers are much less likely to be displaced than other workers, though the relative displacement rates of these groups increased in the 1990s. For Canada, Picot, Lin, and Pyper (1997) showed that Canadians earning high wages are much less likely to be displaced than other Canadians.

^a No = coefficient significant at 5% level but opposite in size to question posed.

^b n.d. = no data available.

^c Insignif. = not significant at the 5% level.

^d Regressions run for men only.

Table 1.6 Displacement and Age: Are Younger Workers More Likely to Be Displaced?

Country	Comparing means		Using regressions		
	Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Yes	2.4	n.d. ^a	—	—
Netherlands	Yes	2.7	Small, nonmonotonic effect	2.9	Gender, tenure, educ., part-time, industry, firm size, wage, occup., job complexity
Japan	No, less likely	3.8	n.d.	—	—
Canada	Yes, but disp. rates rise after 55	3.8	n.d.	—	—
U.K.	Yes, but disp. rates rise after 55	4.2	Yes ^b	4.3	Gender, tenure, educ., part-time, industry, firm size, marital status, children
Australia	No effect	4.13	n.d.	—	—
France	Yes, but disp. rates rise after 50	5.2	No	5.3	Gender, ^c tenure, educ.
Germany	Yes, but disp. rates rise after 50	5.4	No	5.5	Gender, ^c tenure, educ., industry

(continued)

Table 1.6 (continued)

Country	Comparing means		Using regressions		
	Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Belgium	Yes, but not strongly	6.4	Yes, within displacing firms, otherwise no ^d	6.5	Gender, tenure, blue- collar, wage (but the tenure variable groups together all those with 6 or more years)
Denmark	No effect	6.4	Yes	6.5	Gender, tenure, blue- collar, wage (but the tenure variable groups together all those with 6 or more years)

NOTE: Recall that France, Germany, Belgium, and Denmark focus on mass displacements only; others combine individual and mass displacements.

^a n.d. = no data available.

^b But significant only for temporary contract expirations.

^c Regressions run for men only.

^d In a sample of all employed workers, displacement is correlated to age between ages 20 and 59. Teens have lower displacement rates and workers over 60 have higher rates than those 20–59.

Table 1.7 Displacement and Tenure: Are Low-Tenure Workers More Likely to Be Displaced?

Country	Comparing means		Using regressions		
	Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Yes, strongly	2.3	n.d. ^a	—	—
Netherlands	Yes, strongly	2.6	Yes	2.9	Gender, age, educ., part-time, industry, firm size, wage, occ., job complexity
Canada	Yes ^b	—	n.d.	—	—
U.K.	Yes, strongly	4.2	Yes	4.3	Gender, age, educ., part-time, industry, firm size, marital status, children
Australia	Yes	4.12, 4.13	n.d.	—	—
France	Yes, not strongly	5.2	Yes	5.3	Gender, ^c age, educ.
Germany	Yes	5.4	Yes	5.5	Gender, age, educ., industry
Belgium	Yes	6.4	Yes	6.5	Gender, age, blue-collar, wage
Denmark	Yes	6.4	Yes	6.5	Gender, age, blue-collar, wage

NOTE: Recall that France, Germany, Belgium, and Denmark focus on mass displacements only; others combine individual and mass displacements.

^a n.d. = no data available.

^b No results are reported in this volume for Japan or Canada. However, when jobs lasting under a year were excluded from the Canadian displacement counts, displacement rates fell dramatically.

^c Regressions run for men only.

Table 1.8 Joblessness and Gender: Do Displaced Women Experience More Joblessness?

Country	Jobless measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Prob. (positive spell)	n.d. ^a	—	Insignif. ^b	2.12	Age, tenure, educ., married, nonwhite, immig., closure, notice
	Duration of positive spells	Yes	2.10	Yes	2.12	Age, tenure, educ., married, nonwhite, immig., closure, notice
	Unconditional duration	Yes	2.10	Yes	2.12	Age, tenure, educ., married, nonwhite, immig., closure, notice
Netherlands	Duration of positive spells (insured unemployment)	Yes	2.16	Yes	2.17	Age, wage, married, urban, part-time, UI sanction
Japan ^c	Unconditional duration	Yes	3.11	n.d.	—	—
Canada	Unconditional duration	Yes	3.11	n.d.	—	—
U.K.	Duration of positive spells	n.d.	—	Insignif.	4.6	Age, tenure, qualif., married, children, part-time, occup., industry, industry declining, firm size

