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ABSTRACT

This monograph provides empirical evidence of the effects of advance notice. Chapter 1 summarizes theoretical arguments for and against plant closing legislation and the evidence of the extent to which advance notice currently is provided to displaced workers in the United States. Chapter 2 presents a summary of prior empirical research on the effects of legislated, privately bargained, and voluntarily provided advance notice of displacement in the United States and Europe. The next four chapters contain new empirical research, which uses the Bureau of Labor Statistics 1984 Survey of Displaced Workers and other data sources. Findings are that having advance notice appears to reduce the probability that a displaced worker will suffer any spell of unemployment, but that it has no effect on the individual's duration of nonemployment if he/she becomes unemployed or on the individual's earnings if he/she becomes reemployed. Contrary to concerns of critics of advance notice, no evidence is found that advance notice leads a firm's most productive workers to quit prior to their planned displacement date. Chapter 7 discusses implications of these findings for public policy toward displaced workers. It addresses inducements that the federal government might use to encourage employers to provide advance notice voluntarily and types of research that should be undertaken to help evaluate the effectiveness of the new federal legislation. (YLB)

Advance Notice Provisions in PLANT CLOSING Legislation

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The facts presented in this study and the observations and viewpoints expressed are the sole responsibility of the author. They do not necessarily represent positions of the W. E. Upjohn Institute for Employment Research.

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Preface

In July of 1988, Congress passed the *Worker Adjustment and Retraining Notification Act*, which requires employers of 100 or more workers to provide workers and local government officials with 60 days advance notice before they shut down or make large scale layoffs. Although legislation calling for advance notice had been active in Congress every year since 1979, 1988 represented the first year that advance notice legislation passed both houses of Congress, and President Reagan, although philosophically opposed to the legislation, bowed to political pressure and did not veto it.

Currently, Canada and most European nations have some form of legislation relating to plant closings or large scale layoffs. A few states in the Untied States also have their own legislation. Debate over advance notice legislation has been highly emotional, and little substantive empirical evidence existed to help guide policymakers. Our monograph represents a comprehensive treatment of the subject, and we hope it will contribute to future policy debate.

After summarizing the theoretical arguments for and against plant closing legislation and the evidence on the extent to which advance notice currently is provided to displaced workers in the United States in chapter 1, we summarize the results of prior empirical research on the effects of advance notice in chapter 2. The next four chapters contain our own new empirical research, which makes use of the Bureau of Labor Statistics 1984 *Survey of Displaced Workers* and other data sources.

To summarize briefly, we find that having advance notice appears to reduce the probability that a displaced worker will suffer any spell of unemployment, but that it has no effect on the individual's duration of nonemployment if he or she becomes unemployed or on the individual's earnings if he or she finds reemployment. Contrary to concerns expressed by critics of advance notice, we also find no evidence that advance notice leads a firm's most productive workers to quit prior to their planned displacement date, thereby disrupting a firm's operations in its final weeks.

As we discuss in the final chapter, ultimately, given all the evidence we present and cite in the monograph, the position one takes towards advance notice legislation will depend heavily on one's preconceptions as to how labor markets function. Our own position is that given the social costs associated with worker displacement, a strong case appears to exist for a federal policy relating to advance notice. We conclude by discussing both inducements that the federal government might use to encourage employers to voluntarily provide advance notice and the types of research that should be undertaken to help evaluate the effectiveness of the new federal legislation.

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Introduction

Background

Most European nations have some form of legislation relating to plant closings or large scale layoffs (table 1.1). Typically, the legislation calls for advance notice by employers, with the length of notice varying across countries, and for employer negotiations with employees and government over whether the closing can be averted. Often, legislation requires severance pay for displaced workers, and some countries, for example Sweden, have detailed programs of labor market services (retraining, placement, public works, wage subsidies) to facilitate adjustments.[1] In Canada, both federal and provincial legislation similarly require advance notice, and the notice required often depends on a worker's tenure with the establishment (table 1.2). In many of these countries, small establishments with less than 100 employees are exempt from the requirements, perhaps due to the greater failure rate of small businesses or the belief that a shutdown of a small business does not have a substantial effect on a community.

Plant closing legislation in the United States is much more modest. As of early 1988, there was no federal law and only a few state laws. Three states, Maine, Wisconsin and Hawaii, require advance notice of plant shutdowns (with size class exemptions), and Maine also requires one week's severance pay per year of service for workers with greater than three years of tenure. The penalties for noncompliance are low in Maine ($500 per establishment) and Wisconsin ($50 per employee), but high in Hawaii (three months' wages and benefits per laid-off worker). Connecticut does not require advance notice, but does require nonbankrupt firms to maintain health insurance and other benefits for workers unemployed because

Table 1.1
Requirements for Advance Notice for Collective Dismissals in European Legislation, 1983[a]

Country	Definition of collective dismissals	Notice requirements
Belgium	Within a 60-day period a) 6 workers in 20-59 employee firms b) 10% of workforce in larger firms	30 days
Denmark	Within a 30-day period a) 10 workers in 21-99 employee firms b) 10% of workforce in 100-299 employee firms c) 30 workers in ≥300 employee firms	30 days
France	Within a 30-day period a) 2 workers in firms of ≥11 employees	45 days and up
Germany	Within a 4-week period a) 6 workers in 21-49 employee firms b) 10% of workforce or greater than 25 workers in firms employing 50-499 workers c) 50 workers in ≥500 employee firms	30 days
Greece	2-10% of the workforce in firms normally employing ≥50 employees (percentage changes each year)	30 days
Ireland	Within a 30-day period a) 5 employees in 21-49 employee firms b) 10 employees in 50-99 employee firms c) 10% of workforce in 100-299 employee firms d) 30 employees in ≥300 employee firms	30 days
Italy	On the same date 2 workers in any firm employing ≥10 workers	22 to 32 days
Luxembourg	Within a 30-day period, 10 workers Within a 60-day period, 20 workers	60 to 75 days
Netherlands	Within a 3-month period 20 workers	30 days
Sweden	5 workers	2 to 6 months
United Kingdom	1 worker	30 to 90 days (if at least 10 workers are involved

SOURCE. Authors' interpretation of material in "Collective Dismissals in 10 Countries," *European Industrial Relations Review* 76 (May 1980): 19–24; and "Collective Dismissals and Insolvencies," *European Industrial Relations Review* 109 (Feburary 1983): 12–17.

a. In all cases these are minimum notice provisions. Provisions are typically also found for consultations with employee representatives with a view towards avoiding or mitigating the consequences of the dismissals. Relevant public authorities must also be notified and often their authorization is required. Finally, provisions are also in effect often for severance payments. In the event of employer insolvency, these payments typically come from a state fund. The figures for Sweden are as of 1980.

of plant shutdowns for up to 120 days. Massachusetts, Maryland and Michigan all have voluntary programs in which firms are urged to provide advance notice and/or continue benefits. Finally, South Carolina "requires" employers to give workers two weeks, notice before shutting down, but *only* in situations where employees are required to give advance notice prior to quitting.[2]

Interest in plant closing legislation in the United States has grown since the deep recession of the mid-1970's, and the relatively large number of plant closings and permanent layoffs in major manufacturing industries since then undoubtedly stimulated this interest. During the 1975-83 period, over 125 bills relating to plant closings were introduced in 30 states, the majority in the Northeast and Midwest.[3] More than 90 percent of these bills had provisions requiring advance notice of shutdowns, while substantially smaller percentages required severance pay or economic assistance to workers, employers, local governments, or potential buyers.

At the federal level, over 40 bills have been introduced into Congress since 1979. Some have called not only for advance notice, but also for severance pay, maintenance of benefits, and payments to communities to compensate them for lost revenues. In July of 1987, the Senate voted to attach an amendment to the Omnibus Trade Bill (S1420) that would require employers of 100 or more workers to give 60 days advance notice to workers and local government officials of a plant closing, or a layoff involving at least one-third of the employer's workforce that was planned to last at least six months.[4] The notice period could be reduced for employers seeking ways to avoid the shutdown or if "unforeseeable circumstances" occurred. Numerous exemptions were included in this bill, including if the employer relocated the business within a reasonable commuting distance of the old plant, or if a planned layoff of less than 60 days was extended due to "unforeseeable circumstances." Penalties for violations of the act would include pay to displaced employees for each day of violation and a fine of $500 per day of violation for failing to notify local governments.

In June of 1987, the House Education and Labor Committee approved legislation (HR1122) distinct from its trade bill that had

Table 1.2
Notice Requirements for Termination of Employment, Various Jurisdictions in Canada, 1986

	Individual termination		Mass terminations	
Jurisdiction	Minimum length of service	Employer notice	Number of employees	Employer notice
Federal	3 months	2 weeks	≥50	16 weeks
Alberta	3 mos.-2 years	1 week	No special legislation	
	≥2 years	2 weeks		
British Columbia	6 mos.-2 years	2 weeks	No special legislation	
	≥3 years	No. of weeks equal to years of service to maximum of 8 weeks		
Manitoba	>2 weeks	1 pay period	50-100	10 weeks
			101-300	14 weeks
			>300	18 weeks
New Brunswick	6 mos.-5 years	2 weeks	≥25 *if* they represent at least 25% of employer's workforce	4 weeks
	≥5 years	4 weeks		
Newfoundland	1 mo.-2 years	1 week	50-199	8 weeks
			200-499	12 weeks
	≥2 years	2 weeks	≥500	16 weeks
Nova Scotia	3 mos.-2 years	1 week	10-99	8 weeks
	2 years-5 years	2 weeks	100-299	12 weeks
	5 years-10 years	4 weeks	≥300	16 weeks
	≥10 years	8 weeks		
Ontario	3 mos.-2 years	1 week	50-199	8 weeks
	2 years-5 years	2 weeks	200-499	12 weeks
	5 years-10 years	4 weeks	≥500	16 weeks
	≥10 years	8 weeks		
Prince Edward Island	3 months	1 week	No special legislation	
Quebec	3 mos.-1 year	1 week	10-99	8 weeks
	1 year-5 years	2 weeks	100-299	12 weeks
	5 years-10 years	4 weeks	≥300	16 weeks
	≥10 years	8 weeks		
Saskatchewan	3 mos.-1 year	1 week	No special legislation	
	1 year-3 years	2 weeks		
	3 years-5 years	4 weeks		
	5 years-10 years	6 weeks		
	≥10 years	8 weeks		

Table 1.2 (continued)

	Individual termination		Mass terminations	
Jurisdiction	Minimum length of service	Employer notice	Number of employees	Employer notice
Northwest Territories	No notice provisions		No special legislation	
Yukon	6 months	1 week	25-49	4 weeks
			50-99	8 weeks
			100-299	12 weeks
			≥300	16 weeks

SOURCE: Extracted from the *Canadian Labour Law Reporter* (1986)—Termination of Employment (¶1650) and Group Termination of Employment (¶1655).

a. In some cases, employee notice of intent to terminate employment is also required. The federal provisions apply to federal employees and employees in regulated industries. Provincial regulations apply to both public and private employees with certain exemptions. These exemptions are both for temporary layoffs of specified durations and also for certain industries. Some laws also require severance pay. Generally, the penalty for failure to provide the required notice is payment of the employees' regular wages for the specified period. More details on each of these provisions are found in the Bureau of National Affairs, *Daily Labor Reporter* 50 (March 17, 1987) pp. D-1 to D-6.

stricter notification requirements. The House bill would require 90 days notice if 50 or more workers were displaced and 180 days notice if 500 or more workers were displaced. This bill also contained a provision that employers must "consult in good faith" with employees, unions, and local government officials about the effects of plant closings or mass layoffs.[5]

In April of 1988, both the House and the Senate passed the Omnibus Trade Bill that included the Senate's advance notice amendment. President Reagan vetoed the entire bill, stressing among other factors his opposition to the advance notice provisions.

Political pressure for the passage of advance notice legislation continued to grow, however, as advance notice became an important issue during the early stages of the 1988 Presidential campaign. In July of 1988, Congress passed the *Worker Adjustment and Retraining Notification Act* which requires employers of 100 or more workers to give workers and local government officials 60 days' advance notice of a plant closing or a layoff that is planned to last at least six months and that involves at least 500 workers or one-third of an employer's workforce. Although philosophically opposed to the bill, President Reagan bowed to political pressures and did not veto it.

Penalties for failure to provide the required advance notice again include back pay for each day of violation for displaced workers and a fine of $500 per day of violation for failing to notify local governments. Numerous exemptions are included in the Act, including those employers actively seeking to attract new capital or to sell the business, and displacements due to "business circumstances that were not reasonably foreseen." Whether an exemption applies to an employer who has failed to provide the required notice is to be determined by the relevant federal district court upon a suit being filed by employees or a local government.

Arguments For and Against Plant Closing Legislation

Proponents of plant closing legislation argue that advance notice provisions would ease displaced workers' shock and facilitate their search for alternative sources of employment or training. Such notice also would allow employers, workers and the community to see if ways exist to save the jobs, such as wage concessions, tax concessions, or seeking new ownership, including the possibility of employee ownership. If plants do shut, the maintenance of health insurance would be important for individuals during a period when stress leads to increased incidence of physical and mental ailments. Finally, payments by firms to the communities in which the plants were located would help alleviate the extra demands placed on these communities for social services that the shutdowns cause—demands that arise at the same time local property and sales tax revenue are being reduced.[6]

Opponents of the legislation argue that, in addition to restricting the free mobility of capital, advance notice legislation would have a number of other adverse effects on firms.[7] They claim it would increase worker turnover and decrease productivity, as those productive workers with the best opportunities elsewhere would leave and the morale of remaining workers would suffer. It also would decrease the likelihood that buyers of the plant's product would place new orders, that banks would supply new credit, that suppliers would continue to provide services, and that the firm could sell the plant to potential buyers. Finally, it would depress corporate stock prices. Such a provision, as well as others that directly increase the costs of plant shutdowns, effectively increase the cost of reducing employment and thus should encourage firms *not* to expand operation or to substitute overtime hours for additional employment in states where such laws are in effect.

In evaluating the case for plant closing legislation, it is useful to stress the divergence between private and social costs. Employers currently do not bear the full social costs of plant shutdowns, both because unemployment insurance is imperfectly experience rated and because the costs shutdowns impose on communities are not taken into account by them. As such, imposing a "tax" on plant closings, either in the form of advance notice provisions, severance pay requirements, or maintenance of benefits requirements may make sense; it would have the effect of discouraging the action. These efficiency considerations suggest the need for federal, rather than state-by-state rules, to reduce the possibility that locational decisions by firms would be influenced by "tax price" differences. Critics, however, would stress that such legislation might encourage the flight of jobs overseas.

Outline of This Study

In spite of the growth of legislative efforts, there has been surprisingly little empirical effort devoted to analyzing the effects of advance notice; our study seeks to increase our empirical knowledge in this area. We begin the next section by describing the extent to which advance notice was provided to displaced workers in the United States prior to the passage of the *Worker Adjustment and Retraining Notification Act* through existing state legislation, provisions in privately negotiated collective bargaining agreements, or voluntary employer actions. The majority of displaced workers in the

United States are seen not to have received advance notice of any meaningful length.

Chapter 2 presents a summary of prior empirical research on the effects of legislated, privately bargained, and voluntarily provided advance notice of displacement in the United States and Europe. The weaknesses of the previous research are highlighted and the contributions of our subsequent empirical analyses stressed.

The next three chapters summarize our empirical analyses of advance notice provisions. After describing the data used in chapter 3, chapter 4 presents analyses of the determinants of which displaced workers in the United States report receiving advance notice and of whether those workers who do receive notice implicitly "pay" for it in the form of lower predisplacement wages. Chapters 5 and 6 then analyze respectively what the effects of advance notice appear to be on displaced workers' durations of time out of work and, for those who find new employment, on their postdisplacement wages. Chapter 5 also addresses a concern of critics of advance notice requirements, namely, whether such requirements lead firms' most productive workers to quit (once notified) prior to their scheduled displacement dates.

Finally, chapter 7 discusses the implications of our findings for public policy towards displaced workers. This chapter also indicates the directions in which future research might profitably go.

Advance Notice of Displaced Workers in the United States

Two employee-based and two employer-based surveys have recently collected information on the extent to which displaced workers in the United States received advance notice of their displacement. The employee-based surveys were the *Survey of Displaced Workers (SDW)* supplements to the January 1984 and January 1986 *Current Population Surveys;* these covered workers who were displaced during the 1979-83 and 1981-85 periods respectively.[8] The 1984 *SDW* data is used in our analyses in chapters 4 through 6 and is described in some detail in chapter 3.

Table 1.3 presents data on the proportion of displaced workers in these surveys who received advance notice or expected layoffs. For the purpose of this table, displaced workers are defined as persons with tenure of at least three years who permanently lost or involuntarily left a full-time wage and salary job. These data suggest that over half of the displaced workers covered by these surveys received advance notice, with displaced females more likely to have received notice than displaced males, and workers displaced due to a plant closing more likely to have received notice than workers displaced

Table 1.3
Proportion of Displaced Workers Who Received Advance Notice or Expected Layoff in the January 1984 and January 1986 *CPS* Displaced Worker Supplements

	All displaced workers	Displaced due to plant closing	Displaced for other reasons
January 1984 survey (workers displaced in 1979-83)			
Persons 20 and over	.56	.61	.52
Persons 20 to 34	.57	.62	.53
Persons 35 to 54	.56	.60	.52
Persons 55 and over	.56	.61	.48
Males 20 and over	.55	.58	.52
Females 20 and over	.60	.66	.51
January 1986 survey (workers displaced in 1981-85)			
Persons 20 and over	.55	.59	.49
Persons 20 to 34	.58	.63	.53
Persons 35 to 54	.53	.57	.49
Persons 55 and over	.52	.57	.41
Males 20 and over	.53	.57	.47
Females 20 and over	.59	.63	.54

SOURCE: Authors' calculations from U.S. Bureau of Labor Statistics, Bulletin 2240, *Displaced Workers, 1979-83* (Washington, DC: July 1985), tables 6 and B6, and U.S. Bureau of Labor Statistics, Bulletin 2289, *Displaced Workers, 1981-85* (Washington, DC: September 1987), tables 6 and B6.

for other reasons (e.g., layoffs). Younger workers also appear more likely to have received notice than older workers.

While at first glance these data suggest that prior to the passage of federal legislation, advance notice was widespread in the United States, we must caution that the question asked respondents in this survey did *not* distinguish between receipt of formal notice and simply expectation of displacement.[9] The survey also provided *no* information on the duration of the advance notice. This was a second crucial shortcoming, since the effectiveness of advance notice policies presumably depend at least partially on how far in advance notice is given.[10]

The two employer-based surveys suffer from neither of these shortcomings. The first, a September 1986 survey of establishments in seven states that, during the last half of 1985, reported "layoff events" of 30 days or more in which 50 or more initial unemployment insurance claims were filed in a three-week period by former employees, was conducted by the U.S. Bureau of Labor Statistics (BLS).[11] The second, a nationwide random sample of establishments in which layoffs of 100 or more workers occurred in 1983 or 1984, was conducted by the U.S. General Accounting Office (GAO).[12]

Table 1.4 summarizes information from the two employer-based surveys on the frequency and duration of advance notice. The precise definitions of advance notice differ between the two surveys (see table 1.4), however in this table "general notice" relates to notice that a layoff would occur sometime in the future, while "specific notice" relates to individual employees being given specific termination dates.

These data present a much different picture from the employee-based data about the prevalence of advance notice for displaced workers in the United States. While the results differ across the two surveys (which differ in coverage and time frame), it is clear that only a small fraction of employers provide workers about to be displaced with either general or specific advance notice of at least one month. Indeed, over 80 percent of the establishments in the BLS survey provided either no notice or notice of less than two weeks,

Table 1.4
Comparison of the Distribution of Advance Notice Provided by Establishments in the *BLS* and *GAO* Surveys[a]

	General notice[b]		Specific notice[c]	
Length of notice	BLS	GAO	BLS	GAO
No notice	64	24	5	31
1-14 days	16	25	78	34
15-30 days	6	17	9	15
31-90 days	10	17	8	15
91 days and over	4	17	d	5

SOURCE: Sharon Brown, "How Often Do Workers Receive Advance Notice of Layoffs?" *Monthly Labor Review* (June 1987), pp. 13-17 and table 6.

a. *BLS*—Bureau of Labor Statistics September 1986 survey of establishments in seven states that, during the last half of 1985 reported "layoff events" of 30 days or more in which 50 or more initial unemployment insurance claims were filed in a 3-week period by former employees. *GAO*—Government Accounting Office nationwide random sample of establishments that focused on those in which layoffs of 100 or more workers occurred in 1983 and 1984.

b. *General Notice*—In the BLS study, defined as informing individual employees that they will be laid off without specifying the exact date. In the GAO study, defined as informing groups of workers that some or all of them may be laid off.

c. *Specific Notice*—In the BLS study, defined as informing individual employees the specific date on which they will be laid off. In the GAO study, a similar definition was used.

d. Less than .5 percent.

while about 50 (65) percent of the establishments in the GAO survey provided either no general (specific) notice or general (specific) notice of less than two weeks.

