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Welfare and Work: Experiences in Six Cities

Christopher T. King
University of Texas at Austin

Peter R. Mueser
University of Missouri

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1
Welfare and Employment Transitions in the 1990s

The last decade has seen extraordinary changes in the programs supporting indigent parents and their children in the United States, popularly known as welfare. From its inception in the 1930s through the 1960s, the federal/state Aid to Families with Dependent Children (AFDC) program operated on the implicit assumption that a mother's primary "job" should be caring for her children. In recent decades, as women entered the labor force in greater numbers and acceptance of working mothers grew, public opinion has shifted toward an increased emphasis on work as an alternative to welfare, even for mothers with young children.

Although efforts to increase employment of aid recipients date back at least to the 1960s, the shift to an employment-focused system gained serious momentum only in the 1990s. Under federal waivers, many states developed programs that modified the basic structure of AFDC, imposing increasingly stringent work and training requirements on aid recipients. In addition, legislation as well as policy and administrative directives in many states shifted program emphasis away from provision of aid to families and toward finding employment alternatives to public assistance. The national trend culminated with passage of the federal Personal Responsibility and Work Opportunity Reconciliation Act in 1996 (PRWORA), which replaced AFDC with Temporary Assistance for Needy Families (TANF). The new legislation, which Congress passed with bipartisan support, specified explicit program participation and work requirements for participants as well as limitations on the length of time aid could be received. It further expanded state autonomy, allowing states to develop and implement aid under a variety of program structures.

Reform-oriented policy changes at the national and state levels during the 1990s dramatically modified the welfare service delivery system for recipients and administrators alike.[1] National patterns of aid receipt during the 1990s reflect these major changes. After moderate increases

through most of the previous two decades, for the most part tracing the growth of the United States population, the number of families receiving aid under AFDC had reached 4.0 million by 1990. In the next four years, the caseload reached a peak of 5.0 million and then began a decline, falling to 3.9 million in 1997, the year TANF was implemented in most states, and 2.6 million in 1999, a level not seen since 1970.[2]

While it is clear that increasing numbers of families are successfully transitioning from welfare to work, the probability that welfare leavers will successfully achieve stable employment and self-sufficiency over the long term remains in question. Many welfare recipients face significant barriers to employment, including physical disabilities, mental health or substance abuse issues, limited English proficiency, learning disabilities, and domestic violence, as well as poor job skills and inadequate work experience. These barriers are substantial in urban areas, where most aid recipients live. Those who are able to move into employment commonly cycle in and out of work, earn low wages, and often continue to rely on government supports such as the Earned Income Tax Credit (EITC), Food Stamps, and subsidized child care and health care.

Our study examines changes in welfare participation and labor market involvement of female welfare recipients starting in the early 1990s and extending through 1999. We focus particular attention on the dynamics of recipients' employment activities in light of the welfare-to-work emphasis of policy reform. Our detailed analysis is based on data for the core counties in six major urban areas: Atlanta, Baltimore, Chicago, Fort Lauderdale, Houston, and Kansas City. Together, these counties accounted for 5.1 percent of the nation's welfare caseload in 1991, as well as shares of their own state's caseloads ranging from 6 percent in Fort Lauderdale to fully 73 percent in Chicago.[3] These sites provide considerable range and diversity, including cities from a very low-benefit state (Texas), a classic northern urban area (Chicago), two cities on the border of the old South (Baltimore and Kansas City), one traditional southern city (Atlanta), and three cities with significant representation of Hispanics, one of the faster-growing populations on welfare. All experienced significant declines in their welfare caseloads over this period that were broadly consistent with the national trend.

Our analyses are based on administrative data that are unusual in allowing us to examine individual welfare and employment histories for

extended periods using parallel methods across distinct sites. Information on all welfare recipients beginning as early as 1990 is included in the data we obtained from state agencies, so trends over time in flows onto and off of welfare are identified. We matched this individual information with data collected by states in support of their Unemployment Insurance programs, providing information on recipients' detailed employment experiences, both during and after the period of welfare receipt.[4]

In the remainder of this chapter, we provide an overview of our key findings. Our focus is on trends and patterns that are common across states, and we present both national data and data from our sites to investigate the welfare caseload and employment for welfare recipients. We look at flows onto and off of welfare and consider how these have changed in the 1990s. We address issues of what kinds of people are most likely to leave welfare and what kinds of jobs they are likely to hold. We ask how welfare reform has altered the experiences of recipients and the processes of securing work and leaving welfare.

The second chapter delves more deeply into the structure of welfare reform at each of our sites, recognizing the central role that differences across states play in defining the features of reform. By focusing on six major cities, we can examine the extent to which differences in state and local policy, administrative directives, and local labor market conditions contribute to observed trends. It is widely acknowledged that policy and administrative changes designed to move families from the rolls have been facilitated by a growing economy, much more so than in the late 1980s and early 1990s during implementation of work-oriented programs under the Family Support Act of 1988. Other supportive policy changes—including expansions of EITC, Medicaid, and child care subsidies—that might fall within the broad rubric of welfare reform, were also occurring during this period. Comparison across cities will allow us to begin to understand mechanisms inducing change and the interaction between labor market conditions and government action.

Chapter 3 considers the role of demographic characteristics, economic factors, and policy regimes in explaining welfare exit and employment rates of welfare recipients during the 1990s at each of our sites. We also examine the reciprocal relationships between recipient employment and exit from welfare.

In Chapter 4, we turn to an analysis of the job stability of welfare caretakers. We consider explicitly job stability and earnings in particular jobs and trends in both over time. We also look at the jobs obtained by workers who are not welfare recipients but who are employed contemporaneously in the same firms and at the same earnings levels as these welfare recipients. Such analysis has not been attempted before.

Chapter 5 considers the extent to which job stability for welfare caretakers is explained by personal characteristics and the kinds of jobs they obtain. To conduct this analysis, we look at caretakers' demographics as well as key traits of their jobs, such as industry. The analysis also considers caretakers who hold multiple jobs and employers who hire more than one welfare recipient to separate out effects of employer and employee.

In our final chapter, we offer overarching conclusions and discuss the policy implications of our findings.

WELFARE CASELOAD DECLINES

The caseload decline after its peak in the 1990s was both precipitous and almost universal across states.[5] Table 1.1 provides information on the national welfare caseload at its peak in the early 1990s, extending through the end of the 1990s.[6] From that peak, the caseload had declined by just about a third by the end of 1997, and then again by another third in the next two years, for an overall decline of 53 percent by the end of 1999. Our sites show similar patterns.[7] Declines in caseload from the peak vary among our sites, but all are substantial, and they bracket the national decline, ranging from a low of 44 percent in Kansas City to a high of 81 percent in Fort Lauderdale. There is some evidence that federal passage of PRWORA in 1996 may have increased caseload declines. Nationally, the decline in percentage terms is similar before and after 1997, meaning that the annual decline in the more recent period is greater. The trend at our sites is similar, with particularly large caseload declines in the last two years.[8]

Many of the legal and policy changes following welfare reform focused on the activities of recipients, attempting to create both incentives and opportunities for them to obtain employment and exit welfare, as well as an accompanying set of penalties and sanctions if they did

Table 1.1 Welfare Caseload Trends in the United States and Six Areas

	Maximum		Fourth quarter 1997		Fourth quarter 1999		
Area	Year: quarter	Caseload	Caseload	Change from maximum (%)	Caseload	Change from 1997 (%)	Change from maximum (%)
United States[a]	1994:1	5,066	3,431	−32	2,361	−31	−53
Atlanta	1994:3	21,765	14,261	−34	9,298	−35	−57
Baltimore	1992:3	37,291	25,186	−32	14,859	−41	−60
Chicago	1995:3	132,345[b]	106,548	−19	63,283	−41	−52
Fort Lauderdale	1994:1	17,038[b]	7,464	−56	3,194	−57	−81
Houston	1992:4	55,468[b]	24,568	−56	12,278	−50	−78
Kansas City	1994:3	14,405	10,732	−25	8,072	−25	−44

[a] Caseload in thousands.
[b] This was the first quarter for which we have data. See text.
SOURCE: National data from U.S. Department of Health and Human Services (2004a,b).

Table 1.2 Welfare Exit Rates in Six Areas and Impact on Caseload

Area	Exit rates[a]			Caseload decline due to exit rate increase (%)[c]
	At peak caseload[b]	1997	1999	
Atlanta	0.073	0.119	0.167	−56
Baltimore	0.064	0.121	0.178	−64
Chicago	0.075	0.089	0.140	−46
Fort Lauderdale	0.175	0.337	0.431	−59
Houston	0.142	0.196	0.204	−30
Kansas City	0.096	0.146	0.161	−40

[a] Quarterly exit rates averaged over four quarters.

[b] Quarterly exit rate for four quarters preceding peak caseload. Where prior quarters are not available in our data, reported exit rates are based on the first four quarters for which we have data.

[c] Calculated decline in the stable caseload that would result from the observed change in the exit rate. The stable caseload can be written as $C=E/d$, where d is the exit rate and E is the flow of entries. A change in the exit rate from d_1 to d_2 produces a percentage change of $-(d_2 - d_1)/d_2 \times 100$.

not. Time limits created inducements to leave welfare: Although only a small number of recipients could exhaust these limits within the period of our study, recipients may well have decided to leave welfare to "bank" their remaining eligibility.[9] Reforms also included mandatory programs designed to aid recipients in obtaining employment, providing them with job readiness training and job search support, as well as basic skills and vocational training. It is clear that such reforms should increase rates of departures from the welfare rolls.

The Role of Welfare Exits

In all of our sites, we see that, in fact, increases in exit rates are substantial, accounting for large declines in the caseloads. Despite substantial differences across sites in initial departure rates, Table 1.2 shows that there were dramatic increases in exit rates at all sites. For four of the six sites, quarterly exit rates are less than 10 percent at the peak of

the caseload, meaning that fewer than 1 out of 10 recipients in a given quarter were off welfare by the following quarter. By 1999, quarterly exit rates at these sites were between 14 and 18 percent. In Fort Lauderdale and Houston, exit rates were initially higher than in our other sites, but both still increased substantially, with Fort Lauderdale's exit rate exceeding 40 percent in the final year.

The final column of Table 1.2 indicates how much the caseload would be expected to decline based on growth in the exit rate alone.[10] At all of our sites, the projected decline is very large, implying that exit rates play an important role in the observed decline. In three of the sites, the caseload would decline to less than half of its prior level because of the growth in exit rates.

Effects on Long-Term Welfare Recipients

Long-term dependence has been a key concern of welfare reformers for many years. How much have policy and program changes influenced long-term recipients? Table 1.3 presents statistics on exit rates for those who have been on welfare for at least two years. At all our sites, the exit rate for this group is much lower than for all recipients, but what is notable is that the increases in exit rates are substantial for these long-term recipients. In three of our sites, exit rates for long-term recipients increased at least two and one-half times. In most sites, substantial increases in their exit rates continued to occur between 1997 and 1999. The case of Fort Lauderdale is special, since Florida's two-year limit—a "hard" time limit relative to that implemented by most states—was forcing individuals to leave welfare by 1999. The exit rate for long-term recipients is between two and three times that for our other sites. Even if we ignore Fort Lauderdale, the evidence suggests that these reforms have been very successful in changing the behavior of long-term aid recipients.

Welfare Entry Effects

Some elements of welfare reform were also designed to reduce entry onto welfare. Not only were explicit diversion programs adopted by many states, in some cases requiring potential recipients to engage in job search prior to submission of a formal welfare application, but

Table 1.3 Welfare Exit Rates for Long-Run Recipients in Six Areas

	Exit rates[a]		
Area	At peak caseload[b]	1997	1999
Atlanta	0.051	0.102	0.141
Baltimore	0.047	0.099	0.157
Chicago	0.054	0.070	0.120
Fort Lauderdale	0.125	0.257	0.312
Houston	0.094	0.138	0.132
Kansas City	0.066	0.114	0.124

[a] Quarterly exit rates averaged over four quarters. Where prior quarters are not available in our data, reported exit rates are based on the first four quarters for which we have data.

[b] Quarterly exit rate for four quarters preceding peak caseload.

many states restructured their application processes as well. Equally important, reforms focused on recipients may have also influenced welfare applicants. Training and employment requirements adopted as part of these reforms may have had the effect of making welfare receipt less attractive, thus reducing incentives for individuals to enter the program. On the other hand, reforms that moved recipients with tenuous employment off of welfare may have increased the number of individuals returning after disappointing labor market experiences.

In fact, Table 1.4 shows that declines in the number of individuals entering welfare each quarter were large at all our sites. The final column of the table shows that, in the absence of any change in the exit rate, the declines in entries would have caused caseload reductions of between one-fifth and two-thirds. Declines in the numbers entering welfare were particularly important in Fort Lauderdale and Houston, suggesting that larger caseload declines in these sites were driven at least partly by a fall in the flow of new recipients.

Table 1.4 Welfare Entry in Six Areas and Impact on Caseload

Area	Number entering welfare[a]			Caseload decline due to entry decline (%)[c]
	At peak caseload[b]	1997	1999	
Atlanta	1,602	906	1,160	−28
Baltimore	2,556	2,451	2,048	−20
Chicago	7,721	7,463	4,403	−43
Fort Lauderdale	2,379	1,850	1,182	−50
Houston	6,962	3,087	2,019	−71
Kansas City	1,534	1,253	1,197	−22

[a] Quarterly number of entries onto welfare averaged over four quarters.

[b] Quarterly number of entries for four quarters preceding peak caseload. Where prior quarters are not available in our data, reported numbers are based on the first four quarters for which we have data.

[c] Calculated decline in the stable caseload that would result from the observed change in the entry flow. The stable caseload can be written as $C=E/d$, where d is the exit rate and E is the flow of entries. A change in the number of entries from E_1 to E_2 produces a percentage change of $(E_2 - E_1)/E_1 \times 100$.

EMPLOYMENT OF WELFARE RECIPIENTS

Welfare reform signaled a major change in the emphasis placed on employment as an alternative to cash assistance. Prior to the 1990s, AFDC rules specified certain eligibility criteria, and federal courts had ruled that those meeting the criteria were categorically eligible to receive benefits. States therefore had little leverage over recipients, and there were minimal efforts to increase their labor force participation. In practice, recipients in many states appear to have faced implicit pressure not to work while they received AFDC, since working recipients—and their caseworkers—were saddled with additional reporting requirements, a result of efforts to assure that benefits would be adjusted to reflect earnings variation.[11] Following welfare reform, states required most recipients to participate in work or training activities, applying sanctions—often including removal from the rolls—for those who failed to comply. In addition, many states raised the earnings disre-

gard, providing increased pecuniary incentives for individuals to obtain and retain employment.[12]

Table 1.5 provides employment rates for AFDC/TANF recipients for the United States and for our six sites. The welfare employment rate for the United States is provided for comparison, but it must be recognized that it is calculated differently in several respects from our site measures. First, the criteria for a case to be counted are slightly different, but this does not have an important impact on computed employment rates.[13] Second, the national statistics are based on employment rates submitted by state agencies for their caseloads, which are based on reports by caseworkers. In contrast, the employment rates for our sites are based on quarterly earnings reports from employers (i.e., unemployment insurance wage records). Given incentives for caseworkers to underreport employment under AFDC, it is likely that the former measure

Table 1.5 Employment Rates for Welfare Recipients in the United States and Six Areas

	Employment rate (%)		
Area	1994[a]	1997[b]	1999[c]
United States	8.3	18.2	27.6
Atlanta	26.5	34.4	37.0
Baltimore	20.1	28.4	34.7
Chicago	28.1	31.6	42.5
Fort Lauderdale	36.6	37.1	43.3
Houston	28.2	30.6	31.7
Kansas City	38.5	47.0	45.2

[a] Averaged over the four quarters of the federal fiscal year (October 1993–September 1994) except for Fort Lauderdale (January–December 1994) and Chicago (July 1995–June 1996).

[b] Averaged over the four quarters of the federal fiscal year except for national statistics (July–September 1997), and Fort Lauderdale (September 1996–March 1997, July–September 1997).

[c] Averaged over the four quarters of the federal fiscal year except for Houston (July–December 1998).

SOURCE: National data from U.S. Department of Health and Human Services (2004a,b).

may miss some employment. Third, since the national welfare employment figure is based on data for a single month, individuals employed for only one or two months in a quarter are only counted as employed in those months, whereas the quarterly measure that we use for our sites identifies as employed individuals receiving earnings in any month during the quarter. (The appendix contains a detailed description of our data sources and definitions.)

Table 1.5 suggests that, nationally, employment rates for recipients in 1994 were less than 10 percent, whereas our sites exhibit employment rates between 20 and 40 percent. And, the national figures show an increase of about 10 percentage points from 1994 to 1997, whereas our sites show more modest increases. A similar pattern occurs for the period from 1997 to 1999, the national figures again showing nearly a 10-percentage-point growth, and our sites generally displaying more modest growth in the rate of employment. Although it is clear that an increasing share of recipients is actively engaged in the labor market, the official statistics would appear to overstate the growth as we observe it at our sites. We have much greater confidence in the accuracy of UI wage records for documenting recipients' employment patterns, as do most researchers.

EMPLOYMENT OF LEAVERS

Not only did PRWORA specify work requirements for recipients, but the federal Welfare-to-Work program enacted as part of the Balanced Budget Act of 1997 provided additional support for recipients in obtaining employment. Those supporting welfare reform often suggested that reform would facilitate self-sufficiency through employment, improving the lives of those who would otherwise be dependent on government support.

One might expect that the increasing concern with employment would have been associated with higher employment levels for those leaving welfare. On the other hand, policy changes also may have had the effect of discouraging individuals from continuing to receive public assistance even when their employment opportunities were very limited.

States are not required to follow those leaving welfare, so there are no comprehensive national statistics identifying the employment ex-

periences of those who leave welfare. However, states are required to identify the reason that an individual left welfare. Although many individuals do not provide this information, and states have little incentive to provide accurate data (a majority of cases are coded as "other"), these statistics do give a sense of the trend in movements. The first line in Table 1.6 shows that, in 1994, 15 percent of welfare leavers were coded as leaving because of employment (technically, "increased earnings"). The proportion had increased to fully 23 percent by 1999.

The measure of employment we report at our sites indicates the proportion of leavers who are employed at some point in the first quarter following departure from welfare, again as measured by UI wage records.[14] It is important to note that not all individuals who are counted as employed by this measure are employed at the point they exit welfare, since observed employment may begin at any point in the quarter following the welfare exit. The measure is best viewed as an indicator of whether a recipient moving off of welfare is able to obtain a job, even if she exits without one. The table shows that about half of leavers obtain employment in the quarter after leaving. All of the sites display

Table 1.6 Employment Rates for Welfare Leavers in the United States and Six Areas (%)

Area	1994	1997	1999
United States[a]	14.7	17.4	23.0
Atlanta	58.5	64.5	61.2
Baltimore	44.8	54.6	59.7
Chicago	48.6	54.5	56.7
Fort Lauderdale	53.3[b]	53.2	55.4
Houston	43.7	50.4	49.1
Kansas City	57.6	65.2	66.0

NOTE: All measures apply to federal fiscal year (October–September) unless indicated otherwise. Site measures are means for four quarters.

[a] Proportion indicating employment as reason for leaving welfare.

[b] Fiscal year 1996.

SOURCE: National data from U.S. Department of Health and Human Services (2004a,b).

increases over the period of our study, varying from as little as 2 percentage points in Fort Lauderdale to 15 percentage points in Baltimore. In all sites, almost all of the increase occurred before 1997.

It is useful to place these results in context. First, it must be recognized that although the overwhelming majority of those working for employers in the state are covered in our data, some of those leaving welfare obtain employment outside the state, are self-employed, or are working in illegal or informal jobs, which are not covered in our state-specific UI wage record data. Second, our data do not attempt to capture household income. A substantial portion of departures from welfare are associated with changes in household structure, and in many cases this implies that former recipients are supported by other individuals. It is therefore no surprise that more than a third of former recipients are not actually earning income.

Nonetheless, the employment of leavers is of particular concern because national and state welfare reforms placed increased emphasis on this route of exit from welfare. Those supporting the reforms argued that their implementation would both benefit recipients and relieve the public purse. Training and related programs, in conjunction with work requirements, would move welfare families into the world of work, providing them with new opportunities for material betterment. Critics warned that it was more likely that the reforms would merely force those who were ill-prepared to support themselves to seek aid from family, private charities, or less restrictive public programs, causing increased material hardship and ultimately damaging the welfare of children in these families.

Our results do not fit either of these extreme views. The moderate increases in employment rates for welfare recipients in the face of the extraordinary economic growth occurring in this period do not paint a picture of unprecedented opportunity provided to those who exited welfare. On the other hand, given the dramatic increases in the exit rates from welfare, the very fact that employment rates did not decline suggests that the reforms have been at least somewhat successful in achieving reforms' employment goals. There is little support for the view that the reforms have dumped former recipients into a glutted labor market where they face worsening employment prospects. Of course, that judgment is based only on looking at employment rates. One may also ask whether the types of jobs welfare leavers obtain have changed and what

factors determine employment success. That is the focus of Chapters 3 through 5, which we now summarize.

EXPLAINING WELFARE EXITS AND EMPLOYMENT

To what degree are changes in the characteristics of welfare recipients responsible for the increased exit rates from welfare and for the growth in employment for welfare recipients? Prior to welfare reform, observers suggested that as individuals left the rolls, the remaining recipients might differ dramatically from the prior caseload. Our data allow us to identify the age and race of recipients, as well as the number of dependent children on the case and the length of time that the payee or case head has received aid. Consistent with other findings, we observe only modest changes in these measures over the period of our study.

Table 1.7 presents data on the contribution of such changes to observed differences in exit rates and employment between 1994–1995 and 1998–1999.[15] The details of this analysis are provided in Chapter 3.[16] In the case of welfare exit rates, we see that changes in characteristics contribute very little to the observed growth. The negative percentages in the table indicate that, based only on observed changes in recipient characteristics, exit rates would be expected to decline slightly rather than increase over this period. We must attribute essentially all of the increases in exit rates to changes in either the policies or the environment.

Similar results hold in the case of employment rates for welfare recipients. In four of the sites, we observe substantial increases in employment rates, and in each case cohort characteristics contribute very little to the observed increase. Higher rates of employment for recipients must be due to changes occurring over time in either the welfare program or the local labor market.

Since the goals of welfare reform focus jointly on moving people off of welfare and getting them into jobs, it is natural to ask how these goals are related. Our analyses show that many factors jointly influence employment and departures from welfare. Minority recipients are more likely to be employed but substantially less likely to leave welfare. In contrast, other factors tend to induce more welfare exits and higher employment. Those with more children and those who have received aid

Table 1.7 Changes in Exit and Employment Rates for Welfare Recipients: Role of Personal Characteristics and Regime Change in Five Areas

	Probability of exiting welfare				Probability of employment			
			Difference (%) in probability accounted for by				Difference (%) in probability accounted for by	
Variables	1994–95	1998–99	Cohort characteristics	Regime	1994–95	1998–99	Cohort characteristics	Regime
Atlanta	0.072	0.153	−1	101	0.340	0.396	−13	113
Baltimore	0.073	0.191	2	98	0.225	0.333	7	93
Fort Lauderdale	0.177	0.410	−3	103	0.361	0.439	−14	114
Houston	0.141	0.209	−9	109	0.242	0.229	108	−8
Kansas City	0.099	0.165	−0	100	0.413	0.454	13	87

for longer are both less likely to find employment and less likely to leave welfare. Unmeasured factors have similar effects on employment and welfare exits. Those individuals who are particularly likely to be employed are also likely to leave welfare. Of course, in part this reflects the fact that when employment yields sufficiently high earnings, an individual will no longer be eligible for welfare. The details of this analysis are provided in Chapter 3.

LOOKING AT RECIPIENTS' JOBS

A central goal of welfare reform is moving recipients into stable jobs. Chapters 4 and 5 use UI wage record data to examine the stability and earnings of jobs held by recipients in our six areas over the 1990s, before, during, and after the implementation of welfare reform. We are not aware of any systematic analysis of the kinds of jobs held by recipients that would allow this kind of comparison.

It is known that welfare recipients tend to have unstable, short-term jobs, with few benefits and low wages. Although we are not able to determine benefits, the wage record data allow us to determine how long an employee continues to receive earnings from a given employer. Table 1.8 is based on an analysis, presented in Chapter 4, that examines all new jobs that are begun by welfare recipients in the relevant period. We include continued employment with the employer even after an individual leaves welfare, assuring that we do not omit those jobs that lead to self-sufficiency. The first two columns show that only about half of all jobs obtained by welfare recipients last beyond the quarter in which they start. Perhaps of most interest, the proportion did not change appreciably between 1994–1995 and 1998–1999. The two columns on the right show that between 4 and 10 percent of jobs last eight quarters or more. This table displays a modest decline in the share of jobs lasting at least eight quarters at three of the five sites where we have data, and little or no change at the other two.

Although these results might suggest a decline in the quality of jobs welfare recipients are obtaining, in Chapter 4 we show that similar declines occurred for other low-wage workers as well. We also show that even where job stability has declined, earnings have not. We are therefore left to conclude that the kinds of jobs welfare recipients obtain

Table 1.8 Stability of Jobs Held by Welfare Recipients: Six Areas

	Probability that job lasts more than 1 quarter		Probability that job lasts more than 7 quarters	
Variables	1994–95	1998–99	1994–95	1998–99
Atlanta	0.472	0.457	0.050	0.050
Baltimore	0.536	0.525	0.089	0.060
Chicago	0.539	0.561	0.100	0.097
Fort Lauderdale	0.517	0.519	0.075	0.068
Houston	0.533	0.527	0.073	n/a
Kansas City	0.441	0.428	0.044	0.032

have not seriously deteriorated over the 1990s. Nor have there been substantial improvements, either in job stability or in earnings.