	Unconditional duration	n.d.	—	Insignif.	4.6	Age, tenure, qualif., married, children, part-time, occup., industry, industry declining, firm size
Australia	Survey date reemployment	Yes	4.12, 4.13	Yes	4.16	Age, educ., math and reading aptitude, unempl. rate in last occup.
Belgium	Duration of positive spells (insured unemployment)	n.d.	—	Yes	6.8	Age, tenure, last wage, white-collar
Denmark	Duration of positive spells (insured unemployment)	n.d.	—	Insignif.	6.8	Age, tenure, last wage, white-collar (very small sample)

NOTE: The chapter on France and Germany presents results for men only. Except where noted, durations refer to total joblessness following displacement, whether due to unemployment or labor-force withdrawal.

^a n.d. = no data available.

^b Insignif. = not significant at the 5% level.

^c For Japan and Canada, separate duration regressions were run for women and men, but no predictions at common values of the regressors were performed.

Table 1.9 Joblessness and Skills: Do Skilled Displaced Workers Experience Less Joblessness?

Country (skill measure)	Jobless measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A. (education)	Prob. (positive spell)	n.d. ^a	—	Yes	2.12	Gender, age, tenure, married, nonwhite, immigr., closure, notice
	Duration of positive spells	n.d.	—	Yes	2.12	Gender, age, tenure, married, nonwhite, immigr., closure, notice
	Unconditional duration	n.d.	—	Yes	2.12	Gender, age, tenure, married, nonwhite, immigr., closure, notice
Netherlands (predisp. wage)	Duration of positive (insured UI) spells	n.d.	—	Yes	2.17	Gender, age, married, urban, part-time, UI sanction
Japan (education)	Unconditional duration	n.d.	—	Insignif.	3.12	Gender, ^c age, firm size, part-time, industry, age-sex specific U/V ratio.
Canada (education, predisp. wage)	Unconditional duration	n.d.	—	Yes	3.13	Gender, ^c age, tenure, firm size, part-time, industry, region (only the predisp. wage is signif. for men; only education is signif. for women)
U.K. (qualifications)	Duration of positive spells	n.d.	—	Insignif.	4.6	Gender, age, tenure, occup., married, children, part-time, industry, industry declining, firm size
	Unconditional duration	n.d.	—	Insignif.	4.6	Gender, age, tenure, occupation, married, children, part-time, industry, industry declining, firm size

Australia (education)	Survey date reemployment	Yes	4.13	Yes	4.16	Gender, age, math and reading aptitude, unempl. rate in last occup. (aptitude scores are <i>not</i> significant)
France (education)	Duration of positive spells	n.d.	—	Yes	5.7	Gender, ^d age, tenure, year
Germany (education)	Duration of positive spells	n.d.	—	No ^e — educated workers have <i>longer</i> durations	5.9	Gender, ^d age, tenure, year
Belgium (predisp. wage)	Duration of positive (insured UI) spells	n.d.	—	Yes	6.8	Gender, age, tenure, white-collar
Denmark (predisp. wage)	Duration of positive (insured UI) spells	n.d.	—	Insignif.	6.8	Gender, age, tenure, white-collar (<i>very</i> small sample; positive point estimate)

NOTE: Recall that France, Germany, Belgium, and Denmark focus on mass displacements only; others combine individual and mass displacements.

^a n.d. = no data available.

^b Insignif. = not significant at the 5% level.

^c Separate regressions were run for women and men.

^d Regressions were run for men only.

^e No = coefficient significant at 5% level but opposite in sign to question posed.

Table 1.10 Joblessness and Age: Do Older Displaced Workers Experience More Joblessness?