Some data on the prevalence of advance notice broken down by industry, establishment size, occupational category, and union status, are reported in tables 1.5 and 1.6 for the BLS and GAO surveys, respectively. What emerges from these tables is that differences in the frequency and length of advance notice appear to have existed along some of these dimensions. For example, employees at unionized establishments were more likely to have notice than employees at nonunion ones and employees at large establishments were more likely to have notice than employees at small establishments. We will discuss in chapter 4 why such differences might occur.

The conclusion one draws from these data is that many displaced workers in the United States have received either no advance notice

Table 1.5
Percentage of Establishments with Mass Layoffs Providing Advance Notice in the BLS Survey: By Industry and Union Status[a]

	With advance general notice			With specific notice of more than 1 day		
	(A)	(U)	(N)	(A)	(U)	(N)
Total, all industries	35	38	37	52	53	50
	[46]	[51]	[42]	[18]	[13]	[24]
Agriculture	0	—	0	37	—	37
				[40]		
Nonagriculture	37	38	41	58	53	51
	[46]	[51]	[43]	[18]	[13]	[23]
Manufacturing	42	44	48	63	60	56
	[45]	[50]	[43]	[18]	[13]	[22]
Durable	41	38	49	60	60	53
	[54]	[63]	[53]	[19]	[14]	[26]
Nondurable	46	59	45	69	59	65
	[25]	[27]	[13]	[15]	[11]	[12]
Nonmanufacturing	19	17	21	42	35	39
	[54]	[59]	[43]	[18]	[10]	[27]
Wholesale and retail trade	37			37		
	[84]			[18]		
Services	23			50		
	[23]			[19]		
Other Nonmanufacturing	14			41		
	[54]			[18]		

a. Authors' calculations from Sharon P. Brown, "How Often Do Workers Receive Advance Notice of Layoff?" *Monthly Labor Review* (June 1987) tables 1 and 2.

where

(A) all establishments
(U) union establishments
(N) nonunion establishments

Union status was reported by establishments in only six of the seven states. Thus, the numbers in (U) and (N) do not necessarily add up to those in (A).

[ ] mean duration of notice (in days)

of their displacement or notice of only very short duration. Thus, federal advance notice legislation does have the potential to substantially increase both the frequency *and* duration of notice that displaced workers receive.

Table 1.6
Length of Specific Advance Notice Provided By Establishments in the GAO Survey

Category	Percent of establishments that provided: No notice	1 to 14 days	15 to 30 days	30 to 90 days	91 days or more
All establishments	**32**	**34**	**15**	**14**	**5**
Blue-collar workers	31	36	14	14	5
Union	19	40	18	18	5
Nonunion	42	31	13	10	4
Plant closures	29	24	14	22	11
Permanent layoffs	32	39	14	12	3
Manufacturing	34	36	14	12	4
Nonmanufacturing	26	35	16	18	5
Less than 250 employees	35	32	13	15	5
More than 250 employees	23	44	17	11	5
White-collar workers	28	31	19	16	6
Union	21	37	19	17	6
Nonunion	31	26	20	16	7
Plant closures	25	17	21	28	0
Permanent layoffs	29	36	18	12	5
Manufacturing	31	34	17	13	5
Nonmanufacturing	23	26	21	21	9
Less than 250 employees	31	29	17	17	6
More than 250 employees	21	35	21	15	8

SOURCE: United States General Accounting Office, *Plant Closings, Limited Advance Notice and Assistance Provided Dislocated Workers* (Washington, DC: Superintendent of Documents, July 1987), appendix tables VII-6 through VII-19.

NOTES

1. Bjorklund and Holmlund (1987) present a discussion of Swedish policies.
2. See Cline (1984).
3. See Burchell et al. (forthcoming).
4. See Bureau of National Affairs (1987a).
5. See Bureau of National Affairs (1987b).

6. See, for example, Bluestone and Harrison (1982).

7. See, for example, McKenzie (1982).

8. See U.S. Bureau of Labor Statistics (1985; 1987).

9. Moreover, because the question was asked *ex post*, or after the fact, one cannot distinguish between true notice or *ex ante* expectation on the one hand, and *ex post* rationalization on the other hand. As we will discuss in chapter 8, this distinction is crucial in evaluating the potential effects of legislated advance notice provisions.

10. Both shortcomings are remedied in the *Survey of Displaced Workers* that is part of the January 1988 *Current Population Survey.*

11. See Brown (1987).

12. See U.S. General Accounting Office (1987).

2

Do Advance Notice Provisions Matter?

Previous empirical research on the effects of legally mandated, collectively bargained, or voluntarily provided advance notice for displaced workers has been of two types. One set of studies focuses on the effects of advance notice provisions on employment-related variables at the national or community level. The second, and by far more numerous, set of studies examines the effects of provision of advance notice on individual displaced workers and employers. We discuss the two sets in turn and then indicate what our own empirical contributions will be.

Studies at the National or Community Level

Lazear (1987) used annual aggregate data for 23 countries (the U.S., Canada, many European countries, Hong Kong, and Australia) over a 29-year period (1956 to 1984) to estimate what the effects of legally mandated severance pay and advance notice provisions for blue-collar workers were on the aggregate employment/population ratio, unemployment rate, and average weekly hours in manufacturing. The severance pay variable used was a measure of severance pay due displaced workers with 10 years of service, while the advance notice variable used was the number of months of notice required for workers with this job tenure. If a country did not have such provisions in a year, these variables were set equal to zero for the year. Simple fixed-effects models (to control for country-specific omitted variables) were estimated and a small set of control variables (e.g., a linear time trend, cyclical factors, and demographic factors) were included in the analysis.

He found that instituting or increasing the length of an advance notice requirement tended to increase the employment/population ratio and *decrease* average weekly hours, although both effects were statistically insignificant. He attributed these results to the fact that most countries that have laws requiring advance notice exempt part-time employees who are displaced. Thus, the laws encourage the substitution of part-time for full-time employees.[1] One must caution, however, that in most countries in Lazear's sample, changes occurred in advance notice requirements only once or twice during the period.

Folbre, Leighton and Roderick (1984) is the only study of U.S. data that examined the effects of advance notice of plant closings on local area unemployment rates and labor force size. They examined the effects of major plant closings (those involving more than 100 workers) in Maine in a period prior to advance notice becoming mandatory in the state, and found that *voluntary* provision by a firm of at least one month advance notice to displaced workers significantly diminished the closing's impact on the local area unemployment rate in the month of closing. While this *may* be due to more rapid reemployment of displaced workers in the presence of advanced notice, their results also suggest that advance notice was associated with a significant reduction in the size of the local labor force in the month of the closing. The latter reflects either labor force withdrawal or outmigration (and *possible* reemployment elsewhere); they were unable to ascertain which had occurred.

Studies at the Establishment or Individual Level

Two studies have utilized data from a small number of plants that ceased operations. One early study, Weber and Taylor (1963), focused on 32 plant closings in the late 1950s and early 1960s and found that voluntarily provided advance notice rarely led to increased quit rates or decreased productivity of workers. The second study, Holen, Jehn, and Trost (1981), analyzed the experiences of 9,500 displaced workers from 42 plants that closed and found that provision of advance notice was associated with *larger* earnings losses for the displaced workers; at first glance, a somewhat paradoxical result.

In each of the above studies and indeed in all of those discussed below, provision of advance notice was treated as exogenous. If the existence of advance notice provisions is determined by other variables, however, biased estimates of the provisions' true effects may result. For example, suppose that those employers who perceive they would face increased quit rates prior to the shutdown date if they provide their workers with advance notice do not provide such notice, while those employers who perceive they would not face increased turnover do provide notice. To the extent that these perceptions are correct, only low "expected increase in turnover" firms would provide notice and one would not observe turnover increasing in those firms after notice was provided (Weber and Taylor's findings). This would not, however, tell us anything about the likely effects of legally mandated advance notice on turnover. Similarly, if advance notice were to arise primarily in situations in which the employment prospects faced by displaced workers were the worst, a comparison of earnings losses suffered by workers with notice to the losses incurred by those who failed to receive notice would show that the former lost more (Holen, Jehn, and Trost's finding). However, this finding again would *not* imply that legally mandated advance notice would make workers worse off.

Recently several studies have analyzed data from the Bureau of Labor Statistics January 1984 *Survey of Displaced Workers (SDW)*, a supplement to the *Current Population Survey*.[2] The 1984 *SDW* is a special supplement to a national probability sample of households that was administered to workers permanently displaced during the 1979-84 period due to a plant shutdown or layoff. It contains information on the individuals' predisplacement earnings, survey date earnings (if employed), weeks of nonemployment since displacement and whether the individuals received advance notice or expected their displacement. It contains no information on whether the notice was formal or how far in advance it was given. This is a crucial omission, since, as noted in chapter 1, the effectiveness of advance notice policies presumably depends at least partially on how far in advance notice is given.

The various studies yield somewhat mixed findings. Howland (1988) limited her analyses to a small subsample of the displaced workers located in SMSAs so that she could merge in controls for area economic conditions. She found that, on average, workers displaced from manufacturing jobs due to plant shutdowns who received advance notice did not benefit from the notice, although white-collar workers did appear to have shorter durations of nonemployment. In addition, the approximately 10 percent of workers who received advance notice and then quit prior to displacement appeared to suffer smaller wage losses and fewer weeks of nonemployment than those who failed to receive notice.

Addison and Portugal (1986; 1987a) concentrated their attention on workers displaced due to plant shutdowns, did *not* include any controls for area economic conditions in their analyses and found that, *ceteris paribus*, the presence of advance notice was associated with durations of nonemployment that were some 35 percent shorter. For workers who received unemployment insurance (UI) after displacement (which meant, given UI rules in most states, those with more than one week of unemployment) the negative association of advance notice and duration was found only for white-collar employees. For both white- and blue-collar workers who failed to receive UI after displacement, a negative association between advance notice and duration of nonemployment was found. This latter result is not surprising; if advance notice helps some workers to find employment without an intervening spell of unemployment, these workers will never be eligible for UI benefits. Put another way, the presence of advance notice may increase the probability that displaced workers fail to receive UI. While Addison and Portugal (1986) treated the receipt of UI as endogenous, they did *not* allow receipt of advance notice to influence receipt of UI.

Podgursky and Swaim (1987a) restricted their attention to those workers displaced during the 1979-81 period. Using a set of control variables (they included the area unemployment rate in the year of displacement) slightly different from Addison and Portugal (1987a), they found that advance notice significantly reduced nonemployment durations only for white-collar females. Podgursky and Swaim (1987b) studied the determinants of postdisplacement earnings for workers who were subsequently employed full time at the survey date. They found no effects of advance notice, suggesting that such policies have, at best, transitional effects.

Finally, Addison and Portugal (1987b) found that a 10 percent increase in duration of unemployment decreased postdisplacement wages by about 1 percent. Since their results (in a paper which focused on laid-off displaced workers, as well as those displaced by plant closings) also indicate that advance notice reduced duration of unemployment by about 25 percent, one can infer that advance notice increases postdisplacement wages by about 2.5 percent. One must caution, however, that the duration-wage relationship is conditional on a displaced worker's having remained in the same industry and occupation. Moreover, their specifications did not permit advance notice to have a direct effect on postdisplacement wages.

Our Own Research

The research reported in this monograph reanalyzes the 1984 *Survey of Displaced Workers* (*SDW*) data, making a number of methodological innovations. First, in the absence of formal legislation requiring advance notice, one can view advance notice as an explicit or implicit contract provision that is desirable to have from workers' perspective and ask if workers must pay for this provision in the form of lower predisplacement wages? That is, we ask if compensating wage differentials exist for advance notice provisions? If the answer is yes, it is straightforward to show that displaced workers who receive advance notice will appear, *ceteris paribus*, to suffer smaller earnings losses than those who fail to receive notice, even if advance notice has no true effect on postdisplacement wages.

Second, as noted above, the presence of an advance notice provision is likely endogenous and depends upon both employers' willingness to supply, and employee demand for such provisions. We attempt to formally model the determinants of advance notice, including the magnitude of the compensating wage differential, and then test whether treating advance notice as endogenous influences subsequent results. To do this requires us to merge into the *SDW* data an extensive set of industry and area variables.

Third, previous researchers have not stressed that about 10 percent of the males and over 15 percent of the females in the *SDW* suffered *no* spell of nonemployment after displacement. We estimate separately what the effect of advance notice was on the probability of a displaced worker finding a job without any spell of nonemployment and what it was on the duration of nonemployment (conditional on a spell existing).[3] We also estimate what the effects of advance notice were on survey date wages. In each of these analyses we employ a much more extensive set of controls for industry and area characteristics than previous researchers did.

Fourth, since the *SDW* contains data on whether workers who received advance notice quit prior to displacement, we estimate the determinants of predisplacement turnover and ascertain if there is any evidence that turnover among firms' most productive workers occurs. Finally, all of our analyses are done separately for four groups (male/displaced due to a plant shutdown, female/displaced due to a plant shutdown, male/displaced due to a layoff, female/displaced due to a layoff) to see if such policies have differential effects across groups.

NOTES

1. See Ehrenberg, Rosenberg, and Li (1988) for a discussion of other factors that influence the substitution of part-time for full-time employees.

2. See chapter 3 for a more complete description of the *SDW*.

3. After a paper summarizing our research was widely circulated, Swaim and Podgursky (1988) performed a similar analysis, citing their awareness of our work.

3

Data Used in Our Analyses

The 1984 Survey of Displaced Workers

This supplement to the January 1984 *Current Population Survey* contains data for approximately 11,000 adult workers, age 20 and older, who were permanently displaced from jobs during the 1979-83 period because of plant closings (including those due to employers going out to business), layoffs from which they had not been recalled, seasonal factors, self-employed business failures, or other reasons.[1] We restricted our attention to those workers displaced from a full-time job due to a plant closing or layoff. These restrictions reduced the sample we analyzed to 7,365 individuals. Sample size was further reduced in many of our analyses because data were not reported by some individuals for some variables.

Questions were asked of each individual in the *SDW* supplement about the nature of the job the individual was displaced from (including earnings as of the date of displacement), the year displacement took place, the nature of the person's survey date job (including earnings) if employed, and total weeks of nonemployment experienced by the individual since the displacement. In addition, each individual was asked if he or she had expected, or had received advance notice of, the displacement. Individuals who answered in the affirmative were also asked if they quit their job prior to the date at which they would have been displaced. A complete description of the variables used in our analyses from the *SDW* is found in the top panel of table 3.1.

At the time our analyses were undertaken, the *SDW* supplement was the best available source of data to use in research on displaced workers. However, it has a number of weaknesses that should be acknowledged. First, it asks individuals if they had expected, or had

Table 3.1
Variable Definitions and Sources

Variable name	Description
1) *From the January 1984 Current Population Survey—individual data*[a]	
AGET	age in years, at date of displacement
AGET2	age in years squared at the displacement date
REDUC	years of schooling completed
REDUC2	years of schooling squared
AGEED	age times years of schooling
TENURE	years of continuous tenure prior to displacement
TEN2	years of tenure squared
RRACE	1=nonwhite, 0=white
RHISP	1=hispanic, 0=other
RMAR	1=married spouse present, O=other
RVET	1=male veteran, 0=other
RHINST	1=had health insurance at job before displaced, 0=other
MAINE	1=reside in Maine, 0=other (at survey date)
WISC	1=reside in Wisconsin, 0=other (at survey date)
MSMSA	1=reside in an SMSA, 0=other (at survey date)
MANUF	1=displaced from manufacturing job, 0=other
RUI	1=received UI when displaced, 0=other
WCOL	1=white-collar worker when displaced, 0=other
RADV	1=received advance notice of job loss, 0=other
REARNT	usual weekly earnings when displaced, converted to 1984 dollars
REARNN	usual weekly earnings at the survey date (1/84)
SHUT	1=lost job due to plant shutdown, 0=lost job due to layoff
DURNON	weeks out of work since displacement (*maximum is 99*)
YD1	1=displaced in 1979, 0=other (1984 is the omitted year)
YD2	1=displaced in 1980, 0=other (1984 is the omitted year)
YD3	1=displaced in 1981, 0=other (1984 is the omitted year)
YD4	1=displaced in 1982, 0=other (1984 is the omitted year)
YD5	1=displaced in 1983, 0=other (1984 is the omitted year)
R1	1=reside in Northeast, 0=other (West is the omitted region)
R2	1=reside in Northcentral, 0=other (West is the omitted region)
R3	1=reside in South, 0=other (West is the omitted region)

Table 3.1 (continued)

Variable name	Description
2) Data merged in by matching individuals' CPS 3-digit industries prior to displacement with data for 52 broad industry groups from the 1980 Census of Population, a 1978 BLS special report on job tenure, and the March 1984 Current Population Survey[b]	
IMALE	percentage male
IBLACK	percentage black
IHISP	percentage hispanic
IAGE	median age
ICOL	percent college graduates
IHS	percent high school graduates
ITEN	median job tenure
IA55	percentage older than age 55
IPROF	percent professional workers
IADS	percent administrative and technical support workers
IBCS	percent blue-collar skilled workers
IBCU	percent blue-collar unskilled workers
3) Data merged in by matching individuals' predisplacement CPS 3-digit industry[c]	
IUNION	percent unionization in the industry in the year prior to displacement
4) Data merged in by matching individuals' predisplacement industry, year of displacement, and area (SMSA if available, otherwise state) of displacement to area data on unemployment, employment by major (1-digit) industry groups and unionization[d]	
AUNEMP	area unemployment rate two years prior to displacement
AUNEMPC	change in the area unemployment rate in the year prior to displacement
AEMPC1	annual percentage change in nonagricultural employment in the area from 1975 to two years prior to displacement
AEMPC2	percentage change in nonagricultural employment in the area in the year prior to displacement
AEMPC3	same as AEMPC1, but for major industry group employment in the area
AEMPC4	same as AEMPC2, but for major industry group employment in the area
AUNION	percent unionization in the area in the year prior to displacement
AU	area unemployment rate in the year prior to displacement

Table 3.1 (continued)

Variable name	Description
5) Data merged in by matching on the individual's 3-digit census industry prior to displacement	
WDIF	estimated 3-digit industry wage premium, associated with job prior to displacement, from regressions using May 1979 CPS data

a. Individual data is from the *January 1984 Current Population Survey*. The sample is restricted to individuals who lost or left their last job since January 1979 due to a plant closing, slack work, or a position being eliminated, and who were working full time at the date of displacement.

b. Data sources IMALE to IHS—*1980 Census of Population*. ITEN—*1980 Census of Population* and BLS Special Labor Force Report 235 *Job Tenure Declines as Work Force Changes* (Washington, D.C., 1980) (industry job tenure data were for January 1978). IA55 to IBCU—*March 1984 Current Population Survey*. The correspondence of the 3-digit census codes to the broad industry groups is found in appendix A.

c. Unionization data is from Edward Kokkelenberg and Donna Sockell, "Union Membership in the United States," *Industrial and Labor Relations Review* 38 (July 1985): 497-543.

d. Unemployment and employment data come from various issues of *Employment and Earnings*. Unionization data are from the Kokkelenberg and Sockell article cited above.

e. The major industry groups used are manufacturing, mining, construction, transportation-communications-utilities, wholesale and retail trade, finance-insurance-real estate, and service employment (no data were collected for agriculture and government employees).

received, advance notice of their displacement. Researchers have no way of knowing if an affirmative answer meant formal notice was received, or what the length of any such formal notice was.[2] Since the effectiveness of advance notice presumably depends upon the notice being sufficiently long to both facilitate labor market adjustments and to allow for the provision of additional services (job counseling, job placement assistance, etc.), this is a serious limitation which causes the analyses reported in later chapters to likely *understate* the effects of advance notice of, say, 60 days. Moreover, no information is provided in the *SDW* about any services that may have accompanied advance notice.