While the changes over time are modest at best, by any standard the jobs these welfare recipients have been able to secure are very poor ones. Over the life of the job (up to two years), the average cumulative earnings are between $2,000 (for Atlanta) and $5,000 (for Chicago).[17] Few of these jobs lead to economic self-sufficiency for mothers with at least one and often two or more dependents. Some individuals obtain sufficient earnings to move off of welfare and support their families when they succeed in cobbling together multiple low-paying jobs into a semisteady earnings stream. Others may stumble onto a good job after many tries.

FINDING A GOOD JOB

Although opportunities clearly are limited, those recipients who obtain the best jobs have substantial advantages. In all of our areas, the standard deviation of total earnings on a job is at least 50 percent greater than the mean, implying that some jobs provide reasonably good long-term earnings in these urban labor markets. In considering how a particular welfare recipient achieves stable employment, it is natural to ask how important individual characteristics are in procuring a good

job. It may well be that individual characteristics determine who will get the best jobs. In this case, there is little benefit in placing individuals with certain employers, since the only route to achieving economic self-sufficiency will be to augment their human capital. In contrast, certain employers may offer highly desirable jobs, and individuals lucky enough to land them will do relatively well over time.

Chapter 5 looks at the factors determining differences in earnings and job stability across jobs. Our findings confirm that demographic characteristics play a role in determining these job outcomes, but their effects are quite modest. In contrast, we find that the industry of the employer is of substantial importance. Furthermore, when we examine those firms that employ many welfare recipients, we find that employers differ from one another quite dramatically. It appears that some employers offer unstable employment and low wages to all their employees, whereas others offer relative stability and higher wages. Once again, getting a "good job"—one with a "good" employer—makes a real difference.

Naturally, one may ask whether differences between employers may be a result of unmeasured differences between individuals. If some employers hire particularly capable individuals, but differences between individuals are not readily observable, we may mistakenly assume that they offer desirable jobs. If this were the case, there would be no benefit of placing less qualified workers with such employers, since they would be expected to face summary dismissal. Fortunately, we are able to examine the importance of unmeasured individual factors, since many welfare recipients obtain multiple jobs. As might be expected, our analysis confirms that unmeasured differences between individuals do play an important role. But we find even after controlling for such person "fixed effects," substantial differences between jobs remain. As a result, it is possible to say with some confidence that certain types of jobs are "good," and that directing recipients to them will likely provide significant benefits. This implies a role for more targeted workforce development services for welfare recipients, a topic we turn our attention to later.

Although many differences between jobs may be difficult to measure, we do observe that broad industries differ widely in expected earnings. Figure 1.1 provides information about the expected earnings for jobs in six industries, based on a model that controls for unmea-

sured individual characteristics.[18] Although there are clearly differences across our sites, we see that variation in expected earnings across industries is generally consistent. As might be expected, jobs in temporary help services firms provide the lowest expected total earnings, reflecting both shorter duration of employment and lower quarterly earnings. Retail trade provides somewhat greater job stability and higher earnings, while restaurant work is only slightly better. Manufacturing jobs are appreciably better than jobs in these other industries, often with total earnings two or three times those for temporary help jobs. The figure also includes public administration, which generally provides very substantial job stability (except in Fort Lauderdale). Unfortunately, the number of welfare recipients who obtain jobs in public administration is quite small.

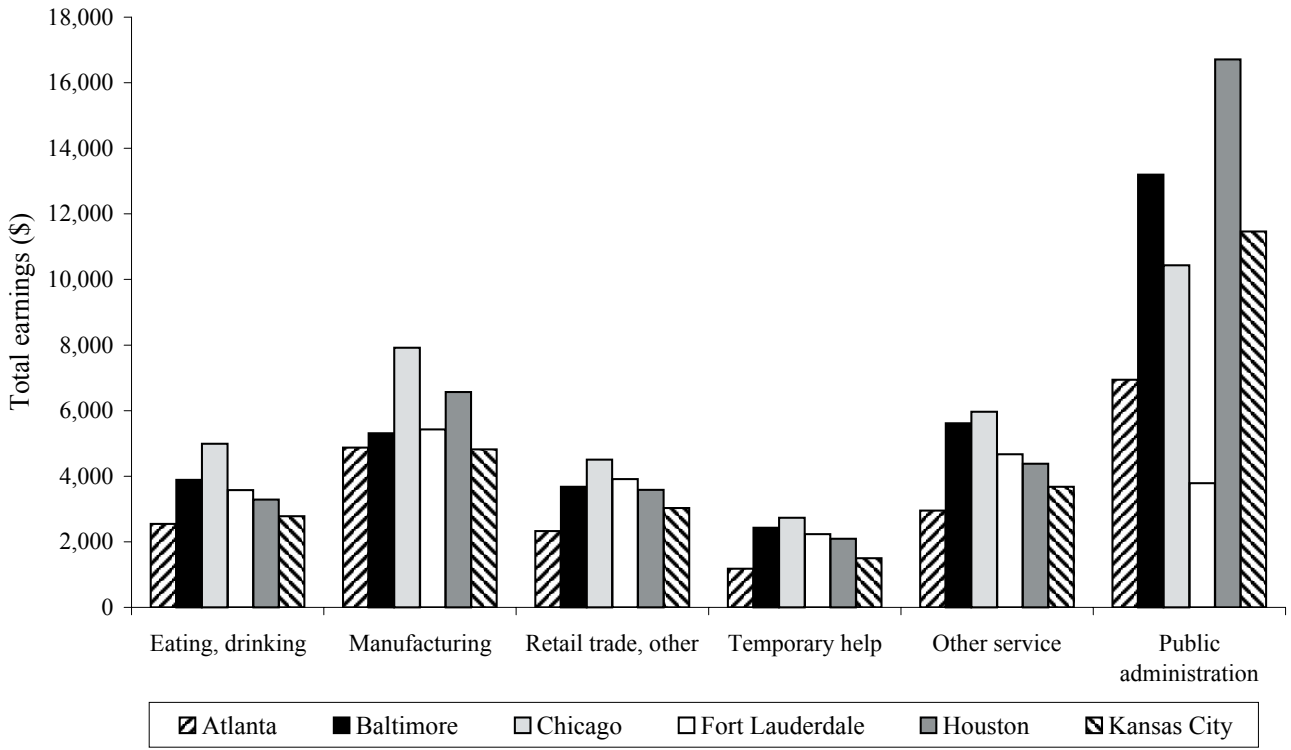
These results support the view that getting a good job is valuable for welfare recipients, as well as for others seeking work in urban labor markets. Although we do not see evidence that welfare reform has improved the stability of the jobs that recipients obtain, we do not see evidence of a deterioration in job quality. This latter observation may be taken as an endorsement of welfare reform, since we might well expect that, with an increasing proportion of welfare recipients obtaining jobs, there would be greater pressure for them to take inferior jobs. After all, the mantra of work-first programs under TANF and related federal programs has too often been, "Get a job, any job," much more so than, "Get a job, get a better job, get a career," as some have advocated.

CONCLUSIONS

The 1990s saw a dramatic shift in the character and focus of welfare in the United States. Our analyses document extraordinary changes in the patterns of movement onto and off of welfare, as well as important changes in the employment of welfare recipients. Nonetheless, patterns of movement from welfare to work have changed only in relatively subtle ways.

During the 1990s, the proportion of recipients working increased substantially, and among those leaving welfare, employment also was more prevalent. However, over this period, the kinds of jobs obtained by welfare recipients did not change dramatically. Expected earnings

Figure 1.1 Predicted Total Earnings for Jobs in Selected Industries



and job stability remained low for the average recipient of cash assistance, and few of the jobs they landed could assure economic self-sufficiency.

Despite the poor prospects offered by the average welfare recipient's job, we find evidence that some jobs do offer greater opportunities. Even recipients who have had a string of dead-end or short-lived jobs may ultimately be able to obtain a job providing a reasonable chance for economic self-sufficiency at some point. Federal and state welfare reforms of the 1990s have not altered this dynamic in a significant way. On one hand, this provides an endorsement of these new policies, since it suggests that they have succeeded in cutting caseloads and increasing labor market involvement of recipients and former recipients without causing a significant deterioration in their job prospects. On the other hand, the findings underscore the fact that reform has not substantially improved economic opportunities for recipients. The goal of reduced dependency has been attained in the sense that fewer individuals now receive cash aid and more are working, but there is no evidence that reform has substantially improved the lives of recipients or former recipients.

Notes

1. As explained below, welfare reform encompassed a broad array of policy and program changes at all levels and, given state-based actions and the widespread use of federal waivers, was implemented over a number of years in the 1990s.
2. Families receiving AFDC or TANF, computed as the average monthly level (United States Department of Health and Human Services 2004a,b).
3. Nationwide caseload information is from the U.S. Department of Health and Human Services (2004a,b). Broward County data are from the Florida Department of Children and Families (2004). Data for other counties are from Allen and Kirby (2000).
4. The study, and our state and site selection, is an outgrowth of our ongoing research as part of the multistate ADARE project, which has been funded by the U.S. Department of Labor since 1998. For more information about this effort, visit the ADARE Web site at www.ubalt.edu/jfi/adare/.
5. Hawaii holds the distinction of being the only state whose welfare caseload did not decline during this period.
6. The caseload for the United States is the average monthly caseload during the specified quarter. The quarterly caseload in each of our sites is the number of female payees receiving any payment during the relevant quarter, who are not in

the two-parent program (AFDC-Unemployment Parent or its TANF successor), and are at least 18 but less than 65. Caseload estimates at our sites could be as much as 10 percent higher if we included all cases. On the other hand, our use of quarters rather than months for site tabulations increases estimates of caseload by 5 to 10 percent. Further discussion of our data is provided in the appendix.

7. In three of our sites, the caseload declines from the initial quarter for which we have data. For Chicago, state data suggest that this first quarter is about 10 percent below the actual peak. In the other sites, the first quarter appears to be close to the actual maximum.
8. Figure 2.2 in Chapter 2 provides caseload patterns for each of our sites.
9. Only in Fort Lauderdale, where a two-year limit came into effect for receipt beginning in 1996, could a substantial number of recipients actually lose welfare eligibility due to time limits. In our other sites, almost all recipients were subject to the federal five-year limit (four years in Atlanta), which would not be directly binding until after 2000. Details are provided in the following chapter.
10. The percentage indicates how the stable caseload level is influenced by the observed change in exit rate. Details are provided in the notes to Table 1.2.
11. See Bane and Ellwood (1994) and Nathan and Gais (1999).
12. Other policy changes may also have encouraged work among welfare recipients. Hotz, Mullin, and Scholz (2001) report that increases in the federal EITC in the 1990s increased employment among welfare recipients in California.
13. Federal numbers include all cases, whereas those for our sites consider only female payees who are 18 but less than 65. The omission of males from our site analyses has a minor effect on employment rates. In 1996, federal statistics indicate that the employment rate for female adults receiving AFDC was 10.1 percent, whereas the full sample employment rate was 11.3 percent. Other sample differences have even smaller impacts.
14. This is the first quarter that the exiter receives no cash payment.
15. Chicago is omitted since our data do not extend back far enough to undertake analyses there.
16. Welfare exit rates and employment rates reported in Table 1.7 differ from those in Tables 1.2 and 1.5 both because the samples differ and because Table 1.7 presents means across individuals rather than means across periods of time. See Chapters 2 and 3.
17. In the discussion here, "total" or "cumulative" earnings on a job refers to the sum of earnings for as long as the job lasts, up to eight quarters. Fewer than 1 in 10 jobs last longer than eight quarters. Earnings are adjusted for inflation and are reported in 1999:4 dollars.
18. Estimated total earnings on a job reported in Figure 1.1 are based on wage records from a subset of major industries. Relative earnings for all major industries are presented in Table 5.7 in Chapter 5.

2
State and City Welfare and Employment Policies in the 1990s

Interaction between state and federal initiatives characterized the welfare reforms of the 1990s. In one sense, the states were the center for reform, since they took on responsibility for designing new welfare programs under federal waivers and then for implementing comprehensive federal legislation. Yet, to view each state program in terms of its specific provisions fails to recognize the extent to which they reflected similar underlying goals and conceptions. Every state program explicitly focused efforts on moving recipients into employment, requiring training or some form of labor market participation, and providing support services such as transportation and child care. Cash payments were viewed as providing only a temporary and explicitly inferior alternative to employment. In each case, the state programs represented substantial departures from the structures—the policies, programs, and services—that had characterized AFDC for nearly six decades.

Of course, the reform structures ultimately adopted by states were responsive to federal requirements under PRWORA, which specified caseload reduction and employment goals, as well as clear time limits for receiving cash assistance. But this alone cannot explain the similarity in programs, since on most dimensions PRWORA's constraints were not binding. The reformed state programs almost universally reflected a belief that a new employment-based approach to providing aid to single parents was necessary. The ideas that drove welfare reform were in large part developed as part of a discussion that swept across the country in the late 1980s and early 1990s.

We begin by considering welfare reform at the national level, focusing on the growing debate and the role of the Clinton administration in setting the agenda. We discuss the principal federal legislative benchmarks during the 1990s, tracing their effects in state policies. We then provide a selective overview of the literature on the impacts of welfare reform, considering both studies that trace state policy and those focusing on impacts on individuals and families.

We then turn to the six sites that are the focus of our study. We examine welfare policies at these sites, showing how states chose disparate paths in attempts to achieve quite similar goals. Next, we turn to an examination of program participation and employment data at each of the sites, identifying the effects of welfare reform in the patterns we observe. Building on the previous chapter, which focused on national trends, we provide more detailed analysis of each site, in part underscoring the similarities across sites, but also showing how patterns differ as a function of local reforms.

WELFARE REFORM: SETTING THE AGENDA AT THE NATIONAL LEVEL

When policymakers and others talk of "welfare reform," they generally are referring to the much-touted Personal Responsibility and Work Opportunity Reconciliation Act that was signed into law by President Bill Clinton in August 1996. But, PRWORA was not the only national action taken in the continuing effort to reform welfare. Despite the administration's failure to effectively package and sell an integrated welfare reform package to Congress, reform legislation that ultimately passed—together with related program expansions in this period—largely embodied the four principles that the Clinton administration had established: 1) make work pay, 2) establish time limits for cash assistance, 3) strengthen child support enforcement, and 4) fight teen pregnancy (see Ellwood 1996). Federal legislation throughout the 1990s lent support to the act and its goal of reducing dependence on welfare. These actions are highlighted in Table 2.1.

Making Work Pay

Key steps taken to "make work pay" included an increase in the federal minimum wage in 1996, as well as major expansions of Medicaid coverage, federal funding for child care, and the federal EITC. Congress enacted an increase in the federal minimum wage from $4.35 to $5.15 per hour in no small part to ensure that low-skilled single mothers leaving welfare would be able to earn an amount that would put them closer to self-sufficiency.[1] Corbett (2001, p. 3) points out that the real

Table 2.1 Decade of Welfare Reforms: A Chronology

1990	States are required to enact JOBS employment training programs in response to federal legislation passed in 1988 (phase-in continues for several years). Child Care and Development Block Grant created. Omnibus Budget Reconciliation Act expands Medicaid coverage to include pregnant women, infants to age one, and children to age six.
1992	Child Support Recovery Act & Ted Weiss Child Support Enforcement Act enacted.
1993	Family and Medical Leave Act enacted. Earned Income Tax Credit expanded. Low Income Housing Tax Credit program permanently extended.
1994	Head Start Act amendments enacted. Empowerment Zone/Enterprise Community Initiative enacted.
1996	Federal minimum wage increased. PRWORA enacted.
1997	Balanced Budget Act enacted: • Welfare-to-Work program established. • SCHIP established. • Medicare+Choice established.
1998	U.S. Department of Labor supplements Welfare-To-Work funding with $712 million in grants. Child Support Performance and Incentive Act enacted.
1999	Balanced Budget Refinement Act modifies Medicare, Medicaid, and SCHIP.
2000	Benefits Improvement and Protection Act of 2000 further refines Medicare, Medicaid, and SCHIP. Consolidated Appropriations Act expands Welfare-to-Work eligibility and extends deadlines.

value of the minimum wage increased by 15 percent in the decade from 1989 to 1999.

Congress also expanded Medicaid coverage beginning in 1993, greatly expanding the number of working poor families (and their children) eligible under this federal/state program. Burke and Abbey (2002) report that the share of low-income adults enrolled in Medicaid but not receiving any form of cash assistance increased from 44 percent in 1995 to 71 percent in 2000 (also see Fossett, Gais, and Thompson 2002). From 1994 to 1998, combined federal and state Medicaid payments rose by some 18.5 percent (U.S. Bureau of the Census 2001). Federal funding for child care surged from only $933 million in federal fiscal year (FY) 1996 to fully $2.3 billion in FY 1997 and $3.1 billion in FY 1998. The real value of the EITC increased dramatically from 1989 to 1999, in the case of a single parent with two children increasing by more than 200 percent, an increase of more than $3,000 relative to a family with no children (Corbett 2001).[2] Taken together, these steps had the effect of making work much more attractive relative to welfare than in years past and contributed substantially to the welfare caseload decline over the 1990s (Meyer and Rosenbaum 2001).

In addition, Congress passed and President Clinton signed into law the Balanced Budget Act of 1997, which contained the Welfare-to-Work program, a $3 billion effort designed to fund expanded employment and training opportunities for welfare recipients and noncustodial parents through the nation's job training system. Distributed as block grants to states and localities, Welfare-to-Work funds allowed administrators notable flexibility in how programs were structured.

Time-Limiting Cash Assistance

Under the provisions of the federal/state AFDC program, recipients in most states were able to access welfare benefits repeatedly over many years, provided they were otherwise eligible (i.e., low family income and young dependent children). PRWORA changed that situation dramatically. The act asserted that welfare would become a "temporary assistance" program for most recipients, limiting assistance over a person's lifetime to a maximum of five years. It also required recipients to begin looking for work within two years of receiving aid and established specific and increasing work participation requirements for

recipients that states would have to meet. States were allowed to exempt single parents from these new work requirements if they had dependents under the age of one, and they were precluded from imposing sanctions for nonparticipation if child care was not available.

Strengthening Child Support Enforcement

Child support and welfare policies are closely linked due to the large number of single-parent families receiving assistance. Numerous modifications to child support law were made during the welfare reform era. The Child Support Recovery Act of 1992 made it a federal crime to intentionally fail to pay a past-due child support obligation for a child living in another state. In the same year, the Ted Weiss Child Support Enforcement Act modified the Fair Credit Reporting Act to include child support delinquencies on credit reports. In 1993, the Omnibus Budget Reconciliation Act required that states establish paternity on 75 percent of the children in their caseload, and in the following year, several bills had child support riders that required states to further strengthen child support enforcement. Legislation passed in 1998 provided penalties for states that failed to meet data processing requirements, and established felony violations for parental willful delinquency.[3]

Fighting Teen Pregnancy

Since more than three-quarters of unmarried mothers end up on welfare, decreasing teen pregnancy was viewed as an important goal for reducing child poverty and welfare dependence (Sawhill 1998). The Family Support Act (FSA) of 1988 made an early attempt to curb the number of teen parents entering the welfare system by implementing an educational requirement for teen parents and allowing states to mandate that teen parents live with a responsible adult. PRWORA subsequently created new incentives for states to reduce teen pregnancy.

State and Local Reforms: The Role of Federal Waivers

In addition to these federal reform responses, numerous states and a number of urban communities embarked on their own versions of welfare reform in the early to mid 1990s. Clinton entered the presidency

with a vision of reconnecting the Democratic Party's social policy with that of the electorate (Weir 1998). By encouraging states to experiment with varied welfare models through waivers, the Clinton administration created momentum for reform and shaped the direction of the debate on welfare. In doing so, Clinton may also have pushed Republicans further to the right—a shift that strongly influenced the final content of the 1996 act, passed following the Republicans' sweeping victory in the 1994 midterm elections.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996

Clinton's decision to sign PRWORA prompted the resignation of several prominent staff at the U.S. Department of Health and Human Services, who felt that in "ending welfare as we know it" with this particular legislation, the president was making a terrible mistake. PRWORA was indeed a watershed event in reforming welfare in the nation. A summary of key provisions of the act is provided in Table 2.2.

IMPACTS OF WELFARE REFORM

Institutional Changes

Nathan and Gais (1999) headed up a team of researchers studying the implementation of PRWORA and state-based reforms in 20 states, including both the largest (e.g., California, Texas, New York), as well as some of the most innovative and influential (e.g., Wisconsin).[4] They concluded the act served to modify behavior of both families and state bureaucracies and that in the early stages its effects were best seen in terms of what they referred to as "the Three S's": signals, services, and sanctions. Stronger, bolder signals of a changed system ranged from new procedures for recipients securing aid (e.g., personal responsibility agreements), to diversion of potential recipients to work before welfare, and state work program participation requirements.

While most states initiated some form of "work-first" program, with a primary goal of getting recipients into any employment, they also offered a broad array of services to recipients, including remedial

Table 2.2 Key Provisions of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (Public Law 104-193)

Goal. Reduce the number of families dependent on federal assistance by ending entitlements, promoting work, and encouraging personal responsibility.

Temporary assistance. Under the act, Aid to Families with Dependent Children (AFDC), a federal entitlement program, is replaced by Temporary Assistance for Needy Families (TANF). The new program institutes a lifetime, 60-month limit on welfare assistance.

Personal responsibility. Includes measures aimed at increasing rates of participation in work-related programs for both one and two-parent families, and programs designed to reduce out-of-wedlock pregnancies and support family formation. Teen mothers are required to live with their families as a condition to receive aid.

Child support. Requires states to operate child support enforcement programs satisfying new federal requirements. States must establish a Federal Case Registry and implement national reporting requirements for all new hires, streamline paternity establishment, enact uniform interstate child support laws, and computerize central collection. New policies promote noncustodial visitation, increase penalties for delinquency, and give families no longer receiving assistance priority in child support collections.

Performance incentives. Includes a $1 billion competitive performance bonus fund to be allocated to states demonstrating excellence in reducing TANF caseload and assuring moves to self-sufficiency. Criteria, updated annually by the U.S. Department of Health and Human Services, include job entry and retention rates, child care subsidy payments, and increased family formation/stability, among others.

education, GED preparation, and English as a Second Language instruction; substance abuse counseling; child care and other family support services; emergency housing; domestic violence and emergency intervention; and mental health services. Job training was downplayed initially, in part due to the immediate demands for all types of workers in the tight labor markets of the late 1990s and 2000. Post-TANF employment retention and advancement services have been added to the service mix more recently.

The reformed system also places far greater stress on sanctions. Among the more important types of sanctions are those for failing to comply with portions of personal responsibility agreements. Caseworkers report that the threat of sanctions may be more important than their imposition since they tend to be imposed selectively and infrequently.

Nathan and Gais also emphasize that PRWORA has resulted in substantial devolution of responsibilities, both from the federal to state and local governments ("first-order"), as well as from governments to private for-profit and nonprofit organizations ("second-order" devolution). In the long run, states and localities face incentives to reduce the generosity of their welfare systems if potential recipients migrate in response to differences in potential benefits. Although the evidence that such migration occurs is mixed, the concern with such migration does influence state policies (Brueckner 2000).

Changes in the Experiences of Welfare Recipients

There has been some controversy as to the importance of economic and institutional changes in explaining caseload declines since the early 1990s. However, it seems clear that the implementation of the 1996 federal welfare reforms, together with related policy actions reviewed above, has had a large impact on caseloads.[5]

A review of recent literature reveals numerous attempts to determine the employment experiences of those leaving welfare in the 1990s. Clearly, a large share of welfare leavers is employed, but their labor market experiences are highly heterogeneous. Some appear to be appreciably better off than they were while on welfare, while others are appreciably worse off. For a summary of recent research findings, see Table 2.3.

Although much is known about the effects of welfare reform, existing studies are limited in a number of important respects. Because studies that allow explicit comparisons of the period prior to and following reform are usually national, there is little way to determine how state differences—demographic, economic, and policy—affect results. There are many local studies, but they tend to focus on the period following reform, and thus fail to provide information on whether recipient or leaver experiences changed. Even some of the most influential national studies focus on limited time periods, as for example Loprest (2001), whose prereform period is 1995–1997, after many states implemented initial reforms.

Our analyses focus on six urban areas, providing detailed information about individual recipients' experiences over an extended period of the 1990s, spanning most of the reform period. We have detailed information on the employment and related experiences of recipients over that period, and we are able to consider patterns of employment in much greater detail than most extant studies.

WELFARE REFORM IN SIX URBAN AREAS

The six sites that are the focus of our empirical analysis provide a broadly representative picture of state variation in welfare reform. Table 2.4 provides a brief chronology for each of our sites, identifying statutory, policy, or administrative changes affecting welfare programs in our sites from 1991 through 2000. Listed changes reflect statewide programs as well as local policies, programs, or administrative actions that may have affected welfare receipt in each of the sites. Our sites represent systems with strong state control (e.g., Missouri, Texas) and those where local counties have substantial discretion (e.g., Georgia). For several of our sites in which changes in the rules prior to 1991 may have influenced the tone of the system, these are listed in the table as well. By the end of 1997, the TANF program was officially in effect in all our sites, although full implementation of some federal rules was delayed because of existing waivers or specially negotiated arrangements.