Country	Jobless measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Prob. (positive spell)	n.d. ^b	—	Yes (borderline significant)	2.12	Gender, tenure, educ., married, nonwhite, immig., closure, notice
	Duration of positive spells	Yes	2.10	Yes	2.12	Gender, tenure, educ., married, nonwhite, immig., closure, notice
	Unconditional duration	Yes	2.10	Yes	2.12	Gender, tenure, educ, married, nonwhite, immig., closure, notice
Netherlands	Duration of positive spells (insured unemployment)	Yes	2.16	Yes	2.17	Gender, wage, married, urban, part-time, UI sanction
Japan	Unconditional duration	n.d.	—	Insignif.	3.12	Gender, ^c educ., firm size, part-time, industry, age-sex specific U/V ratio.
Canada	Unconditional duration	n.d.	—	U-shaped effect	3.13	Gender, ^c educ., firm size, part-time, industry, region (result also holds when tenure, union and wage are included)
U.K.	Duration of positive spells	n.d.	—	Yes	4.6	Gender, tenure, qualif., occup., married, children, part-time, industry, industry declining, firm size

	Unconditional duration	n.d.	—	Yes	4.6	Gender, tenure, qualif., occup., married, children, part-time, industry, industry declining, firm size
Australia	Survey date reemployment	Yes (mostly age 55–64)	4.13	Yes	4.16	Gender, educ., math and reading aptitude, unempl. rate in last occupation [becomes insignif. when year dummies included]
France	Duration of positive spells	n.d.	—	Insignif.	5.7	Gender, ^d tenure, educ., year
Germany	Duration of positive spells	n.d.	—	Yes	5.9	Gender, ^d tenure, educ., year
Belgium	Duration of positive spells (insured unemployment)	n.d.	—	Yes	6.8	Gender, tenure, last wage, white-collar
Denmark	Duration of positive spells (insured unemployment)	n.d.	—	Insignif.	6.8	Gender, tenure, last wage, white-collar (very small sample; point estimates suggest older workers do have longer durations)

^a Except where noted, durations refer to total joblessness following displacement, whether due to unemployment or labor-force withdrawal.

^b n.d. = no data available.

^c Separate regressions were run for women and men.

^d Regressions were run for men only.

Table 1.11 Joblessness and Tenure: Do High-Tenure Displaced Workers Experience More Joblessness?

Country	Jobless measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Prob. (positive spell)	n.d. ^a	—	Insignif.	2.12	Gender, age, educ., married, nonwhite, immig., closure, notice
	Duration of positive spells	Yes	12.0	Insignif.	2.12	Gender, age, educ., married, nonwhite, immig., closure, notice
	Unconditional duration	Yes	2.10	Insignif.	2.12	Gender, age, educ, married, nonwhite, immig., closure, notice
Canada	Unconditional duration	n.d.	—	Yes	3.13	Gender, ^c age, educ., firm size, part-time, industry, region (result also holds when union and wage are included)
U.K.	Duration of positive spells	n.d.	—	Yes	4.6	Gender, age, qualif., occup., married, children, part-time, industry, industry declining, firm size (but age controls are coarse and the only signif. effect is for tenure < 1 year)
	Unconditional duration	n.d.	—	Yes	4.6	Gender, age, qualif., occup., married, children, part-time, industry, industry declining, firm size (see comment above)

Australia	Survey date reemployment	Yes, but largely for tenure >10 years	4.12, 4.13	n.d.	—	—
France	Prob. (positive spell)	No ^d — unemployment spells <i>less</i> likely	5.6	n.d.	—	—
	Duration of positive spells	n.d.	—	Yes	5.7	Gender, ^e age, educ., year
Germany	Prob. (positive spell)	No ^d — unemployment spells <i>less</i> likely	5.8	n.d.	—	—
	Duration of positive spells	n.d.	—	No—senior workers have <i>shorter</i> durations	5.9	Gender, ^b age, educ., year
Belgium	Duration of positive spells (insured unemployment)	n.d.	—	No—senior workers have <i>shorter</i> durations	6.8	Gender, age, last wage, white- collar

(continued)

Table 1.11 (continued)

Country	Jobless measure	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Denmark	Duration of positive spells (insured unemployment)	n.d.	—	Insignif.	6.8	Gender, age, last wage, white-collar (very small sample; point estimates suggest senior workers have <i>shorter</i> durations)

NOTE: No information on tenure is available in the Netherlands or Japan. Recall that France, Germany, Belgium, and Denmark focus on mass displacements only; others combine individual and mass displacements.