Second, while total weeks of nonemployment an individual experienced since the displacement date was reported, no information was collected on whether these weeks were spent in unemployment or out of the labor force. Similarly, no information was collected on whether the individual had experienced multiple spells of nonemployment and hence possibly multiple jobs since the displacement date. Because of this, one cannot be sure that only a single spell of nonemployment was experienced by the displaced workers.[3]

Third, respondents to the survey were asked in January of 1984 to report their durations of nonemployment over the previous five years. As is well known, such retrospective questions are subject to *recall error;* people's perceptions of events "fade" the further away in time the events occurred.[4] Evidence to support the contention that such errors occurred in the *SDW* is that respondents' reported durations of nonemployment were concentrated at round numbers, such as 13 weeks (3 months), 26 weeks (6 months), 39 weeks (9 months), 52 weeks (1 year), and 78 weeks ($1\frac{1}{2}$ years). While some of this concentration is due to the higher likelihood that respondents find work at the date unemployment benefits are exhausted (e.g., after 26 or 39 weeks) than at other dates, the "spikes" that occurred in the distribution of nonemployment durations at longer durations suggest that recall error was a problem.[5]

Fourth, the *SDW* data contain no information on whether the individual was employed in a job covered by a collective bargaining agreement when he or she was displaced and it contains information for only one-quarter of the sample as to whether individuals employed at the survey date were employed in a unionized establishment.[6] The former omission is particularly unfortunate since, to the extent that advance notice provisions are more prevalent in unionized that nonunionized settings (see table 1.6), estimates of effects that we subsequently attribute to the existence of advance notice may instead be due to the presence of a union.

Finally, the *SDW* covers only workers who have been displaced. It contains no information on individuals whose potential displacement was averted due to the presence of an advance notice provision. Thus, the *SDW* does not permit us to test whether the existence of advance notice provisions can help to avert layoffs or shutdowns, as proponents argue. Similarly, since the *SDW* is an individually-based, rather than an establishment-based, data set, it does not permit us to test whether advance notice adversely affects employee productivity, as critics of advance notice argue.

Merging in Industry, Area, and Industry/Area Data

A major advantage of the *SDW* is that it provides information on the detailed (3-digit SIC) industry in which each individual was employed prior to his or her displacement. Information is also provided on the state in which each individual resided as of the survey date and, for individuals who resided in one of the largest 57 standard metropolitan statistical areas (SMSAs), the name of that SMSA. Since approximately 18 percent of the individuals in our sample moved to a different city or county between their dates of displacement and the survey date, this means that we can unambiguously identify which state, and in some cases which SMSA, over 80 percent of our sample resided in at the dates of their displacement.

This information is important to us because characteristics of the industry in which an employee was employed and the area in which an employee resided at the date of his displacement may influence the employee's duration of nonemployment after displacement and postdisplacement earnings, as well as the likelihood that the employee received advance notice of the displacement. For example, other things equal, the higher the rate of unemployment in an area or the slower the rate of growth of employment in an industry in an area, the longer a displaced worker's duration of nonemployment and the lower his postdisplacement earnings might be expected to be. Similarly, other things equal, workers employed in industries that were heavily unionized might be more likely to be covered by a formal advance notice provision than workers in industries that were not heavily unionized. Failure to include area and industry variables in empirical analyses of the determinants of advance notice, duration of nonemployment, and postdisplacement wages for displaced workers leaves open the possibility that the analyses suffer from omitted variable bias; one cannot be sure, if these variables are omitted, that parameter estimates are unbiased.

As indicated in chapter 2, it is somewhat surprising that previous studies using the *SDW* either added only a few industry and area variables to the data or ignored such variables completely. We have gone to considerable effort to our research to merge a large number of industry, area and industry/area variables into the *SDW* data and then to include these variables in our analyses. A complete listing of the variables we collected and used are found in the second through fifth panels of table 3.1.

Data on the demographic characteristics of workers employed in each individual's predisplacement industry of employment were obtained from the *1980 Census of Population,* a 1978 BLS special report on job tenure, and the *March 1984 Current Population Survey.*[7] Data on the percent of unionization in the industry in which each individual was employed prior to displacement and in the area in which he resided were obtained from an article (cited in table 3.1, note c) that used data from a number of *Current Population Survey* data tapes. Data on area unemployment rates and employment changes, as well as area/industry employment changes were obtained from various issues of *Employment and Earnings.* Finally, an estimate of the wage differential associated with employment in the 3-digit industry in which an individual was employed prior to displacement was obtained from the estimation of wage equations that utilized the May 1979 *Current Population Survey* data.[8] Details about how the wage equations were estimated and the rationale for our use of each of the variables in table 3.1 appear in subsequent chapters.

NOTES

1. For a complete description of the data and many tables of cross-tabulation, see U.S. Bureau of Labor Statistics (1985). The tabulations there, however, are limited to those displaced workers who had worked at least three years on their jobs prior to displacement. We do not limit our analyses in subsequent chapters to workers with at least this job tenure. However, we do test in places whether the results that we obtain differ across tenure classes of displaced workers.

2. As noted in chapter 1, the BLS has recently conducted a new displaced worker survey as a supplement to the January 1988 *Current Population Survey* that more precisely asks whether formal notice was received and how long that notice was. This data should become available to researchers in early 1989.

3. This suggests that prior sophisticated statistical analyses of the *SDW* data that act *as if* all individuals in the sample experienced only one spell of nonemployment and interpret their findings as providing information about the probability of displaced workers leaving nonemployment (e.g., Addison and Portugal (1987a)) are somewhat misleading.

4. For discussions of recall error, see Panel on Incomplete Data (1983), Horvath (1982), and Akerlof and Yellen (1985).

5. See Moffit and Nicholson (1982) for evidence that unemployment insurance recipients probability of leaving unemployment rises at the date they exhaust their UI benefits.

6. The union status of respondents employed at the survey date was asked only of individuals in two of the eight rotation groups in the *CPS*.

7. These data were reported in less industry detail than is found in the *SDW*. Appendix A indicates how we merged these industry-level variables into the *SDW*.

8. We also collected other industry-level variables, for example, changes in imports each year by industry. Once we controlled for the variables found in table 3.1, the import variables never proved statistically significant in any of our analyses.

4

The Determinants of Advance Notice—Do Workers Pay For It?

In the absence of advance notice legislation, the collective bargaining process *may* provide workers covered by union contracts with some guarantees of advance notice of impending displacement. If a union wants such a contract provision, it must win it at the bargaining table.

Of course, if the provision is costly to management (as opponents of legislation allege), management would agree to it only if the union agrees to give up something in return. One possibility is that the union may "pay" for the advance notice provision by agreeing to either a lower wage increase or a lower wage level. If this occurs, advance notice provisions would "affect" *preunemployment* wages. To date, no study has examined whether such a form of compensating wage differential arises, although a number of studies have found that unions often win compensating wage differentials for other unfavorable job characteristics.[1]

Now consider nonunion employers. To the extent that their workers also value implicit or explicit promises that advance notice of any impending displacement will be provided, employers who offer such promises should be able to also offer their workers lower predisplacement wages. In this case, market forces would lead to a compensating wage differential for the promise of advance notice.

Suppose advance notice provisions have *no* effect on displaced workers' postdisplacement wages. If displaced workers with advance notice do have lower predisplacement wages than those without advance notice because of the above forces, the former will appear to suffer smaller wage losses.[2] However, this would not imply that advance notice was a desirable policy (since we assumed that it had no effects on postdisplacement wages). Rather it would reflect only the willingness of workers to "pay" for advance notice. It should be clear then why it is important to study whether compensating wage differentials for advance notice arise.

This chapter provides an analysis of the determinants of which displaced workers in the January 1984 *SDW* reported receiving advance notice and whether those who did receive notice implicitly paid for it in the form of lower predisplacement wages. After sketching out our analytical framework, we present the empirical results.

Analytical Framework

Consider first the question of whether unionized workers whose collective bargaining agreements require advance notice of displacements due to layoffs or plant closings must pay for these provisions in the form of lower preunemployment wages. Restricting ourselves to unionized workers *and* assuming that all people who received advance notice had an explicit or implicit advance notice provision in their contract, a naive approach to the problem would be as follows.[3] If the preunemployment wage of the ith individual is W_{iA} if he or she works under a contract which has an advance notice provision and W_{iN} if he or she works under a contract without such a provision, then the relative wage differential the worker implicitly pays for the provision (d_i) can be defined as

$$(4.1) \quad d_i=(W_{iA}-W_{iN})/W_{iN}\approx\log(W_{iA}/W_{iN})$$

and values of d_i that are less than zero would indicate that workers must pay to obtain the provision.

Unfortunately, it is not possible to observe both W_{iA} and W_{iN} in our survey data, because prior to displacement each individual was either covered by an advance notice provision or not. A naive approach to circumvent this problem would be to estimate wage equations separately for individuals covered by and not covered by such provisions, to use the estimated coefficients from these regressions and the characteristics of a representative individual to compute predicted values of the predisplacement wage rate that the individual would receive in both "sectors," and then to estimate the differential by calculating the percentage difference in these predicted values.

More formally, suppose that the wage rate an individual would receive in a job with an advance notice provision is a log linear function of a vector of variables X that represent characteristics of the individual (e.g., education) and employer (e.g., industry) plus an error term ϵ_{iA}

$$(4.2) \quad \log W_{iA} = \sum_{j=1}^{k} \alpha_{jA} X_{ji} + \epsilon_{iA}$$

and that a similar functional relationship describes the individual's wage in the absence of the provision

$$(4.3) \quad \log W_{iN} = \sum_{j=1}^{k} \alpha_{jN} X_{ji} + \epsilon_{iN}.$$

The naive approach would involve estimating the parameters of equation (4.2) by ordinary least squares (OLS) from observations on individuals who work under contracts with advance notice provisions and the parameters of equation (4.3) by OLS from observations on individuals who are not covered by such provisions. Given estimates of these parameters, $\hat{\alpha}_{jA}, \hat{\alpha}_{jN}$, and the characteristics of a representative individual and his employer, X_{ji}, an estimate of the relative wage differential workers must "pay" for advance notice provisions can then be obtained from

$$(4.4) \quad \hat{d}_i = \log \hat{W}_{iA} - \log \hat{W}_{iN} = \sum_{j=1}^{k} (\hat{\alpha}_{jA} - \hat{\alpha}_{jN}) X_{ji}.$$

A simplified variant of the above approach is to assume that all the coefficients in equations (4.2) and (4.3) are equal except for the intercept terms. In that case, one may utilize the data for all individuals together and estimate

$$(4.5)\quad \log W_i = \sum_{j=1}^{k} \alpha_j X_{ji} + \alpha_{k+1} g_i + \epsilon_i$$

where g_i equals one if individual i received advance notice and zero otherwise. In this case, the estimated wage differential associated with advance notice does not vary with workers' characteristics (as in (4.4)), but rather is constant and is given by

$$(4.6)\quad \hat{d}_i = \log \hat{W}_{iA} - \log \hat{W}_{iN} = \hat{\alpha}_{k+1}.$$

It is well known though, that estimates of wage equations from truncated samples (such as in (4.2) and (4.3)) will not necessarily yield unbiased estimates of the underlying structural wage equations, since the assumption that the error terms in each equation are random and uncorrelated with the other explanatory variables is typically violated.[4] The problem occurs here because employees and employers are not randomly assigned to contracts that have advance notice provisions, but rather make explicit choices. Estimates of the wage equation that ignore the underlying choice model will be biased because they will confound the effect of an explanatory variable on wages with its effect on the probability that an individual is employed under a contract calling for advance notice. To adequately correct for this sample selectivity problem requires a model of the underlying economic choice process that determines whether an individual is observed working under such a provision. This problem is complicated by the fact that these provisions are a product of both employee and employer decisions. We develop such a model below.

Consider first the employee, or union, side of the problem. A union's desire to obtain an advance notice clause in a contract is undoubtedly *negatively* related to the price ($-d_i$) that it would have to pay for the provision (in terms of lower wages) and *positively* related to its perceptions that a layoff or plant closing might take place *and* be costly to its members. Suppose that these perceptions can be represented by a vector of variables, Y, that includes characteristics of the employees, the employer's industry, and the labor market in which the employer is located. For example, older employees, those employed in industries where employment is falling, and those who reside in labor markets with high unemployment rates, might all have strong preferences to obtain an advance notice provision to aid their labor market adjustments if a layoff or plant shutdown occurred.

Without loss of generality, we assume that the union's desire to obtain an advance notice provision is given by

$$(4.7) \quad S_{1i}^{*}=\gamma_{10}d_i+\sum_{r=1}^{R} \gamma_{1r}Y_{ri}+V_{1i} \qquad \gamma_{10} > 0$$

$$S_{1i}=1 \quad \text{if} \quad S_{1i}^{*} > 0$$
$$=0 \text{ otherwise.}$$

Here V_{1i} is a random error term and S_{1i}^{*} is an unobserved variable that indicates the union's desire to have an advance notice provision. Although S_{1i}^{*} is not observable, one can arbitrarily scale it so that if it is greater than zero the union would prefer to have the provision ($S_{1i} = 1$), while if it is less than or equal to zero the union would prefer not to have the provision ($S_{1i} = 0$).

Turning next to the employer's side of the market, an employer's willingness to grant an advance notice provision is undoubtedly *positively* related to the price the union is willing to pay in terms of lower wages ($-d_i$) to obtain the provision. Similarly, it is also related to the employer's perception of the other costs and benefits of the provision to him. For example the provision is likely to be less costly if there is little danger (in his view) of a shutdown or layoff or if his employees are skilled workers with good market alternatives who threaten to quit unless such a provision is included in the contract. Suppose his perceptions of these other costs and benefits can be captured by a vector of variables, Z. Then, again without loss of generality, we assume that the employer's willingness to have an advance notice provision is given by

$$(4.8) \quad S_{21}^{*}=\gamma_{20}d_i+\sum_{m=1}^{n} \gamma_{2m}Z_{mi}+V_{2i}\,, \qquad \gamma_{20} < 0$$

$$S_{2i}=1 \quad \text{if} \quad S^*_{2i}>0$$
$$=0 \text{ otherwise.}$$

Once again V_{2i} is an random error term and S^*_{2i} is an unobservable variable that indicates the employer's willingness to have an advance notice provision in his contract. Again this index can be arbitrarily scaled so that when it is greater than zero the employer is willing to have such a provision ($S_{2i} = 1$), and when it is less than zero he is not willing ($S_{2i} = 0$).

Of course the preferences of an employer and a union to have such a provision can only be partially observed. In particular, all we can observe in a particular situation is whether a worker is covered by an advance notice provision. Presumably this occurs only if both the union and the employer feel that it is in their best interest to have such a provision ($S_{1i} = 1$ and $S_{2i} = 1$). It immediately follows that whether or not an advance notice provision occurs can be represented by the single binary random variable

$$(4.9) \quad S_{3i}=S_{1i}S_{2i}.$$

Given "appropriate" assumptions about the distribution of error terms in (4.2), (4.3), (4.7), and (4.8), the system of equations described in (4.2) to (4.4) and (4.7) to (4.9) can be estimated and consistent estimates derived of the wage differentials unions must pay for advance notice.[5] Appendix B sketches how this may be done.

Empirical Estimates: Do Workers Pay for Advance Notice?

We begin our empirical analyses by estimating predisplacement weekly earnings equations for displaced workers, in which we assume that advance notice provisions influence only the intercept terms of the earnings equation (i.e., equation (4.5)). The analyses were done separately for males and females, and for each gender we estimated equations for nine groups (entire sample, those on layoff, those displaced due to a shutdown, those with job tenure at displacement of less than three years, those with job tenure of at least three years at displacement, and four reason-for-displacement/length-of-job-tenure groups). Our goal here was to see whether compensating wage differentials for advance notice arise only for certain subsets of displaced workers.

Estimates of the coefficients of the advance notice variables (the $\hat{\alpha}_{k+1}$) that we obtained appear in table 4.1. Three equations were estimated for each group. The coefficients in column A come from earnings equations that included vectors of personal characteristics, region and year of displacement dummy variables (to control for price differentials), and the percentage of employees who were union members in the three-digit industry in which the individual was employed at the date of displacement.[6] Those in column B come from equations that included all of the above *plus* the area (state or SMSA) unemployment rate, change in the unemployment rate, area employment growth rate, industry employment growth rate in the area, and percentage of employees unionized in the area. These latter variables are all based on knowledge of where respondents resided at the January 1984 *survey* date. However, as noted in chapter 3, approximately 18 percent of respondents changed their city or county of residence between the date they were displaced and the survey data and thus we *may* have imputed the wrong "area" variables to these people's records. To test for this, the coefficients found in column C come from the same equations as those in column B, with the samples restricted in column C to those respondents who had not changed their city or county of residence between the displacement and survey dates.

These estimates in the main do *not* provide much support for the hypothesis that compensating predisplacement wage differentials arise for the explicit or implicit promise of advance notice. For the most part, the differentials for males are positive, which is the opposite of our expectations.[7] In contrast, those for females are negative, but only those for females displaced due to a plant closing who had short job tenure (column A) are statistically significantly different from zero; in this case they are in the range of 5 to 6 percent.[8]

Table 4.1
Estimated Wage Differentials Prior to Displacement Associated With Advance Notice: OLS Estimates
(absolute value of *t*-statistics)

	Males				Females			
	N	(A)	(B)	(C)	N	(A)	(B)	(C)
(1)	3749/2803	.027 (1.8)	.032 (2.2)	.021 (1.3)	1981/1604	–.019 (1.0)	–.005 (0.3)	–.007 (0.3)
(2)	2251/1686	.032 (1.7)	.038 (2.0)	.014 (0.7)	1036/830	.006 (0.3)	.018 (0.7)	.020 (0.7)
(3)	1497/1116	.019 (0.8)	.021 (0.9)	.023 (0.9)	944/773	–.050 (1.7)	–.033 (1.1)	–.037 (1.1)
(4)	1925/1352	.042 (2.0)	.051 (2.4)	.048 (1.9)	1095/851	–.029 (1.1)	–.006 (0.2)	–.010 (0.4)
(5)	1823/1450	.004 (0.2)	.004 (0.2)	–.014 (0.7)	885/752	–.017 (0.5)	–.014 (0.5)	–.011 (0.3)
(6)	672/455	–.008 (0.8)	–.004 (0.1)	.000 (0.0)	459/353	–.067 (1.6)	–.037 (0.9)	–.081 (0.7)
(7)	663/548	.042 (1.3)	.033 (1.0)	.025 (0.7)	379/332	–.003 (0.1)	–.003 (0.1)	–.003 (0.1)
(8)	1252/896	.072 (2.8)	.080 (3.0)	.062 (2.0)	635/497	–.009 (0.3)	.023 (0.7)	.017 (0.5)
(9)	740/606	–.003 (0.1)	–.005 (0.0)	–.034 (1.0)	281/244	–.030 (0.7)	–.055 (1.0)	–.039 (0.7)

where N = sample size for (A) and (B)/sample size for (C)

Samples
(1) entire sample of displaced workers
(2) displaced due to layoff
(3) displaced due to plant closing
(4) job tenure at displacement less than 3 years
(5) job tenure at displacement at least 3 years
(6) plant shutdown and job tenure less than 3 years
(7) plant shutdown and job tenure at least 3 years
(8) layoff and job tenure less than 3 years
(9) plant shutdown and job tenure at least 3 years

and
(A) wage equations include vectors of personal characteristics, region and year dummy variables, and the percentage of employees unionized in the respondent's 3-digit industry
(B) wage equations include the variables in (A) and SMSA or state unemployment level and change, employment growth, industry employment growth, and percentage of employees unionized in the area in which the individual resided at the survey date
(C) same as (B) but restricted to individuals who had not changed their city or county of residence between the date of displacement and the survey date

Inclusion of the geographic variables and limiting the samples to nonmovers (columns B and C), however, results in all of the estimated female differentials becoming statistically insignificantly different from zero. The magnitudes and signs of the coefficients (in contrast to their statistical significance) appear to be relatively insensitive to the specification used and in what follows we restrict our attention to the specification in column A.

Might these results be due to our aggregating displaced workers from all industries together and not controlling adequately for industry-specific effects on earnings? One crude way to get at this issue is to reduce the heterogeneity of industries in the sample and reestimate the model that underlies column A, restricting the sample to those displaced workers who were employed prior to displacement in either manufacturing industries or, reducing the heterogeneity still further, in durable manufacturing industries. This was done and the resulting coefficients of the advance notice variable appear in table 4.2. Inspection of these coefficients (they are typically positive and are never both negative and statistically significantly different from zero) yields no support for the presence of compensating wage differentials.