It is clear that there are substantial differences in the timing as well as the impetus for changes across our sites. Although it provides some

Table 2.3 Recipients' Experiences under Reform: A Selective Survey

Issue	Research
Response to time limits	Blank (2002), Moffitt and Pavetti (2000), Grogger (2001), and Grogger and Michalopoulos (2003) found that welfare recipients respond to time limits, often exiting before limits bind.
Patterns of welfare use	Following reform, leavers are less likely to return to welfare (Carrington, Mueser, and Troske 2002). Those diverted from welfare have a high chance of returning at a later point (Schexnayder et al. 2002).
Employment	Welfare-related policy changes are associated with substantial increases in labor force participation (Bishop 1998; Blank 2002; Moffitt 1999; Kaushal and Kaester 2001; Meyer and Sullivan 2001). Single mothers' labor force participation rates rose by 10 percentage points from 1994–1999; the share of former recipients employed increased from 19.8 percent in 1990 to 44.3 percent in 2000 (Blank 2002). Studies based on prereform data show that employment played a larger role in exits than previously believed (Blank 1989; King et al. 1991; Lane and Stevens 1995; Harris 1993; Hoynes 1996). High rates of employment for welfare leavers are confirmed in local and national studies (e.g. Bavier 2001; Acs and Loprest 2001; Schexnayder et al. 2002; Loprest 2001; Tweedy et al. 1999), although only a minority of leavers are employed continuously for the full year following leaving (Schexnayder et al. 2002). Bavier (2001) suggests that high employment rates for welfare leavers are largely due to the strong economy; however, Lerman and Ratcliffe (2001) point out that employment for affected groups has not declined substantially following the economic decline after 1999.
Earnings/ poverty	Schoeni and Blank (2000) and Blank (2002) found that welfare-related policy changes resulted in increased earnings and decreased poverty among welfare-eligible families. But Bavier (2001) found that only half of leavers averaged higher post- than preexit household incomes (see also Boushey 2001). Schexnayder et al. (2002) report that earnings for welfare leavers in Texas increased in all six quarters postexit, with average quarterly earnings of $2,500 in the sixth quarter measured. Hanson and Hamrick (2004) estimated that, in the period 1996–2000, entry of former recipients into the labor market due to welfare reform reduced overall wage growth for all workers in affected labor markets by 2–7 percentage points.

Wages and employment stability	Following welfare reform, welfare leavers' wages are low, although a large share works full time (Lawson and King 1997; Acs and Loprest 2001; Pavetti and Acs 2001). There is considerable cycling in and out of work among former recipients (Acs and Loprest 2001). Temporary Help Services provide an increasing share of employment for welfare recipients and leavers (Autor and Houseman 2002; Heinrich, Mueser, and Troske forthcoming).
Household arrangements	Bavier (2001) found that nearly two-thirds of all leavers reside with other household members with incomes.
Use of in-kind assistance programs	Acs and Loprest (2001) found that over one-third of leavers received Food Stamps and about two-fifths utilized Medicaid. Medical insurance rates among adult leavers and their children ranged from 60% to 80%. Meyer and Rosenbaum (2001) showed that the expansion of Medicaid and other social services to low-income, nonwelfare families offered single mothers a significant new incentive to work. Schexnayder et al. (2002) reported for Texas that while 68% of leavers received food stamps in the year after leaving TANF, only 30% did so after 18 months. Only 20% of adults and children continued to receive Medicaid when their TANF case was closed.
Barriers to employment	Many of those leaving welfare include long-term recipients and others with substantial barriers to obtaining employment (Kalil et al. 1998). Common barriers include physical disabilities, mental health or substance abuse issues, limited English proficiency, learning disabilities, and domestic violence (Holcomb and Martinson 2002; Danziger and Seefeldt 2002). Bavier (2001) found that self-reported work-preventing health conditions were more prevalent among welfare recipients in 1999 than in 1996.

Table 2.4 Legislative and Administrative Changes Affecting Welfare Programs at Study Sites

Atlanta	
Prior to 1991	Positive Employment and Community Help (PEACH) plan enacted, providing education, transportation, child care (1986).
1993	November: Personal Accountability and Responsibility Project waiver granted, strengthening work requirements, setting family cap, increasing access to Medicaid and cash payments for working recipients.
1995	July: Inception of Work First, focused on income assistance for employment and job diversion. October: Second waiver approved, providing income disregards and allowing vehicle ownership for commuting. New work requirements applied to those receiving welfare in 24 of previous 36 months.
1996	August: Work requirement strengthened (exemption requires child age 1 or less rather than 3 or less).
1997	January: TANF state plan approved. March: State welfare reform enacted (Act 389) with four-year cash assistance maximum, family cap, work requirements. July-August: Georgia Work connection, collaboration between DHR, U.S. Dept. of Labor, and state training agencies, signals expansion of welfare-to-work efforts. August: TANF recipients dropped if they failed to sign personal responsibility agreement.
Baltimore	
1992	July: Primary Prevention Initiative creates incentives for preventive health care and school attendance for children receiving AFDC.
1995	November: State welfare reform Family Investment Program, implemented in Baltimore, with up-front job search requirements and child support provisions.
1996	August: Statewide waiver granted specifying work requirements, increasing work incentives. October: TANF implemented. Welfare avoidance grants and child care only provisions implemented to aid working parents.
1997	January: Federal time limits effective start date.

Chicago	
Prior to 1991	Employment initiative implemented under WIN (1986). Self-sufficiency demonstration waiver approved (1989). JOBS implemented (1990).
1993	November: Work Pays program implemented, increasing earnings disregard, along with related reforms.
1995	October: State legislation makes JOBS work exemptions and sanctions more stringent.
1996	February: New time limit (24 months) becomes effective but only applies to those with oldest child at least age 13.
1997	July: TANF implemented with five-year time limit, increased work requirements, support services and work incentives.
1998	March: Job-ready applicants required to seek work as part of TANF application.
1999	Community Partners Diversion Program implemented for job-ready TANF applicants.
Fort Lauderdale	
Prior to 1991	Project Independence welfare reform (1986).
1991	January: Hiring freeze at Department of Health and Rehabilitative Services disrupts program. October: Changes in job readiness criteria. Caseload reduction measures initiated.
1993	Family Transition Act passed and signed by governor, specifying that waivers be sought and time-limit experiments begun.
1996	October: TANF implemented. Work and Gain Economic Self-Sufficiency (WAGES) becomes effective statewide and time limits become effective. Florida Dept. of Labor takes over workforce-related issues for TANF clients, providing screening and work support arrangements (e.g., child care) in the first six weeks after entry onto TANF. Local WAGES coalitions, overseen by the state WAGES board, take on primary responsibility of moving TANF recipients to work.
1997	September: Parents Information Resource Center (Broward County nonprofit) awarded contract for WAGES intensive case management. October: Local workforce coalition established in Broward County.
1999	July: Lockheed-Martin awarded contract for WAGES intensive case management in Broward County.
2000	State WAGES Board abolished and replaced by State Workforce Development Board. Workforce Florida Inc. takes over work-related and transitional services for TANF recipients. All workforce development services are required to be provided by privately owned firms.

Table 2.4 (continued)

Houston	
Prior to 1991	Job Opportunities in the Business Sector initiative begins. Transitional child care and Medicaid benefits begin. Federal JOBS program implemented (1990).
1991	April: Child Care Management System offers improved access to child care.
1995	September: JOBS and Food Stamps transferred from Department of Human Services to newly created Texas Workforce Commission. October: JOBS programs shifts to work-first orientation.
1996	March: Statewide waiver granted specifying work requirements and time limits, increasing work incentives. April: Child care programs transferred to TWC. September: Age of child exemption dropped to 48 months. November: TANF implemented. December: Time limits implemented.
1997	November: Texas Works initiative diverts applicants to work and community services. December: Welfare applicants required to attend workforce orientation.
1998	August: One-time payments in lieu of TANF implemented.
1999	August: Fingerprint imaging of TANF and Food Stamp applicants implemented to deter/detect fraud. October: Maximum TANF benefits for 3-person family increased to $201/month.
2000	January: Age of child exemption dropped to 36 months. March: Earned income disregard increased from 33.3% to 90% with a $1,400 cap. September: Age of child exemption dropped to under 24 months.
Kansas City	
1991	July: Federal JOBS program implemented.
1994	June: 21st Century Program initiated (federal waiver), focusing efforts on employment for welfare recipients. October: Missouri welfare reform bill (HB 1547) formally takes effect, requiring JOBS participation and signing of self-sufficiency pacts for most welfare recipients, allowing increased asset ownership, requiring minor parents to live with parents, and providing for wage supplementation. Initially there is little effective enforcement of these provisions.

1995	Wage supplementation and job placement program develop as unified system throughout the year. April: Statewide waiver specifying work requirements and increasing work incentives granted. June: First participants enrolled in self-sufficiency pacts.
1996	November: New rules require that JOBS clients called to participate must respond within five days or face sanctions. December: TANF implemented.
1997	February: Post-employment case management developed. July: Caseworker specialization instituted. TANF time limits take effect. October: Further training to shift caseworker emphasis to employment. December: Work-first approach adopted in JOBS.
1998	November: New rules limit sanctions; caseworkers are required to meet with clients prior to imposing sanctions. New sanctions decline dramatically in the short run.
1999	October: Two-thirds income disregard implemented for recipients obtaining new employment.

SOURCE: State and local administrative directives and interviews with government officials.

indication as to the extent of program changes, the identification of particular milestones may be somewhat misleading in those cases where changes were largely continuous. For example, the 21st Century Program, initiated in Kansas City in 1994 with a federal waiver, would have had its primary impact as it expanded over the next two years. The situation was similar in Houston. Texas was granted a federal waiver in late 1996 for its Achieving Change for Texans (ACT) demonstration. However, Texas' welfare employment programs began to emphasize immediate labor force attachment over training as early as the fall of 1995, when Texas began implementing state enabling legislation.

Although state policy changes prior to the 1996 federal welfare reform were of substantial importance, in most of our sites, implementation of the TANF program was associated with additional program changes, many of which were substantial. Most states passed new legislation to implement TANF reforms, often further strengthening work requirements or imposing more stringent time limits. An exception among our sites is Kansas City. In contrast to most states, Missouri enacted no major legislation to facilitate implementation of the 1996 federal welfare reform, so program changes necessary to make Missouri's welfare program consistent with the federal law were made at the administrative level.

In order to indicate the extent of changes associated with TANF implementation, Table 2.5 compares 10 types of provisions in effect in July 1996, prior to TANF implementation in our sites, to those in July 1997, subsequent to implementation. These provisions have been selected because of their likely effects on welfare receipt and labor market participation.

Column 1 shows that differences between the sites in payment levels that existed under AFDC were not substantially altered by TANF implementation. For a mother with two dependent children, the maximum benefit levels in 1997 for families with no other resources were as follows: Atlanta, $280; Baltimore, $377; Chicago, $377; Fort Lauderdale, $303; Houston, $188; and Kansas City, $292. Except in Baltimore, where benefits increased to $399 by 1999, these levels have remained essentially unchanged in nominal terms since 1990.[6] Adjusted for inflation, maximum real benefit levels have dropped significantly in all sites. Columns 2 and 3 show that the initial eligibility thresholds and the earned income disregard changed in only two of the sites, Baltimore

and Fort Lauderdale, whereas in Chicago changes from AFDC rules had occurred under waivers approved by the U.S. Department of Health and Human Services previously. Notably, in three of the sites the rules remained largely as they had been under AFDC.

All sites experienced the introduction of time-limited cash assistance with the implementation of TANF or related policies under federal waivers (column 4). TANF imposed a five-year lifetime limit on welfare receipt, and none of the sites previously had this kind of limitation. In Florida, substantially more restrictive time limits were imposed than at other sites. In Texas, shorter time limits were imposed in the year following TANF implementation under its federal waiver program, although they included exemptions that allowed most recipients to avoid binding time limits. In the year prior to TANF implementation, Illinois imposed restrictive time limits, but these applied only to cases where the youngest child was at least age 13.

All sites show substantial changes in the child age exemption from work or training requirements (column 5). Prior to TANF implementation, a mother with a child under age 3 was not required to participate in work or training programs. In three of our sites, this figure declined to 12 months with TANF, and in Fort Lauderdale to 3 months. In Missouri, there was no decline with TANF implementation, although the age did decline in the following year. In Texas, the youngest child's age actually increased in response to a perceived budget shortfall for child care assistance.

The other listed categories differ across sites, with changes generally implying more severe penalties for undesired behaviors (sanctions increase, a family cap is imposed) but also greater rewards for work by allowing recipients to maintain higher asset levels.

State and local politics combined with federal initiatives to produce reform. It is worth considering briefly the debates occurring in the states at the time of reform and the processes underlying the development of new welfare legislation and policy.

Atlanta

Georgia administers TANF through its 159 counties, which have a great deal of discretion in program operations within a state-administered system.[7] Each county welfare office operates under the auspices

Table 2.5 Changes in Welfare Rules Associated with TANF Implementation at Study Sites

	Maximum monthly benefits (family of 3) ($)	Initial eligibility threshold ($)	Earned income disregard	Time limit (months)	Youngest child age for working exemption (months)	Most severe sanction	Asset limits ($)	Vehicle exemption[b] ($)	Diversion pay[c]	Family cap[d]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Atlanta										
Pre-TANF	280	514	AFDC[a]	None	36	Adult share, 6 mo.	1,000	1,500	No	Yes
1997	Same	Same	Same	60/48[e]	12	Total grant, perm.	Same	4,650	No	Yes
Baltimore										
Pre-TANF	373	607	AFDC[a]	None	36	Adult share, 6 mo.	1,000	1,500	No	Yes
1997	377	471	26%	60	12	Total grant, indef.	2,000	1 vehicle	Yes	Yes
Chicago										
Pre-TANF	377	467	66.7%	None[f]	36	Adult share, 6 mo.	1,000	1,500	No	Yes
1997	Same	Same	66.7%	60[f]	12	Total grant, 3 mo.	3,000	1 vehicle	No	Yes

Fort Lauderdale										
Pre-TANF	303	574	AFDC [a]	None	36	Adult share, 6 mo.	1,000	1,500	No	No
1997	Same	806/ 393 [i]	$200 + 50%	24/48 [g]	3	Total grant, 3 mo.	1,000/ 2,000	1,500/ 8,500 [i]	Yes	Yes
Houston										
Pre-TANF	188	400	AFDC [a]	None	36	Adult share, 6 mo	1,000	1,500	No	No
1997	Same	Same	Same	60 [h]	60/48 [i]	Same	3,000	4,650	No	No
Kansas City										
Pre-TANF	292	558	AFDC [a]	None	36	Adult share, 6 mo	5,000	1 vehicle	No	No
1997	Same	Same	Same	60	36/12 [i]	Same	Same	Same	No	No

[a] AFDC rules are $120 and 33.3% of remainder for first 4 months, $120 next 8 months, $90 thereafter.
[b] All pre-TANF values are equity in vehicle. Values for Atlanta and Houston in 1997 are fair market value; the values for Fort Lauderdale are equity values. The 1997 value for Atlanta requires that the vehicle be used for work or transportation to work or school; otherwise the value is $1500.
[c] Diversion payments provide funds in lieu of welfare to help families facing short-term crises.
[d] The family cap limits incremental payments for those who have additional children while they are receiving welfare payments.
[e] A time limit of 60 months became effective with implementation of TANF in Janaury of 1997, but it was reduced to 48 months in March, when Georgia's state welfare reform legislation became effective.
[f] In February of 1996, Illinois instituted restrictive time limits (24 months) that applied only to those with children 13 or over. By 1998, families with earned income and 20 hours per week of work faced no time limits.
[g] Florida imposes a 24-month time limit in any five-year period and a 48-month lifetime limit.
[h] Texas imposed shorter time limits in September 1997 (12, 24, or 36 months, depending on recipient circumstances), but relatively few individuals were subject to these. In practice, the 60-month limit became effective for recipients beginning in 1999.
[i] The rule was different in 1997 and 1998; both rules are shown.
SOURCES: Rowe (2000). Time-limit information also from Crouse (1999).

of its own board nominated by the county commission and appointed by the state commissioner of the Georgia Department of Human Resources. Georgia, like many states, implemented a series of major bipartisan welfare reforms prior to TANF, beginning with Positive Employment and Community Help (PEACH), which started as an education and training oriented pilot in 1986 and expanded to include all Georgia counties by 1993, and Work First, which was instituted in 1995. Both PEACH and Work First were highly collaborative in nature, engaging a broad array of partners at the state and local levels. Traditionally, Democrats have made political decisions in Georgia. Until the 2002 elections, Georgia was the only state in the Old South that had not elected a Republican governor since Reconstruction, and both branches of its legislature were controlled by Democrats. The Georgia legislature, however, was growing increasingly conservative over the 1990s, with rising shares of both houses in Republican hands.

While Atlanta looms large in terms of its influence on state social policy, it does not dominate Georgia as Chicago does Illinois in policymaking. Nearly one in three welfare households resides in the Atlanta metropolitan area; many but not most of them are in Atlanta (see Rich 1999).

Baltimore

Maryland's political environment in the early 1990s reflected the national consensus that moving welfare recipients into jobs was critically important and that reforms of the system were necessary to effect this goal. In the early 1990s, a state reform commission solidified this consensus, forging concrete agreements that would result in state reform legislation in 1995. Notwithstanding the emphasis on work, the state's approach to reform reflected its liberal, strongly Democratic, political environment. Although Maryland adopted a family cap, limiting payments to women who had additional children while receiving welfare, a provision allowed such payments to a third party who could target them to child-related expenses. Maryland is one of the few states that increased nominal cash assistance levels during the 1990s, with payments increasing by 10 percent in the period 1996–1999.

Local areas in Maryland have substantial flexibility in the structure of welfare implementation. In Baltimore, which accounts for more than

half of the state's caseload, recipients with identifiable barriers are generally exempted from work requirements, and activities such as looking for child care count toward work requirements.

Chicago

Illinois began instituting its own welfare reforms several years before the enactment of PRWORA, starting with a liberalization of earnings disregards and the implementation of its Work Pays program in late 1993. The approach initially tended to favor "carrots" over "sticks" but was followed by increasingly stringent work requirements and sanctions enacted by the state legislature in late 1995 and 1996, as Illinois became caught up in the fervor to reform welfare. Unlike most of our states, these largely noncontroversial changes appear to have been driven by bureaucrats within the state's human services agency who were closely monitoring related developments in other states and attempting to keep Illinois abreast of these policy trends. The state legislature appears to have played a smaller role than in some other states in determining the details of welfare policy.

It is worth noting that Chicago dominates the Illinois landscape for most state policy issues, with the possible exception of agriculture. On economic and social issues, policies generally are framed for Chicago and the rest of the state.[8] Welfare is largely seen as an urban issue in Illinois, in large part due to the fact that around 70 percent of Illinois' welfare caseload resides in the Chicago area, most of it in Cook County.

Fort Lauderdale

Reflecting the basically conservative flavor of Florida state politics, by 1992 a consensus existed against continuing unlimited access to public support through the AFDC program. Under Democratic leadership, the first legislative step toward reform was passage of the Family Transition Act, which began the process of seeking waivers for reform efforts, including setting up time-limited welfare in demonstration projects. Based in part on the results of such pilot projects, by 1996, legislators were convinced that work requirements and time limits, combined with increased services, provided workable reform elements that would not devastate families.

The Work and Gain Economic Self-Sufficiency (WAGES) legislation was passed unanimously in 1996, and implementation of TANF under this legislation occurred in October. The state WAGES board had overall responsibility for implementing TANF, and local WAGES coalitions had responsibility at the local level. In some regions of the state, however, separate local boards oversaw job training and welfare activities. In Broward County, a single board assumed both roles. When the existing board assumed responsibility for TANF, there was relatively little disruption.

In part, changes occurring in the late 1990s reflected attempts to improve program effectiveness by increasing the autonomy of local boards and moving the provision of social services to the private sector. This trend continued when Republicans obtained control of the state legislature in 1997, and later the governorship. With implementation of TANF, a nonprofit was awarded the contract for intensive case management in Broward County, and, in 1999, a division of Lockheed-Martin was awarded the contract. In 2000, new legislation gave responsibility for welfare implementation at the state level to Workforce Florida Inc., a public/private entity reporting to the governor.

Houston

Texas enacted major bipartisan welfare and workforce development reform legislation (House Bill 1863) in June 1995 that directed the state to request and secure a federal waiver in 1997 to implement its ACT demonstration.[9] The Texas legislation and subsequent waiver provided for relatively lenient "tiered" time limits based on a recipient's education and recent work experience, and considerably increased resource and asset limits. The Houston area historically has accounted for between one-fifth and one-quarter of Texas's welfare caseload. Houston was among the more active areas of the state in terms of reforming its welfare and related systems, participating in pilot welfare employment initiatives to train and place recipients in jobs paying self-sufficiency wages as early as 1988. Welfare employment programs in the Houston area began shifting toward a work-first orientation in the fall of 1995. The main provisions of TANF were implemented in Houston in late 1996 and the early months of 1997.

Kansas City

Observers trace welfare reform in Missouri during the 1990s to implementation of the 1988 federal Family Support Act, which began a process that gradually shifted the structure of the welfare system. By the early 1990s, the view that program reform was necessary to move AFDC recipients into employment was gaining wide support among policy actors in the state. However, when Mel Carnahan replaced John Ashcroft as governor in 1992, with obvious major changes in the political climate, the welfare reform momentum in the state was not substantially altered. The head of the Department of Social Services under Ashcroft remained in his position through most of the 1990s. It appears that interest in reform at the national level was also reflected in the state.

State welfare reform, passed in 1994, reflected a broad consensus that welfare should become a transitional program, with most recipients moving into employment after some period of time. Although advocacy groups protested the new legislation, and conservative lawmakers argued for less generous provisions, those who had serious disagreements with the basic legislation were clearly in the minority.

In Kansas City, the Local Investment Commission, a community-based organization with substantial representation from local business, played a central role in implementing welfare reform. The 21st Century Program, which included wage supplementation as well as other local innovations, was among pilot programs implemented in Kansas City prior to state welfare reform. The Local Investment Commission continued to play an important role in administering welfare-to-work activities throughout the 1990s.

When PRWORA was passed in 1996, there was little interest among legislators in returning to welfare reform, and as a result TANF was implemented without any new state legislation. Although often listed as implementing TANF in December 1996, after extended negotiations the federal government agreed that time against the federal lifetime limit of 60 months not begin until July 1997.

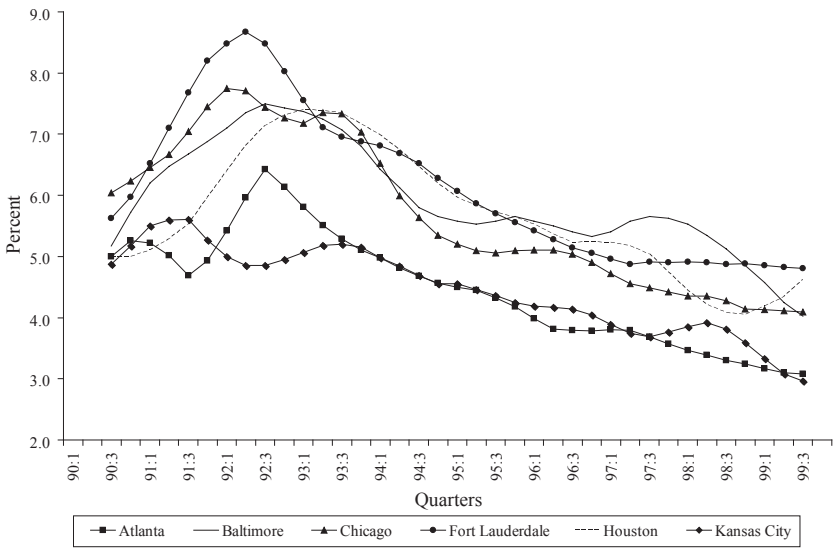
LABOR MARKET AND CASELOAD DYNAMICS

Welfare reform played out against a backdrop of economic growth that had few parallels in the prior century. In what follows, we first consider the economy at each of our six areas. We then examine the patterns of flows onto and off of welfare, considering explicitly the role of reform and the economy. We also consider patterns in recidivism across our sites and employment for welfare leavers.

Labor Markets

Unemployment rates for all six areas are presented in Figure 2.1. Since our concern is with general economic conditions in the relevant labor market, unemployment rates apply to the entire primary metropolitan area, not just the central county that is our focus. In order to remove seasonal effects and reduce quarter-to-quarter volatility, we present four-quarter moving averages.[10]

Figure 2.1 Unemployment Rates for Six Metropolitan Areas (4-quarter moving average)



In all six areas, the data suggest very healthy economic growth since the early to mid 1990s. Atlanta and Kansas City appear to have had the tightest labor markets through most of the 1990s, both with unemployment rates dipping below 4 percent in the 1997–1999 period. The other sites had unemployment rates about a point higher than these, although the rate in Chicago was generally lower than that in the other sites in the second half of the decade. In Baltimore, until 1997, the recovery was not as pronounced as in the other sites, but in the last two years of our data, unemployment rates there declined substantially.