^a n.d. = no data available.

^b Insignif. = not significant at the 5% level.

^c Separate regressions were run for women and men.

^d In a sample of all employed workers, displacement is correlated to age between ages 20 and 59. Teens have lower displacement rates and workers over 60 have higher rates than those 20–59.

Table 1.12 Wage Loss and Tenure: Do High-Tenure Displaced Workers Experience Larger Wage Losses?

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Weekly earnings	Yes	2.19	Yes	2.21	Gender, age, educ., married, nonwhite, immig., closure, notice, union
Netherlands	Monthly earnings, within an employment spell	n.d. ^b	—	Yes, though significant only for tenure under vs. over 1 year	2.22	Gender, age, educ., married, immig., spell length, type of displacement
Canada	Hourly wage rate	Yes	3.16	Yes	3.20	Gender, ^c age, educ., firm size (pre and post), industry change, visible minority, union (pre and post), province
U.K.	Weekly earnings	Yes, except lowest tenure category	4.8a	Yes, but signif. only in Table 4.10 (which includes all separation reasons)	4.9, 4.10	Gender, age, educ., change in firm size, industry decline, unempl. duration, part-time predispl., part-time postdispl.
	Weekly earnings, full-time–full-time only	Yes	4.8b	Yes, but signif. only in Table 4.10 (which includes all separation reasons)	4.9, 4.10	Gender, age, educ., change in firm size, industry decline, unempl. duration

(continued)

Table 1.12 (continued)

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Belgium	Daily wage rate	n.d.	—	Insignif., point estimates consistently show higher losses though	16.11	Gender, age, blue-collar, predispl. firm size, closure, region, occup. ^d
Denmark	Hourly wage rate	n.d.	—	Insignif.; point estimates consistently show higher losses though	6.11	Gender, age, blue-collar, predispl. firm size, closure, region, occup. ^d

NOTE: Data on predisplacement tenure are unavailable for Australia or Japan. Wage regressions for France and Germany do not interact wage losses with demographic characteristics.

In all cases the dependent variable is a wage measure (pay per unit of time worked), not an earnings measure. Details are as follows:

U.S.: Change in log wages, up to three years after displacement

Netherlands: Change in log wages, up to four months after displacement

Japan: Worker's self-reported percentage change in wages, within a year of displacement

Canada: (calculated) percentage change in wages, from actual pre- and postdisplacement wages up to 16 months apart

U.K. Change in log wages, one year after displacement

- ^a Australia: Change in log wages, one to two years after displacement
Belgium: Log hourly wage rate two years after displacement
Denmark: Log hourly wage rate two years after displacement
- ^b n.d. = no data available.
- ^c Separate regressions were run for women and men.
- ^d Regression is for postdisplacement wage, controlling for predisplacement wage.

Table 1.13 Wage Loss and Age: Do Older Displaced Workers Experience Larger Wage Losses?

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Weekly wages	Yes (except for oldest group)	2.19	Yes	2.21	Gender, tenure, educ., married, nonwhite, immig., closure, notice, union
Netherlands	Monthly earnings, within an employment spell	n.d. ^b	—	Insignif. ^c	2.22	Gender, tenure, educ., married, immig., spell length, type of displacement
Japan	Wage rate per month, excluding bonuses	Yes, though primarily among men	3.14	Yes	3.18, 3.19	Gender, ^d educ., firm size (pre and post), type of displacement (<i>shukko</i> vs. layoff), industry change, year

Canada	Hourly wage rate	Yes (both men and women)	3.15	Yes	3.20	Gender, ^d educ., firm size (pre and post), industry change, visible minority (result is robust to additional controls for tenure and union)
U.K.	Weekly wages	Yes	4.8 ^a	Insignif.	4.9	Gender, tenure, educ., change in firm size, industry decline, unempl. duration, part-time
	Weekly wages, full-time–full-time only	Yes	4.8 ^b	Yes	4.9	Gender, tenure, educ., change in firm size, industry decline, unempl. duration
Australia	Average weekly earnings	n.d.	—	Yes	4.19	Sex, educ., immigrant, full-time–part-time changes, unempl. rate in predispos. occup. ^e

(continued)

Table 1.13 (continued)

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Belgium	Daily wage rate	n.d.	—	Yes, but only over age 60	6.11	Gender, tenure, blue-collar, predispl. firm size, closure, region, occup. ^f
Denmark	Hourly wage rate	n.d.	—	Yes, but only over age 60	6.11	Gender, tenure, blue-collar, predispl. firm size, closure, region, occup. ^f

NOTE: Wage regressions for France and Germany do not interact wage losses with demographic characteristics.