Table 4.2
Estimated Wage Differentials Prior to Displacement Associated With Advance Notice (Manufacturing and Durable Manufacturing Sample) OLS Estimates[a] (absolute value of t-statistics)

Males			Females		
N_m/N_D	Mfg.	Durable mfg.	N_m/N_D	Mfg.	Durable mfg.
(1) 2063/1164	.026 (1.37)	.014 (0.60)	1346/447	.026 (1.18)	.014 (0.41)
(2) 1635/736	.024 (1.16)	.023 (0.78)	1164/265	.043 (1.76)	.057 (1.23)
(3) 1326/427	.040 (1.69)	.019 (0.47)	1080/181	.021 (0.81)	−.027 (0.47)
(4) 1392/493	.019 (0.81)	−.019 (0.50)	1109/210	.034 (1.34)	.013 (0.23)
(5) 1569/670	.037 (1.77)	.019 (0.67)	1135/236	.029 (1.17)	−.023 (0.49)
(6) 1047/148	.018 (0.67)	−.100 (1.27)	962/63	.033 (1.17)	−.072 (0.54)
(7) 1132/233	.051 (2.00)	.024 (0.46)	996/97	.032 (1.20)	−.060 (0.82)
(8) 1243/344	.034 (1.37)	.032 (0.69)	1045/146	.042 (1.61)	.051 (0.81)
(9) 1191/297	.024 (0.97)	−.008 (0.19)	977/78	.040 (1.47)	.050 (0.58)

a. The samples in each row corresponds to those defined in each row of table 4.1. The differentials correspond to those found in column (A) of that table.

Of course, these results may still be due to our failure to control adequately for industry-specific effects on wages and to our assumption (up to this point) that the presence of advance notice provisions affects only the intercept, not the other coefficients in the earnings equations. To test for this, we extended our analyses in two ways First, separate earnings equations were estimated for the displaced workers who received advance notice and for those who did not receive notice and then an estimated compensating differential was obtained for each individual, as described in the more general model ((4.2) to (4.4)).

Second, an earnings equation was estimated for all employed individuals represented in the May 1979 *CPS* data using as explanatory variables the individuals' personal characteristics and a set of dichotomous variables for the 3-digit industries in which they were employed. The estimated coefficients of these 200-plus industry variables, which we shall refer to as the "industry wage differentials," are estimates of "unexplained" industry-specific effects on earnings, and we experimented with including each displaced worker's industry wage differential in the predisplacement earnings equation.

Table 4.3 presents the estimated compensating wage differentials (equation 4.4) that we obtained when these extensions were undertaken. These were done separately for four groups; males displaced due to a layoff, females displaced due to a layoff, males displaced due to a plant shutdown and females displaced due to a shutdown. A list of the explanatory variables that appear in each question is found at the bottom of the table. Estimates are presented in the table of the mean (across individuals) compensating wage differential for advance notice and the minimum and maximum differentials. The range between the latter two is typically quite large and we discuss only estimates of the mean differential below.

We focus our attention initially on the rows in the table denoted $\hat{w}_1$ and $\hat{w}_3$ for each group. Estimates in these rows are obtained as described above; the former are based on estimating equations that include the industry wage differential, while the latter are based on equations that exclude it. Comparisons of the numbers in the two

Table 4.3
Estimated Wage Differentials Prior to Displacement Associated With Advance Notice, Separate Wage Equations by Sector[a]

Group	Estimated wage differentials		
Model [Sample size]	Mean	Minimum	Maximum
Male/Layoff			
$\hat{W}1$ [2459]	.027	−.396	.513
$\hat{W}2$ [2330]	.063	−.391	.714
$\hat{W}3$ [2459]	.028	−.396	.514
$\hat{W}4$ [2330]	.063	−.391	.741
Female/Layoff			
$\hat{W}1$ [1121]	−.007	−.362	.815
$\hat{W}2$ [1036]	−.075	−.584	.727
$\hat{W}3$ [1121]	−.007	−.362	.815
$\hat{W}4$ [1036]	−.075	−.585	.727
Male/Shutdown			
$\hat{W}1$ [1696]	.023	−.282	.836
$\hat{W}2$ [1629]	−1.100	−2.145	.117
$\hat{W}3$ [1696]	.023	−.283	.837
$\hat{W}4$ [1629]	−1.100	−2.145	.118
Female/Shutdown			
$\hat{W}1$ [1028]	−.077	−.650	.649
$\hat{W}2$ [976]	−.268	−1.178	.651
$\hat{W}3$ [1028]	−.078	−.650	.650
$\hat{W}4$ [976]	−.268	−1.178	.652

where

$\hat{W}1$—separate wage equations, no sample selection correction, estimated 3-digit census industry wage differential included in each equation

$\hat{W}2$—separate wage equations, sample selection correction, estimated industry wage differential included in each equation

$\hat{W}3$—same as $\hat{W}1$, but no industry wage differential included

$\hat{W}4$—same as $\hat{W}2$, but no industry wage differential included

a. All wage equations include personal characteristics (age, age squared, education, education squared, age-education interaction, tenure prior to displacement, tenure squared, race, marital status, hispanic status, health insurance status prior to displacement), dummy variables for Maine and Wisconsin, dummy variables for region and year of displacement, an SMSA dummy, area (SMSA or state) unemployment and unionization rates, a manufacturing dummy variable, and the unionization rate nationally in the individual's 3-digit industry. All variables refer to the year prior to displacement.

rows for each group, however, make it clear that industry-specific effects do *not* substantially influence the estimated compensating wage differentials that exist for advance notice.

Quite strikingly, the mean estimated effects are positive for both male groups; on average, displaced males who received advance notice did not "pay" for the notice in the form of lower predisplacement earnings. In contrast, the mean estimated effects *are* negative for both female groups. On average, displaced females who received advance notice also received predisplacement wages that were about .7 percent (displaced due to layoffs) to 7.7 percent (displaced due to plant shutdowns) lower than the predisplacement wages of displaced workers who failed to receive advance notice.

As discussed above, these estimates may still be suspect because they treat whether displaced workers received advance notice as being random. To control for potential selectivity bias problems, we attempted to implement the econometric framework described in (4.7), (4.8) and (4.9) that explicitly modeled employees' demand for and employers' willingness to provide advance notice provisions. Unfortunately, this effort proved unsuccessful because to identify the complete model requires one to specify some variables that enter into the employees' demand function (4.7) but not the employers' willingness to supply function (4.8) and vice versa. When one examines the available data on individual, industry, and area variables (table 3.1), it becomes clear that virtually any of the variables one might want to use to explain preferences probably enter both the employee and employer side of the market.

As such, we were forced to adopt a simpler approach. Suppose one is willing to assume that the employer and employee choice of advance notice rules specified in (4.7) to (4.9) can be approximated (after substitution of (4.1) to (4.3) into these equations) by the single decision rule

$$(4.10) \quad S_{4i}^{*}=\sum_{t=1}^{T} \theta_t R_{ti}+V_{4i}$$

$$S_{4i}=1 \quad \text{if} \quad S_{4i}^{*}>0$$
$$=0 \text{ otherwise}$$

where V_{4i} is a random error term, the R_{ti} are all of the individual, industry, and area variables that enter either the earnings equations, the employer willingness to supply advance notice equation, or the employee demand for advance notice equation, and when S^*_{4i} is greater than zero one observes the presence of advance notice ($S_{4i} = 1$), otherwise one does not ($S_{4i} = 0$).

Under suitable assumptions about the distribution of error terms in (4.2), (4.3), and (4.10), one can obtain consistent estimates of the earnings equations using a now well-known two-step procedure.[9] Technical details are described in appendix B. Basically this involves first estimating the reduced form determinants of advance notice equation (4.10), using estimates of it to compute a variable that controls for the probability of observing advance notice, and then estimating the earnings equations in each sector with this additional variable (or a transformation of it) added to correct for "sample selection bias."

Our estimates of the determinants of the probability of observing that a displaced worker received advance notice will be presented and discussed in the next section. These estimates were used, however, to compute the controls for sample selection bias, and the estimated wage differentials that arise for advance notice when the sample selection bias correction method was used are reported in the rows denoted $\hat{w}_2$ and $\hat{w}_4$ of table 4.3.

The estimated mean wage differentials for males and females displaced due to a layoff are somewhat larger (in absolute value) than those obtained before. In contrast, while negative, the estimated average differentials for the males and females who were displaced due to a plant shutdown are much too large to be considered plausible.[10] We conclude, therefore, that one cannot put too much faith in these estimates; the ordinary least square estimates reported in rows $\hat{w}_1$ and $\hat{w}_3$ are preferable.

Empirical Analysis: The Determinants of Advance Notice

Table 4.4 presents our estimates of the determinants of the probability that each displaced worker in the sample received advance

Table 4.4
Reduced Form Probit Probability of Advance Notice Coefficients
(absolute value of *t*-statistics)

Variable/ Sample	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	–.561 (0.4)	2.985 (1.7)	–1.239 (0.9)	.426 (0.2)
IMALE	–.223 (1.0)	–.535 (2.1)*	–.394 (1.5)	–.751 (2.7)*
IBLACK	–.175 (0.1)	–1.364 (0.8)	–.127 (0.1)	–2.553 (1.5)
IHISP	–2.146 (1.0)	–4.809 (1.8)	–.348 (0.1)	–8.625 ([illegible].0)*
IAGE	.021 (0.8)	–.013 (0.4)	.006 (0.2)	–.017 (0.5)
IA55	–.044 (1.8)	–.043 (1.3)	.014 (0.5)	.006 (0.2)
ITEN	–.020 (0.6)	.078 (1.4)	.054 (1.4)	.103 (1.6)
ICOL	.020 (2.3)*	.020 (1.9)	.004 (0.3)	.013 (1.1)
IHS	–.001 (0.1)	–.004 (0.3)	.008 (0.7)	.001 (0.0)
IPROF	–.002 (0.2)	–.013 (1.6)	–.005 (0.7)	–.013 (1.5)
IADS	–.004 (0.8)	–.005 (0.6)	–.011 (1.7)	–.011 (1.4)
IBCS	.007 (2.5)*	.007 (1.4)	.004 (1.2)	.004 (0.8)
IBCU	–.006 (0.5)	–.041 (2.2)*	–.022 (1.7)	.005 (0.3)
IUNION	.005 (2.1)*	.004 (0.9)	.001 (0.3)	–.002 (0.6)
MANUF	–.038 (0.5)	–.197 (1.5)	–.137 (1.4)	–.111 (0.8)
AUNEMP	–.038 (1.9)	.049 (1.6)	.006 (0.2)	.005 (0.1)
AUNEMPC	.037 (1.2)	.118 (2.5)*	.025 (0.6)	.002 (0.3)
AEMPC1	–1.087 (0.6)	–4.158 (1.2)	.392 (0.2)	.732 (0.2)
AEMPC2	–.982 (0.8)	1.405 (0.9)	–.370 (0.2)	1.053 (0.8)
AEMPC3	–.466 (0.5)	5.485 (2.2)	–.373 (0.3)	–1.928 (0.8)
AEMPC4	–.067 (0.1)	–1.769 (1.8)	–.336 (0.5)	.074 (0.1)
AUNION	.007 (1.3)	–.021 (2.5)*	–.011 (1.7)	.002 (0.2)
MSMSA	.117 (2.0)*	.159 (1.7)	–.074 (1.0)	.129 (1.3)
YD1	–.081 (0.4)	.398 (1.0)	.527 (1.3)	–.594 (0.7)
YD2	–.731 (0.3)	.172 (0.5)	.613 (1.5)	–.707 (0.8)
YD3	–.291 (1.4)	–.062 (0.2)	.623 (1.5)	–.471 (0.6)
YD4	–.165 (0.8)	–.012 (0.1)	.547 (1.4)	–.410 (0.5)
YD5	–.180 (0.9)	–.075 (0.2)	.461 (1.1)	–.408 (0.5)
R1	–.111 (1.1)	.025 (0.2)	–.078 (0.7)	.328 (2.1)*
R2	–.149 (1.8)	–.108 (0.8)	–.018 (0.1)	.147 (1.1)
R3	–.073 (0.9)	–.272 (1.9)	–.098 (0.9)	–.105 (0.7)
AGET	–.002 (0.1)	–.032 (1.0)	–.003 (0.1)	.015 (0.5)
AGET2	.000 (0.7)	.000 (0.1)	.000 (1.1)	.001 (0.8)
REDUC	.067 (0.8)	–.143 (1.1)	.072 (0.9)	.185 (1.2)
REDUC2	–.002 (0.8)	.002 (0.7)	.000 (0.1)	–.003 (0.6)
AGEED	–.001 (0.5)	.002 (1.1)	–.002 (1.8)	–.002 (1.6)

Table 4.4 (Continued)

Variable/ Sample	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
TENURE	.020 (1.5)	.043 (1.9)	.029 (2.3)*	.032 (1.7)
TEN2	−.001 (1.4)	−.002 (1.9)	−.006 (1.6)	−.002 (1.9)
RRACE	−.019 (0.2)	−.001 (0.1)	−.205 (1.9)	.200 (1.4)
RHISP	−.089 (0.7)	.281 (1.4)	−.017 (0.1)	−.182 (0.9)
RMAR	.024 (0.4)	.042 (0.5)	−.089 (1.2)	.185 (2.1)*
RVET	−.051 (0.8)		−.014 (0.2)	
RINST	.053 (0.9)	.262 (2.8)*	.172 (2.3)*	.252 (2.7)*
MAINE	.042 (0.2)	−.015 (0.1)	.397 (1.2)	−.486 (1.3)
WISC	−.064 (0.3)	.371 (1.3)	.121 (0.4)	.063 (0.2)
WDIF	−.089 (0.3)	−.037 (0.9)	−.975 (2.5)*	.295 (0.6)
RADV = 0	1186	562	737	401
RADV = 1	1144	474	892	575
X^2(DOF)	65.62 (45)	85.81 (44)	74.19 (45)	87.23 (44)

*Coefficients statistically significantly different from zero at .05 level; two-tail test.
See table 3.1 for all variable definitions.

notice of his or her displacement. The probability is assumed to depend upon characteristics of the individual, characteristics of the workforce in the industry the individual was displaced from, and labor market characteristics in the area (state or SMSA) in which the individual resided at the time of displacement. These equations are best interpreted as approximations to the process that generates advance notice, each variable enters these equations either because it influences employer preferences, employee preferences, or predisplacement earnings. If the error term in these probability equations is assumed to be normally distributed, they become the familiar probit model.

Definitions of all the variables included in the analysis are found in table 3.1. The industry level variables (which start with the letter "I") represent the demographic and occupational composition of employees in the industry, as well as the percent unionization in the industry and whether the industry is in manufacturing. The area (state or SMSA) characteristics (which start with the letter "A") include recent unemployment rate levels and changes and recent employment changes; the latter two variables are both for the entire area and the industry in which the individual was employed in the area. Region (R) and year (YD) dummy variables are also included. Finally, a host of characteristics of the individual displaced workers are included; among these are dummy variables that indicate whether an individual was employed prior to displacement in Maine or Wisconsin—the two states that had mandatory advance notice legislation "on the books" during the 1979-83 period.

Separate analyses are reported in the table for four gender/reason-for-displacement groups. In each case, a chi-square test indicates that one can reject the hypothesis that these models have no explanatory power. Many of the explanatory variables are highly correlated with one another and therefore most of the coefficients of the individual variables are statistically insignificantly different from zero. Our primary interest in these equations is whether or not one can predict the presence of advance notice, so in a "reduced form" sense the estimation is successful. While in general one cannot separately identify many specific important determinants of advance notice, three particular results found near the bottom of the table do stand out.[11]

First, prior coverage by an employer-provided health insurance plan (RHINST) increases the probability of receiving advance notice. This undoubtedly reflects union bargaining power and/or the correlation of "good" contract provisions across employers. Second, displaced workers in Maine and Wisconsin were not significantly more (or less) likely to receive advance notice than displaced workers in other states; apparently, at least during the 1979-83 period, laws requiring advance notice in these states did not have a large impact. Finally, industries in which the industry wage premium (WDIF) was high were less likely to provide their displaced workers with advance notice, at least for male workers displaced due to shutdowns.

Concluding Remarks

In a sense, our findings in this chapter are somewhat negative ones. We found very little evidence to support the view that displaced workers who received advance notice had to pay for it in the form of lower predisplacement wages when ordinary least squares were used. Hence, analyses of the effect of advance notice on postdisplacement earnings and durations of nonemployment can probably safely ignore this possibility. Such compensating wage differentials may have existed for some groups of displaced females, but the magnitudes were very small and we have no ready explanation for why only this group's wages would be affected.

Our a· mpts to explain which displaced workers received advance notice also did not meet with much success. While one can reject the hypothesis that the models we estimated had no explanatory power, very few of the large number of individual, industry and area explanatory variables used were shown individually to have statistically significant effects. Moreover, when we used the estimates of the determinants of advance notice to try to correct for sample selection bias in the estimation of the predisplacement wage equations, implausibly large wage differentials were estimated for males and females displaced due to plant shutdowns. Thus we are skeptical of our ability to use the estimates obtained here to control for the endogeneity of advance notice in the duration of nonemployment and postdisplacement wage equations estimated in the next two chapters. As the reader shall see, such skepticism is justified.

NOTES

1. See Ehrenberg and Schumann (1982) for evidence that unions win positive compensating wage differentials for distasteful mandatory overtime provisions and Dickens (1984) for evidence that similar compensating differentials for on-the-job injury risk exist in the union sector.

2. A numerical example should make this point clear. Suppose two equally skilled workers were displaced. One, working for an employer who did not provide advance notice, was earning $10.00/hour, while the second, working for an employer who did provide advance notice, received $9.75/hour. The difference is the assumed compensating wage differential for advance notice. Suppose further that they both wind up in $9.00/hour jobs (i.e., advance notice has no effect). The worker with notice would appear to have a smaller wage loss ($.75/hour rather than $1.00/hour). However, this would reflect only that part of his predisplacement compensation was in the form of a promise of advance notice—in this example notice was assumed to have *no* effect on postdisplacement earnings.

3. For expository convenience, we focus on the union sector in what follows. Similar logic can be used to discuss the nonunion sector if one treats advance notice as an implicit contract provision.

4. See Heckman (1979) for a detailed discussion of this point.

5. If the error terms in (4.7) and (4.8) are jointly normally distributed, the model in (4.7)-(4.9) is called the truncated bivariate probit model; this model was first discussed by Poirer (1980).

6. The personal characteristics variables include age, age squared, education, education squared, tenure with the employer prior to displacement, tenure squared, race, marital status, hispanic status, and coverage by health insurance status prior to displacement.

7. We must caution here that the *SDW* did *not* contain data on the respondents' union status as of the date of displacement. If union status is an omitted variable that (a) leads to higher wages and (b) is correlated with the presence of advance notice, the advance notice coefficients will be biased in a positive direction. While we attempted to control for union status in the earnings equations by including the percentage of employees unionized in each respondent's area and industry, these controls may not be adequate.

8. In other studies, for example, Ehrenberg and Schumann (1982), compensating wage differentials are seen to arise only for workers with short job tenure.

9. See Heckman (1979).

10. Since $\hat{d}_i = \log \hat{w}_{1A} - \log \hat{w}_{1N}$, the estimated percentage wage differential workers must pay for advance notice is given by $\hat{w}_{1A} - \hat{w}_{1N} = (\exp(\hat{d}i)-1)*100$. Since the sample selection correction estimate of $\hat{d}_i$ is -1.1 for males displaced due to a plant shutdown, the estimated percentage predisplacement wage reduction associated with advance notice is $(\exp(-1.1)-1)*100$ or -67 percent. It is simply implausible that workers would accept a wage cut of 2/3 in return for a promise of advance notice if they were displaced. Similarly, the $\hat{d}_i$ of $-.268$ for females displaced due to a plant shutdown implies that the estimated percentage predisplacement wage reduction associated with the promise of advance notice is $(\exp(-.268)-1)*100$ or -23 percent. Even this figure seems implausibly high.

11. By restricting the set of explanatory variables, it is possible to estimate advance notice equations in which most coefficients are statistically significantly different from zero. The results reported in subsequent chapters are insensitive to such restrictions and the more inclusive specifications reported in table 4.4 are preferred for the compensating differential analyses reported in this chapter.

5

Advance Notice and Nonemployment Duration

Table 5.1 presents background data on the durations of nonemployment that displaced workers in the 1984 *SDW* experienced between their displacement dates and the January 1984 survey date. These data were reported by complete weeks of nonemployment, and the top row of the table, 0 weeks, refers to individuals who had either no nonemployment or a spell that lasted less than one week. Quite strikingly, 7 to 8 percent of the displaced workers who were laid off reported 0 weeks and 12 to 14.5 percent of those displaced due to a plant shutdown also reported 0 weeks. Moreover, in each gender/reason-for-displacement group for whom data are tabulated in table 5.1, a greater percentage of the workers who received advance notice reported 0 weeks than did the workers who failed to receive notice.

Do these results imply that advance notice increases the probability that a displaced worker will suffer no spell of nonemployment? Or do they simply mean that workers who receive advance notice are more likely to have skills that make them rapidly employable or are more likely to reside in areas where there are better job opportunities? The following section addresses these questions.