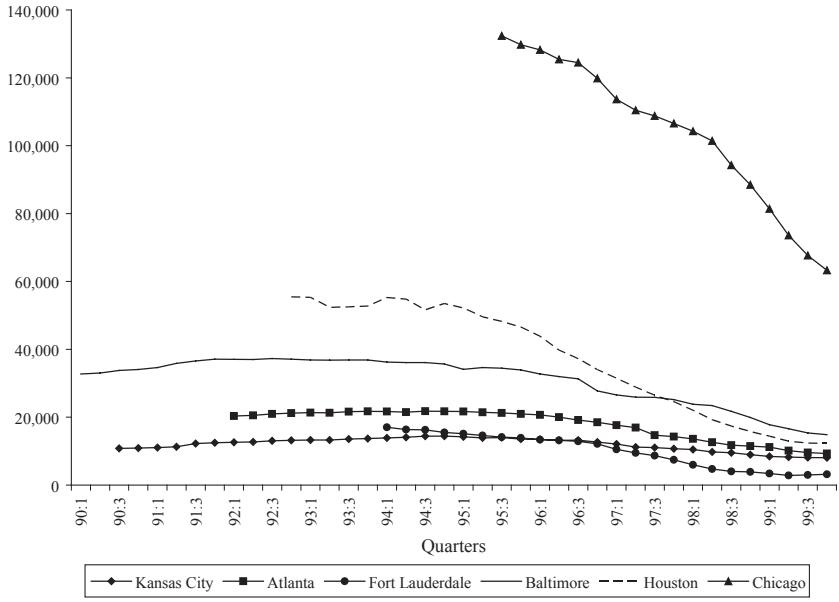
Most sites experienced modest further declines or steady unemployment in the 1997–1999 period. The exception was Houston, where unemployment increased substantially in the last year of our data. In short, while all areas have enjoyed economic growth, there are substantial inter-area differences that could influence the experiences of AFDC/TANF recipients.

Caseload Trends

Figure 2.2 presents the welfare caseload over the 1990s for each area. For three of our sites, the caseload increases from its level at the start of our period to a peak in the early to mid 1990s, followed by a continuous decline to the current level.[11] For Houston, we identify two local peaks, one at the beginning of our data series in the second quarter of 1992, and a second only slightly lower one in the first quarter of 1994, followed by a decline to the 1999 level. Although we do not have comparable data for prior periods, trends in the state suggest that these peaks are close to the maximum caseload. Similarly, we believe the actual peak in Fort Lauderdale is close to our observed initial caseload. In Chicago, the maximum caseload in our data occurs in the first quarter for which we have data, the third quarter of 1995. Statewide data for Illinois suggest that this level may be substantially below the state's peak caseload.[12]

Fort Lauderdale's welfare caseload displays the greatest reduction, declining 56 percent from the peak (at the beginning of our data) to 1997, and an additional 57 percent from 1997 to 1999. The decline for Houston is only slightly smaller. Declines for Atlanta, Baltimore, and Kansas City are smaller but still substantial. It seems likely that Chicago would appear similar to these three if we had data for the early 1990s.

Figure 2.2 Basic Welfare Caseloads



Welfare Entry and Exit Rates

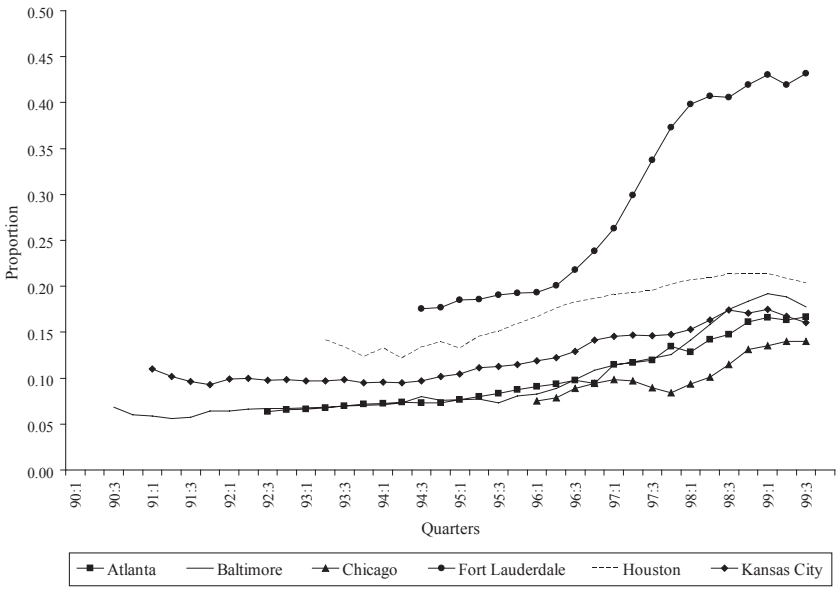
Figure 2.3 depicts the exit rate from welfare, while Figure 2.4 shows the number entering welfare. The time trend in the exit rate is strongly positive for each site, although substantial differences exist among them. The lowest quarterly exit rates are in Atlanta, Baltimore, and Chicago. Up through 1994, Atlanta and Baltimore had exit rates that averaged around 7 percent, increasing gradually and matching Chicago with average rates around 8 to 9 percent through the mid 1990s. By the late 1990s, welfare exit rates in Atlanta and Baltimore were more than 15 percent, while the rate in Chicago was only slightly lower. In contrast, average exit rates in Kansas City in the early to mid 1990s were generally over 10 percent, with the rate exceeding 15 percent in the most recent quarter of 1999. Houston had an average exit rate in the 10–15 percent range in the early to mid 1990s, with exit rates over 20 percent in 1998–1999. Exit rates in Fort Lauderdale were somewhat higher than

in the other sites in the mid 1990s, nearly 20 percent, but they increased dramatically after 1996, averaging over 40 percent in 1998–1999.

Many of these patterns are clearly tied to legislative, policy, and administrative decisions. In Atlanta during the third quarter of 1997, the exit rate increased to 18 percent, from 8 percent in the previous quarter, and then declined to 11 percent in the following quarter.[13] This reflects the fact that the Georgia Department of Human Resources dropped all welfare recipients in that quarter who had not signed personal responsibility agreements. Also, the dramatic increase in exit rates in Fort Lauderdale is probably the result of Florida's welfare reform legislation, WAGES, which included a maximum limit of two years of welfare receipt in any five-year period, a restriction that was effective statewide with few exceptions. Illinois and Florida have the most generous income disregard policies in this period, which would tend to reduce exits, although in Florida, stringent time limits undoubtedly overwhelm this effect.

Much of the growing concern in the last decade has focused on welfare dependency, i.e., long-term welfare receipt with little work. Pro-

Figure 2.3 Overall Welfare Exit Rates (4-quarter moving average)



grams designed to encourage employment among welfare recipients have frequently specified that long-term recipients be among the first served. PRWORA provided explicit lifetime time limits for welfare receipt, and, as indicated, a number of our sites were in states that adopted even more stringent limits. In Chapter 1, we showed that welfare exit rates for long-term recipients, although remaining lower than those for other recipients, increased as well. In fact, exit rates for those who had been on welfare for at least two years exhibit a basic pattern that is almost indistinguishable from that shown in Figure 2.3. The large increase in exit rates for long-term recipients suggests that the special attention focused on this group has borne some fruit.[14]

Figure 2.4 shows that the number of welfare entries is declining at all sites in the 1990s. The decline is smallest for Atlanta, Baltimore, and Kansas City, which show 20–30 percent decreases in the number of entries over the period for which we have data. The decline is more than 40 percent for Chicago, Houston, and Fort Lauderdale. Notwithstanding important variation across sites, it is clear that changes in both entry and exit rates contributed to the marked decline in the welfare caseload at all sites.

Figure 2.4 Number Entering Welfare (4-quarter moving average)

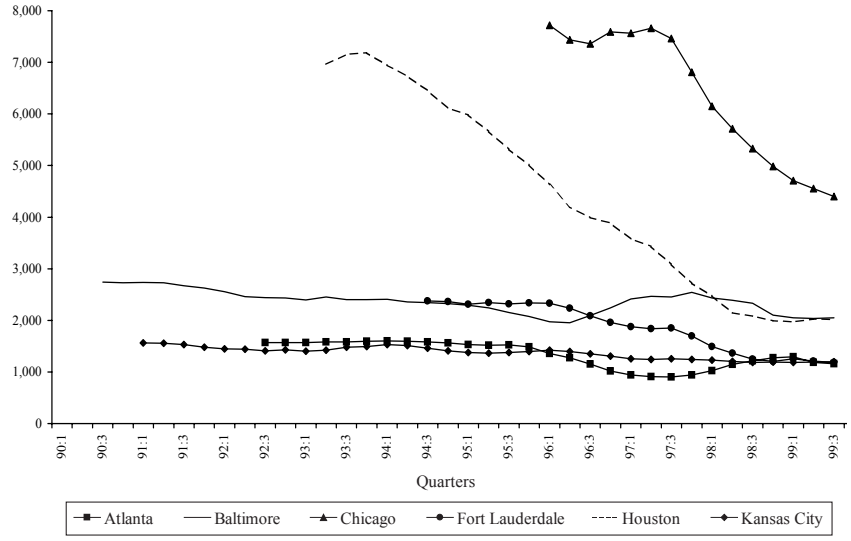
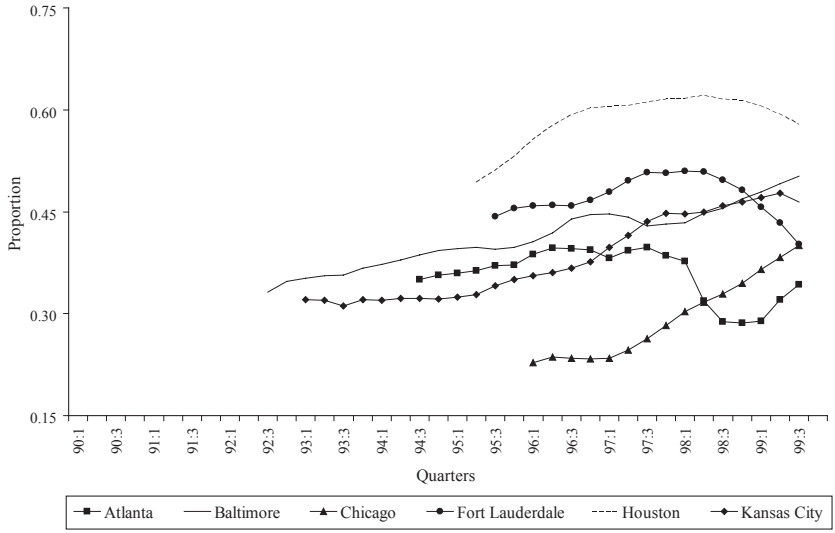


Figure 2.5 Proportion of Entries Who Received Welfare in the Previous Two Years (4-quarter moving average)



Recidivism

One concern is that many of those leaving the welfare rolls following reform may be returning within relatively short periods. In the presence of a booming economy, one would expect that people entering welfare would be those who have the most difficulty finding jobs, including those on welfare sometime in the recent past. As a result, the number of reentrants as a percentage of all individuals entering welfare should rise. On the other hand, once time limits on lifetime receipt of welfare take effect, prior recipients might be less likely to enter welfare, either because they have exceeded their allowable time or because they wish to "bank" remaining eligibility.[15]

Figure 2.5 shows the proportion of those entering welfare who had received welfare at some point in the prior two years for each site. Through the mid 1990s, the trend is clearly positive at all sites, but in the most recent two years trends clearly differ. In Chicago, increases in the proportion of repeat recipients continue in the last two years of our data. Although the trend in Baltimore is more variable and in Kansas City is less strong, the share of repeat recipients continues to increase in

these sites in the last two years. The trend over the last two years for Atlanta is irregular, while Houston and Fort Lauderdale reverse the earlier trend, showing a moderate decline in the share of recidivists.

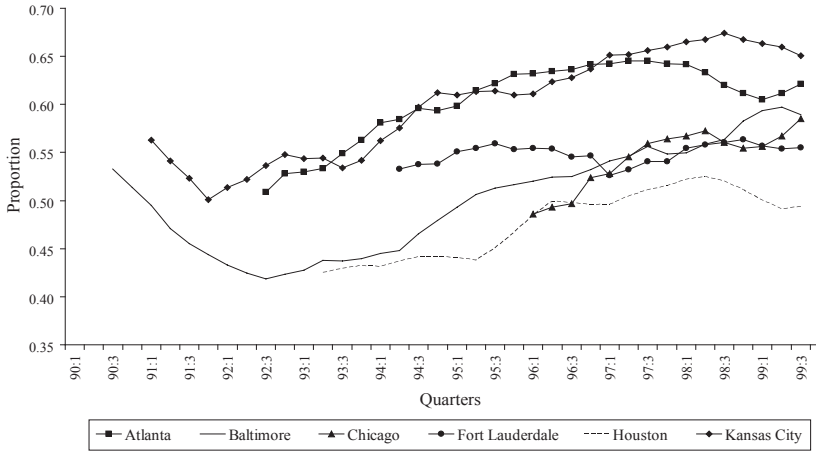
It is useful to recognize that the number of those entering welfare declined quite dramatically over this period, while the number of individuals with prior welfare history may have increased. Hence, return recipients could become more important even if the chance of returning to welfare for those leaving did not increase. In fact, Carrington, Mueser, and Troske (2002) show that in Missouri, the chance that an individual will return to welfare declined between the early 1990s and the late 1990s, despite an apparent increase in the relative importance of returns.

WELFARE-TO-WORK TRANSITIONS

The focus on work as an alternative to welfare is a striking element of the reforms of the past decade. However, it is not clear whether employment drives caseload declines or merely represents a response to new constraints placed on recipients. Do exits from welfare reflect increasingly attractive employment opportunities for welfare recipients in a strong economy, pulling them into the labor market? Or have welfare reforms operated primarily by pushing individuals from the rolls with little regard for their employment prospects? Of course, these explanations are not mutually exclusive or fully distinct conceptually, and determining their relative importance is not easy in practice. Since sanctions are a part of many programs designed to encourage and support employment, successful job programs may rely on coercion to some degree. Equally important, even if individuals are forced from the welfare rolls with little support and poor labor market prospects, a portion would doubtless obtain employment anyway.

Employment rates—and their changes—for individuals leaving welfare give an important indication of the role played by employment in recent caseload declines. Figure 2.6 presents trends in the number of recipients who discontinue a welfare spell and are employed or become employed, as a proportion of those exiting the welfare rolls.[16] Prior to 1997, this proportion is highest in Atlanta and Kansas City. In all of the sites except Fort Lauderdale, the proportion has increased until the last

Figure 2.6 Employment Rates for Welfare Leavers (4-quarter moving average)



two years of our data (i.e., 1998–1999). We observe a decline in the employment levels in Atlanta and Houston in this period, while in the other sites we see that there is little change or continued growth.

These trends are consistent with the view that through most of the 1990s, individuals have been attracted to or deflected from welfare by employment opportunities. Or, if individuals were forced to leave welfare, their efforts to obtain jobs have been at least somewhat successful.

One may wonder whether differences in rates of employment across sites reflect differences in economic growth. In Figure 2.1, we see that, by the mid 1990s, unemployment rates in Fort Lauderdale are similar to those in most of the other sites, so the failure of employment rates to increase there does not appear to be due to labor market conditions. Similarly, we do not see evidence of an economic downturn in Atlanta associated with the decline in employment of welfare leavers we observe there. Thus, it seems unlikely that differences in labor market conditions are the primary reason for variation across sites in the employment rates of those leaving welfare.

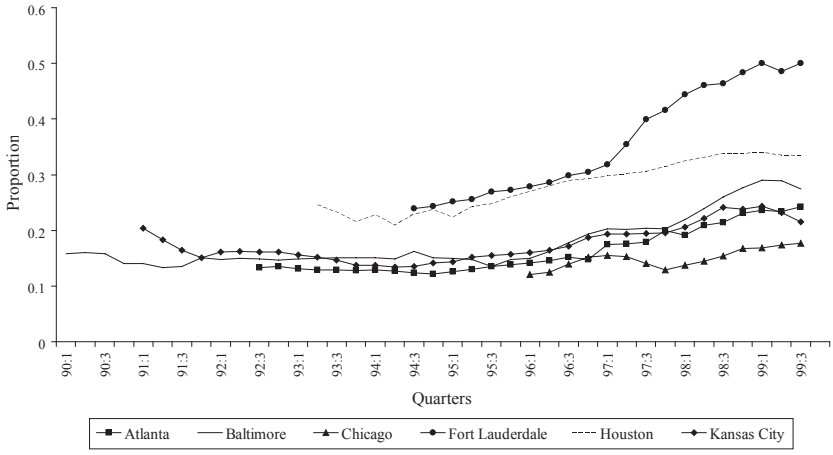
Returning to the patterns observed in the welfare caseloads, it would appear that the dramatic declines in the Fort Lauderdale and Houston welfare caseloads reflect increasingly stringent standards rather than

improved job opportunities. Welfare reforms in Florida are of clear importance, and major changes in the administration of the program in Texas may have been critical both in forcing recipients from the welfare rolls and in discouraging new applicants.[17] The patterns we observed in both these sites do not suggest that these administrative changes reduced the welfare caseload by helping recipients access better job opportunities.

Looking across all our sites, the fact that employment rates for welfare leavers did not decline consistently at a time of dramatic caseload declines may well be viewed in positive terms. Atlanta appears to be the only site where welfare policy changes have clearly led to the departure of recipients who had serious difficulty obtaining jobs. The appreciable decline in welfare leavers' employment rates corresponds to implementation of TANF and state welfare reforms.

Another comparison may be useful in gauging the extent to which welfare exits are a function of labor market opportunities. Figure 2.7 reports the proportion of individuals who leave welfare among those who remain employed or become employed.[18] There appears to have been a substantial increase in this proportion at all sites. In Houston and Fort Lauderdale, nearly 35 percent and 50 percent of employed individuals, respectively, exit welfare in a given quarter. This contrasts

Figure 2.7 Welfare Exit Rates for Employed Recipients (4-quarter moving average)

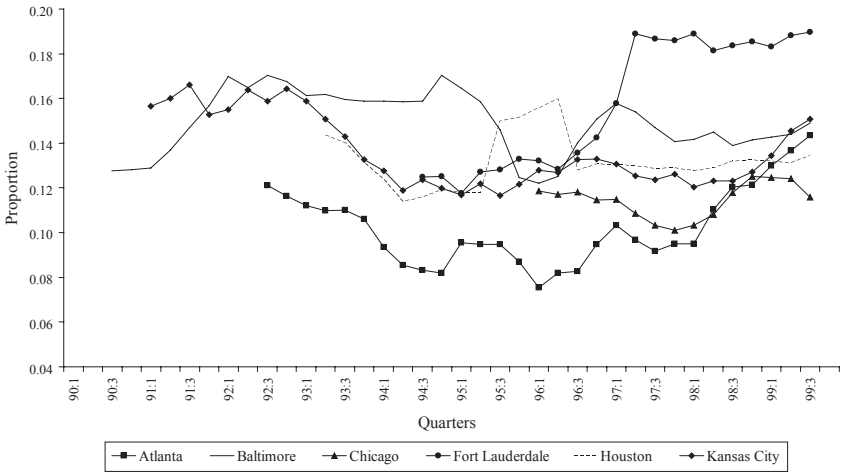


with employed exit rates at the other sites that are generally in the range of 17–30 percent. Of course, these exits from welfare reflect in part the fact that anyone who obtains a job that pays well enough will become ineligible for welfare. For Houston, the initial eligibility threshold is low compared to the other sites (see Table 2.5), so only low levels of earnings are consistent with welfare receipt, and the maximum benefits are also low, so retaining eligibility is of relatively little value. Such factors do not explain the very high departure rates in Fort Lauderdale, since, if anything, the rules would imply that welfare is compatible with higher earnings than in Houston. Rather, it appears clear that strictly enforced time limits are of critical importance in Fort Lauderdale.

One additional measure provides a rough sense of the extent to which individuals who obtain jobs are nonetheless being forced off of welfare. Among those who left welfare and were employed, a portion received no earnings in the last quarter in which they received welfare payments. These individuals were likely to experience a "support gap" between the time they discontinued welfare and the time they began to receive earnings from employment.[19] If such individuals are a growing share of those exiting welfare with jobs, this suggests that shifts in the stringency of welfare standards are forcing people off welfare and into employment. In contrast, if this proportion is not growing, it suggests that most of those who are employed may have found employment opportunities more attractive than cash assistance.

Figure 2.8 shows the proportion who are likely to have experienced a support gap, among those leaving welfare who have jobs in the following quarter. Although the measure is not altogether stable, it is clear there are some initial differences, with levels higher in Baltimore and lower in Atlanta. In the last two years (1998–1999), the proportion experiencing a support gap increased dramatically in Fort Lauderdale and modestly in Atlanta. This supports the view that employment rates for welfare leavers in those sites reflect, at least in part, an increase in the number of individuals who are pushed off of welfare yet who ultimately find jobs. For the other sites, the proportions show very little trend. Even as exits from welfare accelerated, these sites continued to facilitate relatively successful welfare-to-work transitions for those leaving welfare.

Figure 2.8 Proportion of Employed Exits with Support Gap (4-quarter moving average)



CONCLUSIONS

The relationships identified here provide a window into the dynamics underlying the dramatic decline in welfare caseloads over the 1990s. On one hand, the public policy emphasis on moving welfare recipients into jobs is reflected in the observed trends, with an increasing number of those who leave welfare reporting earnings in the quarter following their exit from welfare. On the other hand, it is clear that employment is not the only path off of welfare. A large portion of those who leave welfare, some 35–50 percent in 1999, do not appear to obtain jobs at all. Although our earnings data miss certain kinds of employment, including employment outside the state, such employment clearly accounts for only a small share of those leaving welfare.[20]

The impact of state policies is clearly reflected in the patterns of welfare receipt and employment that we observe. There is little doubt that Florida's time limits were important in inducing the dramatic movements off of the rolls, with long-term residents leaving at remarkably high rates. Atlanta's implementation of TANF appears to have signaled a major shift in state policy, and, although caseload declines are not as great there, the statistics show the impact on employment of a much

harsher set of policies than had been in place earlier in the 1990s. The results in both these sites suggest that the goal of assuring that former recipients obtain employment has suffered as policies that stress caseload reduction are implemented.

Nonetheless, looking across all sites, although TANF implementation and related federal waiver programs in our sites induced more dramatic declines in the caseload than prior policy changes, the employment position of leavers does not appear to have suffered much. The caseload declines were undoubtedly facilitated by the continued strong economy, but it is clear that policy changes played a primary role.

We suspect that the paths by which new policy affected recipient experiences and behaviors may be partly indirect. New regulations, as specified by both federal and state legislation, are implemented by agency employees whose understanding of policy is structured by the general social context and local imperatives. There is evidence that often the rules themselves are communicated to recipients as vague concepts, and that their impact may stem in part from the general sense they convey.[21] Given the national media focus on welfare reform, recipients and prospective recipients may be responding initially not so much to specific rule changes as to a general message that their options within the welfare system are receding. The general expectation now is that welfare is temporary and that work is expected for just about all mothers, regardless of the ages and numbers of their children, their education, and their work experience.

Notes

1. The possibility that an increase in the minimum wage might substantially reduce employment opportunities for low-skilled workers is hotly debated among economists but is not reflected in the views of the median voter in Congress.
2. The EITC has been referred to as "the most effective safety net program for children in working poor families" (Center on Budget and Policy Priorities 1998).
3. Yates (1997) discusses child support enforcement and welfare reform.
4. The principal authors of this book served as field researchers for Texas and Missouri, respectively.
5. An extended literature uses data across states to examine the relative importance of economic growth versus policy changes in explaining the variation in the welfare caseload over time (see Mayer 2000 for a review). Studies of particular interest focusing on the period prior to the 1990s include Peskin (1993) and

Black, McKinnish, and Sanders (2003). Studies examining the 1990s include Blank (1997); Council of Economic Advisors (1997, 1999); Martini and Wiseman (1997); Figlio and Ziliak (1999); Moffitt (1999); Wallace and Blank (1999); Bartik and Eberts (1999); Mueser et al. (2000); Ribar (2000); Ziliak et al. (2000); and Blank (2001). Blank (2002) reviews this literature.

6. In Houston, the comparable benefit increased by $13 in October 1999.
7. Rich (1999) and his colleagues at Emory University provide a detailed look at welfare reform in Georgia, examining events at the state level (pp. 19–29) and in the Atlanta area (pp. 29–36). Rich is the Georgia Field Associate for the Rockefeller Institute's State Capacity Study.
8. Our ADARE project colleague John Baj of Northern Illinois University's Center for Governmental Studies articulated this phenomenon and has also been helpful in enhancing our understanding of welfare reforms in Illinois.
9. For a summary of Texas' welfare reforms, see Schexnayder (2003).
10. In all the figures that follow, except for Figure 2.2, we present four-quarter moving averages. In each case, the moving average is indexed by the third quarter in the year.
11. Details of the measures used here are provided in the appendix.
12. See Lee, Goerge, and Dilts (2000). If the trend for Chicago's caseload corresponds to that for Illinois, the data they present suggest that the actual peak may be about 10 percent above our initial caseload and that it occurred at some point in 1994.
13. These patterns are hidden in the graphs due to the use of moving averages.
14. We also performed preliminary analyses using a five-year definition for long-term recipients. Although rates of departure are lower, the general patterns are similar. We use the two-year definition because, for most of our sites, a five-year definition reduces the number of years for which we can report results.
15. Blank (2002) and Grogger and Michalopoulos (2003) provide excellent discussions of this phenomenon.
16. Of those who receive welfare in quarter t but not $t + 1$, this is the proportion who are employed in quarter $t + 1$. These definitions are further explained in the appendix.
17. It should be noted that the most important changes in the formal policy governing Houston's welfare system did not take effect until late 1997. But the "mood" was certainly changing in prior years, as signaled by the administrative milestones shown in Table 2.4.
18. In Figure 2.7, the population at risk is all those meeting our criteria who are welfare recipients at time t and are employed at $t + 1$. The proportion of these who are not receiving welfare payments in time $t + 1$ is the welfare exit rate for this group.
19. These individuals are defined as those receiving welfare in quarter t but not in $t + 1$, and employed in quarter $t + 1$ but not t, and so the definition excludes any individual whose employment and welfare receipt overlap. Such an individual would not experience a support gap if she received the last welfare check in the third month of one quarter and started a job immediately at the start of the next

quarter. It should also be recognized that we have no measure of income from other household members, so many individuals may receive other support as well. Despite these limitations, the measure may still provide a valid indicator of changes in the number of those whom we count as employed but who do not have continuous support when they exit welfare.