^a See notes to Table 1.12 for more detail on the dependent variables.

^b n.d. = no data available.

^c Insignif. = not significant at the 5% level.

^d Separate regressions were run for women and men.

^e Sample includes quitters, with a dummy for displaced.

^f Regression is for postdisplacement wage, controlling for predisplacement wage.

Table 1.14 Wage Loss and Gender: Do Displaced Women Experience Larger Wage Losses?

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A.	Weekly wages	Yes ^b	2.19	Insignif.	2.21 (col. 3)	Age, tenure, educ, married, nonwhite, immig., closure, notice, union
Netherlands	Monthly earnings, within an employment spell	n.d.	—	Insignif.	2.22	Age, tenure, educ, married, immig., type of displacement
Japan	Wage rate per month, excluding bonuses	No ^c —men have bigger losses	3.14	n.d.	—	—
Canada	Hourly wage rate	Yes, but difference is small	3.15	n.d.	—	—
U.K.	Weekly wages	Yes	4.8a	Insignif.	4.9	Age, tenure, educ., change in firm size, industry decline, unemp. duration, part-time predisp., part-time postdisp.
	Weekly wages, full-time–full-time only	Yes	4.8b	Insignif.	4.9	Age, tenure, educ., change in firm size, industry decline, unempl. duration

(continued)

Table 1.14 (continued)

Country	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Australia	Average weekly earnings	No difference	4.18	Insignif.	4.19	Age, educ., immigrant, full-time-part-time changes, unempl. rate in predispl. occup. (sample includes quitters)
Belgium	Daily wage rate	n.d.	—	Yes	6.11	Age, tenure, blue-collar, predispl. firm size, closure, region, occup. ^d
Denmark	Hourly wage rate	n.d.	—	Yes	6.11	Age, tenure, blue-collar, predispl. firm size, closure, region, occup. ^d

NOTE: Wage regressions for France and Germany do not interact wage losses with demographic characteristics. For Japan and Canada, separate duration regressions were run for women and men, but no predictions at common values of the regressors were performed.

^a See notes to Table 1.12 for more detail on the dependent variables.

^b Men experience a small wage gain; women, a small loss.

^c n.d. = no data available.

^d Regression is for postdisplacement wage, controlling for predisplacement wage.

Table 1.15 Wage Loss and Skills: Do Skilled Displaced Workers Experience Smaller Wage Losses?

Country and skill measure	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
U.S.A. (education)	Weekly wages	n.d. ^b	—	Insignif. ^c	2.21	Gender, tenure, age, married, nonwhite, immig., closure, notice, union
Netherlands (education)	Monthly earnings, within an employment spell	n.d.	—	Insignif.	2.22	Gender, tenure, age, married, immig., spell length, type of displacement
Japan (education)	Wage rate per month, excluding bonuses	n.d.	—	No—educated workers have <i>larger</i> losses	3.18, 3.19	Gender, ^d age, firm size (pre and post), type of displacement (<i>shukko</i> vs. layoff), industry change, year

(continued)

Table 1.15 (continued)

Country and skill measure	Wage loss measure ^a	Comparing means		Using regressions		
		Result	Source (table no.)	Result	Source (table no.)	Variables held constant
Canada (education)	Hourly wage rate	n.d.	—	No real pattern, mostly insignif.	3.20	Gender, ^d age, firm size (pre and post), industry change, visible minority (result is robust to additional controls for tenure and union)
U.K. (education)	Weekly wages	Yes, though small effect	4.8a	Insignif.	4.9	Gender, tenure, educ., change in firm size, industry decline, unempl. duration, part-time predispl., part-time postdispl.
	Weekly wages, full-time–full-time only	Yes, large effect	4.8b	Insignif.	4.9	Gender, tenure, age, change in firm size, industry decline, unempl. duration