The remaining rows of table 5.1 report the distribution of weeks of nonemployment for displaced workers who had durations of at least one week. Individuals reported all durations of 99 weeks or more as 99 in the *SDW* and, depending upon the gender/reason-for-displacement group, 8.5 to 14.7 percent of the displaced workers fell in this category. If one treats these people as having durations of 99 weeks, one can compute lower bound estimates of the mean durations of nonemployment experienced for each group. This ranges

Table 5.1
Distributions of Durations of Nonemployment in the January 1984 *CPS* Displaced Worker Sample

	Percentages											
	Male Layoff			Female Layoff			Male Shutdown			Female Shutdown		
Number of weeks	(All)	(A)	(N)	(All)	(A)	(N)	(All)	(A)	(N)	(All)	(A)	(N)
0	7.2	8.8	5.6	7.8	9.6	6.3	14.5	18.0	10.3	12.1	13.7	9.7
1-13	38.7	37.2	40.2	31.7	29.8	33.2	36.1	33.8	39.0	34.3	33.7	35.2
14-26	17.4	17.3	17.5	17.3	17.6	17.0	16.4	14.4	18.8	14.6	13.3	16.5
27-39	8.3	8.0	8.5	7.7	8.8	6.8	6.7	7.0	6.4	7.4	8.3	6.1
40-52	10.2	9.7	10.8	10.5	9.8	11.2	9.6	9.2	10.0	10.0	10.3	9.5
53-78	7.7	7.1	8.2	8.8	8.1	9.4	6.6	7.1	6.0	5.6	5.9	5.2
79-98	1.4	1.5	1.3	1.4	0.9	1.8	1.6	1.5	1.7	1.7	2.5	0.7
≥99	9.2	10.4	8.0	14.7	15.4	14.2	8.5	9.0	7.8	14.2	12.3	17.0
Mean duration reported	29.3			35.0			26.9			31.9		
Sample size	2481	1232	1249	1138	533	605	1699	932	707	1034	611	423

SOURCE: Authors' computations from the January 1984 *CPS* Displaced Worker Supplement sample used in the analyses. Mean duration calculations treat 99 (greater than or equal to 99) as 99.

(All)–All displaced workers; (A)–displaced workers with advance notice; (N)–displaced workers without notice.

from 26.9 weeks for males displaced due to plant shutdowns to 35.0 weeks for females displaced due to layoffs. Visual comparisons of the distributions for the workers who received and those who failed to receive advance notice do not suggest any obvious differences between the two groups. Such comparisons do not control for any differences in other variables between the groups, and this chapter will address what the effects of advance notice on duration of nonemployment (*given* that duration was positive) were, after one controls for other factors expected to influence duration.

If advance notice increases the probability that a displaced worker will experience no spell of nonemployment, it does so because it permits search for new employment prior to displacement. If soon-to-be-displaced workers are successful in this endeavor, they are apt to quit their jobs prior to the planned displacement date. Among the arguments that opponents of advance notice legislation put forth is that notice of impending displacement would encourage a firm's most productive workers, who may well be those with the best alternative employment opportunities, to quit their jobs prior to the planned displacement date. Thus, it is argued, the demise of firms that already are in serious trouble would be hastened.

While creative approaches can be devised to minimize the chances this would happen, such as providing severance pay only to workers who are still employed as of the shutdown or layoff date, critics of advance notice still stress this point. Hence, knowledge of which displaced workers would actually leave after being given advance notice prior to displacement is important for the policy debate. This chapter also addresses this issue.

Advance Notice and the Probability of Observing a Positive Duration of Nonemployment

What determines whether a displaced worker suffers a positive duration of nonemployment? On the one hand, personal characteristics of the worker are likely to matter. For example, skilled workers may typically have an easier time finding work than nonskilled workers. On the other hand, characteristics of the labor market, such as the area unemployment rate, will determine the worker's success at quickly finding work. Finally, to the extent that advance notice permits the worker to search for work prior to the date of displacement and/or if it is accompanied by other services that facilitate job matching, advance notice should also matter.

These ideas are captured in table 5.2, which presents the results of the estimation of probit probability of experiencing positive duration of nonemployment equations for the displaced workers in the *SDW*.[1] The personal characteristics of the workers included in the model are marital status (RMAR), race (RRACE), age at the displacement date and its square (AGET, AGET2), veteran status (RVET), years of schooling (REDUC), whether the worker is hispanic (RHISP), years of job tenure with the employer prior to displacement and its square (TEN, TEN2) and whether the worker was covered by health insurance (RHINST), was a white-collar worker (WCOL), or was employed in manufacturing (MANUF). Year of displacement (YD) and region of employment at displacement (R) dummy variables are included to partially control for economic conditions the worker may have faced. Area-(state or SMSA) specific variables included are the area unemployment rate in the year of displacement (AU), the percentage of the area's workforce that was unionized (AUNION), and whether the individual resided in an SMSA (MSMSA). Finally, a variable indicating the receipt of advance notice (RADV, 1=yes, 0=no) is included.

These equations were estimated separately for males displaced due to layoffs, females displaced due to layoffs, males displaced due to plant shutdowns and females displaced due to plant shutdowns. For all four groups, chi-square statistics reported at the bottom of the table suggest that one cannot reject the hypothesis that these equations have significant explanatory power and at least some of the variables have statistically significant nonzero coefficients in each equation. For example, as one might expect, other things equal, in all four groups nonwhites had a higher probability of experiencing a spell of nonemployment than did whites (perhaps due to discriminatory factors) and more highly educated workers had a lower probability than less highly educated workers. Similarly, workersdisplaced from manufacturing had a higher probability than did other workers.

Table 5.2
Probit Probability of Positive Duration of Nonemployment Equations for Displaced Workers
(absolute value *t*-statistics)

Variable/Group	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	1.891 (3.0)	2.612 (2.9)	1.102 (1.5)	4.117 (1.0)
RMAR	–.149 (0.1)	–.005 (0.1)	–.216 (2.2)	.183 (1.2)
RRACE	.309 (2.2)	.524 (2.7)	.302 (1.9)	.388 (2.1)
AGET	–.011 (0.3)	–.071 (1.5)	–.009 (0.3)	.006 (0.1)
AGET2	.000 (0.4)	.001 (1.4)	–.000 (0.6)	–.000 (0.4)
RVET	–.037 (0.4)		.060 (0.6)	
REDUC	–.086 (4.7)	–.061 (2.2)	–.058 (3.0)	–.082 (3.0)
RHISP	–.216 (1.3)	.791 (1.8)	.194 (1.0)	.313 (1.1)
RHINST	.043 (0.5)	–.233 (1.7)	.015 (0.1)	.093 (0.8)
YD1	.577 (2.1)	.781 (1.7)	.096 (0.2)	–2.663 (0.6)
YD2	.352 (1.4)	.881 (2.0)	.274 (0.6)	–2.595 (0.6)
YD3	.367 (1.5)	.727 (1.7)	.532 (1.1)	–2.451 (0.6)
YD4	.593 (2.5)	.842 (2.0)	.584 (1.2)	–2.363 (0.6)
YD5	.515 (2.2)	.842 (2.0)	.584 (1.2)	–2.439 (0.6)
TENURE	–.029 (1.6)	–.034 (0.7)	–.005 (0.3)	–.004 (0.1)
TEN2	.001 (0.6)	.003 (1.0)	.000 (0.6)	–.000 (0.1)
R1	.165 (1.3)	.227 (1.1)	–.127 (1.0)	.206 (1.2)
R2	.105 (1.0)	–.020 (0.1)	.001 (0.6)	.204 (1.4)
R3	.139 (1.2)	–.153 (0.8)	.255 (2.0)	.064 (0.4)
WCOL	.128 (1.3)	.047 (0.3)	–.129 (1.3)	.012 (0.1)
MANUF	.062 (0.7)	.309 (2.3)	.203 (3.2)	.172 (1.5)
AU	.024 (1.1)	.047 (1.2)	–.002 (0.1)	–.015 (0.4)
AUNION	.006 (0.9)	–.010 (0.9)	.023 (3.1)	.011 (1.1)
MSMSA	.005 (0.1)	–.098 (0.8)	–.076 (0.8)	.221 (2.0)
RADV	–.144 (1.9)	–.235 (2.0)	–.381 (4.5)	–.167 (1.5)
DUR = 0	204	94	249	135
DUR > 0	2146	969	1299	794
X^2(DOF)	68.107 (24)	51.804 (23)	109.427 (24)	38.074 (23)

See table 3.1 for variable definitions and sources.

Most important, receipt of advance notice is seen to have reduced the probability of experiencing a positive length spell of nonemployment for all groups. Moreover, this relationship is statistically significantly different from zero for three of the four groups.

Several extensions were conducted to see how robust these results concerning the effects of advance notice are; these extensions are summarized in table 5.3. The first row simply reproduces the RADV coefficients from table 5.2. The second row presents the estimated effects of advance notice when the model in table 5.2 was reestimated with an instrument, $\widehat{\text{RADV}}$ (obtained from the reduced form probability of advance notice equations in table 4.4) used in place of RADV. When the instrument was used, its coefficients suggested that advance notice did not appear to significantly influence the probability that the displaced workers experienced a nonzero spell length. While the instrument should be preferred if one is concerned about the endogeneity of advance notice, the lack of statistically significant coefficients in table 4.4, reduces our confidence that it is at all meaningful. Formal statistical tests reported in the next section and appendix B suggest that it is legitimate for us to treat advance notice as exogenous.

Table 5.3
Comparison of Coefficients of Advance Notice Variable in Probit Probability of Positive Duration of Nonemployment Equations: Various Specifications (absolute value *t*-statistics)

Model/Group	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
(1) RADV	–.144 (1.9)	–.235 (2.0)	–.381 (4.5)	–.167 (1.5)
(2) $\widehat{\text{RADV}}$	.066 (0.1)	.234 (0.7)	1.067 (1.5)	.563 (0.9)
(3) RADV	–.117 (1.3)	–.220 (1.7)	–.420 (4.2)	–.169 (1.4)
RADV*T	–.080 (0.6)	–.073 (0.3)	.098 (0.7)	.005 (0.0)
(4) RADV	–.047 (0.5)	–.184 (1.0)	–.208 (2.1)	–.219 (1.3)
RADV*WC	–.372 (2.1)	–.095 (0.4)	–.512 (2.9)	.098 (0.5)

where

(1) specification in table 5.2, RADV treated as exogenous,

(2) specification in table 5.2, instrument for RADV used which was obtained from the reduced form probability of advance notice equation,

(3) same as (1), but RADV is also interacted with whether the individual had at least 3 years tenure on the job prior to displacement (T = 1, otherwise T = 0),

(4) same as (1), but RADV is also interacted with whether the individual was a white–collar employee prior to displacement (WC = 1, otherwise WC = 0).

The third row reports the results of experiments that allow the affect of advance notice to depend on whether individuals had at least three years of job tenure prior to their displacement. As is evident, the marginal effect of advance notice on the probability of experiencing a positive spell length did *not* depend on workers' prior tenure. When similar experiments were conducted to see if the effects differed for white-collar and blue-collar workers (row 4), the answer appears to be yes, at least for males. Advance notice appears to have reduced the probability that males who were displaced due to either layoffs or plant shutdowns experienced nonzero durations of nonemployment by more for white-collar workers than it did for blue-collar workers.

Some idea of the magnitude of these effects can be found in table 5.4. The underlying probit coefficient estimates and the values of the explanatory variables for each displaced worker can be used to compute what his or her expected probability of experiencing a positive duration of nonemployment would be, both with and without advance notice.[2] The difference between these two probabilities is our estimate of the effect of advance notice on that individual's nonemployment duration. The average effect over all individuals is found in the column headed "Mean." These calculations are presented for all workers, and separately for blue-collar and white-collar workers; in the latter case, the coefficient estimates that underlie row 4 of table 5.3 are used.

One can interpret the numbers in table 5.4 as follows: Males displaced due to a layoff who received advance notice had, on average, a probability of experiencing a nonzero spell length that was .022 lower than that of otherwise comparable individuals in the group who failed to receive notice. This number should be contrasted to the overall probability of having a *zero* spell length of .087 for the group.[3] The analogous pairs of numbers for females on layoff (–.035, .088), males displaced due to a shutdown (–.084, .161), and females displaced due to a shutdown (–.043, .145) are roughly of the same order of magnitude. Given that roughly 60 percent of the displaced workers in the sample actually received advance notice, elimination of advance notice would have decreased the proportion of displaced

Table 5.4
Marginal Effect of Having Had Advance Notice on the Probability of a Displaced Worker's Having a Positive Duration of Nonemployment

	Computed effects by individual			
	Mean	Std. Dev.	Minimum	Maximum
Male Layoff [.087]				
All workers[a]	−.022	.099	−.0009	−.054
White-collar[a]	−.055	.024	−.0020	−.152
Blue-collar	−.007	.003	−.0003	−.018
Female Layoff [.088]				
All workers[a]	−.035	.018	−.0004	−.086
White-collar[a]	−.041	.021	−.0004	−.101
Blue-collar[a]	−.028	.014	−.0003	−.068
Male Shutdown [.161]				
All workers[a]	−.084	.030	−.007	−.151
White-collar[a]	−.165	.053	−.017	−.281
Blue-collar[a]	−.045	.016	−.004	−.083
Female Shutdown [.145]				
All workers	−.043	.014	−.0002	−.080
White-collar	−.026	.009	−.0001	−.043
Blue-collar	−.048	.016	−.0002	−.088

[ ] fraction in the group who had *zero* durations of nonemployment.
a. Based on RADV coefficient that was statistically significantly different from zero at at least the .10 level; two-tail test.

workers observed to have zero duration of nonemployment by roughly 15 to 30 percent.[4] Moreover, as table 5.4 indicates, the effect of advance notice on this probability for males is roughly twice as large for white-collar workers as it is for blue-collar workers.

Advance Notice and Displaced Workers' Durations of Nonemployment

The evidence presented in the previous section suggests that displaced workers who received advance notice had lower probabilities of experiencing a positive length spell of nonemployment than displaced workers who failed to receive notice. In this section we focus on workers who experienced positive spell lengths and ask what the effect of having had advance notice was on these workers' durations of nonemployment.[5]

Table 5.5 presents estimates of equations in which the logarithm of the displaced workers' durations of nonemployment were specified to be functions of the same set of variables that entered into the previous section's probability of positive duration equations. The econometric method used here takes account of the fact that the *SDW* questionnaire did not permit individuals to report durations of more than 99 weeks and that some displaced workers' durations of nonemployment were truncated because they were still in progress at the survey date.[6] Although the sample is confined to those workers with positive durations, no econometric correction has been made here for any potential sample selection bias that may arise. We address this issue later in the section. The method used here assumes an exponential distribution for the error terms; we also illustrate the sensitivity of the results to this assumption below.

Numerous variables in this table are seen to be important determinants of duration of nonemployment. For example, other things equal, nonwhites (RRACE) had longer durations, while highly educated workers (REDUC) and white-collar workers (WCOL) had shorter durations in all four gender/reason-for-unemployment groups. Area labor market conditions also mattered; for three of the four groups, the higher the area unemployment rate was the longer displaced workers' durations of nonemployment were. Most important to us, however, is that, conditional on a positive duration having occurred, receipt of advance notice did *not* significantly reduce these displaced workers' durations of nonemployment.

How sensitive is this latter result to various assumptions we have made? Table 5.6 presents comparisons of how the coefficient of the advance notice variables in the log duration equations changed when a number of the assumptions were altered. First, for each of the four gender/reason-for-displacement groups, we reestimated the duration equations using alternative assumptions about the distribution of error terms. In particular, in addition to the exponential distribution, we used the gamma and lognormal distributions.[7] Second, we used the instrument for the receipt of advance notice variable obtained

Table 5.5
Logarithm of Duration of Nonemployment Equations for Displaced Workers With Positive Durations of Nonemployment: Exponential Error Distribution (standard error)

Variable/Group	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	.786 (.493)	2.511 (.725)	.111 (.663)	.327 (.969)
RMAR	−.361 (.061)	.079 (.087)	−.323 (.079)	.182 (.092)
RRACE	.682 (.098)	.856 (.133)	.445 (.120)	.739 (.155)
AGET[a]	.034 (.023)	.017 (.032)	.036 (.026)	.023 (.032)
AGET2	−.024 (.033)	−.003 (.048)	−.040 (.038)	−.012 (.047)
RVET	.026 (.067)		.255 (.079)	
REDUC	−.072 (.013)	−.063 (.021)	−.084 (.015)	−.085 (.023)
RHISP	.100 (.131)	.275 (.190)	−.136 (.147)	.317 (.210)
RHINST	.064 (.063)	−.130 (.093)	.138 (.076)	.028 (.095)
YD1	2.177 (.284)	1.399 (.449)	2.886 (.467)	3.196 (.739)
YD2	2.105 (.280)	1.008 (.443)	2.690 (.466)	2.886 (.740)
YD3	2.113 (.270)	1.233 (.426)	2.948 (.461)	3.082 (.730)
YD4	2.150 (.266)	1.009 (.418)	2.894 (.458)	2.695 (.725)
YD5	1.663 (.266)	.459 (.410)	2.362 (.458)	2.103 (.723)
TENURE	.057 (.014)	.034 (.029)	.033 (.015)	.042 (.023)
TEN2	−.002 (.001)	−.001 (.002)	−.001 (.001)	−.001 (.001)
R1	.124 (.090)	.326 (.132)	.100 (.113)	−.024 (.133)
R2	.424 (.075)	.595 (.120)	.191 (.095)	.316 (.125)
R3	.268 (.081)	.349 (.129)	−.011 (.095)	.229 (.136)
WCOL	−.255 (.068)	−.266 (.095)	−.257 (.081)	−.197 (.095)
MANUF	−.004 (.059)	−.077 (.094)	.063 (.074)	.136 (.099)
AU	.053 (.018)	.001 (.026)	.042 (.022)	.087 (.029)
AUNION	.019 (.055)	.014 (.008)	.024 (.006)	.002 (.007)
MSMSA	.074 (.060)	.203 (.092)	.167 (.073)	−.004 (.101)
RADV	.055 (.055)	.048 (.085)	.053 (.067)	−.030 (.089)
Loglikelihood	−2915.74	−1324.94	−1931.26	−1175.7
DUR < 99	1390	593	933	558
DUR ≥ 99	756	376	366	236

a. Coefficient and standard error have been multiplied by 100.

from the reduced form probit equation in chapter 4 to control for the possible endogeneity of advance notice. Third, we reestimated the log duration equations to see if the impact of having had advance notice depended on the displaced workers' previous job tenure.

Perusal of table 5.6 suggests that the following conclusions may be drawn from these extensions. First, the conclusion that, having advance notice does not affect displaced workers' durations of nonemployment, given that duration of nonemployment is positive, is robust, and was not sensitive to which of the distributions was assumed (row 1). Second, when the instrument for advance notice was used, its coefficients implied that receipt of advance notice actually *lengthened* displaced workers' durations of nonemployment for three of the four groups (row 2). However, the implied marginal effects were so large here that all one can really conclude is that the instrument is picking up something spurious.[8] Moreover, formal statistical tests described in appendix B suggest that since it is legitimate to treat advance notice as exogenous, the instrument is *not* needed. Finally, when the effect of advance notice is allowed to vary with previous job tenure (row 3), it appears that for at least one group, females displaced due to a layoff who had at least three years prior tenure, advance notice may have reduced nonemployment durations. However, this effect was statistically significant for only one error distribution assumption.

Two final extensions warrant brief mention before we conclude this section. Table 5.7 shows what the coefficients on a (1,0) received unemployment insurance (UI) variable was when it was added to the log duration of nonemployment equation for displaced workers with positive durations of nonemployment. Even when we confine the sample to those whose durations are greater than one week (to avoid a spurious correlation due to people with short durations not receiving UI because of the waiting week that exists in most states) and to those whose predisplacement job tenure was at least one year (to assure that they were all eligible for UI), we still find that displaced workers who received UI did have longer durations of nonemployment as theory predicts.

Finally, table 5.8 tests whether our failure so far to control for the fact that we have restricted our sample to those with positive durations of nonemployment has led to a sample selection bias problem. Row (1) in this table merely repeats the advance notice variable's coefficients from table 5.5. Row (2) shows that ignoring both the

Table 5.6
Comparison of Advance Notice Coefficients in Logarithm of Duration of Nonemployment Equations
Various Specifications

Group	Male Layoff			Female Layoff			Male Shutdown			Female Shutdown		
Model/Dist.	(E)	(G)	(L)	(E)	(G)	(L)	(E)	(G)	(L)	(E)	(G)	(L)
(1) RADV	.054	.028	.019	.048	.077	.079	.053	−.020	−.025	−.031	−.061	−.058
(2) $\widehat{RADV}$	1.288*	1.291*	1.340*	.902**	.824	.809	1.479*	1.126	1.078	−.411	−.188	−.190
(3) RADV	.052	.022	.016	.117	.141	.141	.041	−.046	−.050	−.072	−.096	−.095
RADV*T	.010	.017	.009	−.342*	−.286	−.274	.030	.063	.060	.140	.110	.116

where

(1) specification in table 5.5, RADV treated as exogenous,

(2) specification in table 5.5, instrument for RADV used which was obtained from the reduced form probability of advance notice equation,

(3) same as (1), but RADV is also interacted with whether the individual had at least 3 years of tenure on the job prior to displacement (T = 1, otherwise T = 0),

and the distribution of error terms is assumed to be

(E) exponentially distribution,

(G) gamma distribution,

(L) lognormal distribution.