20. Welfare mothers are far less mobile geographically than are their noncustodial parent counterparts (see O'Shea et al. 2001 and Schexnayder et al. 2001).
21. See the research on frontline welfare workers by Meyers, Glaser, and MacDonald (1998), for example.

3
The Determinants of Welfare Exits and Employment

(Coauthored with Julie Hotchkiss)

The success of welfare reform is largely a function of whether aid recipients 1) leave welfare, and 2) obtain employment. This chapter explores the dynamics of and the relationship between these outcomes for welfare recipients at our sites. In addition, we examine the role of demographics in the interaction of leaving welfare and employment. A desire to use limited resources as efficiently as possible has driven some states to target services where they have been found to be most effective (Eberts 1997). A key element of this targeting effort is to identify those who are most in need and who benefit most from particular services; the use of demographics as determinants of the outcomes examined moves us closer to that goal. Finally, since the typical expectation is that positive employment outcomes lead to increased personal earnings, we investigate both short-term and longer-term earnings outcomes. The earnings analysis serves as a precursor to the following chapter, which focuses on job stability and earnings.

One strand of the welfare-to-work literature examines the importance of administrative and economic factors in reducing caseloads and/or increasing the incidence of leaving. A second strand focuses on the effect of administrative and economic factors on increasing employment among welfare recipients. These studies typically find that administrative changes in the welfare system have had a discernible and important effect on getting people off of welfare and/or into employment. While these studies make it clear that administrative policies can be manipulated to facilitate both exit from welfare and employment of recipients, independently, it is not clear from the literature which efforts lead to the most effective attainment of leaving welfare and being employed. An understanding of the factors associated with each of these outcomes is crucial for targeting policy or structuring programs for greater effectiveness.

This chapter begins by examining the importance of demographic and family characteristics in predicting welfare leaving and employment rates. The success (or lack thereof) of certain, identifiable groups of welfare recipients can help policymakers target resources toward those groups. On the other hand, if individual demographics play but a minor role in the actual flow between welfare and work (i.e., time trends and unobservables guide the process more than individual characteristics), then policies and resources focused on providing for an accommodating economy (i.e., available jobs) may make the most sense. We also consider how employment and welfare exits are related, and the degree to which each of these outcomes appears to facilitate the other. The chapter then explores the importance of changes in cohort characteristics and changes in policy regimes in accounting for the observed trends in welfare leaving and employment. A bridge to the next chapter is made with an initial look at the quality of employment outcomes for recipients, both currently and over longer time periods.

Our data pertain to female-headed AFDC/TANF cash assistance cases in the central counties in the Atlanta, Baltimore, Chicago, Fort Lauderdale, Houston, and Kansas City metropolitan areas, as described in Chapter 2. Table 3.1 presents statistics from each site for the variables used in the analysis of this chapter. The units of analysis are quarters spent receiving AFDC or TANF.[1] Over all quarters, the percent leaving welfare in any quarter ranges from an average of 10 percent (Atlanta) to 24 percent (Fort Lauderdale). Average employment rates range from 27 to 44 percent. The average age and number of children are fairly consistent across sites. The degree of minority representation varies quite a bit (from 69 percent in Kansas City to 96 percent in Atlanta), reflecting the populations of those areas.[2] We also see that the proportion of recipients who have received aid for the prior eight quarters differs dramatically across sites, from a low of 21 percent in Fort Lauderdale to a high of 63 percent in Chicago.[3] Since a recipient will appear in our data for each quarter in which she receives welfare, the number of unique individuals is smaller than the total number of observations.

Table 3.1 Sample Statistics for Recipients

Variable		Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Leave = 1	Mean	0.10	0.11	0.11	0.24	0.18	0.13
Employment = 1	Mean	0.37	0.27	0.38	0.38	0.29	0.44
Age	Mean	31.6	32.0	31.0	31.0	32.7	30.9
		(9.8)	(9.4)	(8.2)	(8.1)	(9.1)	(9.3)
Number of children	Mean	2.14	1.92	2.22	2.09	2.08	1.98
		(1.29)	(1.17)	(1.36)	(1.23)	(1.23)	(1.19)
Minority = 1	Mean	0.96	0.89	0.90	0.76	0.84	0.69
Longterm = 1 (received welfare for the preceding 8 quarters)	Mean	0.59	0.61	0.63	0.21	0.45	0.46
Real earnings ($) (if > 0)	Mean	1,190	1,260	1,862	1,761	1,116	1,864
		(1,297)	(1,384)	(1,803)	(1,638)	(1,263)	(2,038)
Number of observations		393,412	615,226	826,115	214,939	621,485	264,705
Number of unique individuals		40,597	57,709	141,785	40,865	87,970	30,817
Years of data available for analysis		1994:1–1999:3	1994:1–1999:3	1997:3–1999:4	1994:1–1999:3[a]	1994:4–1999:3	1994:1–1999:3

NOTE: Standard deviation in parentheses.

[a] Excludes 1995:3 and 1997:2, for which data were missing.

LEAVING WELFARE AND EMPLOYMENT: THE IMPORTANCE OF DEMOGRAPHICS

The previous chapter examined welfare exit and employment rates across time for each site. The analysis suggested that policy changes associated with welfare reform played an important role in getting women off of welfare. This chapter extends the analysis of the previous chapter by making use of individual demographic characteristics to better understand the dynamics of movement between welfare and employment. It is of particular policy importance to determine whether certain groups of recipients have different experiences in these transitions in order to focus more resources on those who might find the transition from welfare to work systematically more difficult. This section explores the question of how important individual characteristics are in explaining the experience of welfare exits and employment.[4]

Empirical Specification

The incidences of leaving welfare ($L = 1$) and securing employment ($E = 1$) are estimated using simple univariate probit models for each site both with and without individual demographic covariates.[5] This allows us to determine how much knowing about the demographic characteristics of individual welfare recipients can help determine their probability of leaving and becoming employed.

Empirical Results and Implications

Table 3.2 contains coefficient estimates based on the probit equations. Table 3.2 also reports the value of the log-likelihood function obtained from estimating the equations without demographic regressors.[6]

From a statistical perspective, the demographic coefficients show that these factors are useful in predicting which recipients will leave welfare and obtain employment.[7] Focusing on the determination of leaving welfare, we find some consistent results across geographic sites.[8] In general, older, nonminority recipients are significantly more likely to leave welfare and those with more children are less likely to leave.[9] These results are consistent with others' findings (for example, see King et al. 1991; Wallace 2000; and Ribar 2000). When we translate

the coefficients into marginal effects on the probability of exit, they are of modest size. In Fort Lauderdale, where the exit probability averages around 0.24, the predicted probability is about 0.03 higher at age 35 than age 25. Effects at other sites are smaller but are comparable when viewed relative to their lower exit rates. Estimated effects of race are similarly modest, with minorities having lower exit rates.

A recipient who has been receiving welfare for at least eight consecutive quarters (Longterm = 1) has a lower probability of leaving welfare. In practical terms, this effect is substantial. In Fort Lauderdale and Houston, long-term recipients have a chance of exiting welfare that is about 10 percentage points below others, while the comparable figure for other sites varies from 4 to 7 percentage points. In each case, the increment is between a third and a half of the mean exit rate. This lower probability of leaving for those who have been receiving welfare for some time could be due to unmeasured heterogeneity, perhaps tied to the influences of neighborhoods or peers (see, for example, Osterman 1991). It could also result from positive duration dependence among welfare recipients, in which time on welfare actually increases the difficulty of exiting (see Chapter 3 of Bane and Ellwood 1994).

Turning to the probability of employment, we find that minority recipients are more likely to be employed across all geographic sites. Differences are as large as 16 percentage points. This may be related to the fact that minorities are more likely—given other characteristics—to receive welfare for extended periods of time and so may be employed during their periods of recipiency. Further evidence on differential employment by race for welfare recipients can be found in Heinrich et al. (forthcoming). Older recipients are more likely to be employed in Houston but are less likely to be employed in the other sites. We also observe that having more children lowers the probability of employment. The decline in probability ranges across our sites from half a percentage point to two percentage points for each child. While the effect of other demographic characteristics on employment is not entirely consistent in the literature, the negative effect of children is robust across numerous studies (for example, see King et al. 1991; Ribar 2000; Wallace 2000; and Eberts 1997).

Both economic theory and common sense suggest that work and welfare participation decisions are made simultaneously, with a variety of measured and unmeasured factors influencing both. We do not have

Table 3.2 Probit Results for the Probability of Leaving and the Probability of Employment

	Atlanta		Baltimore		Chicago		Fort Lauderdale		Houston		Kansas City	
Variable	Leaving	Employ	Leaving	Employ	Leaving	Employ	Leaving	Employ	Leaving	Employ	Leaving	Employ
Intercept	−1.891	−0.210	−1.536	−0.720	−1.657	−1.505	−1.494	−0.675	−1.173	−0.242	−1.793	−0.318
Age	0.058	−0.009	0.022	−0.012	0.046	0.076	0.043	0.028	0.026	−0.014	0.048	−0.001
Age squared[a]	−0.863	−0.073	−0.256	0.145	−0.559	−1.308	−0.541	−0.705	−0.446	0.361	−0.683	−0.133
Number of children	−0.069	−0.034	−0.065	−0.038	−0.127	−0.012	−0.075	−0.024	−0.037	−0.012	−0.042	−0.056
Minority = 1	−0.133	0.241	−0.142	0.301	−0.165	0.120	−0.100	0.241	−0.089	0.106	−0.177	0.410
Longterm = 1	−0.285	−0.301	−0.210	−0.267	−0.245	−0.008	−0.349	−0.262	−0.376	−0.310	−0.305	−0.254
Log-likelihood												
Full model	−126,820	−252,696	−203,832	−350,693	−282,333	−542,824	−112,298	−139,263	−281,859	−356,397	−99,520	−175,621
Time only	−129,383	−258,828	−206,076	−355,751	−289,288	−547,630	−114,159	−142,447	−288,629	−362,593	−1,011,585	−180,830
Intercept only	−131,870	−259,813	−212,951	−359,428	−292,206	−549,888	−118,441	−143,072	−290,230	−363,502	−102,513	−181,656
Likelihood ratio index	0.04	0.03	0.04	0.02	0.03	0.01	0.05	0.03	0.03	0.02	0.03	0.03
Total observations	393,412		615,226		826,115		214,939		621,485		264,705	
Unique individuals	40,597		57,709		141,785		40,865		87,970		30,817	

NOTE: Parameter coefficients reported are from a model that includes a time dummy variable for all but one quarter represented in the data for each site.
[a] Coefficient multiplied by 1,000.

sufficient information to estimate direct effects of one decision on the other, but we can expand our model to account for unmeasured factors that influence both. This estimation strategy is taken up in the next section.

JOINT LEAVING AND EMPLOYMENT DETERMINATION

If employment and welfare exit are indeed the two desired outcomes, policymakers have some choices about how to pursue them. In particular, there may be some choice about whether to channel resources to the leaving outcome ($L = 1$), expecting that employment will follow, or to the employment outcome ($E = 1$), expecting that leaving will follow. In order to evaluate which process might be most fruitful in achieving the most desired outcome ($L = 1$ and $E = 1$), the leaving and employment outcomes are modeled simultaneously since there are likely many factors that influence both outcomes. Through such a specification, not only can we determine which factors contribute positively to the most desired outcome ($L = 1$ and $E = 1$), we can also evaluate the conditional probabilities of employment given that the recipient leaves welfare ($E = 1|L = 1$) and the conditional probability of leaving welfare given that the recipient is employed ($L = 1|E = 1$).

Empirical Specification

The empirical specification of this model takes the form of a bivariate probit. The probabilities that person i leaves welfare and is employed in time period t are jointly determined by a set of demographic and time period–specific regressors.[10] The bivariate estimation strategy, which allows for correlation between the error terms in the two equations, is appropriate if there are unobserved factors that affect both the probability of leaving welfare and that of being employed. Maximum likelihood estimates will be used to predict how the probabilities of being in each, both, and neither of the two categories are affected by the different demographic variables. In addition, conditional probabilities are also easily calculated from the model.[11]

The experiment of interest is to determine how important leaving welfare is to the expected probability of employment,

$$\frac{\partial \Pr[E = 1]}{\partial L} = \Pr[E = 1] \mid L = 1] - \Pr[E = 1 \mid L = 0],$$

and how important being employed is to the expected probability of leaving welfare,

$$\frac{\partial \Pr[L = 1]}{\partial E} = \Pr[L = 1] \mid E = 1] - \Pr[L = 1 \mid E = 0].$$

These partial derivatives measure the difference in expected outcomes given the environment in which those outcomes are determined.

The model takes the leaving and employment outcomes as determined simultaneously, with neither directly influencing the other. Rather, events and factors in each recipient's life affect both outcomes simultaneously, which is reflected in the correlated error structure. The partial derivatives indicate how the probability of a particular outcome differs as the other outcome varies, based on inferences about how unmeasured factors are likely to differ. Of course, it is uncertain whether policies that affect one of these outcomes will in fact operate in this way, but the model is nonetheless a convenient way to summarize the empirical relationships.

Empirical Results and Implications

The bivariate probit model is estimated separately across each site. This procedure produced coefficient estimates that parallel those presented in Table 3.2, but the estimates differ because this model takes account of the correlations between the error terms. In fact, the two models produce estimates where differences are statistically significant, but there are no practical differences in estimated coefficients. In almost all cases, estimates differ by only a few percent, and in no case is the difference of any substantive importance. The implication is that, from a practical standpoint, the coefficients obtained from estimating the leaving and employment probabilities separately are not seriously

biased. We therefore do not present coefficient estimates for the bivariate probit.

Despite the similarities in estimated coefficients, the estimated correlation between the unobserved determinants of leaving welfare and of employment (σ_{EL}) is significantly different from zero, positive, and substantial for each site (see Table 3.3). The similarity in the correlation estimates across sites, varying from 0.28 to 0.36, is notable. The positive estimate of σ_{EL} indicates that unobserved factors that increase the probability of leaving welfare also increase the probability of employment. For example, a recipient who is more likely in any quarter to leave welfare for some unmeasured reason (e.g., high motivation) is also more likely to be employed in that quarter.

The parameter coefficients from estimation of the bivariate specifications are used to calculate the expected joint, conditional, and unconditional probabilities presented in Table 3.3. As noted above, although employment and leaving welfare are not modeled as having a causal relationship (they are modeled simultaneously), the importance of one outcome in the determination of the probability of the other outcome can be calculated through differences in conditional probabilities.

The partial derivatives reported at the bottom of Table 3.3 are interpreted as follows:

(a) Among recipients, how does the probability of leaving welfare differ between those who are employed and those who are not? Where unmeasured factors induce employment, we may interpret this as indicating the extent to which employment is associated with a change in the probability of leaving.

(b) Among recipients, how does the probability of being employed differ between those who are leaving welfare and those who are not? Where unmeasured factors induce welfare exit, we may interpret this as indicating the extent to which welfare exit is associated with a change in the probability of employment.

The partial derivatives of the leaving probability, line (a), indicate that an employed recipient is more likely to leave welfare than a nonemployed recipient, with the differential between 9 (Chicago) and 17 (Houston) percentage points. The implication is that employed recipients have a tremendous advantage over nonemployed recipients in leaving welfare. The importance of leaving welfare on the probability of being employed is reported in line (b) of Table 3.3. These figures indicate

Table 3.3 Predicted Probabilities and Partial Derivatives, Leaving and Employment

	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Error covariance						
$\mathbf{F}_{EL}$	0.350	0.356	0.277	0.289	0.360	0.297
Unconditional probabilities						
Pr[L=1]	0.10	0.11	0.11	0.24	0.18	0.13
Pr[E=1]	0.37	0.27	0.38	0.38	0.27	0.44
Pr[L=1,E=1]	0.07	0.06	0.06	0.12	0.09	0.08
Pr[L=1,E=0]	0.04	0.05	0.05	0.11	0.09	0.05
Pr[L=0,E=1]	0.31	0.21	0.32	0.25	0.19	0.36
Pr[L=0,E=0]	0.59	0.68	0.57	0.51	0.64	0.51
Conditional probabilities						
Pr[L=1|E=0]	0.06	0.08	0.08	0.18	0.13	0.09
Pr[L=1|E=1]	0.17	0.20	0.17	0.33	0.30	0.19
Pr[E=1|L=0]	0.34	0.24	0.36	0.34	0.23	0.41
Pr[E=1|L=1]	0.62	0.50	0.57	0.53	0.47	0.63

Partial derivatives							
(a)	$\Pr[L=1\|E=1] - \Pr[L=1\|E=0]$	0.11	0.12	0.09	0.14	0.17	0.10
	(As proportion of overall leaving probability)	(1.10)	(1.09)	(0.81)	(0.58)	(0.94)	(0.77)
(b)	$\Pr[E=1\|L=1] - \Pr[E=1\|L=0]$	0.28	0.26	0.21	0.19	0.24	0.22
	(As proportion of overall employment probability)	(0.76)	(0.96)	(0.55)	(0.50)	(0.89)	(0.50)

NOTE: Based on model with demographic controls estimated for entire sample period. Terms in parentheses are the difference above divided by the unconditional probability. For line (a), the divisor is $\Pr[L=1]$, and for line (b) the divisor is $\Pr[E=1]$. Probabilities reflect the expected probability for a recipient drawn at random.

that a recipient leaving welfare has from a 19- to a 28-percentage-point increment in probability of being employed relative to a nonleaving recipient. It is notable that the patterns across sites are similar, even though exit probabilities are much higher in Fort Lauderdale and Houston than in the other sites.

Certainly, these results show that employment and welfare exits are closely associated, and that achieving one of these outcomes brings a recipient a long way toward achieving the other. If one simply asks how one obtains the highest probability of exiting welfare and being employed, it is clear that, if we observe an exit for the average person, the chance is around 50 percent that the individual will also be employed ($\Pr[E = 1|L = 1]$). On the other hand, if one identifies employed individuals, the chance that such an individual also leaves welfare in a given quarter is less than a third, and as low as 17 percent in two sites ($\Pr[L = 1|E = 1]$). Since the welfare exit is the smaller probability event, achieving it moves one farther toward achieving the joint goal.

However, this comparison may understate the value of obtaining employment. Rather than looking at the simple probability, one may wish to apply an adjustment for the overall probability of each outcome. The figures in parentheses in line (a) divide the calculated differential for the chance of leaving by the overall probability of leaving. We see that, relative to the overall probability of leaving, the observed probability differences vary from about half to more than the full probability. The figures in line (b) show the differential for the chance of employment divided by the overall chance of employment. The percentage-point gain in chance of employment across all locations amounts to from 50 to 96 percent of the overall employment probability. By these relative measures, it appears that the effect of getting a job is more substantial than it would otherwise appear.

It is important to note that these results say nothing about the quality of employment. For example, a recent MDRC study shows that employment mandates without income supplements are not effective in improving overall family welfare (Morris et al. 2001).[12] This would certainly be expected if the job paid too little to disqualify a recipient for welfare or to bring a recipient above poverty levels. As a transition to the next chapter, which explores employment outcomes in more detail, the last part of this chapter takes an initial look at longer-range employment and earnings outcomes. We first turn to examining how

the process of obtaining employment and leaving welfare has changed over time.

LEAVING AND EMPLOYMENT DETERMINATION IN DIFFERENT TIME PERIODS

Much of the recent welfare-to-work literature has been focused on whether the welfare reform of 1996 has had any effect on welfare-to-work dynamics or outcomes (for example, Schoeni and Blank 2000; and Loprest 2001). We would like to see that the emphasis that welfare reform has on employment and training leads to better alternatives to welfare. Of course, given that our analyses of post-welfare reform outcomes are necessarily performed in an era of unprecedented economic expansion, it is difficult to identify the effects of welfare reform or related policy changes, particularly since the cohort of welfare recipients before and after reform may differ in systematic ways. In spite of this limitation, it is of interest to see whether we can identify any systematic differences in transition dynamics before and after welfare reform. The goal of this effort is to see whether there are any differential trends or effects of demographics in explaining leaving and employment behavior of welfare recipients.

Empirical Specification

Comparisons across pre- and postreform time periods will be made using simultaneous bivariate probit results obtained from reestimating the empirical model for three distinct time periods: prereform (1994–1995), mid-reform (1996–1997), and postreform (1998–1999). Each of these time periods is characterized by the demographic characteristics of welfare recipients and by the mechanism by which recipients transition out of welfare and into employment. This mechanism is determined by both the economy and the policy structure in place during the time period. Table 3.4 contains sample means for each site for the three time periods.

In general, across the three time periods, the recipient cohorts are becoming older, have more children, are more likely to be minority and are generally less likely to be long-term recipients.[13] Changes in these characteristics will affect leaving and employment in different ways.

Table 3.4 Sample Means for Three Time Periods: Welfare Recipients

	Atlanta			Baltimore			Chicago		
Variable	1994–1995	1996–1997	1998–1999	1994–1995	1996–1997	1998–1999	1994–1995	1996–1997	1998–1999
Leave = 1	0.08	0.11	0.16	0.08	0.11	0.20	—	0.08	0.12
	(0.26)	(0.31)	(0.36)	(0.27)	(0.31)	(0.40)		(0.28)	(0.33)
Employment = 1	0.34	0.39	0.40	0.23	0.29	0.34	—	0.35	0.39
	(0.48)	(0.49)	(0.49)	(0.42)	(0.45)	(0.47)		(0.48)	(0.49)
Age	30.87	31.68	33.02	31.52	32.59	32.00	—	30.94	30.98
	(9.32)	(9.78)	(10.81)	(9.29)	(9.83)	(8.56)		(8.20)	(8.21)
Number of children	2.11	2.15	2.18	1.91	1.92	1.97	—	2.16	2.25
	(1.26)	(1.31)	(1.34)	(1.14)	(1.16)	(1.24)		(1.33)	(1.37)
Minority = 1	0.96	0.96	0.97	0.88	0.89	0.91	—	0.89	0.90
	(0.20)	(0.19)	(0.18)	(0.32)	(0.31)	(0.29)		(0.32)	(0.30)
Long term = 1	0.59	0.62	0.56	0.62	0.63	0.55	—	0.63	0.63
	(0.49)	(0.48)	(0.50)	(0.49)	(0.48)	(0.50)		(0.48)	(0.48)
Total observations	171,662	141,599	80,151	282,258	226,295	106,673	—	215,261	610,854
Unique individuals	30,341	26,218	19,650	49,301	41,858	26,775	—	115,993	127,400

Variable	Fort Lauderdale			Houston			Kansas City		
	1994–1995	1996–1997	1998–1999	1994–1995	1996–1997	1998–1999	1994–1995	1996–1997	1998–1999
Leave = 1	—	0.26	0.41	0.15	0.19	0.22	0.10	0.14	0.17
		(0.44)	(0.49)	(0.36)	(0.39)	(0.41)	(0.31)	(0.34)	(0.38)
Employment = 1	—	0.38	0.44	0.29	0.29	0.31	0.42	0.46	0.46
		(0.47)	(0.50)	(0.45)	(0.45)	(0.46)	(0.49)	(0.50)	(0.50)
Age	—	30.84	31.46	31.62	32.74	34.85	30.58	30.91	31.55
		(8.11)	(8.43)	(9.02)	(8.98)	(9.20)	(8.96)	(9.28)	(9.89)
Number of children	—	2.09	2.19	2.00	2.09	2.20	1.98	1.98	2.00
		(1.22)	(1.32)	(1.17)	(1.23)	(1.31)	(1.17)	(1.19)	(1.21)
Minority = 1	—	0.77	0.80	0.84	0.84	0.84	0.67	0.69	0.72
		(0.42)	(0.40)	(0.37)	(0.37)	(0.37)	(0.47)	(0.46)	(0.45)
Long term = 1	—	0.32	0.20	0.41	0.47	0.46	0.47	0.48	0.44
		(0.47)	(0.40)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Total observations	—	78,309	27,846	247,534	262,395	111,556	108,572	94,941	61,192
Unique individuals	—	23,292	11,797	70,930	60,340	30,046	21,251	19,977	15,156

NOTE: In this analysis, the first quarter of data for Fort Lauderdale is 1996:1. The data for other sites are as listed in Table 3.1. — = data are unavailable. Standard deviations are in parentheses.

Based on the results in Table 3.2, an older cohort implies a greater chance of leaving welfare but lower employment, a greater proportion of minorities implies a harder time leaving but greater employment, more children implies decreases in both leaving and employment, and fewer long-term recipients implies a greater chance of both leaving and employment.

By estimating leaving and employment probabilities separately for each time period, we can simulate the differences across policy regimes, holding recipient characteristics constant. This allows us to answer the question, "How much of the change in employment probabilities can be accounted for by changes in the policy and economic regime?"[14] In addition, a comparison of cohorts can be made by evaluating the probabilities of employment and leaving within one regime but for the different (1994–1995 versus 1998–1999) cohorts.[15]

Using the calculated probabilities, the overall changes in employment and leaving probabilities can be "decomposed" into a portion that is accounted for by differences in cohort characteristics (differences between $X_{94\text{-}95}$ and $X_{98\text{-}99}$) and by differences in regimes (differences between $\beta_{94\text{-}95}$ and $\beta_{98\text{-}99}$).

Empirical Results and Implications

Table 3.5 contains predicted leaving and employment probabilities based on coefficients for models estimated separately by time period. These analyses provide information on the role of recipient characteristics and regime change in explaining variation over time in employment and leaving probabilities. Chicago and Fort Lauderdale are omitted, since information for 1994–1995 is not available for those sites.