Australia (education)	Average weekly earnings	n.d.	—	Insignif.	4.19	Gender, age, immigrant, full-time–part-time changes, unemployment rate in predispl. occup. ^c
Belgium (white-collar)	Daily wage rate	n.d.	—	Yes	6.11	Gender, age, tenure, predispl. firm size, closure, region, occup. ^f
Denmark (white-collar)	Hourly wage rate	n.d.	—	Insignif.	6.11	Gender, age, tenure, predispl. firm size, closure, region, occup. ^f

NOTE: Wage regressions for France and Germany do not interact wage losses with demographic characteristics.

^a See notes to Table 1.12 for more detail on the dependent variables.

^b n.d. = no data available.

^c Insignif. = not significant at the 5% level.

^d Separate regressions were run for women and men.

^e Sample includes quitters, with a dummy for displaced.

^f Regression is for postdisplacement wage, controlling for predisplacement wage.

**Table 1.16 Who Loses Most from Displacement across Countries?
The Effects of Gender, Age, Tenure, and Skill Level**

Attribute \ Outcome measure	Frequency of displacement	Postdisplacement joblessness	Displacement-induced wage losses
Gender	Men	Women	Either no difference, or women lose more (except Japan)
Age	Young (except Japan ^a)	Old	Old
Tenure	Junior ^a	Varies	Senior (in U.S.A., U.K., and Canada)
Skill level	Unskilled	Unskilled	No consistent difference

^a In Japan, no tenure information is available. Thus, we cannot say whether the observed age effect is purely due to lower tenure among the young, or what the “true” tenure effect is.

Table 1.17 Annual Displacement Rates: International Comparisons

	Data source	Displacement definition	Period	Population	Rate (annual)	Source (table no.)	Comments
U.S.A.	Retrospective survey of persons	Self reports of permanent job loss due to plant closing, slack work, position or shift abolished (includes individual <i>and</i> mass layoffs)	1993–95	Tenure greater than 1 year, age 20–64	4.9 (M) 4.3 (F) 4.6 (A)	2.3	
				All tenure levels, age 20–64	5.3 (M) 4.5 (F) 4.9 (A)		
Netherlands	Panel survey of firms	Firms’ reported layoffs, <i>plus</i> : workers moving into new jobs, early retirement, or disability insurance from firms shrinking 30% or more	1993–95	All tenure levels, age 60 and under	4.2 (M) 4.0 (F) 4.1 (A)	2.6	Layoffs during probationary periods excluded
Japan	Panel survey of firms	Firms’ reports of separations due to “management convenience” only	1995	Establishments with more than 5 employees, tenure one month or more, age 15 and over	1.3 (M) 1.1 (F) 1.2 (A)	3.8	—

(continued)

Table 1.17 (continued)

	Data source	Displacement definition	Period	Population	Rate (annual)	Source (table no.)	Comments
		“Management convenience,” plus “contract finished,” plus mandatory retirement	1995	Establishments with more than 5 employees, tenure one month or more, age 15 and over	3.7 (M) 3.2 (F) 3.5 (A)	3.8	
Canada	Administrative data on separations (from Employment Insurance system)	Firms’ reports of layoffs (separations due to “short work,” excluding workers who return to the original firm within a year)	1995	Establishments with more than 5 employees, tenure one month or more, age 15 and over	6.1 (M) 3.4 (F) 4.9 (A)	3.8	Same list of industries as Japan May include a small number of fixed-term contract expirations
U.K.	Household panel survey	Workers’ reported job loss due to redundancy or dismissal	1990–96	Age 18 to pensionable age	6.4 (M) 2.9 (F) 4.7 (A)	4.1, 4.2	Includes a small number of dismissals for cause
Australia	Labor mobility survey, 1995	Workers ceasing a job due to retrenchment, ill health, seasonal or temporary job ended.	1995	Employed workers	5.2 (M) 3.9 (F)	4.12	