*(**) Coefficient is statistically significantly different from zero at the .05 level; two-tail test.

Table 5.7
Effect of Receipt of Unemployment Insurance on Logarithm of Duration of Nonemployment (standard error)

Model	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
(1)	.821 (.072)	.808 (.112)	.971 (.087)	.501 (.116)
(2)	.542 (.071)	.587 (.110)	.753 (.086)	.356 (.112)
(3)	.635 (.101)	.902 (.176)	.711 (.114)	.410 (.149)

where

(1) same specification is in table 5.5, but (1,0) receive unemployment insurance when displaced variable added,

(2) same as (1), but restricted to people whose duration of nonemployment exceeded one week,

(3) same as (2), but further restricted to people whose predisplacement job tenure exceeded one year.

truncation of duration at 99 weeks and that some spells were still in progress at the survey data does not substantially alter the estimated advance notice variable's coefficient in most cases. Finally, row (3) shows that use of Heckman's (1979) two-stage procedure to control for the exclusion of displaced workers with zero weeks of

Table 5.8
Testing for Sample Selection Bias in the Log Durations of Nonemp. .yment Equations

	Coefficients of advance notice variable			
	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
(1)	.055	.048	.053	−.130
(2)	−.001	.082	.033	−.034
(3)	−.050	.130	−.013	−.067

where

(1) specification in table 5.5, exponential error distribution, takes account of truncation at 99 weeks and of spells in progress,

(2) specification in table 5.5, normal error distributions, OLS,

(3) specification in table 5.5, normal error distribution, inverse Mills ratio correction factor to control for exclusion of displaced workers with zero weeks of nonemployment.

*(**) Coefficient statistically significantly different from zero at .05 (.10) level; two-tail test.

nonemployment similarly does not substantially alter the results. Put another way, there is no evidence here that receipt of advance notice significantly reduced displaced workers' durations of nonemployment for those displaced workers with positive durations of nonemployment.

Does Advance Notice Cause a Firm's Most Productive Workers to Quit Prior to Displacement?

The January 1984 *SDW* asked displaced workers who had received advance notice if they subsequently quit their jobs prior to the planned displacement date. The bottom two rows of table 5.9 tabulate the number of people with advance notice in each of our four gender/reason-for-displacement groups who quit prior to displacement (Quit = 1) and who remained employed until the displacement date (Quit = 0). Depending upon the group, these estimates imply that between 9.3 and 16.3 percent of those people who received advance notice quit prior to their displacement.

What are the factors that influence whether a worker quits prior to displacement? Table 5.9 also presents estimates of probit probability of quit prior to displacement equations for full-time workers who received advance notice. The explanatory variables included in these equations include characteristics of the workers (age at displacement (AGET), job tenure at displacement (TENURE), education levels (REDUC), race (RRACE), Spanish ethnicity (RHIS), marital status (RMAR), weekly earnings in 1984 dollars (REARNT), coverage by employer-provided health insurance (RHINST) and whether the worker was a white-collar worker (WCOL)), year (YD) and region (R) dummy variables, and area of employment characteristics (the previous year's area unemployment rate (AUNEMP), the annual change in the area unemployment rate (AUNEMPC), recent and lagged changes in area employment and employment in the worker's major industry in the area (AEMPC1, AEMPC2, AEMPC3, AEMPC4), and whether the individual resides in an SMSA (MSMSA)).

Table 5.9
Probit Quit Prior to Displacement Equations for Full-Time Workers Who Received Advance Notice (absolute value t-statistics)

Explanatory variables	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	–2.581 (1.4)	–.504 (0.4)	–.424 (0.5)	–3.004 (0.6)
AGET	–.013 (1.9)	–.019 (1.9)	.002 (0.2)	–.015 (2.2)*
TENURE	.006 (0.4)	.006 (0.2)	–.011 (1.0)	–.027 (1.4)
REDUC	.011 (0.4)	.046 (1.1)	.053 (2.1)*	–.020 (0.5)
RRACE	–.493 (2.1)*	.167 (0.7)	–.207 (0.9)	–.212 (0.5)
RHISP	–.067 (0.2)	–.345 (0.8)	.308 (1.2)	–.237 (0.6)
RMAR	–.056 (0.4)	.016 (0.1)	–.063 (0.5)	–.195 (1.3)
RHINST	–.078 (0.6)	.298 (1.5)	–.028 (0.2)	.092 (0.6)
REARNT	.000 (0.7)	–.001 (0.9)	–.001 (1.8)	.001 (1.0)
WCOL	.331 (2.2)*	–.139 (0.7)	.423 (3.2)*	.368 (2.2)*
AUNEMPC	.087 (1.2)	–.147 (1.2)	–.021 (0.3)	.061 (0.6)
AUNEMP	–.040 (0.8)	–.001 (0.0)	.033 (0.8)	.024 (0.4)
AEMPC1	4.876 (1.0)	4.094 (0.6)	–5.133 (1.1)	5.517 (0.8)
AEMPC2	–.538 (0.1)	1.740 (0.3)	6.044 (1.8)	–1.886 (0.9)
AEMPC3	–2.012 (1.0)	–1.633 (0.3)	–1.381 (0.6)	–2.055 (0.4)
AEMPC4	.198 (0.2)	3.720 (1.6)	–2.772 (1.0)*	–.214 (0.1)
MSMSA	.019 (0.2)	–.110 (0.6)	.147 (1.0)	–.160 (1.0)
YD1	2.167 (1.1)	–.432 (0.6)	–.335 (0.5)	2.796 (0.5)
YD2	2.070 (1.1)	–.604 (0.8)	–.796 (1.1)	3.005 (0.6)
YD3	1.716 (0.9)	–.129 (0.2)	–.747 (1.0)	2.495 (0.5)
YD4	1.786 (1.0)	–.691 (1.0)	–1.126 (1.6)	2.097 (0.4)
YD5	1.555 (0.8)	–.061 (0.1)	–1.126 (1.6)	2.097 (0.4)
R1	–.311 (1.4)	.095 (0.3)	–.174 (0.9)	–.064 (0.3)
R2	–.306 (1.6)	–.019 (0.6)	–.016 (0.1)	–.424 (1.7)
R3	.040 (0.2)	–.265 (0.9)	–.199 (1.1)	–.089 (0.3)
X^2 (DOF)	49.732 (25)	27.501 (25)	65.954 (25)	31.576 (25)
Quit = 1	98	54	129	49
Quit = 0	959	405	663	453

*Coefficient statistically significantly different from zero at .05 level, two–tail test.
See table 3.1 for variable definitions.

Quite strikingly, the explanatory power of these models is quite low.[9] There is evidence for three of the four groups that, other things held constant, older workers were less likely to quit than younger workers and white-collar workers were more likely to quit than blue-collar workers. However, job tenure and earnings levels of these workers did not influence their quit probabilities and only for one group, males displaced due to plant shutdowns, was there any evidence that a worker's schooling level was positively associated with his probability of leaving prior to displacement. Area employment and unemployment conditions also bore no relationship to the probability of quitting.

The evidence presented in this section cannot be interpreted as providing strong support for the hypothesis that provision of advance notice would lead a firm's most productive workers to quit their jobs prior to displacement. Based upon the available data in the *SDW*, for the most part the quit decision was not highly correlated with observed determinants of productivity and was almost a random process. Although advance notice does permit some workers to find new employment, and thus quit prior to their displacement dates, these workers do not appear to systematically be among firms' most productive workers.[10]

Conclusions

The conclusions obtained from this chapter can be summarized briefly. Having advance notice *did* significantly increase the probability that a displaced worker in the 1984 *SDW* did not experience any weeks of nonemployment. The largest increase was for males who had been displaced due to a plant shutdown and the major beneficiaries within this group were white-collar workers. In contrast, once a displaced worker experienced any weeks of nonemployment, the presence of advance notice had no effect on his or her ultimate duration of nonemployment. Advance notice thus seemed to help displaced workers in the sample *only* if they found new employment prior to displacement. Analyses of which displaced workers who received advance notice actually quit their jobs prior to the displacement date do not provide strong support for the hypothesis that advance notice leads a firm's most productive workers to quit prior to displacement.

NOTES

1. Technical details of the estimation of this model appear in appendix B.

2. See appendix B for details.

3. The proportions with zero spell length in table 5.4 differ from those found in table 5.1 because the former refer only to individuals whose data were used in the analyses in table 5.2. Due to missing data on some explanatory variables, the sample size used in table 5.2 is smaller than that used in table 5.1.

4. These percentages are calculated as follows:
male layoff (–.022)(.6)/.087 = –.152
female layoff (–.035)(.6)/.088 = –.239
male shutdown (–.084)(.6)/.161 = –.313
female shutdown (–.043)(.6)/.145 = –.178

5. The analyses in this section are restricted to workers aged 55 and under as of the displacement date, to avoid complications due to the retirement behavior of older workers. Preliminary tests suggested, however, that inclusion of older workers in the analyses did not alter significantly any of the results that follow.

6. See appendix B for details.

7. See appendix B for details.

8. See appendix B for details.

9. Experimentation with the inclusion of quadratic terms in AGET and TENURE indicated that neither improved the model's explanatory power.

10. Put another way, productivity on the job and success in job search do not appear to be highly correlated.

6

Advance Notice and Survey Date Employment and Earnings

Background data on the January 1984 survey date employment status of those workers in the *SDW* for whom we had weekly earnings information as of their displacement dates are found in table 6.1. The bottom row of the table indicates that in each of the four gender/reason-for-displacement groups, from 51 to 62 percent of these workers who had been displaced during the 1979-83 period were employed at the January 1984 survey date.[1]

What about the earnings levels of the reemployed workers vis-a-vis their predisplacement earnings? All predisplacement earnings figures have been converted by us to January 1984 dollars by multiplying them by the ratio of the January 1984 Consumer Price Index (CPI) to the CPI in the year of displacement. The remainder of table 6.1 presents summary statistics on the ratio of survey date to predisplacement real weekly earnings for those displaced workers who were reemployed at the survey date.[2]

The top row suggests that, on average, those displaced workers who found reemployment experienced only small real earnings losses, as compared to their own predisplacement earnings. The mean ratio of survey date to predisplacement real earnings varied across the four gender/reason-for-displacement groups from .95 to .97. Put another way, on average these reemployed workers' real earnings fell by only 3 to 5 percent.[3] Averages, however, mask considerable variation in individual experiences. The data reported in the table on the ratio of survey date to displacement date real earnings indicate that 25 percent of each group's reemployed workers suffered real earnings losses of at least 8 to 10 percent, while another 25 percent experienced at least moderate real earnings increases of 1 to 2 percent.

Table 6.1
Ratio of Survey Date to Predisplacement Real Weekly Earnings for Workers Reemployed as of the Survey Week in the January 1984 *CPS* Displaced Worker Sample

	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
Mean (std. dev.)	0.96 (.09)	0.95 (.11)	0.97 (.09)	0.96 (.11)
Distribution				
Minimum	0.29	0.49	0.39	0.48
25 percentile	0.92	0.90	0.92	0.92
Median	0.98	0.97	0.98	0.98
75 percentile	1.02	1.01	1.02	1.01
Maximum	1.66	1.63	1.28	1.33
Fraction of sample employed	.57	.51	.62	.55

SOURCE: Authors' computations from the January 1984 *CPS Survey of Displaced Workers* sample used in the analyses. Distributions for workers with and without advance notice are very similar.

Did the receipt of advance notice of impending displacement influence displaced workers' probabilities of being reemployed at the survey date? For those displaced workers who are reemployed at the survey date, did advance notice influence the ratio of their survey date to predisplacement real weekly earnings? This chapter provides answers to these questions.

Probability of Employment at the Survey Date

Table 6.2 presents the results of our estimating probit probability of being employed as of the survey date equations for the displaced workers in our sample.[4] The explanatory variables used are those found in the duration of nonemployment equations presented in the last chapter, with the addition of the unemployment rate in 1984 in the area in which the individual resided (AU84). The latter is entered to control for economic conditions in the area.

Table 6.2
Probit Probability of Employment at Survey Date Equations
(absolute value *t*-statistics)

Explanatory variables	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	−1.166 (1.5)	−1.296 (1.1)	1.387 (1.5)	−3.676 (2.3)*
AGET	.008 (0.3)	.066 (2.1)*	−.001 (0.1)	.119 (3.9)*
AGET2	−.000 (0.7)	−.001 (2.6)*	−.001 (2.4)*	−.001 (5.0)*
REDUC	.192 (2.2)*	.085 (0.7)	−.063 (0.7)	.298 (1.9)
REDUC2	−.003 (1.2)	.000 (0.1)	.000 (0.1)	−.008 (1.6)
AGEED	−.002 (1.5)	−.002 (1.3)	.003 (2.3)*	−.002 (1.4)
TENURE	−.007 (0.5)	.020 (0.9)	.020 (1.5)	.010 (0.5)
TEN2	−.000 (0.2)	−.001 (1.3)	−.001 (2.2)*	−.001 (1.3)
RRACE	−.475 (5.7)*	−.721 (6.3)*	−.341 (3.1)*	−.498 (3.8)*
RVET	.080 (1.2)		.012 (0.1)	
RHISP	−.086 (0.7)	−.235 (1.3)	.145 (1.0)	−.143 (0.7)
RMAR	.302 (5.1)*	−.320 (3.9)*	.193 (2.5)*	−.390 (4.5)*
RHINST	.126 (2.0)*	.317 (3.6)*	.135 (1.8)	.142 (1.6)
YD1	.970 (4.3)*	.579 (1.7)	−.398 (0.9)	.898 (1.1)
YD2	.807 (3.8)*	.523 (1.6)	−.205 (0.4)	1.126 (1.4)
YD3	.716 (3.6)*	.302 (0.9)	−.301 (0.7)	.993 (1.3)
YD4	.602 (3.1)*	.138 (0.4)	−.512 (1.2)	.963 (1.2)
YD5	.006 (0.0)	−.358 (1.1)	−.774 (1.9)	.852 (1.1)
R1	−.089 (1.0)	−.344 (2.7)*	−.181 (1.8)	.307 (2.2)*
R2	−.121 (1.6)	−.226 (2.0)*	−.112 (1.2)	.060 (0.5)
R3	.114 (1.5)	−.008 (0.7)	.221 (2.5)*	.184 (1.7)
WCOL	.057 (0.8)	.347 (3.9)*	.007 (0.1)	.258 (2.8)*
MANUF	.121 (2.0)*	−.010 (0.1)	.051 (0.2)	−.131 (1.4)
AU	.028 (1.5)	.075 (2.5)*	.002 (0.9)	−.101 (3.1)*
AU 84	−.098 (6.3)*	−.103 (4.1)*	−.051 (2.6)*	−.019 (0.7)
RADV	−.013 (0.2)	.078 (1.0)	.127 (2.0)*	.006 (0.1)
X^2 (DOF)	411.622 (25)	187.311 (24)	189.555 (25)	168.496 (24)
Emp = 1	1408	582	1063	573
Emp = 0	1084	560	654	475

*Coefficient statistically significantly different from zero at .05 level; two-tail test.
AU84—area unemployment rate in 1984. See table 3.1 for all other variable definitions.

The estimates in this table suggest that many of the variables other than receipt of advance notice have important explanatory power. For example, reemployment probabilities appear to have declined monotonically with age for males displaced due to plant shutdowns. For both female groups, the probabilities first increased and then decreased with age, with the decline starting at about age 33 for those who had been laid off and at about age 60 for those displaced due to a shutdown. Nonwhites in all four groups had lower survey date reemployment probabilities than otherwise comparable whites. Married men were more likely and married females less likely, other things equal, to be reemployed at the survey date. The estimates also suggest that respondents covered by a health insurance policy at their predisplacement jobs and, for females, white-collar workers, were more likely to be reemployed.

For workers displaced due to a layoff, other things held constant, the further the layoff date was in the past, the more likely the worker was to be reemployed as of the survey date. Somewhat surprisingly, however, this result did not hold for males displaced due to a plant shutdown. Finally, as expected, the higher the unemployment rate in the local area as of the survey date, the lower the probability that the displaced workers were reemployed.

Most important to us, only for males displaced due to a plant shutdown did advance notice appear to significantly increase the probability of being employed at the survey date. Since this is the group (see chapter 5) for whom advance notice led to the greatest reduction in the probability that a displaced worker experienced positive weeks of nonemployment, this result is not surprising.

Some idea of the magnitude of the effect of advance notice is found in table 6.3. Here we have used the coefficient estimates from table 6.2 and the value of all the other explanatory variables for each individual to compute each individual's marginal increase in the probability of being employed at the survey date if he or she received advance notice of displacement.[5] For males displaced due to plant shutdowns, the increase ranged from .004 to .051 with an average increase of .044. Since approximately 60 percent of this group received advance notice, we can infer that advance notice increased the proportion of the group that was employed by about .026; this should be contrasted with the actual reemployment rate of the group of .62. At best then, advance notice had only a marginal effect on this group's survey date reemployment rate.

Table 6.3
Marginal Effects of Having Received Advance Notice on the Probability of Being Employed at the Survey Date

		Computed effects by individual			
		Mean	Std. dev.	Minimum	Maximum
Male Layoff	[.57]	−.0045	.0008	−.0052	−.0008
Female Layoff	[.51]	.0271	.0047	.0010	.0311
Male Shutdown	[.62]	.0442*	.0063*	.0041*	.0506*
Female Shutdown	[.55]	.0022	.0004	.0000	.0025

[ ] fraction in the group who were employed at the survey date.
*Based on RADV coefficient that was statistically significantly different from zero at the .05 level; two-tail test.

Survey Date Earnings for Those Reemployed

Table 6.4 presents estimates of the logarithm of survey date weekly earnings equations that used the subsample of displaced workers who were reemployed at the survey date. The variables included in these equations are the same as those included in the probability of employment equations (table 6.2), as well as the logarithm of the individual's weekly earnings (converted to 1984 dollars) as of his or her date of displacement. As the table indicates, predisplacement earnings were obviously an important determinant of postdisplacement earnings.

Turning to other results, the coefficients of the age variables suggest that earnings *losses* increased with age, although job tenure *per se* did not seem to have any independent effect. Other things held constant, married males' earnings were higher than nonmarried males' and workers covered by an employer-provided health insurance policy prior to displacement did better than workers who did not have such policies. As expected, higher local area unemployment rates were associated with lower survey date earnings; on average, a 1 percentage point increase in the area unemployment rate was associated with 2 to 3½ percent lower survey date earnings.

The results in this table suggest, however, that advance notice was *not* associated with higher survey date earnings. As this is an important negative finding, we performed a number of extensions to

Table 6.4
Logarithm of Survey Date Earnings Equations for Individuals Employed at the Survey Date (absolute value *t*-statistics)

Explanatory variables	Male Layoff	Female Layoff	Male Shutdown	Female Shutdown
INTERCEPT	1.198 (2.4)	1.567 (2.1)	2.032 (4.3)	1.438 (1.3)
AGET	.045 (3.7)	.036 (1.8)	.025 (2.1)	.043 (2.1)
AGET2[a]	–.049 (4.0)	–.031 (1.5)	–.034 (2.4)	–.056 (3.1)
REDUC	.162 (3.2)	.127 (1.7)	.020 (0.4)	–.019 (0.2)
REDUC2	–.004 (2.6)	–.000 (0.4)	.001 (0.5)	.002 (0.7)
AGEED	.001 (0.8)	–.002 (1.8)	–.000 (0.0)	–.000 (0.2)
TENURE	–.012 (1.6)	.018 (1.1)	.002 (0.3)	–.015 (1.1)
TEN2[a]	.034 (1.1)	–.093 (1.1)	–.030 (1.0)	.018 (0.2)
RRACE	–.097 (1.8)	–.057 (0.6)	–.077 (1.3)	.043 (0.5)
RHISP	.096 (1.4)	.017 (0.1)	–.062 (0.8)	–.054 (0.7)
RMAR	.157 (4.8)	.062 (1.2)	.157 (4.0)	.010 (0.2)
RVET	–.005 (0.1)		–.029 (0.8)	
RHINST	.104 (2.9)	.083 (1.4)	.051 (1.3)	.158 (2.8)
YD1	–.170 (1.2)	–.409 (1.8)	–.051 (0.3)	.030 (0.1)
YD2	–.116 (0.8)	–.229 (1.0)	–.155 (0.9)	.003 (0.0)
YD3	–.137 (1.0)	– 298 (1.4)	–.161 (0.9)	–.078 (0.1)
YD4	–.177 (1.3)	–.255 (1.2)	–.161 (0.9)	–.070 (0.1)
YD5	–.239 (1.7)	–.256 (1.2)	–.143 (0.8)	–.230 (0.4)
R1	–.054 (1.1)	–.191 (2.4)	.001 (0.1)	–.014 (0.2)
R2	–.115 (2.8)	–.297 (4.3)	–.057 (1.2)	–.043 (2.0)
R3	–.097 (2.5)	–.191 (2.9)	–.007 (0.2)	–.111 (1.7)
WCOL	.031 (0.8)	.022 (0.4)	.151 (3.7)	.069 (1.3)
MANUF	–.100 (3.3)	–.003 (0.1)	–.041 (1.2)	.050 (0.9)
AU	.016 (1.7)	–.013 (0.7)	.009 (0.8)	.038 (1.7)
AU84	–.035 (4.2)	–.025 (1.6)	–.019 (2.1)	–.034 (2.1)
LREARNT	.479 (13.8)	.485 (7.3)	.590 (13.1)	.562 (9.2)
RADV	–.031 (1.1)	.045 (0.9)	–.033 (1.0)	.029 (0.6)
R^2	.307	.296	.361	.341
DOF	1257	502	908	498

[a]Coefficient has been multiplied by 100.