The probability differences in lines (i) and (iv) confirm results presented earlier, showing that postreform recipients have higher probabilities of both leaving and being employed than prereform recipients. These increases in probability for leaving range from 7 percentage points in Houston and Kansas City to 12 percentage points in Baltimore. Increases in employment probabilities range from 4 percentage points in Kansas City to 11 percentage points in Baltimore. These gross probability increases result both from changes in cohort characteristics, and from changes in policy structure and economic environment ("regime").

Table 3.5 Predicted Probabilities of Leaving Welfare and Employment across Regimes and Cohorts

	Atlanta	Baltimore	Houston	Kansas City
Probability of leaving				
(a) Pr $L_{94\text{-}95}\|X_{94\text{-}95}$	0.072	0.073	0.141	0.099
(b) Pr $L_{98\text{-}99}\|X_{98\text{-}99}$	0.153	0.191	0.209	0.165
(c) Pr $L_{94\text{-}95}\|X_{98\text{-}99}$	0.071	0.076	0.135	0.098
(d) Pr $L_{98\text{-}99}\|X_{94\text{-}95}$	0.161	0.191	0.225	0.167
i. Gross change in leaving probability (b) − (a)	0.081	0.118	0.068	0.066
ii. Change in probability accounted for by differences in cohort characteristics (c) − (a)	−0.001	0.003	−0.006	−0.000
	−1%	2%	−9%	0%
iii. Change in probability accounted for by differences in regimes (b) − (c)	0.082	0.115	0.074	0.066
	101%	98%	109%	100%
Probability of employment				
(e) Pr($E_{94\text{-}95}\|X_{94\text{-}95}$)	0.340	0.225	0.242	0.413
(f) Pr($E_{98\text{-}99}\|X_{98\text{-}99}$)	0.396	0.332	0.287	0.455
(g) Pr($E_{94\text{-}95}\|X_{98\text{-}99}$)	0.332	0.233	0.229	0.418
(h) Pr($E_{98\text{-}99}\|X_{94\text{-}95}$)	0.403	0.330	0.307	0.450

Table 3.5 (continued)

	Atlanta	Baltimore	Houston	Kansas City
iv. Gross change in employment probability (f) − (e)	0.056	0.107	0.045	0.041
v. Change in probability accounted for by differences in cohort characteristics (g) − (e)	−0.007	0.008	−0.013	0.005
	−13%	7%	−29%	13%
vi. Change in probability accounted for by differences in regimes (f) − (g)	0.064	0.099	0.058	0.036
	113%	93%	129%	87%

NOTE: Reported percentages use the gross change as the base.

Decomposing the changes in leaving probabilities indicates that in two of the sites, cohort characteristic changes would lead to declines in the probabilities of leaving. In contrast, in Baltimore, cohort characteristics are changing in such a way as to make it easier for recipients to leave, whereas characteristics have essentially no effect in Kansas City. In all sites, the effect of characteristics is very small. The overwhelming bulk of the increase in leaving probability can be accounted for by differences in regime (line [iii]).

Decomposing the changes in employment probabilities indicates that cohort characteristic changes reduce employment probabilities in two of the sites and increase employment in the other two. In either case, the effects are modest (line [v]). As in the case of the leaving probability, the change in regime again must take most of the credit in the employment probability increases.

The number of demographic variables available in this analysis is clearly limited. Most notably, measures of education, labor market experience, marital status, and health are missing; these are either not available or not reliably available from the different state agencies during this time period. Our results might differ if such measures were included. However, given the small effect of available measures and the fact that unmeasured factors are expected to be at least weakly associated with measured factors, we doubt that our main conclusions would be altered.

LONG-TERM EMPLOYMENT DETERMINATION

In policy formation or in targeting resources, it is often necessary to measure success in terms of longer-term outcomes. Knowing the relative importance of different demographic characteristics in determining immediate versus future employment outcomes could be useful in targeting resources for long-term success. This section focuses on employment outcomes eight quarters after the current quarter. Leaving is still defined relative to the current quarter.

Empirical Specification

This section estimates the joint leaving and employment bivariate model following the same structure as the previous models, except that E^* is the employment outcome eight quarters into the future ($t + 8$). The sample consists of all recipients at time t. The questions addressed by this specification are 1) how the probability of employment in eight quarters is influenced by demographic and time variables, and 2) how this effect differs from the results presented earlier pertaining to the current quarter. As above, the bivariate specification allows for unmeasured factors to influence both outcomes. Estimates obtained are comparable to those presented in Table 3.3, but since employment is measured after the quarter in which the recipient may leave welfare, our conditional measures focus on the effect of exit on employment.

Empirical Results and Implications

Table 3.6 contains the predicted probabilities of being employed eight quarters out, plus the employment probabilities conditional on current leaving status. Whether or not recipients leave welfare now, there is a substantial chance they will be employed in two years. Comparing the overall employment probabilities ($\Pr[E = 1]$) in Table 3.6 with those in Table 3.3, employment is more likely over the long term than in the current quarter. Figures in the bottom two lines of Table 3.6 are much smaller than comparable measures reported in Table 3.3, implying that the importance of leaving for employment outcomes diminishes over the long term. In other words, leaving welfare this quarter is more important in employment determination this quarter than in employment determination eight quarters in the future. Of course, many of those who do not leave in the current quarter will leave sometime in the next two years. It should be recognized that the employment probabilities remain in the range of 50 percent, so it is clear that forcing recipients off welfare does not necessarily mean they will eventually find employment.

Table 3.6 Predicted Probabilities and Partial Derivatives, Current Leaving and Employment Eight Quarters Out

	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Unconditional probabilities						
Pr[L=1]	0.09	0.09	0.08	0.21	0.17	0.12
Pr[E=1]	0.37	0.41	0.51	0.45	0.42	0.54
Pr[L=1,E=1]	0.06	0.05	0.04	0.11	0.08	0.07
Pr[L=1,E=0]	0.03	0.04	0.04	0.11	0.08	0.05
Pr[L=0,E=1]	0.31	0.36	0.46	0.35	0.33	0.47
Pr[L=0,E=0]	0.60	0.55	0.45	0.44	0.50	0.42
Conditional employment probabilities						
Pr[E=1|L=0]	0.47	0.40	0.50	0.44	0.40	0.53
Pr[E=1|L=1]	0.56	0.50	0.55	0.49	0.49	0.59
Partial derivatives						
Pr[E=1|L=1] − Pr[E=1|L=0]	0.09	0.10	0.05	0.05	0.08	0.06
(As proportion of overall employment probability)	(0.24)	(0.24)	(0.10)	(0.12)	(0.19)	(0.11)

NOTE: Probabilities evaluated at sample means for each site separately.

SHORT-TERM AND LONG-TERM EARNINGS OUTCOMES

We are interested not only in the dynamics of welfare-to-work transitions but also in the type of job that a recipient obtains. A recent MDRC summary of welfare-to-work programs argues that insufficient attention is paid to the quality of welfare recipient and leaver jobs (Morris et al. 2001). This section examines the quarterly earnings of employed welfare recipients in a given quarter. It serves as a link with the analysis of the next two chapters, which expands on the quality of outcomes by looking in detail at the longevity and earnings of jobs obtained by welfare recipients.

Empirical Specification

We examine total quarterly earnings in the next quarter ($t + 1$) and total quarterly earnings eight quarters out ($t + 8$) for a sample of welfare recipients. Since many recipients at time t will not be employed at $t + 1$ or at $t + 8$, many observations have zero earnings. In order to account for this censoring of earnings at zero, and so that we might generalize the results to all welfare recipients (not just those who become employed), we use a tobit specification for earnings.[16]

Empirical Results and Implications

Table 3.7 contains the partial derivatives calculated at the sample means for each geographic location (see McDonald and Moffitt 1980). The middle panel of the table provides the predicted quarterly earnings for a recipient drawn at random ($E[W]$) and for an individual who has a job ($E[W|W > 0]$). The conclusion that the types of jobs welfare recipients find are fairly low-paying is consistent across sites, with the highest expected earnings in quarter $t + 1$ for those with a job in Kansas City ($2,176) and the lowest expected quarterly earnings in Houston ($1,245). The effect of age is not consistent across sites, with earnings at age 35 exceeding earnings at 25 for half of the sites, and the reverse at the others. Earnings differences by age are modest, never exceeding $70 per quarter. Minorities uniformly have higher quarterly earnings than whites. Having more children and being a long-term recipient lowers expected earnings across all sites.

The negative effect of having more children on earnings may suggest that alleviating recipients' child care needs could improve not only the incidence of employment but also its quality.

The right side of Table 3.7 presents results from estimating the equation for long-term earnings. The effect of demographic variables is generally magnified over time, so their importance is emphasized for long-term outcomes, although there are a fair number of exceptions. One partial exception is the effect of age. The difference between long-term earnings at age 25 and age 35 is negative in five of the six sites; overall, older workers experience greater earnings disadvantages two years out than initially.

Expected earnings are larger two years from time *t* than in the quarter just following *t,* largely due to increased rates of employment. If we look at those with jobs, increases in earnings are modest, which suggests that the observed employment is not providing real economic self-sufficiency for any but a small minority of recipients.

CONCLUSIONS

The purpose of this chapter has been to examine the relationship between leaving welfare and becoming employed and to explore the role that individual demographic characteristics play in this relationship. Determining which demographics are important in the dynamics of leaving welfare and obtaining employment helps to identify the most effective targets for scarce resources. For example, a consistent result across all specifications in this chapter, as well as across other cited research, is that women with more children have both lower exit rates and lower employment rates. This is clearly a call for assistance in integrating the needs of the children of welfare recipients to be cared for and the need for the recipient to get off of welfare. The effectiveness of providing child care subsidies on employment outcomes has recently been addressed by Blau and Tekin (2001), who found that the provision of child care subsidies significantly increases the probability that a welfare recipient obtains employment. The authors caution that tying eligibility for the child care subsidy to receipt of welfare appears to increase welfare participation.

Table 3.7 Partial Derivatives from Tobit Estimates of Model Predicting Real Quarterly Earnings

	Earnings ($) next quarter ($t+1$)					
Variable	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Intercept	−888.2	−472.5	−2,136.5	−1,180.1	−170.8	−796.4
Age	16.4	−4.3	117.3	62.4	−3.9	17.5
Age^2	−0.29	0.11	−1.88	−1.14	0.00	−0.18
Number of children	−80.2	−25.3	−7.8	−40.7	−10.2	-88.1
Minority = 1	341.9	174.7	125.9	237.5	56.0	419.5
Long term = 1	−579.2	−168.6	−77.3	−335.0	−193.2	−290.2
Expected earnings ($)						
E[W]	504	382	887	821	395	980
E[W|W>0]	1,299	1,287	2,163	1,968	1,245	2,176
Total observations	393,412	615,222	826,115	189,813	584,254	264,705
Unique individuals	40,597	57,709	141,785	40,053	85,862	30,817

Table 3.7 (continued)

	Earnings ($) eight quarters out ($t + 8$)					
Variable	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Intercept	−1235.0	−455.5	−1,967.9	−1,297.4	−317.4	−949.2
Age	67.3	8.0	150.2	81.9	16.0	52.2
Age^2	−1.24	−0.17	−2.61	−1.56	−0.38	−0.84
Number of children	−66.7	−30.9	−42.2	−67.5	−8.8	−87.5
Minority = 1	446.6	210.5	120.2	333.6	107.8	383.4
Long term = 1	−584.4	−211.6	−132.9	−298.7	−220.1	−318.0
Expected earnings ($)						
E[W]	753	633	1,511	1,301	681	1,452
E[W|W>0]	1,575	1,551	2,946	2,647	1,579	2,764
Total observations	313,261	508,503	215,261	172,480	494,404	203,513
Unique individuals	35,420	56,479	115,993	36,596	83,222	27,437

NOTE: Sample is welfare recipients at time t. Standard errors are not reported since parameter coefficients are highly significant with rare exception. Tobit estimation includes time dummies for quarters. Expected earnings are calculated for each person, then averaged over the sample at each geographic location.

Our results from the estimation of the bivariate model predicting both leaving welfare and becoming employed underscore the strong association between these two important policy outcomes, and they suggest that each outcome will likely facilitate the other. These results should be interpreted with an eye to the economic environment in which they were generated, however. While there was variation in economic activity across geographic regions of the country, all sites experienced sustained employment growth over the time period evaluated. The results might be quite different in a period when jobs are not as plentiful. In addition, this conclusion does not consider the costs associated with alternative strategies for reducing dependence or boosting employment. In looking at longer-term employment outcomes, we found that leaving welfare in the current quarter was strongly associated with employment eight quarters later, although the relationship was, as expected, diminished relative to that with current employment.

Despite the substantial effects that demographic factors have in explaining the probability of welfare exit and employment for particular recipients, changes in demographics are of little or no importance in explaining changes in exit and employment probabilities that occurred in the 1990s. The overwhelming bulk of the increases in both leaving and employment probabilities can be accounted for by changes in regime, which include both economic and policy changes occurring over the period of our study. Changes in cohort characteristics actually worked against these changes in some sites, tending to reduce leaving and employment probabilities, but these effects were substantively small and were overwhelmed by the effects of regime changes. All recipients appear to be more likely to leave in the more recent period.

As a preliminary look at the quality of employment outcomes, we explored the determinants of quarterly earnings. For those with jobs (not just a recipient drawn at random), the quality of employment is low, with quarterly earnings ranging from only $1,245 in Houston to $2,176 in Kansas City. Of course, looking just at the next quarter will typically underestimate the ultimate labor market success of recipients. Expected real quarterly earnings (of those with jobs) two years from the current quarter were higher than in the current quarter. The effects of individual characteristics in determining earnings are magnified over time, suggesting that the importance of addressing the difficulties

of certain groups of recipients (e.g., the young with children) is even greater the longer the time frame being considered.

Overall, the results from this chapter may be useful in telling us 1) on which recipients resources might best be focused, 2) whether policies should emphasize getting people off welfare or into jobs first, and 3) how important recipient characteristics versus the economic and policy environment are in determining post-welfare outcomes. Of course, decisions on resource allocation must consider the marginal effects of programs as well as their costs. However, if a program has similar effects and costs for different individuals, we might choose to focus efforts on those who are least likely to exit welfare and obtain jobs in the absence of intervention. By this standard, resources devoted to getting people off welfare should be targeted to young, minority welfare recipients with more children and those who have been on welfare for long periods of time. Resources devoted to getting people into jobs should be targeted to older recipients with children and those who have been on welfare a long time. Welfare exit and employment chances are closely associated, and efforts to facilitate one will likely facilitate the other.

Notes

1. The sample for analysis is the universe of all cases meeting our inclusion criteria in the given quarter but is slightly different than that used in Chapter 2 since we have omitted those cases for which data on relevant variables were not available.
2. "Minority" is a dummy variable set equal to one for any race other than white. Hispanics are coded as nonwhites.
3. Due to a data problem, the 21 percent figure may underestimate the proportion of recipients in Fort Lauderdale who have received aid for eight or more quarters. Although we have not been able to determine the extent of this problem, we can confirm that the actual percentage of long-term recipients is much lower in Fort Lauderdale than in the other sites—certainly less than 30 percent. This data problem has no effect on the substantive results we report.
4. The terminology "leaving welfare" and "welfare exit" are used interchangeably throughout the chapter.
5. A recipient is said to leave welfare during quarter t if she is receiving welfare in quarter t and is not receiving welfare in quarter $t + 1$, consistent with the definition used in Chapter 2. In contrast to the analysis in Chapter 2, which examines employment of welfare leavers in quarter $t + 1$, the current measure of employment refers to welfare recipients and applies to quarter t. Simple probit models

are estimated separately for each site as follows:

$$E^*_{it} = X_{it}\beta_E + \sum_{j=1}^{n}\gamma_{Ej}YRQTR_j + \varepsilon_{Eit}, \qquad E_{it} = 1 \text{ if } E^*_{it} > 0, \text{ 0 otherwise}$$

$$L^*_{it} = X_{it}\beta_L + \sum_{j=1}^{n}\gamma_{Lj}YRQTR_j + \varepsilon_{Lit}, \qquad L_{it} = 1 \text{ if } L^*_{it} > 0, \text{ 0 otherwise}$$

where E^*_{it} refers to the unobserved employment propensity of person *i* in quarter *t*, L^*_{it} refers to the unobserved leaving propensity of person *i* in quarter *t*, X_{it} is a vector with individual demographic characteristics, $YRQTR_j$ is a dummy variable equal to one for quarter *j*, *n* is the total number of quarters (minus one) available for analysis from a given site, β and γ are parameters to be estimated, and ε_{Eit} and ε_{Lit} are normally distributed random errors with means of zero and variances equal to one. One could choose to model the process of leaving welfare as a hazard. However, since our data are discrete, the differences between results produced by a probit model and a hazard model are small.

6. Since the number of unique recipients represents from 9 to 17 percent of the total number of observations, statistical significance will be overstated since we know errors will not be independent across quarters for a given individual. Most reported coefficients are highly significant, and given the large sample sizes, adjustments to take into account the error structure do not influence any substantive conclusions.
7. Likelihood ratio tests reject the null hypothesis that these covariates contribute nothing at highly significant levels.
8. It is not our intention to test for significant differences in coefficients across sites. The large sample sizes mean that differences that are highly statistically significant may be of little substantive importance.
9. The effects of age are considered by examining the difference in predicted leaving probability at ages 25 and 35. This difference is slightly over a standard deviation. The mean age across sites varies from 31 to 33.
10. The equations may be written in the form specified above for the simple probit, except in this model ε_{Eit} and ε_{Lit} are potentially correlated normally distributed random errors with means of zero and variance $\Sigma = \begin{bmatrix} 1 & \sigma_{EL} \\ \sigma_{EL} & 1 \end{bmatrix}$.
11. For example,

$$\Pr[E = 1 \mid L = 1] = \frac{\Phi(\beta'_E X, \beta'_L X, \sigma_{EL})}{\Phi(\beta'_L X)} = \left\{ \begin{array}{l} \text{predicted probability of the recipient being} \\ \text{employed given that she leaves welfare} \end{array} \right.$$

$$\Pr[E = 1 \mid L = 0] = \frac{\Phi(\beta'_E X, \beta'_L X, \sigma_{EL})}{1 - \Phi(\beta'_L X)} = \left\{ \begin{array}{l} \text{predicted probability of the recipient being} \\ \text{employed given that she does not leave welfare} \end{array} \right.$$

where Φ is the cumulative normal distribution. The conditional probabilities of $\Pr[L = 1|E = 1]$ and $\Pr[L = 1|E = 0]$ are analogously defined. See Greene (2000, pp. 849–852).

12. This is also consistent with the conclusions offered by Brauner and Loprest (1998) regarding the quality of employment outcomes of welfare leavers.
13. Similar changes in recipient characteristics are also found by Loprest (2001) in her national sample. See also Moffitt and Stevens (2001) and Zedlewski and Anderson (2001).
14. This question is answered by calculating the following probabilities:

$$\Pr[E_{94\text{–}95} = 1 \mid X_{98\text{–}99}] = \Phi\,(\beta'_{94\text{–}95} X_{98\text{–}99}) = \left\{ \begin{array}{l} \text{predicted probability of the average} \\ \text{postreform recipient being employed} \\ \text{in the prereform environment} \end{array} \right.$$

where Φ is the cumulative normal distribution and X_t is the mean characteristics level in period t and β_t is the estimated coefficient for period t.

$$\Pr[E_{98\text{–}99} = 1 \mid X_{98\text{–}99}] = \Phi\,(\beta'_{98\text{–}99} X_{98\text{–}99}) = \left\{ \begin{array}{l} \text{predicted probability of the average} \\ \text{postreform recipient being employed} \\ \text{in the postreform environment} \end{array} \right.$$

15. The employment probabilities corresponding to this comparison are:

$$\Pr[E_{94\text{–}95} = 1 \mid X_{94\text{–}95}] = \Phi\,(\beta'_{94\text{–}95} X_{94\text{–}95}) = \left\{ \begin{array}{l} \text{predicted probability of the average} \\ \text{prereform recipient being employed} \\ \text{in the prereform environment} \end{array} \right.$$

$$\Pr[E_{94\text{–}95} = 1 \mid X_{98\text{–}99}] = \Phi\,(\beta'_{94\text{–}95} X_{98\text{–}99}) = \left\{ \begin{array}{l} \text{predicted probability of the average} \\ \text{postreform recipient being employed} \\ \text{in the prereform environment} \end{array} \right.$$

16. The tobit specification is as follows:

$$W^*_{it+1} = X_{it}\beta_1 + \sum_{j=1}^{n} \gamma_{1j} YRQTR_j + \varepsilon_{it+1},$$

$$W^*_{it+8} = X_{it}\beta_8 + \sum_{j=1}^{n} \gamma_{8j} YRQTR_j + \varepsilon_{it+8},$$

where $W^*_{it+k} = W_{it+k}$ (the observed earnings) if $W_{it+k} > 0$. When $W_{it+k} = 0$, W^*_{it+k} is negative and can be interpreted as capturing a continuous propensity that identifies how close an individual is to obtaining actual earnings. W_{it+k} refers to the quarterly real earnings of a welfare recipient in quarter $(t + k)$ $(k = 1,8)$. All regressors are in reference to the quarter in which the recipient receives benefits (t). ε_{it} is assumed to be normally distributed with mean of zero and variance σ. The coefficients obtained from the tobit estimation essentially identify the combined effect of earnings and chance of employment. One could model this earnings outcome as part of a two-step selection process. However, when one applies a two-step methodology, the censoring selection equation only facilitates identification of selection effects in the earnings equation when one imposes identification restrictions. The value of fitting such an equation here, where our potential identifiers are severely limited, is low.

4
Job Stability for Welfare Recipients

A Comparison of Matched Job Spells

(Coauthored with Shiferaw Gurmu)

In this chapter, our focus is on the character of jobs obtained by welfare recipients during the 1990s in our six urban areas. We consider explicitly job stability and earnings within a job, and changes in these measures over time. As a benchmark, we also examine jobs obtained by workers who are not welfare recipients but are employed by the same employers as welfare recipients, allowing a control for the economy in the local areas.

A small literature has examined the dynamics of employment for welfare recipients, most of it focused on the period prior to welfare reform. Gault, Hartmann, and Yi (1999) show that, even in the 1980s, prior to the latest round of welfare reforms, work was common among welfare recipients. About a third of recipients worked while receiving welfare at some point over a two-year period, while a slightly smaller number were seeking work. Of those who did not work while receiving welfare, approximately half were individuals who cycled between work and welfare.

Moffitt and Rangarajan (1989) compared wage profiles of welfare recipients to those for other single parents, showing that although welfare recipients had lower earnings initially, their earnings increased over time, approaching those of nonwelfare individuals for many cohorts. Gladden and Taber (2000) examined the earnings-experience profiles of low-skilled individuals, considering the impact of welfare receipt. Their results confirm the view that welfare recipients do not experience lower levels of wage growth than others once actual experience is controlled. These results, in conjunction with those of Loeb and Corcoran (2001), suggest that, insofar as welfare recipients experience lower levels of wage growth, this can be traced to their intermittent work history.

Our work differs from prior analyses in one important respect: We focus on a particular "job," defined as a continuing relationship between an employer and an employee. This approach allows us to examine and compare the importance of employee and employer characteristics, as we do in Chapter 5. Jobs are clearly the building blocks from which economic self-sufficiency is constructed, but it should be recognized that information about job stability and earnings provides only one part of the picture. An individual may move from one employer to another, obtaining continuous employment even if each job is short-lived. In addition, under some conditions, workers may benefit when they leave a job, trading a low-wage job for one that is higher-paying or offers health insurance and other fringe benefits. So, job instability may not always indicate a problem. Nonetheless, there is little doubt that a stable job with high earnings provides valuable benefits to most individuals.

ANALYTICAL CONTEXT: EMPLOYMENT PATTERNS

It is important to provide some context for the analysis that follows. For our purposes, a job or job spell is defined as one or more consecutive quarters during which an individual receives earnings from a given employer. Since job stability is central in helping individuals leave welfare, we examine job spells beginning during a quarter in which an individual is receiving welfare payments, but we follow that job even if it continues after the individual leaves welfare. For comparison, we also examine job spells of those not receiving welfare but who secured a job with the same employer during the same quarter.

Before turning to job spells, it is useful to examine employment levels of welfare recipients and the extent to which recipients enter jobs while receiving welfare. Our initial examination of the data showed that employment and new job entry are highly seasonal, reflecting general economic variation in the kinds of low-skill service and retail positions recipients are likely to hold. In five of our six sites, the smallest number of new jobs occurs in the first quarter of the year, and the greatest number occurs in the third or fourth quarters, with this seasonal variation in the range of 5–10 percent. The exception is Fort Lauderdale, where the peak in new jobs occurs in the first quarter, reflecting Florida's distinctive high tourist season.

Figure 4.1 provides information on the level of employment for welfare recipients over the period of the study in our six sites, presented as a four-quarter moving average to remove seasonal effects.[1] This figure represents the proportion of those on welfare who have jobs in that quarter, without regard to whether the job began in that quarter or a prior one, or whether the job began while the individual was receiving welfare. In each site, changes in the welfare system have encouraged or required recipients to seek employment, so it is not surprising that, consistent with national statistics, there has been growth in employment at all sites since 1994. Nonetheless, there are substantial differences among the sites, with Kansas City displaying higher employment levels than the other sites. In the early 1990s, welfare employment rates were approximately 20 percent in Baltimore and over 30 percent in Kansas City, with the other sites between these. By 1999, while the ranking remained similar, welfare employment in Baltimore was over 30 percent, whereas employment in Kansas City was approaching 50 percent. The growth in employment in Houston was smaller than that at the other sites, with an increase only from 27 percent to 31 percent.