France	Panel of administrative social security records	Workers separating from dying <i>firms</i> , in the calendar year of “death” and the preceding calendar year Adjustments made for “false firm deaths” Excludes workers who return to original firm within a year	1984	Age 25–50, tenure at least 4 years	2.8 (M)	5.2	
Germany	Panel of administrative social security records	Workers separating from dying <i>plants</i> , in the calendar year of “death” and the preceding calendar year Adjustments made for “false firm deaths”	1984–90	Age 25–50, tenure at least 4 years	1.0 (M)	5.4	Value is calculated by dividing the 7-year displacement rate in Table 5.4 (for 1984–1990) by 7
Belgium	Panel of administrative social security records	Workers separating from dying <i>firms</i> in the calendar year of death (firms with at least 5 employees)	1983	Tenure at least 3 years All tenures	1.6 (A) 2.1(A)	6.3	Adjustments made for “false firm deaths,” excludes public sector

(continued)

Table 1.17 (continued)

	Data source	Displacement definition	Period	Population	Rate (annual)	Source (table no.)	Comments
Denmark	Panel of administrative records	Workers separating from dying <i>plants</i> with at least 5 employees in the calendar year of death	1988	Tenure at least 3 years	0.6 (A)	6.3	Adjustments made for “false firm deaths,” excludes public sector
				All tenures	1.6 (A)		

NOTE: (M) denotes “men”; (F), “women”; and (A), “all workers.”

Table 1.18 Postdisplacement Unemployment: International Comparisons

Country (displacement type)	Population	Probability of a positive jobless spell		Jobless durations conditional on a positive spell		Unconditional jobless durations		Source for durations
		Result	Source (table no.)	Prob. ^a jobless after 6 months	Prob. jobless after 12 months	Prob. jobless after 6 months	Prob. jobless after 12 months	
U.S.A. (all layoffs)	All tenure levels	0.84	2.3	n.d.	n.d.	n.d.	n.d.	—
	Tenure greater than 1 year	.0.85	2.3	0.39	0.28	0.33	0.24	Tab. 2.10
	Tenure 3 years or more ^b	n.d.	—	0.41	0.30	0.34	0.25	Tab. 2.10
Netherlands (all layoffs)	All tenure levels	0.30	2.13	0.45	0.27	n.d.	n.d.	Tab. 2.18
	Tenure greater than 1 year	0.30	2.13	n.d.	n.d.	n.d.	n.d.	—
Japan (all “layoffs” ^c)	All tenure levels	n.d.	—	n.d.	n.d.	0.23 (M) ^d 0.25 (F)	0.14 (M) 0.11 (F)	Tab. 3.11
Canada (all layoffs)	All tenure levels	n.d.	—	n.d.	n.d.	0.47 (M) 0.68 (F)	0.0 (M) 0.41 (F)	Tab. 3.11 ^e
U.K. (all layoffs)	All tenure levels	0.63	4.5	0.32	0.19 ^f	0.20	0.12 ^f	Tab. 4.5

(continued)

Table 1.18 (continued)

Country (displacement type)	Population	Probability of a positive jobless spell		Jobless durations conditional on a positive spell		Unconditional jobless durations		Source for durations
		Result	Source (table no.)	Prob. ^a jobless after 6 months	Prob. jobless after 12 months	Prob. jobless after 6 months	Prob. jobless after 12 months	
Australia (all layoffs)	All tenure levels, young workers only	n.d.	—	0.27	0.15	n.d.	n.d.	Tab. 4.14
France (firm closures only)	Tenure at least 4 years	0.78 (M)	5:6	0.62 (M)	0.45 (M)	n.d.	n.d.	Fig. 5.1 ^g
Germany (plant closures only)	Tenure at least 4 years	0.39 (M)	5:8	0.52 (M)	0.40 (M)	n.d.	n.d.	Fig. 5.2 ^g
Belgium (firm shrinkage and closures only)	Tenure at least 3 years	0.65 ^h	6:6	n.d.	0.63 (M)	0.72 (M)		Tab. 6.7
Denmark (plant shrinkage and closures only)	Tenure at least 3 years	0.31 ^h	6:6	n.d.	0.28 (M)	0.37 (M)	n.d.	Tab. 6.7

^a n.d. = no data available.