LREARNT of weekly earnings (expressed in 1984 dollars) at the displacement date. See tables 5.1 and 6.2 for other variable definitions.

see how sensitive the result was to different econometric specifications and the results of these efforts are summarized in table 6.5.

Table 6.5
Coefficients of RADV in Survey Date Logarithm of Earnings Equations Various Specifications

	Male Layoff		Female Layoff		Male Shutdown		Female Shutdown	
	(O)	(S)	(O)	(S)	(O)	(S)	(O)	(S)
LREARNT–EXOG								
(1) RADV	−.031	−.030	.045	.042	−.033	−.080	.029	.031
(2) RADV	−.039	−.037	−.094	−.106	−.039	−.088	.029	.030
RADV*WC	.027	.027	.249*	.253*	.018	.022	.001	.003
(3) RADV	−.027	−.025	−.007	−.019	−.023	−.071	.013	.010
RADV*WC	.024	.024	.193**	.197**	.012	.017	.009	.015
RADV*MAN	−.030	−.030	−.149	−.148	−.041	−.038	.030	.037
LREARNT–END								
(4) RADV	−.031	−.028	.022	.010	−.053	−.118**	.029	.028
(5) RADV	−.039	−.036	−.117	−.139	−.065	−.134*	.029	.029
RADV*WC	.032	.032	.259*	.265*	.038	.045	−.001	−.001
(6) RADV	−.026	−.025	−.040	−.062	−.053	−.122**	.021	.018
RADV*WC	.029	.029	.213*	.219*	.034	.041	.004	.004
RADV*MAN	−.031	−.031	−.129	−.127	−.030	−.027	.015	.020

(1) same specification as table 6.4,
(2) same as (1), but interaction of RADV and WC added,
(3) same as (2), but interaction of RADV and MAN added,
(4) same as (1), but LREARNT treated as endogenous,
(5) same as (2), but LREARNT treated as endogenous,
(6) same as (3), but LREARNT treated as endogenous,
and
(O) no correction for being employed at survey date.
(S) correction for being employed at survey date using inverse Mills ratios computed from equations in table 6.2.

First, since the equations in table 6.4 were estimated using samples of displaced workers who were reemployed at the survey date, they are subject to possible sample selection bias. As such, we reestimated the model using the two-stage method first suggested by Heckman (1979) to control for the probability a worker was employed at the survey date.[6] A comparison of the coefficients of the advance notice variables from the equations that used OLS (column O) and those that used this two-stage method (column S) indicates that using the latter method provides no evidence that having advance notice increased survey date earnings (row 1).

Second, we reestimated the earnings equations, interacting the advance notice variable with whether the worker was a white-collar worker and with whether he or she worked in manufacturing prior to displacement. Our goal here was to see if there was any differential effect of advance notice on earnings across occupations and industries. When this was done (rows 2 and 3), the results suggested that having advance notice was associated with an increase in survey date weekly earnings of roughly 15 to 20 percent for white-collar females who had been displaced due to a layoff. No such effects were found for other gender/occupational groups, however, and the effects of advance notice did not appear to depend upon whether the worker was displaced from a manufacturing job.

Third, all of the previous results treat predisplacement earnings as exogenous. In reality these earnings were probably determined by many of the other variables that appear in table 6.3, as well as other variables. The last three rows of table 6.5 show what happens to the advance notice variables' coefficients when we treat the logarithm of predisplacement earnings as endogenous and use an instrument for it in the analysis.[7] Doing so alters none of the previous results, save that advance notice now appears to have been associated with lower survey date earnings for males displaced due to a shutdown. Given the perverse nature of this result, we place little significance in it.[8]

One final extension we conducted was to include a variable indicating whether the displaced worker had received unemployment insurance (UI) in the estimated survey date earnings equation. The sample was restricted here to individuals who had postdisplacement nonemployment durations of at least one week and predisplacement job tenures of at least one year, to assure us that all individuals in the sample should have been eligible to receive UI payments. As table 6.6 indicates, those people who actually had received UI did not wind up in better paying jobs, even though evidence presented in table 5.7 showed that they had longer durations of nonemployment.

Table 6.6
Marginal Effect of Having Receive[illegible] [illegible]ment Insurance on Survey Date [illegible]

Group	Ma[illegible] effect
Male Layoff	−.036 (1.0)
Female Layoff	−.096 (1.1)
Male Shutdown	−.007 (0.1)
Female Shutdown	.101 (1.4)

[a] Sample confined to individuals with predisplacement tenure of at least one year and post-displacement nonemployment duration of greater than one week.

Concluding Remarks

The results reported in this chapter suggest that having received advance notice had very little impact on the January 1984 survey date employment status and earnings of workers displaced during the 1979-83 period. Only for males who had been displaced due to a plant shutdown was there any evidence that advance notice led to an increased probability of being employed at the survey date. While a substantial fraction of displaced workers come from this group, even here the marginal effect was small. Only for white-collar females who had been displaced due to layoff was there any evidence that advance notice led to higher survey date weekly earnings. While the implied effects of advance notice on earnings of members of this group were in the order of 15 percent, females who were laid off made up only 16 percent of all the displaced workers for whom we had earnings data (see table 6.4) and not all of them were white-collar workers. Hence, the overall effect of advance notice on survey date earnings of displaced workers in the *SDW* was quite small.

NOTES

1. The Bureau of Labor Statistics (1985) reports that about 60 percent of workers in the January 1984 *SDW* were reemployed as of the survey date. Our sample, which yields a somewhat lower overall percentage, is confined to those displaced workers who were working full-time at

the displacement date and for whom data on predisplacement earnings were reported. In addition, the BLS restricted their attention to workers who had at least three years of tenure with their employers prior to displacement.

2. No obvious differences occurred in these summary statistics between people with and without advance notice, so we report only the overall group statistics here.

3. We should caution that this comparison ignores nonwage benefits. It also is strictly a predisplacement/postdisplacement comparison and does *not* take account of any real earnings increases the individual may have received if he or she had remained employed, due to either increases in seniority, or to economy-wide or firm-specific productivity increases.

4. See appendix B for details.

5. See appendix B for details.

6. See appendix B for details.

7. See appendix B for details.

8. A possible "explanation" for the result is as follows: Suppose these males displaced due to shutdowns came primarily from unionized employment and were reemployed after displacement in nonunion jobs. The estimated wage loss associated with advance notice of around 11 to 13 percent could be interpreted simply as a union/nonunion wage differential - mate; panel data estimates of union wage differentials often lie in this range. Since we ..ave no data, however, on these displaced workers' predisplacement union status, this explanation is purely speculative.

7

Implications of Findings

Summary of Findings

Empirical analyses of the January 1984 *SDW,* which are reported in chapters 4 through 6, indicate that there is very little support for the proposition that workers who received advance notice paid for it in the form of lower predisplacement wages. They also suggest that it is difficult to explain which displaced workers received advance notice prior to displacement using data on characteristics of the individuals, data on characteristics of the workforce in the industries in which they were employed, and data on employment growth and unemployment in the areas and in the industries in the areas in which they were employed. While some variables proved statistically significant in formal probit models of the probability of receiving advance notice (and the equations do have some predictive power), in the main, few consistent patterns were observed across the four groups. Thus, analyses of the effects of advance notice provisions that use the 1984 *SDW can* legitimately treat the existence of advance notice as exogenous.

Our analyses do suggest that having advance notice did significantly increase the probability that a displaced worker would *not* experience any unemployment. The largest increase was for males who were displaced due to a plant shutdown and the major beneficiaries within this group were white-collar workers. In contrast, once an individual experienced any unemployment, the presence of advance notice had no effect on his or her ultimate duration of nonemployment. Advance notice seemed to help, then, only if individuals could find employment prior to displacement.

Analyses of the effects of advance notice on survey date earnings echoed the findings of Podgursky and Swaim (1987b) that, on average, receipt of advance notice had no affect on subsequent earnings once a displaced worker was reemployed. Only for white-collar females who had been displaced due to a layoff was there any evidence that advance notice led to higher survey date weekly earnings, and this group made up less than 15 percent of the *SDW* sample. The major effect of advance notice on workers in the *SDW* sample, then, was through decreasing the probability of observing positive nonemployment spell lengths.

Finally, for the people in the *SDW* sample who received advance notice, we found no systematic evidence that variables that might be proxies for productivity (job tenure, education, previous earnings) systematically were associated with the probability that a worker would quit prior to the date of his or her displacement. Thus, we found no evidence that advance notice would lead a firm's most productive workers to quit prior to the displacement date, thereby disrupting a firm's operations in its final weeks.

Implications for Public Policy and for Future Research

A number of studies suggest that there are large private costs of displacement to displaced workers.[1] While some fraction of the costs may represent the dissipation of rents, a substantial portion represent true social costs. Often these costs are transitory in nature; witness the fact that real reemployment earnings of displaced workers in the *SDW* who *were* reemployed at the survey date, on average, were within 5 percent of their predisplacement earnings levels (table 6.1).[2] However, some workers suffer large losses which last for an extended period of time; witness the large fraction of workers in the *SDW* displaced during the 1979-83 period who were *not* reemployed at the January 1984 survey date.

The literature surveyed in chapter 2 and our own empirical results suggest that advance notice may well facilitate labor market adjustments by allowing displaced workers to find employment prior to their date of displacement. Advance notice appears to reduce the probability that displaced workers suffer any spell of unemployment and thus may well moderate temporary increases in area unemployment rates. In both a number of the surveyed studies and our own, "advance notice" included notice of very short duration and results in these studies likely understate the effects of mandated notice of longer duration. The individual worker-based data used in most of the studies we discussed also did not permit analyses of whether advance notice of pending displacements can lead to actions (e.g., reorganization, wage concessions, employee ownership) that help avert displacements.

Although opponents of advance notice cite potential costs of such policies, empirical studies have found no evidence that advance notice causes firms' most productive workers to leave and that the productivity of the remaining workers suffers. Moreover, save for Lazear (1987), who found no statistically significant relationships, no systematic empirical evidence has been provided on the other potential adverse effects of advance notice that opponents have enumerated.

While at first glance this discussion suggests support for the new federally mandated advance notice for displaced workers, several cautions are in order. First, the effects of voluntary provision of advance notice in situations where workers expect impending displacement anyway may be very different from the effects of mandated advance notice in situations where the impending displacement is completely unexpected by workers.[3] Indeed, one should recall that the *SDW*, which our research and most of the research we surveyed was based upon, asked only if workers received advance notice *or* expected their displacement.

Future researchers will have access to the January 1988 *Survey of Displaced Workers* which specifically asks displaced workers if they received formal advance notice and, if so, how long the notice was. However, even with these data, to adequately estimate the effects of advance notice *per se* will require researchers to try to model what displaced workers' expectations of displacement would have been in the absence of advance notice. Put another way, researchers need to estimate if formal advance notice actually communicates new information to workers.

Second, the observation that the voluntary provision of advance notice appears to reduce the probability a displaced worker will suffer any spell of unemployment does not necessarily imply that mandated advance notice will increase employment and decrease unemployment rates. Indeed, one can conceive of situations in which displaced workers compete for a fixed number of vacant positions that only a fraction of them can obtain. Advance notice gives those workers who receive notice an advantage; it increases their probability of finding one of these jobs. However, if the number of vacant positions is truly fixed, by necessity the probability that workers who failed to receive notice find jobs would have to go down. In this case, the gains to those workers who received notice would come solely at the expense of those workers who failed to receive notice. There would be no social gains from advance notice in the sense that, on average, it would not influence aggregate employment levels and/or unemployment rates.

Studies that use individual-based data sets, such as our own and the others that used the *SDW*, cannot test for the possibility of such displacement effects. The only study of U.S. data that addressed this issue, Folbre, Leighton and Roderick (1984) did find evidence that voluntary provision of advance notice led to smaller temporary increases in area unemployment rates. However, Lazear's (1987) cross-country study found no significant effects of mandated advance notice on national employment levels and unemployment rates. Clearly more studies that focus on the effects of advance notice on area economic outcomes are needed.

Suppose for a moment, however, that all voluntarily provided advance notice actually does is "reshuffle" jobs among displaced workers from those people who fail to receive notice to those people who do receive it. In fact, evidence of this might *strengthen* the case for federally mandated advance notice if the people who receive notice voluntarily are the ones least in need of such assistance. For example, *if* high-wage, unionized workers were more likely to receive notice than comparably-skilled, lower-wage nonunion workers (table 1.6), implementation of federal legislation would allow the latter a "better shot" at competing with the former for the available jobs when they are displaced. One thus might be in favor of advance notice legislation because of its potential redistributive effects, even if one believes it will have no net effect on employment or unemployment.

Ultimately, given all the evidence presented and cited above, the position one takes towards advance notice legislation will depend heavily on one's preconceptions as to how labor markets function. If one believes labor markets in the main are competitive and operate primarily in an efficient manner, one might argue that the onus is on those who propose government intervention to document empirically what the benefits of the proposed legislation are and to document that its adverse side effects will be small. Given such a view, one might argue that the evidence presented here does not support government intervention; there are too many results whose implications are ambiguous and too many yet unanswered questions.

If, on the other hand, one believes that labor markets in the main are not competitive and/or that important externalities exist when workers are displaced, one will find the results presented here very supportive of some form of intervention, perhaps in the form of advance notice legislation.[4] Such individuals may claim that we have documented at least some private benefits that advance notice seems to produce, without uncovering any evidence of its costs.

It is important when designing an intervention, however, to be clear about the source of public concern. If the major concern is the externality imposed on a local community due to a plant closing or large scale layoff, then public policy should specifically address this concern. Such a concern may argue for advance notice legislation. However, in this case, exemptions should not be based on absolute size, as is the case in the recently enacted *Worker Adjustment and Retraining Notification Act*, but rather on the basis of size relative to the local labor market. In contrast, if the source of concern is the private costs workers suffer from displacement, then severance pay provisions may be a viable alternative and/or addition to advance notice legislation.

Our own position is that given the social costs associated with worker displacement, a strong case appears to exist for a federal policy relating to advance notice. One possibility is for the federal government to encourage advance notice by providing inducements for employers to voluntarily do so.[5] For example, the federal government could reduce the costs to firms of providing such notice by funding a share of the unemployment benefits received by notified workers and/or by reducing the firms' corporate profit tax rates.

Alternatively, federal legislation mandating advance notice of plant closings or permanent layoffs (as has recently been enacted) could be undertaken. Well-designed research is needed, however, to more adequately address issues relating to the macro labor market effects of the legislation, including whether advance notice of impending displacement can serve to help prevent displacement from occurring, as proponents of the legislation often assert. Moreover, since so much of prior research has focused on the potential benefits of advance notice legislation, subsequent studies might also focus on research issues that opponents have been concerned about, namely, those relating to the costs of the legislation.

NOTES

1. See Daniel Hamermesh (1987) for a review of the literature in this area.

2. As was done in chapter 6, we stress that such comparisons ignore any real wage increases the displaced workers would have received if they had not been displaced due to increases in their job tenure and general firm-specific real wage increases.

3. We are grateful to Sherwin Rosen for stressing this point to us.

4. See chapter 1 for a discussion of worker displacement and externalities.

5. We are grateful to Louis Jacobson for suggesting this alternative to us.

Appendix A

The Matching of CPS 3-Digit Industries to 52 Broader Industry Groups Used in Table 3.1

Agriculture, Forestry, Fisheries (010 to 031)
Mining (040 to 050)
Construction (060)
Food & Kindred Products (100 to 122)
Textile Mill Products (132 to 150)
Apparel & Other Finished Textiles (151 to 152)
Paper & Allied Products (160 to 162)
Printing, Publishing & Allied Products (171 to 172)
Chemicals & Allied (180 to 192)
Rubber & Miscellaneous Plastic (210 to 212)
Other Nondurable Manufacturing (130, 200, 201, 220, 221, 222)
Lumber & Wood, Except Furniture (230 to 241)
Furniture & Fixtures (242)
Stone, Clay, Glass & Concrete (250 to 262)
Primary Iron & Steel (270 to 271)
Primary Nonferrous (272 to 280)
Fabricated Metal (281 to 301)
Machinery, Except Electric (310 to 332)
Electric Machinery (340 to 350)
Motor Vehicles (351)
Aircraft & Space Vehicles (352)
Other Transport Equipment (360 to 370)
Other Durable Goods (371 to 392)
Railroads (400)
Trucking Services (410)
Other Transportation (401, 402, 411 to 432)
Communication (440 to 442)
Utilities & Sanitary Services (460 to 472)
Wholesale Trade, Total (500 to 549, 552 to 571)
Groceries & Farm Products (550 to 551)
General Merchandise Stores (591 to 600)
Food Bakery & Dairy Stores (601 to 611)
Automobile Dealers & Gas Stations (612 to 622)
Apparel & Accessory Stores (630 to 631)
Eating & Drinking Places (641)
Other Retail Trade (580 to 590, 632 to 640, 642 to 691)
Banking & Credit Agencies (700 to 702)
Insurance (711)
Other Finance & Real Estate (710, 712)
Business Services (721 to 742)
Automotive Repair & Services (750 to 751)
Other Repair Services (752 to 760)
Hotels & Lodging (762 to 770)
Other Personal Services (761, 771 to 791)
Entertainment & Recreation Services (800, 802)
Hospitals (831)
Health Services, Except Hospitals (812 to 830, 832 to 840)
Educational Services, Government (842, 850, 852)
Educational Services, Private (851, 860)
Social Services (861 to 871)
Other Professional Services (841, 872 to 892)
Public Administration (900 to 932)

Appendix B

Technical Appendix to Chapters 4, 5 and 6

Chapter 4

Consider the following model:

(A4.1a) $y_1^* = x'\beta_1 + u_1$

(A4.1b) $y_2^* = x'\beta_2 + u_2$

Asterisks will denote variables that are only partially observed or never observed throughout this appendix. Let y_1^* and y_2^* be the logarithms of the predisplacement wage rate for a worker with and without advance notice, respectively. The vector x contains exogenous characteristics of the worker, the firm in which he is employed and the area in which he works, that are postulated to influence wages. For notational simplicity we adopt the convention that if a variable does not appear in an equation, then its coefficient is zero. The disturbance term $u = (u_1, u_2)'$ has mean 0 and is assumed to be independent of x. We will denote the (i,j) element of its covariance matrix, Σ, by σ_{ij}.

The wage differential a worker pays for advance notice is $(y_1^* - y_2^*)$. Suppose the worker's desire for advance notice is a function of the wage difference, his characteristics, characteristics of his employer and area, and unobservable factors that we treat as a random disturbance term (e_3).

(A4.2) $y_3^* = \alpha(y_1^* - y_2^*) + xb_3 + e_3 = x\beta_3 + u_3$,

where $u_3 = e_3 + \alpha(u_1 - u_2)$ and $\beta_3 = b_3 + \alpha(\beta_1 - \beta_2)$.

Suppose also for now that if a worker wants notice, then he gets it. We write this as,

(A4.3) $y_3 = 1$ if $y_3^* > 0$ and 0 otherwise,

where $y_3 = 1$ denotes the presence of advance notice.[1] Provided that x contains an intercept, the choice of 0 as the threshold in equation (A4.3) is inconsequential.