Figure 4.1 Employment Rate for Welfare Recipients (moving average)

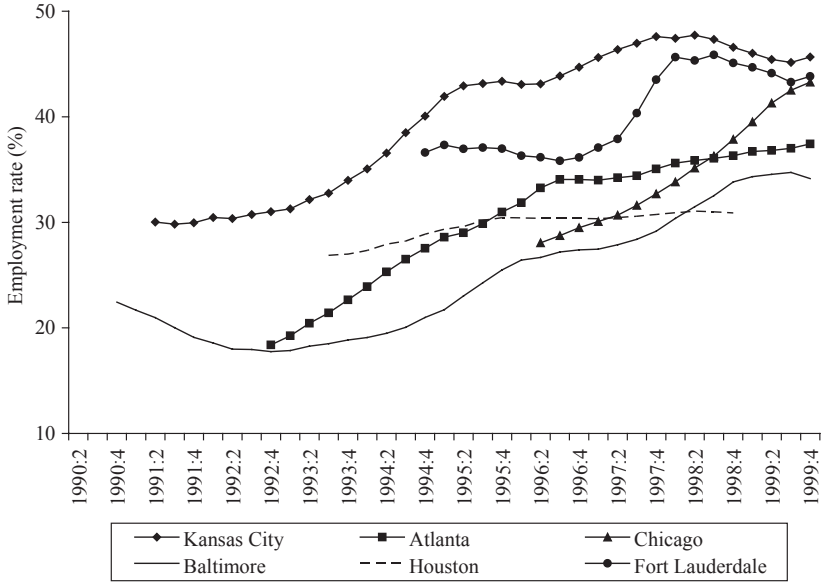
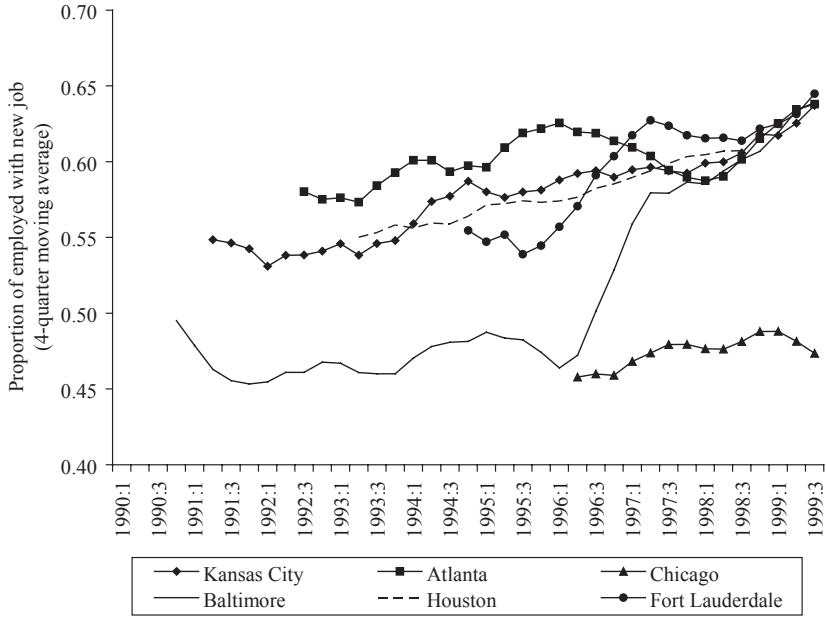


Figure 4.2 shows the proportion of employed recipients who are in a new job, also presented as a four-quarter moving average. It is well known that job turnover is high for welfare recipients, and the levels here confirm the view that a substantial number of employed recipients are obtaining new jobs, i.e., they are in the first quarter of a job spell. In Atlanta, Kansas City, Fort Lauderdale, and Houston, the proportions are in the range of 55 percent in the early 1990s, increasing gradually to nearly 65 percent by the end of the decade. In contrast, we see little change in the importance of new jobs in Chicago, where the proportion of new jobs remains slightly below 50 percent. Since our employment data for Chicago begin in 1996, a change could have occurred prior to that time.

An interesting pattern is observed in Baltimore, where the proportion remains unchanged at around 50 percent through 1995. This shifts dramatically in 1996 and 1997, as the percentage of new jobs increases to over 60 percent, approaching that in Kansas City, Houston, and Fort Lauderdale. The pattern suggests that policy changes may have played

Figure 4.2 Employed Recipients with New Job (moving average)



a particularly important role in Baltimore. As noted in previous chapters, in late 1995, Maryland implemented major welfare reform, the first major policy change in the state increasing emphasis on employment.[2]

JOB SPELLS FOR WELFARE RECIPIENTS

In order to examine job stability for welfare recipients, we consider the characteristics of job spells, changes in those characteristics over time, and differences across sites. As noted above, a job spell is defined as one or more consecutive quarters in which an individual received earnings from one employer. A spell is defined as a "welfare job spell" if the employee was a welfare recipient at any time during the first quarter that she received earnings from a given employer. We do not require that the individual remain a recipient after the initial quarter of employment. Since we expect that stable employment will cause many individuals to leave welfare, longer welfare job spells often include extended periods after the individual has left welfare. The job, as we define it here, is a particular match or relationship between a worker and an employer, which ends when the individual leaves (for whatever reason). It should not be confused with a position in a firm, which may exist independent of its incumbent.

Figure 4.3 presents survival functions based on welfare job spells at each of the sites, aggregated over all the spells we observe that begin in the period 1992–1999.[3] Survival functions estimate the probability that an event will last more than a specified period of time. We see that the probability that a job lasts more than one quarter varies from 45 percent for Kansas City to 56 percent for Chicago. The basic shapes of the survival curves are similar at all sites, with the ranking in stability maintained over all job lengths. Jobs in Chicago are most stable, those in Baltimore, Fort Lauderdale, and Houston similar to one another and slightly less stable, those in Atlanta less stable still, and those in Kansas City least stable.[4]

One might ask whether the survival curve differences are driven by differences in the early chance of job loss, since sites differ so dramatically on this dimension. Figure 4.4 presents the same data in terms of the hazard that a job spell will discontinue at each point in the spell. As expected, the hazard of job loss declines dramatically in the first two

Figure 4.3 Survival for Welfare Job Spells

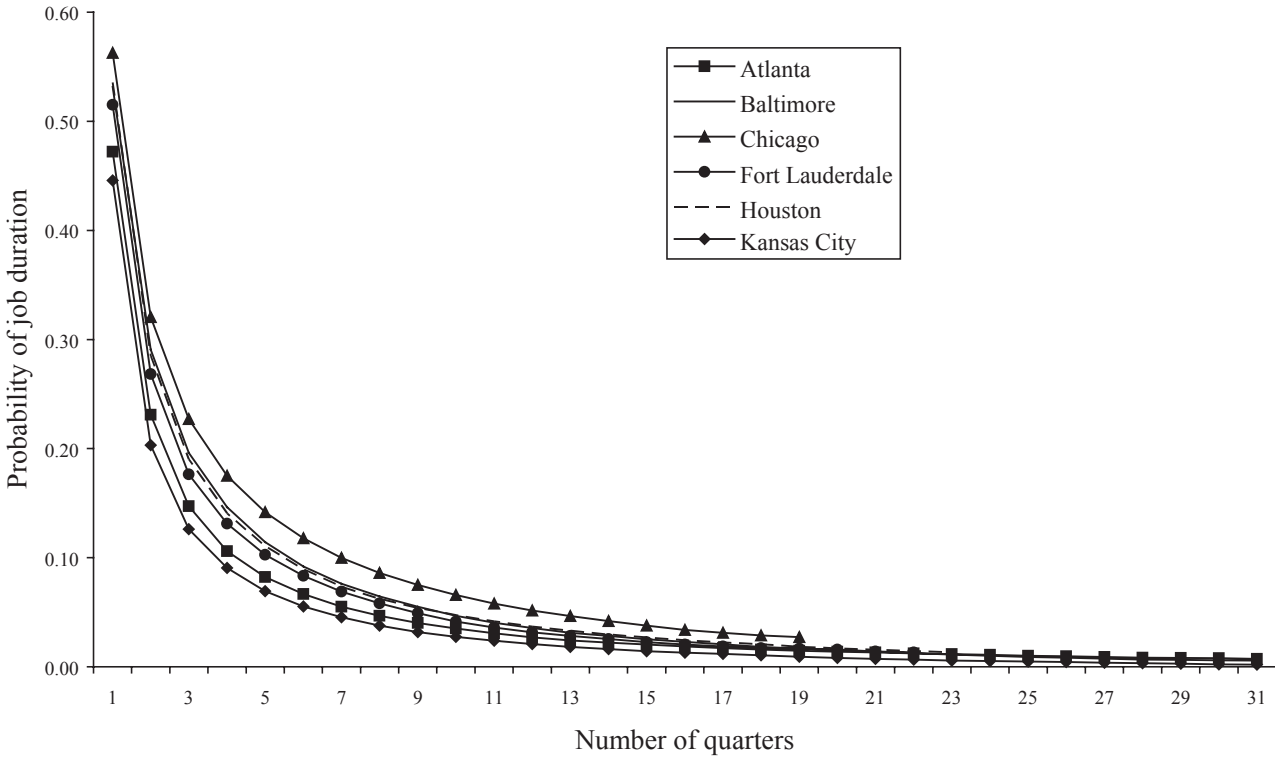
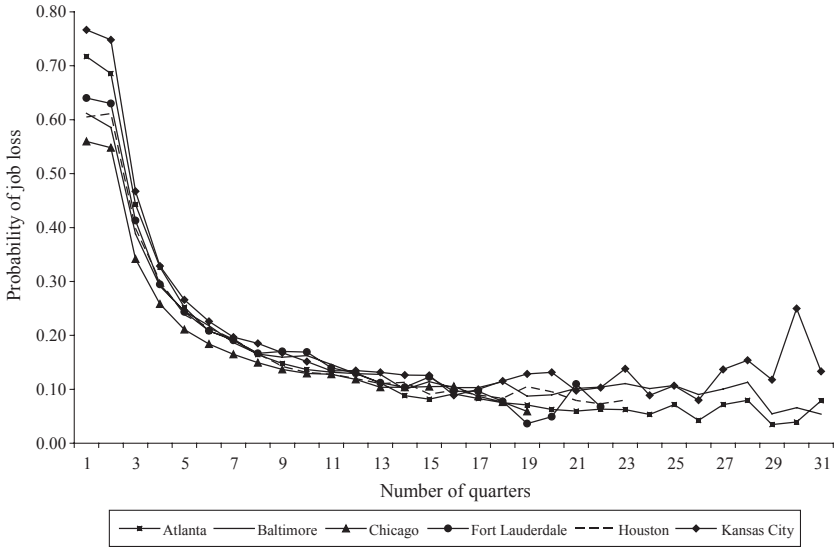


Figure 4.4 Hazard of Job Loss for Welfare Job Spells

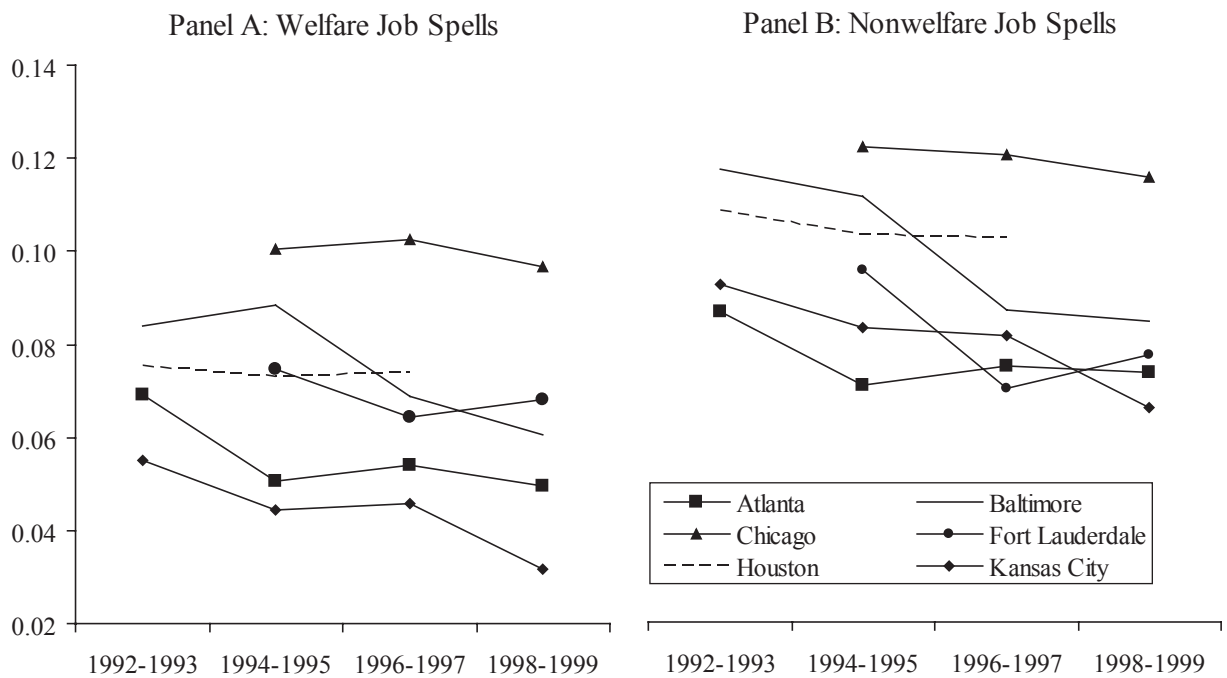


years on the job, remaining in the neighborhood of 10 percent after that. It should be noted that because a job can begin at any time during the first quarter, the calculated hazard of job loss is artificially depressed by our use of the standard hazard formula. A rough correction for this would imply that actual job loss hazard, following the first quarter, would be twice the reported figures.[5]

It is clear that the hazard of job loss is not only higher in Kansas City at the beginning of a job spell, but it remains at least slightly higher than at the other sites at greater spell lengths. Similarly, the likelihood of a job loss in Chicago remains lower. In short, there are consistent, if modest, differences in the stability of jobs across these sites.

All the analyses reported so far combine job spells beginning any time during the period 1992–1999. We also examined survival curves for welfare job spells sorted by the starting quarter and found basic shapes of these curves are very similar. Hazard functions are also basically similar to those for the full period. In order to summarize the changes over time and to compare sites, it is convenient to choose a single measure of job stability. Panel A of Figure 4.5 graphs the chance that a job spell survives for eight quarters (two years) or more, again

Figure 4.5 Job Spell Chance of Survival Eight or More Quarters by Beginning Year



sorted by starting quarter. (We return to panel B of Figure 4.5 later.)

Figure 4.5 underscores differences among the sites. The chance an individual will hold a job for two years is about 10 percent in Chicago but is less than half of that in Kansas City. Of the six sites, Kansas City and Baltimore exhibit the clearest changes over time. In Baltimore, the chance that a welfare job spell lasts two years declines from nearly 9 percent in 1994–1995 to only 6 percent in 1998–1999. In Kansas City, the decline is from 5.5 percent in 1992–1993 to 3.2 percent in 1998–1999. Changes in Atlanta are somewhat smaller, and those at the other sites are much more modest.[6] Although unstable jobs are generally less desirable, if declines in stability are associated with an increase in voluntary turnover, this could indicate movement to more desirable jobs.

Table 4.1 provides information about earnings received by welfare recipients in a given job spell. As a measure of the overall quality of the job, we present the total real earnings provided by the job during its entire duration, or the first eight quarters if it lasts more than eight quarters.[7] Also presented are measures of earnings per quarter. We see that earnings per quarter (e.g., line c) differ across sites, with Atlanta having appreciably lower earnings than the other sites. Some of the observed differences may be due to differences in cost of living, although it is unlikely that this explains much of the observed differences. As there is no accepted way to adjust for cost of living differences across our sites, we will focus on changes over time across sites, and on the relationship between the earnings of welfare recipients and others.[8]

In considering changes over time, we might expect that declines in stability would reduce total earnings, but this does not appear to be the case. In Kansas City, although Figure 4.5 implies a decline in job stability, the table shows that overall earnings for a job spell actually increased in the last period. We also see that while job stability did not change appreciably for Chicago welfare recipients, their total earnings increased dramatically. For both Kansas City and Chicago, the increase in earnings is driven by an increase in earnings per quarter. We also observe a modest improvement in earnings in Houston, whereas there is a decline in Atlanta, Baltimore, and Fort Lauderdale.[9] Overall, we conclude that declines in job stability over time do not appear to signal a serious deterioration in the quality of jobs obtained by welfare recipients.

Table 4.1 Earnings Measures for All Welfare Job Spells and Matching Spells

	A. Welfare job spells			B. Matching job spells[c]			C. Ratio welfare/match		
Date spell begins	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97
Atlanta									
a. Total number of spells	23,953	38,158	37,455	23,567	37,767	37,055			
b. Mean earnings in 1st quarter ($)	513	479	499	818	836	859	0.63	0.57	0.58
c. Mean earnings in 2nd quarter ($)	1,022	1,001	1,037	1,486	1,536	1,569	0.69	0.65	0.66
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	629	575	597	950	905	957	0.66	0.64	0.62
e. Mean of total earnings in 1st 8 quarters ($)[b]	2,370	1,998	2,096	3,950	3,607	3,851	0.60	0.55	0.54
Baltimore									
a. Total number of spells	28,113	35,964	39,455	27,462	35,201	38,712			
b. Mean earnings in 1st quarter ($)	1,113	1,041	958	1,862	1,669	1,563	0.60	0.62	0.61
c. Mean earnings in 2nd quarter ($)	2,040	1,980	1,908	3,105	2,976	2,872	0.66	0.67	0.66
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	1,305	1,245	1,190	2,089	1,946	1,884	0.62	0.64	0.63
e. Mean of total earnings in 1st 8 quarters ($)[b]	5,031	5,006	4,535	8,946	8,654	7,715	0.56	0.58	0.59

Chicago									
a. Total number of spells	n/a	42,543	157,159	n/a	40,526	149,294			
b. Mean earnings in 1st quarter ($)	n/a	798	876	n/a	1,249	1,352	n/a	0.64	0.65
c. Mean earnings in 2nd quarter ($)	n/a	1,647	1,836	n/a	2,345	2,643	n/a	0.70	0.69
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	n/a	1,691	1,863	n/a	2,590	2,897	n/a	0.65	0.64
e. Mean of total earnings in 1st 8 quarters ($)[b]	n/a	4,379	5,002	n/a	7,270	8,272	n/a	0.60	0.60
Fort Lauderdale									
a. Total number of spells	n/a	19,257	19,327	n/a	17,613	17,374			
b. Mean earnings in 1st quarter ($)	n/a	966	977	n/a	1,453	1,468	n/a	0.67	0.67
c. Mean earnings in 2nd quarter ($)	n/a	2,041	1,999	n/a	2,744	2,704	n/a	0.74	0.74
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	n/a	1,211	1,216	n/a	1,788	1,747	n/a	0.68	0.70
e. Mean of total earnings in 1st 8 quarters ($)[b]	n/a	4,687	4,343	n/a	7,597	6,662	n/a	0.62	0.65
Houston									
a. Total number of spells	45,129	77,280	30,364	44,211	75,790	29,838			
b. Mean earnings in 1st quarter ($)	874	845	878	1,390	1,446	1,318	0.63	0.58	0.67
c. Mean earnings in 2nd quarter ($)	1,695	1,768	1,825	2,539	2,533	2,588	0.67	0.70	0.71
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	2,147	2,185	2,262	3,228	3,192	3,299	0.67	0.68	0.69
e. Mean of total earnings in 1st 8 quarters ($)[b]	4,048	4,052	4,274	7,691	7,260	7,349	0.53	0.56	0.58

Table 4.1 (continued)

Date spell begins	A. Welfare job spells 1992–93	1994–95	1996–97	B. Matching job spells[c] 1992–93	1994–95	1996–97	C. Ratio welfare/match 1992–93	1994–95	1996–97
Kansas City									
a. Total number of spells	22,822	35,601	35,086	22,492	35,073	34,618			
b. Mean earnings in 1st quarter ($)	773	751	828	1,184	1,130	1,313	0.65	0.66	0.63
c. Mean earnings in 2nd quarter ($)	1,655	1,670	1,850	2,438	2,359	2,682	0.68	0.71	0.69
d. Mean quarterly earnings in 1st 8 quarters ($)[a]	940	905	1,004	1,482	1,403	1,603	0.63	0.65	0.63
e. Mean of total earnings in 1st 8 quarters ($)[b]	3,316	3,061	3,423	6,310	5,876	6,569	0.53	0.52	0.52

NOTE: All earnings expressed in real dollars for 1999, quarter 4.

[a] Calculated for quarters with earnings.

[b] For a spell lasting less than eight quarters, this is the total earnings.

[c] Spells matched by employer and beginning quarter.

MATCHED JOB SPELLS: COMPARISONS OF WELFARE RECIPIENTS AND OTHERS

In interpreting differences across sites and over time, it is difficult to determine the relative role of the local economy and welfare policy. One might wish to have information on job spells for those who are not welfare recipients for comparison. Of course, a random sample of all jobs would be inappropriate for comparative purposes because welfare recipients differ dramatically from the average job holder, both in terms of their demographic characteristics (e.g., sex, age, education) and in terms of family characteristics, since most recipients are single parents, by definition. Given that our data lack demographic information on employees who are not welfare recipients, we cannot compare similar individuals.[10]

On the other hand, our data do allow us to address the question of how characteristics of the employer affect individual success. Research suggests that welfare recipients who obtain jobs in traditionally high-paying and stable industries are much more likely to be successful in remaining employed (Bartik 1997). By matching workers on welfare with others working for the same employer, we control for industry and other differences between employers.

In order to control both for employer and the time when the job spell begins, for each welfare job spell we have chosen at random a job spell that begins in the same quarter with the same employer but for an employee who is not a welfare recipient. Of course, a small number of welfare recipients obtained jobs in such small firms that there are not enough other workers to provide matches, but we find that over 95 percent of job spells are matched by this process.

It should be stressed that this approach does not control for individual characteristics or occupation, since most firms hire a wide range of workers. It also does not fully control for the geographic area, since some employers who hire welfare recipients hire workers at other plants/locations throughout the state.[11] However, the matching process does capture the industrial structure of welfare recipients' jobs, which differ dramatically from those of workers in general. Welfare recipients are much more likely to be working in firms classified as service and retail trade. Since these firms tend to hire low-skill, female workers, the matching provides some degree of control for these characteristics.

When we examine this matched set of job spells for each site, we find that the shapes of the survival and hazard functions are qualitatively similar to those for welfare recipients, although nonwelfare workers are much more likely to have stable employment. The survival function for the matched spells is higher everywhere, confirming the view that those who are not welfare recipients experience consistently greater job stability, even when they obtain jobs with the same employers during the same quarter.

Panel B of Figure 4.5 presents the chance that a matched job spell will last eight quarters or more. Most striking is the fact that the changes over time in a given site are similar for welfare job spells and matched nonwelfare job spells. The chance that a matching job spell will last eight quarters is declining in Kansas City and Baltimore, following the same pattern observed for welfare job spells. This suggests that the patterns observed over time cannot be attributed primarily to the structure of welfare reform.

The relationships between welfare and matching spells are somewhat different across sites. If we compare welfare and matched job spells by taking the ratio of the chance that a spell will last eight quarters (panels A and B of Figure 4.5), we find that it is as low as 0.5 for Kansas City, around 0.7 for Baltimore and Houston, and over 0.8 for Chicago and Fort Lauderdale. In Atlanta, the ratio shifts over time, with an initial ratio of 0.8, declining to 0.65 in the last period. However, when we focus on earnings, these differences are less striking. Panel B in Table 4.1 presents total earnings (up to eight quarters) for matched nonwelfare job spells, and panel C indicates the ratio between earnings for welfare job spells and matched spells. Comparing total earnings received during the spell (row e), we see that its lowest value is 0.52, for Kansas City in 1994–1995 and 1996–1997, and its maximum is 0.65, for Fort Lauderdale in 1996–1997.

The similarities in the observed patterns across the sites are clearly more striking than the differences. Welfare recipients' job spells provide lower levels of earnings both because they are shorter and because they provide lower earnings each quarter. For example, during the first quarter of the spell, recipients earn about two-thirds as much as those in matched spells.[12] The number is slightly higher in the second quarter, suggesting that when recipients hold jobs through the second quarter,

their relative earnings increase. It is clear that welfare recipients land very different jobs with a given employer than do others.

One obvious question is whether welfare recipients who manage to keep their jobs do as well as others. Table 4.2 provides a partial answer to this question. In constructing this table, we have selected all welfare job spells that extend for at least three quarters. Similarly, we have selected among matched spells those that last at least three quarters.[13] The second quarter for both sets of spells is "interior," so that earnings are not reduced because the job began or ended during the quarter. Panel C indicates that earnings in the second quarter for welfare job spells (row c) are 23 to 33 percent below those in the sample of matched spells.

What we have seen here confirms the view that welfare recipients have markedly different job experiences, even when they work for the same employer, and that their earnings do not approach those of non-welfare workers even if they keep a job for three quarters. The character of the results differs very little across our sites. Perhaps most surprising, we see little evidence that the relationship between welfare recipient jobs and jobs of others is changing over time, even in the face of dramatic welfare policy changes. Most of the variation over time in the job experiences of welfare recipients is paralleled in those for the matched spells of those not receiving welfare. Labor market structures rather than welfare policies appear to be determining here, but we still need to explore the extent to which these differences are real under stricter matching procedures.