^b Simple average of the proportions for 3–4, 5–9, and more than 10 years, respectively (these do not differ markedly).

^c Includes mandatory retirement and contract expirations; does not include *shukko*.

^d M = men; F = women.

^e Simple average of the “Canada A” and “Canada B” figures (these do not differ markedly).

^f After 10 months.

^g Numerical estimates based on figure.

^h Percent experiencing *any* unemployment in the three years after displacement.

Table 1.19 Displacement-Induced Percentage Wage Changes: International Comparisons

Country (displacement type)	Relative to controls? ^a	Tenure on the predisplacement job (years) ^b												All	Source	
		<1	1	2	3	4	5	6	7	8	9	10+				
U.S.A. (all displacements)	No		+11	+11	5	5	0	0	0	0	0	0	-19	0	Tab. 2.19	
U.S.A. (mass, from Jacobson, LaLonde, and Sullivan 1993) ^c	No														Fig. 1 ^c	
	Yes							-23	-23	-23	-23	-23	-23	-30		
Japan (all) ^d	No														-4(M) ^e 0(F)	Tab. 3.14
Canada (all)	No	3(M) 1(F)	0(M) 1(F)		-5(M) -8(F)		-5(M) -4(F)	-5(M) -4(F)	-5(M) -4(F)	-5(M) -4(F)	-5(M) -4(F)	-5(M) -4(F)	-11(M) -7(F)	-1(M) -2(F)	Tab. 3.16	
U.K. (all) ^f	No	-6	1	-5			-6								-4	Tab. 4.7, 4.8b
	Yes				-5	-5		-6	-6	-6	-6	-6	-6	-6		
France (mass)	No					17	17	12	12	14	14	10	10		Tab. 5.10	
	Yes					12	2	8	8	10	10	7	7			
Germany (mass)	No					2	2	6	6	6		2	2		Tab. 5.11	
	Yes					-1	-1	2	2	-3	-3	-1	-1			

(continued)

Table 1.19 (continued)

Country (displacement type)	Relative to controls? ^a	Tenure on the predisplacement job (years) ^b												Source
		<1	1	2	3	4	5	6	7	8	9	10+	All	
Belgium	No				-6	-6	-5	-6	-6	-6	-6	-6	-6	Tab. 6.10b,
	Yes				-3	-3	-2	-3	-3	-3	-3	-3	-3	6. 11 ^g
Denmark (mass)	No				-1	-1	-1	-1	-1	-1	-1	-1	-1	Tab. 6.10,
	Yes				-6	-6	-6	-6	-6	-6	-6	-6	-6	6.11 ^g

NOTE: In all cases, percentage change estimates condition on reemployment and measure rates of pay per unit of time worked.

^a In all cases where a control group is used, it is continuously employed workers (not necessarily in the same plants as the displaced workers).

^b <1 = less than 1 yr.; 1 = at least a year but less than two years; 2 = at least 2 yrs. but less than 3 yrs; and so forth.

^c Estimates are from Jacobson, LaLonde, and Sullivan (1993) for 3 years after displacement, for workers with positive earnings in each year after displacement. Percentage changes are based on the following estimates taken from their Figure 1: mean (quarterly) predisplacement earnings of \$6,000 for both displaced workers and controls; mean quarterly earnings six years after displacement of \$4,600 for displaced workers and \$6,600 for controls.

^d Excluding *shukko*.

^e M and F indicate statistics for men and women respectively. Wage-change statistics for Australia (Chapter 4, Table 4.17) are not included because they apply to a sample of very young workers only and are not directly comparable. Wage-change statistics for the Netherlands (provided in the discussion of Table 2.22, Chapter 2) are not included because they are based on a very small sample. While some of their estimates are large in magnitude, none are significantly different from zero.

^f Full time in both the predisplacement and postdisplacement job.

^g Figures calculated from sample means in Table 6.10b (for wages two years after displacement) in combination with regression coefficients in Table 6.11. The estimates with controls simply subtract off the two-year earnings growth of continuously employed workers reported in Table 6.10b. Due to the very small sample size and the resulting high standard errors on Table 6.11 tenure coefficients, I report here only the totals for all workers in their sample for Denmark.

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