Assume that $u=(u_1, u_2, u_3)'$ is multivariate normal with mean 0 and variance Σ and is independent of x. Then the probability that a worker is observed having advance notice is given by

$$P(y_3=1|x)=P(y_3^*>0|x)=P(u_3> -x\beta_3).$$

If we standardize the expression above by dividing both sides by the standard deviation of u_3, σ_3, we obtain

$$\text{(A4.4)}\quad P(y_3=1|x)=P[u_3/\sigma_3> -x\beta_3/\sigma_3]=F[x\beta_3/\sigma_3],$$

where F( ) is the standard normal distribution function.

The model in (A4.4) is a *probit* model and estimation is easily carried out using maximum likelihood methods. Note that we can only identify $\gamma_3=\beta_3/\sigma_3$, that is, we can identify the slope coefficients β_3 only up to a scaling factor.

Now consider the wage equations (A1.1). Let y_1 and y_2 denote the observed values of y_1^* and y_2^*, respectively. Clearly, we observe y_1 only when $y_3=1$ and observe y_2 only when $y_3=0$. Consider the y_1 equation

$$E(y_1|x)=E(y_1^*|x, y_3=1)=E(y_1^*|x, y_3^*>0)=x\beta_1+E(u_1|u_3> -x\beta_3).$$

The problem is the last term. If u_1 does not have a zero mean in the subsample with advance notice, then least squares estimates of the wage equation for this subsample will suffer from selectivity bias. The "correct" wage equation to estimate is

$$\text{(A4.5)}\quad E(y_1|x)=x\beta_1+\sigma_3\ \varrho_{13}m_1\ (x\beta_3/\sigma_3),$$

where $\varrho_{13}=\sigma_{13}/\sigma_{33}$, $m_1(z)=f(z)/F(z)$, f( ) is the standard normal density function, and, as defined above, σ_{ij} is the (i,j) element of the covariance matrix of u. The last term in equation (A4.5) is the source of the bias, for if it is nonzero, then conventional OLS procedures, which ignore it, omit an explanatory variable that is correlated with the other explanatory variables, leading to bias in the estimates of β_1. The bias disappears only when u_1 and u_3 are independent, so that ϱ_{13} is 0. Since $u_3=e_3+\alpha(u_1-u_2)$, it is unlikely that u_1 and u_3 are independent.

A similar argument applies to estimating the wage equation for the subsample of workers who do not have advance notice. The analogous equation for wages without notice is

$$(A4.6)\ E(y_2|x, y_3=0)=x_2\beta_2+\sigma_3\ \varrho_{23}\ m_2\ (x\beta_3/\sigma_3),$$

where $m_2(z)=-f(z)/(1-F(z))$ and $\varrho_{23}=\sigma_{23}/\sigma_{33}$. Again the source of the bias (the presence of the last term) should be obvious.

Estimation of the wage equations and probability of advance notice can be done using the method of maximum likelihood. We employ a less computationally intensive multistep procedure, initially developed by Heckman (1979) that proceeds as follows:

Step 1. Apply probit analysis to equation (A4.4). This yields estimates $\hat{\gamma}_3$ of $\gamma_3=\beta_3/\sigma_3$. These estimates are found in table 4.4 in the text.

Step 2. Use $\hat{\gamma}_3$ to construct estimates of $m_1(x\beta_3/\sigma_3)$ and $m_2(x\beta_3/\sigma_3)$. Denote these estimates as $\hat{m}_1$ and $\hat{m}_2$, respectively.

Step 3. Estimate the wage equations separately on the appropriate subsamples, adding $\hat{m}_1$ ($\hat{m}_2$) to the regressor variables in the y_1 (y_2) equation. Denote the coefficient of m_i as δ_i. If we fail to reject the null hypothesis that $\delta_i=0$ then there is no bias. If we reject the null, then the coefficients $\hat{\beta}_i$ are consistent estimates of β_i but the estimated standard errors are incorrect.

Step 4. For each individual use the $\hat{\beta}_1$ and $\hat{\beta}_2$ from step 3 to construct $\hat{y}_1=x'\hat{\beta}_1$ and $\hat{y}_2=x'\hat{\beta}_2$. These are consistent estimates of the predicted values of y_1* and y_2* from the corrected wage equations and thus we can obtain a consistent estimate of the wage differential.

Now consider a more general model that corresponds to equations (4.7) to (4.9) in the text. Firms have a propensity to offer advance notice which depend on observed characteristics x. Write this as

$$(A4.7)\ y_4^*=x\beta_4+u_4$$

and add u_4 to the vector of disturbances. Firms will be willing to offer workers notice only if y_4* is large enough, so

$$(A4.8)\ y_4=1 \text{ if } y_4^*>0 \text{ and 0 otherwise,}$$

where $y_4=1$ denotes that a firm is willing to offer notice and $y_4=0$ if it is not willing.

We observe advance notice only when both workers want it *and* firms are willing to offer it. Let $y_5=1$ denote the presence of advance notice. Then

(A4.9) $y_5=y_3y_4$.

The probability of observing advance notice is

$$\text{(A4.10)}\ P(y_5=1)=P(y_3=1 \text{ and } y_4=1)$$
$$=P(y_3^*>0,\ y_4^*>0)=P(u_3>-x\beta_3,\ u_4>-x\beta_4).$$

After appropriate normalization, this is a truncated bivariate probit model (see Poirer (1980)) and it can be estimated by maximum likelihood methods. Given the estimates from this problem one can proceed in a multistep manner similar to that outlined above.

Identification in this model, however, requires that at least one variable found in the y_3^* equation be excluded from the y_4^* equation and vice versa. Unfortunately, as described in the text, it did not prove possible for us to identify the model.

Chapter 5

The Probit Probability of Experiencing Positive Duration of Nonemployment Model

Consider the following model

(A5.1) $y^*=x\beta+u$

where u is normally distributed with mean zero and variance σ^2. y^* can be interpeted as the individual's unobserved propensity to experience a positive duration of nonemployment and the vector x includes those explanatory variables expected to influence this propensity. We observe only whether the individual experiences a positive duration and let y equal one if he does and zero if he does not. Without loss of generality, we assume that

$$\text{(A5.2)}\ y=\begin{cases}1 & \text{if } y^*>0\\ 0 & \text{if } y^*\leq 0.\end{cases}$$

The probability of seeing $y=1$ is

$$P(y=1|x)=P(y^*>0|x)=P(u>-x\beta)=P(u/\sigma>-x\beta/\sigma),$$

so

$$\text{(A5.3)}\quad P(y=1|x)=F(x\beta/\sigma)=F(x\gamma)$$

where $F(\cdot)$ is the standard normal distribution function and $\gamma=\beta/\sigma$. Note that we can only identify $\gamma=\beta/\sigma$, that is, we can only identify the coefficients up to a scaling factor. Similarly, we have

$$\text{(A5.4)}\quad P(y=0|x)=1-F(x\gamma).$$

Equations (A5.3) and (A5.4) define the sample likelihood and the method of maximum likelihood can be used to obtain an estimate $\hat{\gamma}$ of γ. Such estimates appear in table 5.2 of the text.

Interpretation of the Parameters of the Probit Model

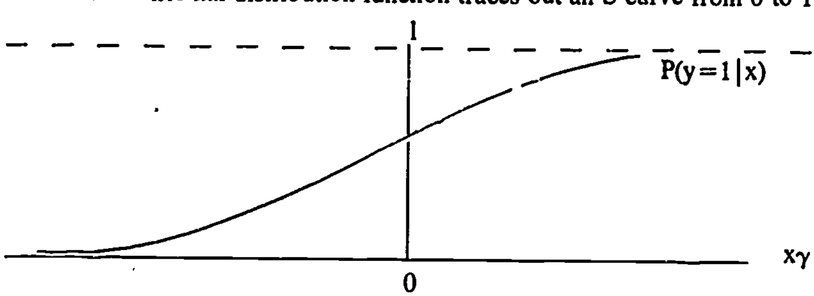
The coefficients γ are not sufficient to answer questions concerning the effect of a change in x on the probability that an individual experiences a positive duration ($y=1$). To see this, first consider the linear model in equation (A5.1). Clearly $\partial y^*/\partial x$ does equal γ in this model. But now consider the censored model in equation (A5.3):

$$\text{(A5.5)}\quad \partial P/\partial x\ (u=1|x)=\partial F(x\gamma)/\partial x=f(x\gamma)\ \gamma,$$

where $f(\ )$ is the standard normal density function. That is,

$$\text{(A5.6)}\quad f(z)=(2\pi)^{-1/2}\exp(-1/2\ z^2).$$

The effect of a change in x depends on the value of x in this censored model. The standard normal distribution function traces out an S-curve from 0 to 1



as $x\gamma$ ranges from minus infinity to plus infinity as indicated in the picture above. The derivative of interest is the slope of this S-curve, which depends on the point at which we examine the slope. That point depends on the particular value of x.

Now consider two thought experiments. The first is to consider an individual with a particular value of x, for example, $\bar{x}=x$ where $\bar{x}$ is the sample mean of x. *For this individual* the derivative of interest is

$$\text{(A5.7)}\quad \partial P(y=1 \mid x=\bar{x})/\partial x = f(\bar{x}\gamma)\gamma.$$

The second thought experiment is to draw an individual at random from the population and perturb his x's. On average, what is the effect on the probability that $y=1$? The answer here is the mean derivative in the sample, *not* the derivative evaluated at the mean x. That is, the relevant effect in this second experiment is

$$\text{(A5.8)}\quad E[\partial P(y=1 \mid x)/\partial x] = E[f(x\gamma)\gamma]$$

where the expectation is over the distribution of the characteristics x. The estimators for these two quantities are respectively

$$\text{(A5.7}'\text{)}\quad f(\bar{x}\gamma)\gamma$$

and

$$\text{(A5.8}'\text{)}\quad (1/N)\sum_{i=1}^{N} f(x_i\gamma)\gamma.$$

The answers to these two thought experiments need not be similar. Consider, for example, the case in which the sample is such that the $x_i\gamma$ are clustered in the two tails of the S-curve. Since the S-curve is relatively flat in the tails, the derivatives for each individual will be small, and hence the average derivative will be small. However, it is possible that $\bar{x}\gamma$ will lie in the middle, where the S-curve is steep. Hence the derivative at the mean will be much larger than the mean derivative, and focusing on the former will yield misleading inference about the effects of changes in x on any individual's probability of incurring a positive spell of nonemployment.

Finally, note that when the explanatory variable of interest is a dummy variable, the effect of a change in the variable (from zero to one) is computed in a different way. Let x_2 be the dummy variable of interest and let x_1 be the other explanatory variables, and partition $\gamma=(\gamma_1, \gamma_2)$ to match. Then the appropriate formulas that correspond to the two thought experiments are

$$\text{(A5.7'')}\quad F(x_1\hat{\gamma}_1+\hat{\gamma}_2) - F(x_1\hat{\gamma}_1), \text{ and}$$

$$\text{(A5.8'')}\quad (1/N) \sum_{i=1}^{N} [F(x_{i1}\hat{\gamma}_1+\hat{\gamma}_2) - F(x_{i1}\hat{\gamma}_1)].$$

respectively.

The estimated mean marginal effects of having had advance notice on the probability of a displaced worker's having a positive duration of nonemployment found in table 5.4 in the text are obtained using equation (A5.8'').

Testing for Exogeneity

We are concerned with whether we can treat the advance notice variable as exogenous in estimating the probability of nonemployment. The procedure for testing is easiest to describe for a linear model, so we do this first. Write equation (A5.1) as

$$\text{(A5.9)}\quad y^*=x_1\beta+x_2\beta_2+u$$

where x_2 is the variable that we wish to test for exogeneity. Write x_2 as

$$\text{(A5.10)}\quad x_2=\hat{x}_2+v$$

where $\hat{x}_2$ is an instrumental variable predictor of x_2 and v is the residual. Substituting into (A5.9) we obtain

$$\text{(A5.11)}\quad y^*=x_1\beta_1+\beta_2\hat{x}_2+\beta_2\, v+u.$$

Under the null hypothesis that x_2 is exogenous, the coefficients on $\hat{x}_2$ and v are the same. If x_2 is endogenous, then the coefficient on v, say β_3, will not be equal to β_2. We add and subtract $\beta_2 v$ in (A5.11) to get

$$\text{(A5.12)}\quad y^*=x_1\beta_1+\beta_2(\hat{x}_2+v)+(\beta_3 - \beta_2)\, v+u=x_1\beta_1+x_2\beta_2+\alpha(x_2 - \hat{x}_2)+u$$

where $\alpha=(\beta_3 - \beta_2)$. If x_2 is exogenous then $\alpha=0$. Hence the test for endogeneity is based on this coefficient.

The same testing procedure applies to the probit model. We construct an estimate of x_2, say $\hat{x}_2$, and ask whether adding $(x_2 - \hat{x}_2)$ to the probit equation adds to the explanatory power. Either the likelihood ratio test or the Wald test ("t-test") on α can be used since both are asymptotically equivalent.[2] In addition, the choice of the instrument for x_2, $\hat{x}_2$, does not affect the properties of the test. We used both linear probability and probit models to construct an instrument for the advance notice variable in the text, with little difference in results.

Finally, notice that in a linear framework we can replace $(x_2 - \hat{x}_2)$ in equation (A5.12) with x_2 and have an algebraically identical test statistic. In the probit model the two methods are not identical algebraically but are asymptotically equivalent. Regardless of whether we used RADV and $\widehat{\text{RADV}}$, or RADV and (RADV − $\widehat{\text{RADV}}$), we always concluded that it was legitimate to treat advance notice as exogenous. Similar methods were used in the text to test for the exogeneity of RADV in duration of nonemployment analysis and again we concluded that one could treat advance notice as exogenous.

Analyses of Duration of Nonemployment

The General Model

Let T be the random duration of nonemployment, conditional on some positive amount of nonemployment time. We assume that duration T has a probability distribution characterized by a density function f(t) and a distribution function $F(t)=P(T<t)$.

Initially, suppose that nonemployment time comes in a single spell. If a completed spell is observed, that is, we see the spell and or know the length of the entire spell, then the contribution to likelihood is the density of the duration time t which is f(t). On the other hand, if the spell is in progress at the survey date, then we do not know the length of the complete spell, but only the current length. Therefore we only observe a "censored" duration, and we only know that the completed duration T is at least as long as the observed current length t. In this case, the contribution to the likelihood is $P(T>t)=1 - P(T<t)+P(T=t)=1 - F(t)$, since we assume continuous distributions so that $P(T=t)=0$.

We wish to condition on a set of explanatory variables x which are thought to affect the distribution of duration. We must specify a particular form for that distribution, which will depend on a set of parameters θ. Therefore, we write the density function for duration as $f(t|x,\theta)$. Let c be an indicator variable that takes the value 1 if the spell is complete and 0 if the spell is censored. We observe (t, c, x) in the data and write the likelihood of a single observation as

$$\text{(A5.13)}\quad L=[f(t|x,\theta)]^{c}\,[1-F(t|x,\theta)]^{(1-c)}.$$

Maximizing the log of equation (A5.13) yields maximum likelihood estimates $\hat{\theta}$ of the parameters θ.

The Importance of Censoring

A simple example helps illustrate the importance of allowing for censoring. For simplicity we ignore any explanatory variables and consider a homogeneous population whose durations follow an exponential distribution:

$$\text{(A5.14a)}\quad f(t)=\alpha\exp(-\alpha t),\ \alpha>0.$$

$$\text{(A5.14b)}\quad F(t)=P(T\leq t)=\exp(-\alpha t).$$

The parameter α in this model is the "hazard rate," that is, the rate at which durations end. In this simple model the escape rate is constant over time, and the mean duration is

$$\text{(A5.15)}\quad E(T)=1/\alpha.$$

Letting i index individuals, the maximum likelihood estimate $\hat{\alpha}$ from a sample of size N is

$$\text{(A5.16)}\quad \hat{\alpha}=\sum_{i=1}^{N} c_i/\sum_{i=1}^{N} t_i,$$

that is, the number of *completed* spells divided by the total *observed* duration. If we ignored the censoring and treated all spells as if they were complete, then our estimate a of α would be

$$\text{(A5.17)}\quad a=N/\sum_{i=1}^{N} t_i,$$

that is, the number of *individuals* divided by total observed duration. Note that $a>\hat{\alpha}$, and therefore $(1/a)<(1/\hat{\alpha})$. Thus we would *systematically* *underestimate spell length* by ignoring censoring. Intuitively, the censored spells are longer than what we observe, and ignoring that leads us to believe that there are too many short spells, relative to the true population.

Distributional Assumptions

We employ a number of different distributional assumptions about $f(t|x,\theta)$ in order to ensure that our results are not an artifact of a particular choice. Our procedure differs slightly from the simplified ones outlined above, so we sketch it here.

We work with the natural logarithm of duration rather than duration itself. Let T_0 be the random duration time for an individual whose covariates x are all equal to zero. We then assume that the covariates enter as follows

(A5.18) $T(x)=T_0 \exp(x'\beta)$,

where β is a vector of parameters. Then we have

(A5.19) $\log T=x'\beta+\log T_0$.

Define $\mu\equiv x'\beta$, $W\equiv \log T$, and $W_0\equiv \log T_0$, and rewrite equation (A5.19) as

(A5.19′) $W=\mu+W_0$.

In general we allow for a scale effect σ, so that the general model takes the form

(A5.20) $W=\mu+\sigma W_0=x'\beta+\sigma W_0$.

This model is equivalent to writing duration as

(A5.18′) $T(x)= \exp(x'\beta)T_0^{\sigma}$.

These models are called "location-scale models for log t."[3]

The distributions we consider are the exponential, the lognormal, and the gamma. The first is characterized by a constant exit rate while the latter two allow for exit rates that vary over time. The exponential and the lognormal are special cases of the gamma. We will illustrate the procedure and the basis of inference using the exponential distribution. The details for the others are similar but rather messy.

Start from the model in equation (A5.14) but now write the exit rate α as

(A5.21) $\alpha = \exp(-\mu) = \exp(-x'\beta)$.

Conventional methods for driving the distribution of a function of a random variable yield the following expression for the density of the log of duration W=log T:

(A5.22) $g(w) = \exp(w-\mu) \exp[-\exp(w-\mu)]$

where $\mu = x'\beta$. The survival probability is

(A5.23) $1-G(w) = \exp[-\exp(w-\mu)]$,

where $G(w)=P(W<w)$.

Given these probabilities one can write down the likelihood function and maximize it using standard methods. The estimates presented in tables 5.5 and 5.6 in the text are obtained in this manner.

Interpretation of Parameters

We are interested in the change in expected duration that would occur due to a change in an explanatory variable, x. For the exponential model we have

(A5.24) $\partial E(t|x)/\partial x = \beta/\alpha = \beta \exp(x'\beta)$.

Note that this effect will be different for individuals with different values of x. The effect is β for the (nonexistent) individual for whom all x's are zero (including the intercept). However, the sign of the effect will be the same as the sign of β, and the effect is zero only if β is zero. Our interest is in the coefficient of the advance notice variable and for our purposes it is sufficient to test the null hypothesis that its coefficient is zero. If we fail to reject that hypothesis, as we do in the text, then we have no evidence in favor of the proposition that advance notice, affects duration of nonemployment once a worker loses his or her job, conditional on having some nonemployment time.

Multiple Spells

The *SDW* data do not allow us to distinguish between the case of a single spell of nonemployment of length t and the case of multiple spells whose length sum to t. For this reason, one cannot give the usual structural interpretation to the parameters in our duration equations that is usually given in duration analyses. Except in special cases, the sum of two random variables does not have the same distribution as the original random variables. Therefore the appropriate interpretation of our procedure is a "curve-fitting exercise." We have approximated the true distribution with a variety of functional forms. Since our results are robust to changes in functional form, we are confident that they are not merely artifacts of a particular model.

Determination of Censoring

As noted above, the proper treatment of censored durations is important in this type of analysis. The maximum duration reported in the data is 99 weeks; durations that were longer are also recorded as 99 weeks. As such, it is clear that a 99 also represents a censored observation.

Chapter 6

Details of most of the econometric issues that arise in this chapter have been discussed in the sections above. Estimation of the probit probability of employment as of the survey date equations and interpretation of the resulting coefficients follows the discussion above in (A5.1) to (A5.8) about the probit probability of experiencing positive duration of nonemployment equations. Corrections for possible sample selection bias in the estimated survey date earnings equation are implemented in an analogous manner to the corrections for possible sample selection bias in the predisplacement earnings equations discussed in (A4.1) to (A4.6) above. Finally, treating predisplacement earnings as endogenous in the analyses reported in table 6.5 in the text required us to use an instrument for it. This instrument was obtained using the variables described in the notes to table 4.1.

NOTES

1. This model corresponds to equation (4.10) in the text. We consider the more general selection model ((4.7 to (4.9)) in the text below.

2. Silvey (1975), chapter 7.

3. See Lawless (1982), especially chapter 6, for a further discussion of these models.

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