Job Spells Matched by Earnings

Given the dramatic differences in earnings between welfare recipients and others working for the same employer, we consider the degree to which welfare recipients' disadvantages can be traced to the kinds of jobs they obtain. Since our data do not provide us with information about the occupational classification or hourly wage of a welfare recipient's job, we have chosen to match recipients' job spells with job spells for other workers by the earnings received initially on the job. Unfortunately, the first quarter does not provide a good measure of expected earnings in subsequent quarters. Since a job does not necessarily begin at the start of the quarter, matching by initial earnings would match low-wage workers who worked for the full quarter with

Table 4.2 Earnings Measures for Welfare Job Spells and Matching Spells of Length 3 or Greater

	A. Welfare job spells			B. Matching job spells[b]			C. Ratio welfare/match		
Date spell begins	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97
Atlanta									
a. Total number of spells	6,366	8,681	8,333	7,014	10,274	9,899			
b. Mean earnings in 1st quarter ($)	818	836	859	1,298	1,342	1,399	0.63	0.62	0.61
c. Mean earnings in 2nd quarter ($)	1,486	1,536	1,569	2,115	2,179	2,333	0.70	0.70	0.67
d. Mean earnings in 3rd quarter ($)	1,287	1,296	1,338	1,878	1,962	2,095	0.69	0.66	0.64
e. Mean quarterly earnings in all interior quarters ($)	1,355	1,373	1,412	1,947	2,017	2,158	0.70	0.68	0.65
f. Mean of total wages in 1st 8 quarters ($)[a]	7,236	6,927	7,369	11,191	11,164	12,060	0.65	0.62	0.61
Baltimore									
a. Total number of spells	8,263	10,917	11,469	9,430	12,292	12,523			
b. Mean earnings in 1st quarter ($)	1,780	1,661	1,496	3,039	2,724	2,480	0.59	0.61	0.60
c. Mean earnings in 2nd quarter ($)	2,828	2,746	2,699	4,135	4,007	3,933	0.68	0.69	0.69
d. Mean earnings in 3rd quarter ($)	2,538	2,430	2,382	3,807	3,637	3,610	0.67	0.67	0.66
e. Mean quarterly earnings in all interior quarters ($)	2,881	2,803	2,767	4,194	4,030	4,024	0.69	0.70	0.69
f. Mean of total wages in 1st 8 quarters ($)[a]	14,301	13,973	12,965	22,725	21,843	20,429	0.63	0.64	0.63

Chicago									
a. Total number of spells of length 3 or greater	n/a	13034	51520	n/a	14192	53813			
b. Mean earnings in 1st quarter ($)	n/a	1,249	1,335	n/a	1,916	2,164	n/a	0.65	0.62
c. Mean earnings in 2nd quarter ($)	n/a	2,335	2,552	n/a	3,229	3,655	n/a	0.72	0.70
d. Mean earnings in 3rd quarter ($)	n/a	2,088	2,283	n/a	2,954	3,377	n/a	0.71	0.68
e. Mean quarterly earnings in all interior quarters ($)	n/a	3,612	3,906	n/a	5,244	5,911	n/a	0.69	0.66
f. Mean of total wages in 1st 8 quarters ($)[a]	n/a	13,973	12,965	n/a	21,843	20,429	n/a	0.64	0.63
Fort Lauderdale									
a. Total number of spells	n/a	5,439	5,191	n/a	5,731	5,250			
b. Mean earnings in 1st quarter ($)	n/a	1,551	1,488	n/a	2,338	2,387	n/a	0.66	0.62
c. Mean earnings in 2nd quarter ($)	n/a	2,837	2,751	n/a	3,749	3,737	n/a	0.76	0.74
d. Mean earnings in 3rd quarter ($)	n/a	2,538	2,492	n/a	3,465	3,481	n/a	0.73	0.72
e. Mean quarterly earnings in all interior quarters ($)	n/a	2,870	2,820	n/a	3,827	3,797	n/a	0.75	0.74
f. Mean of total wages in 1st 8 quarters ($)[a]	n/a	13,832	12,968	n/a	20,308	18,426	n/a	0.68	0.70

Table 4.2 (continued)

	A. Welfare job spells			B. Matching job spells[b]			C. Ratio welfare/match		
Date spell begins	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97
Houston									
a. Total number of spells	12,063	20,136	7,444	14,183	22,677	8,051			
b. Mean earnings in 1st quarter ($)	1,276	1,283	1,297	1,923	1,895	1,836	0.66	0.68	0.71
c. Mean earnings in 2nd quarter ($)	2,382	2,483	2,504	3,319	3,332	3,277	0.72	0.75	0.76
d. Mean earnings in 3rd quarter ($)	2,071	2,097	2,116	3,040	2,934	2,875	0.68	0.71	0.74
e. Mean quarterly earnings in all interior quarters ($)	2,439	2,510	2,547	3,500	3,372	3,356	0.70	0.74	0.76
f. Mean of total earnings in 1st 8 quarters ($)[a]	10,907	10,607	10,021	17,626	15,627	14,034	0.62	0.68	0.71
Kansas City									
a. Total number of spells	5,228	7,030	7,200	6,826	9,717	9,895			
b. Mean earnings in 1st quarter ($)	1,312	1,358	1,500	1,973	1,987	2,257	0.67	0.68	0.66
c. Mean earnings in 2nd quarter ($)	2,504	2,659	2,896	3,480	3,468	3,833	0.72	0.77	0.76
d. Mean earnings in 3rd quarter ($)	2,103	2,226	2,431	3,001	3,089	3,351	0.70	0.72	0.73
e. Mean quarterly earnings in all interior quarters ($)	2,548	2,711	2,957	3,490	3,526	3,869	0.73	0.77	0.76
f. Mean of total wages in 1st 8 quarters ($)[a]	11,510	11,975	13,022	17,909	18,151	19,507	0.64	0.66	0.67

NOTE: All earnings expressed in real dollars for 1999:4.

[a] For spells lasting less than eight quarters, this is total earnings.

[b] Spells with three or more quarters among all spells matched by employer and beginning quarters.

high-wage workers who worked only a portion of the quarter. Similarly, for any job that lasts only two quarters, the second quarter most likely reflects earnings over only a portion of the quarter. Hence, we limit consideration to welfare job spells that last at least three quarters, matching second quarter earnings only.

In limiting consideration to job spells lasting at least three quarters, we are only focusing on the most stable job spells of welfare recipients. As indicated in Figure 4.3, in Kansas City, only 20 percent of spells last this long; in the other sites, the proportion is between 23 and 32 percent. The matching method is similar to that indicated above. For each welfare job spell lasting at least three quarters, we examine job spells lasting at least three quarters for individuals hired by the same employer in the same quarter who are not welfare recipients. The matching spell is the one with second quarter earnings that are as close as possible to the second quarter earnings for the welfare job spell. If no matching spell exists for which earnings are within $150 of the welfare spell, we omit that welfare spell from further analysis. Across the sites, we eliminated between 20 and 30 percent of welfare job spells lasting at least three quarters because no appropriate match was available.[14]

When we observed the survival functions for welfare and other job spells matched by earnings, it was clear that differences were smaller than those based on the simple match. Some basic information on the job spells matched by earnings is provided in Table 4.3. We first consider the extent to which the matching actually produced spells with the same earnings in the second quarter. We see for all sites that mean earnings in the second quarter are within $2 for the welfare job spells and spells matched to them (compare row c for panels A and B), showing that, as a mechanical matter, the matching is successful. When we look at first quarter earnings, we see that welfare recipients earn 4 to 8 percent less than their nonwelfare counterparts in that quarter (row b in panel C). Earnings in the third quarter are within 5 percent in all sites, with most sites within 2 percent (row d). This implies that once we have controlled for (or matched on) earnings in a given quarter, earnings in the following quarter are very similar. If we look at earnings in a typical quarter, they are also quite close (row e).

As a measure of the overall value of a job, we consider the total earnings obtained for the life of the job or, for jobs lasting more than eight quarters, for the first eight quarters (row f). The difference be-

Table 4.3 Earnings Measures for Welfare Job Spells of Length 3 or Greater and Nonwelfare Spells Matched by Second Quarter Earnings

	A. Welfare job spells			B. Matching job spells[b]			C. Ratio welfare/match		
Date spell begins	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97
Atlanta									
a. Total number of spells	5,111	7,112	6,498	5,111	7,112	6,498			
b. Mean earnings in 1st quarter ($)	789	812	856	855	870	914	0.92	0.93	0.94
c. Mean earnings in 2nd quarter ($)	1,426	1,483	1,567	1,427	1,484	1,568	1.00	1.00	1.00
d. Mean earnings in 3rd quarter ($)	1,243	1,258	1,328	1,269	1,297	1,375	0.98	0.97	0.97
e. Mean quarterly earnings in all interior quarters ($)	1,306	1,330	1,406	1,313	1,363	1,450	0.99	0.98	0.97
f. Mean of total earnings in 1st 8 quarters ($)[a]	6,968	6,788	7,405	7,018	7,027	7,673	0.99	0.97	0.97
Baltimore									
a. Total number of spells of length 3 or greater	5,556	7,600	8,451	5,556	7,600	8,451			
b. Mean earnings in 1st quarter ($)	1,782	1,655	1,461	1,912	1,784	1,577	0.93	0.93	0.93
c. Mean earnings in 2nd quarter ($)	2,751	2,702	2,637	2,752	2,703	2,637	1.00	1.00	1.00
d. Mean earnings in 3rd quarter ($)	2,475	2,403	2,316	2,517	2,422	2,370	0.98	0.99	0.98
e. Mean quarterly earnings in all interior quarters ($)	2,811	2,758	2,694	2,843	2,784	2,739	0.99	0.99	0.98
f. Mean of total earnings in 1st 8 quarters ($)[a]	14,213	13,932	12,669	14,585	14,095	13,005	0.97	0.99	0.97

Chicago									
a. Total number of spells of length 3 or greater	n/a	9292	36085	n/a	9292	36085			
b. Mean earnings in 1st quarter ($)	n/a	1,193	1,297	n/a	1,267	1,357	n/a	0.94	0.96
c. Mean earnings in 2nd quarter ($)	n/a	2,247	2,481	n/a	2,248	2,481	n/a	1.00	1.00
d. Mean earnings in 3rd quarter ($)	n/a	2,004	2,206	n/a	2,041	2,238	n/a	0.98	0.99
e. Mean quarterly earnings in all interior quarters ($)	n/a	2,634	2,853	n/a	2,687	2,888	n/a	0.98	0.99
f. Mean of total earnings in 1st 8 quarters ($)[a]	n/a	11,994	12,884	n/a	12,336	13,161	n/a	0.97	0.98
Fort Lauderdale									
a. Total number of spells	n/a	1,960	2,067	n/a	1,960	2,067			
b. Mean earnings in 1st quarter ($)	n/a	1,480	1,420	n/a	1,564	1,488	n/a	0.95	0.95
c. Mean earnings in 2nd quarter ($)	n/a	2,720	2,686	n/a	2,722	2,687	n/a	1.00	1.00
d. Mean earnings in 3rd quarter ($)	n/a	2,291	2,307	n/a	2,414	2,409	n/a	0.95	0.96
e. Mean quarterly earnings in all interior quarters ($)	n/a	2,224	2,215	n/a	2,342	2,289	n/a	0.95	0.97
f. Mean of total earnings in 1st 8 quarters ($)[a]	n/a	12,380	11,553	n/a	13,607	12,286	n/a	0.91	0.94

Table 4.3 (continued)

	A. Welfare job spells			B. Matching job spells[b]			C. Ratio welfare/match		
Date spell begins	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97	1992–93	1994–95	1996–97
Houston									
a. Total number of spells	9,345	16,163	11,353	9,345	16,163	11,353			
b. Mean earnings in 1st quarter ($)	1,223	1,235	1,283	1,301	1,307	1,379	0.94	0.94	0.93
c. Mean earnings in 2nd quarter ($)	2,284	2,404	2,526	2,285	2,405	2,527	1.00	1.00	1.00
d. Mean earnings in 3rd quarter ($)	2,019	2,056	2,214	2,061	2,127	2,256	0.98	0.97	0.98
e. Mean quarterly earnings in all interior quarters ($)	2,354	2,446	2,614	2,382	2,492	2,658	0.99	0.98	0.98
f. Mean of total earnings in 1st 8 quarters ($)[a]	11,232	11,204	12,014	11,618	11,951	12,600	0.97	0.94	0.95
Kansas City									
a. Total number of spells	3,569	4,814	5,008	3,569	4,814	5,008			
b. Mean earnings in 1st quarter ($)	1,257	1,325	1,488	1,362	1,413	1,598	0.92	0.94	0.93
c. Mean earnings in 2nd quarter ($)	2,440	2,629	2,871	2,441	2,629	2,871	1.00	1.00	1.00
d. Mean earnings in 3rd quarter ($)	2,016	2,176	2,410	2,156	2,249	2,499	0.94	0.97	0.96
e. Mean quarterly earnings in all interior quarters ($)	2,481	2,669	2,929	2,517	2,713	2,954	0.99	0.98	0.99
f. Mean of total earnings in 1st 8 quarters ($)[a]	11,075	11,824	12,974	12,231	12,985	13,963	0.91	0.91	0.93

NOTE: All earnings expressed in real dollars for 1999:4.

[a] For spells lasting less than eight quarters, this is total earnings.

[b] Spells matched by employer, beginning quarter and second quarter earnings.

tween earnings for welfare and nonwelfare spells is greatest in Fort Lauderdale and Kansas City, where it approaches 10 percent. The difference is 3–6 percent in Houston, and only 1–3 percent in Atlanta, Baltimore, and Chicago.[15]

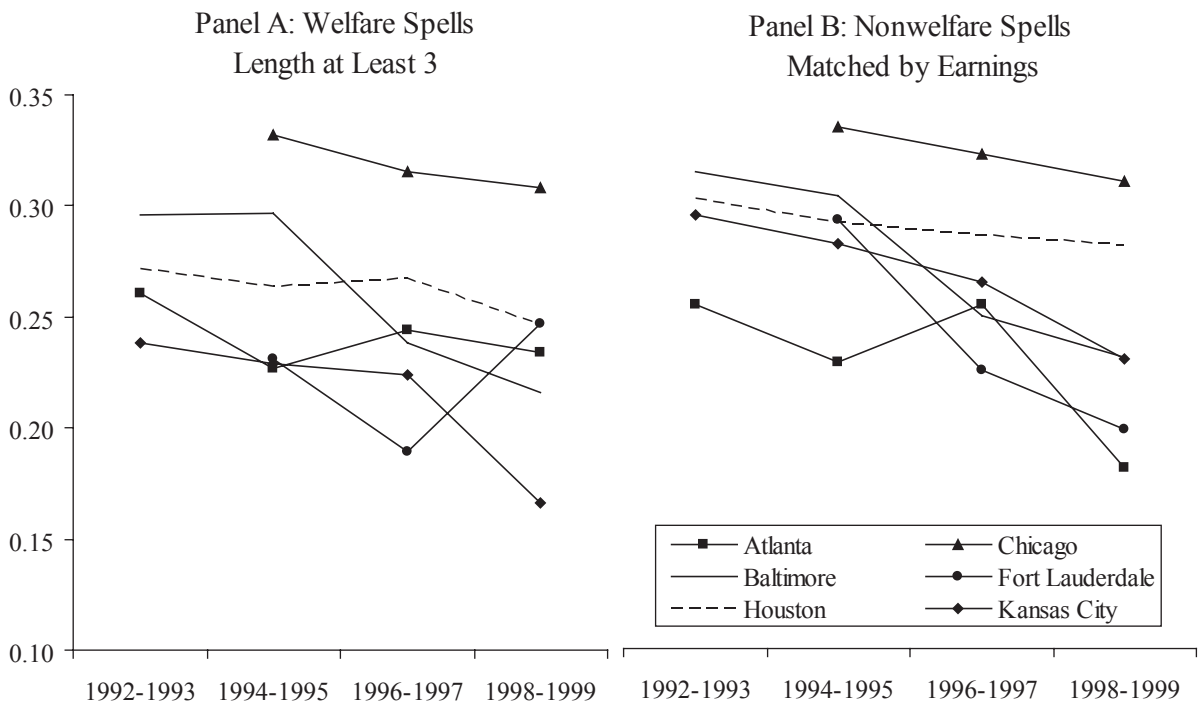
Figure 4.6 presents the likelihood that a job spell will last eight or more quarters for welfare and nonwelfare job spells matched by earnings for the four time periods we have considered. Note that since probabilities are contingent on the spell lasting at least three quarters, the numbers are much higher than those for all spells (Figure 4.5): both welfare and nonwelfare job spells are relatively stable representatives of their respective groups. There are some differences among the sites. For Baltimore and Chicago, there are almost no differences between job spells of welfare recipients and their matches. For example, for the period 1996–1997 in Chicago, the chance that a welfare job spell will last at least eight quarters is 31.5 percent, whereas the chance for matching spells is only marginally higher at 31.9 percent. For Houston, the difference is in the range of 2–3 percentage points, still modest. In Kansas City, the difference is in the range of 4–6 percentage points, with the greatest difference in the final period. The difference in Fort Lauderdale is 6 points in the earliest period, but the relationship actually reverses in the final period.

Figure 4.6 also shows that time trends for job spells matched by earnings parallel those for all job spells (Figure 4.5). This indicates that the patterns reported in the previous section are not driven solely by differences in spell survival over the first three quarters of the spell, since all spells used in Figure 4.6 are at least three quarters in length.

The results for Atlanta are somewhat anomalous. For two of the four periods, the chance that a welfare job spell will last at least eight quarters is actually greater than that for matching spells, with this difference substantial in the most recent period. These results imply that, in the final period, welfare recipients have a 23 percent chance of remaining in a job for at least eight quarters as compared with only 18 percent for those in matching jobs. We are not sure what to make of this anomaly.

We turn next to several comparisons that allow us to quantify the extent to which matching by earnings captures job differences.

Figure 4.6 Job Spell Chance of Survival Eight or More Quarters by Beginning Year for Spells Matched by Earnings



Comparison of Simple Matching and Matching by Earnings

A natural question to ask is the extent to which matching by earnings removes differences between welfare recipients' job experiences and those of others. Table 4.4 reports the ratio of earnings for welfare recipient job spells and others when various matching criteria are considered, using job spells from 1992 through 1997 as available at each site. The first column shows that, over the course of a job, welfare recipients earn between 52 and 63 percent as much as others who obtained jobs with the same employers in the same quarter. When we limit consideration to jobs that last at least three quarters, the figure increases to an average of about 65 percent (column 2). When job spells are matched by second quarter earnings, the number increases to over 90 percent.

Of course, the finding that matching by earnings reduces the difference in earnings between welfare recipients and others is partly an arithmetic necessity, since earnings in the second quarter are part of the earnings being measured. Columns 4–6 show that first quarter differences follow a very similar pattern, confirming that matching by second quarter earnings identifies jobs that provide similar earnings in all quarters.

To what degree does matching by earnings remove or account for differences in job stability? Table 4.5 compares expected job spell length for welfare recipients and matched nonwelfare spells, based on survival estimates for each site.[16] The first two columns show that welfare recipients' job spells are generally between 0.4 and 0.8 quarters shorter than are matched spells, a difference of 15 to 30 percent. The exception is Fort Lauderdale, where the difference is only 0.2 quarters.

Since we can match by earnings only for jobs lasting at least three quarters, it is useful to focus on spells for such jobs. Columns 4–6 show that in five of the six sites there remain notable differences in expected spell length even contingent on the spell lasting for at least three quarters. This confirms our earlier conclusion that welfare recipients are in jobs that are less stable even after an initial trial period.

A substantial portion of these differences is explained by differences in earnings. Once jobs are matched by second quarter earnings (columns 7–9), we see that the differences in expected spell length decline markedly.[17] Column 10 reports the percentage decline in the difference, a measure of the extent to which the earnings match explains this dif-

Table 4.4 Earnings Ratios of Welfare Job Spells and Matching Spells with Alternative Matching Criteria, Spells 1992–1997

Cumulative conditions on spells	Ratio of total earnings for welfare job spells and matching spells			Ratio of first-quarter earnings for welfare job spells and matching spells		
	Spells matched by employer and quarter (1)	Length of 3 or more quarters (2)	Matched by 2nd-quarter earnings (3)	Spells matched by employer and quarter (4)	Length of 3 or more quarters (5)	Matched by 2nd-quarter earnings (6)
Atlanta	0.56	0.62	0.97	0.59	0.62	0.93
Baltimore	0.58	0.63	0.98	0.61	0.60	0.93
Chicago	0.60	0.64	0.98	0.65	0.62	0.95
Fort Lauderdale	0.63	0.69	0.92	0.67	0.64	0.95
Houston	0.55	0.66	0.95	0.61	0.68	0.94
Kansas City	0.52	0.66	0.92	0.65	0.67	0.93

Table 4.5 Expected Spell Length in Quarters for All Available Spells, 1992–1999

	All spells			Contingent on length at least 3						
							Spells matched by earnings			Percent explained by earnings match[a]
	Welfare job spells (1)	Matched spells (2)	Difference (3)	Welfare job spells (4)	Matched spells (5)	Difference (6)	Welfare job spells (7)	Matched spells (8)	Difference (9)	(10)
Atlanta	2.77	3.20	0.43	7.62	8.08	0.46	7.89	7.63	−0.26	157
Baltimore	3.02	3.48	0.45	7.13	7.64	0.50	7.18	7.20	0.02	95
Chicago	3.48	3.83	0.34	7.98	8.29	0.31	8.11	8.20	0.09	72
Fort Lauderdale	2.90	3.10	0.20	7.16	7.02	−0.13	6.29	6.84	0.54	—
Houston	2.90	3.66	0.75	6.80	8.16	1.36	7.84	8.38	0.54	60
Kansas City	2.34	3.15	0.81	6.41	7.73	1.33	6.39	7.26	0.86	35

[a] Calculated as 100 × (column 6 − column 9) / column 6.

ference. There is substantial variation across sites. In Kansas City the difference declines by about a third, and in Houston it declines by three-fifths. The decline is 72 percent in Chicago and fully 95 percent in Baltimore; in Atlanta, the relative spell lengths reverse, with welfare jobs actually estimated to last slightly longer than matched nonwelfare jobs. In Fort Lauderdale, matching by earnings reveals a gap that was not apparent in the simple comparisons.[18]

We conclude that in Atlanta, Chicago, and Baltimore, welfare recipients who remain on a job at least three quarters have jobs that are almost indistinguishable—in terms of job stability and expected earnings—from others working for the same employers who have similar initial earnings. In contrast, welfare recipients have substantially less stable jobs than nonwelfare individuals with initially comparable jobs in Fort Lauderdale, Kansas City, and Houston.

CONCLUSIONS

These results confirm the view that not only do welfare recipients suffer less stable employment than others, but this is, in part, a function of lower levels of stability in the particular jobs they hold. Our analysis shows that this difference cannot be attributed to industry or other employer characteristics, since differences remain large when welfare recipients are matched with nonwelfare individuals working for the same employers. The difference between welfare recipients and others is marked across all of our sites. As noted above, some of the difference may be accounted for by the fact that welfare recipients of necessity must deal with child care and a number of other challenges as poor single parents that nonwelfare workers may not be faced with. Lacking demographic and family information for our nonwelfare matches, we cannot fully account for such differences.

In all sites, we observe modest declines over time in the apparent stability of jobs for welfare recipients, but these are similar to declines observed for nonwelfare jobs with the same employers, so it appears that these are not attributable to welfare reform. Perhaps equally important, even in the face of declines in job stability, earnings declined little or not at all in most sites, suggesting that growing instability does not reflect declines in job quality.

As expected, when welfare recipients' jobs were matched with other jobs with the same employers providing similar earnings, observed differences in job stability declined markedly. In three of our sites, substantial differences remain, implying that even when welfare recipients have the same job, their chance of remaining with that job for an extended period is smaller. Nonetheless, the variation across jobs for welfare recipients clearly dwarfs differences between welfare recipients and others. The next chapter focuses on the extent to which recipient and employer characteristics contribute to the observed success on a job.

Notes

1. We index the moving average by the third quarter of the year for which the average applies. The employment data for Fort Lauderdale are missing in 1995:3 and 1997:2. Moving averages for periods including these quarters merely omit them from the average.
2. This also corresponds to the implementation of a new data management system, and the shift may reflect incompatibilities between systems.
3. Survival and hazard calculations are consistent with normal life table methods, although we have treated censored data in a slightly different way than is conventional to reflect the character of our data. We write the conditional probability that the spell is terminated at t quarters as

   $$q_t = (d_t - w_t) / (n_t - w_t),$$

   where d_t is the number of spells terminating at length t, n_t is the number of spells of length t or greater, and w_t is the number of spells of length t but which are censored at that length because data for quarter $t + 1$ are not available. This treatment of censored spells differs from the conventional approach in that here all censored spells are removed from the risk set. The conventional formula assumes that censored observations can be distinguished from spells where the event of interest occurs. In our data, when a spell is censored at length t because data for quarter $t + 1$ are unavailable, this wipes out all information about whether the job terminated due to job loss in quarter t. Hence, the formula above, which we use, ignores all spells that are censored in quarter t.
   The survival function is calculated as

   $$S(t) = \Pi_{j=1}^{t} (1 - q_j).$$

   The hazard of job loss we report is calculated in the usual way using the quarter as the time unit,

   $$h_t = q_t / (1 - q_t/2).$$

This approximation is based on the assumption that the hazard of job loss is constant throughout quarter t. For $t > 1$, this is a reasonable approximation in our data, but it is not correct for $t = 1$ because jobs do not generally begin at the start of the quarter. We discuss this issue below.

4. Missing data in quarters 1995:3 and 1997:2 in Fort Lauderdale required special attention. Survival and hazard calculations omit any spell beginning in a quarter for which data are missing as well as spells beginning in the following quarter, since it is not possible to determine whether employment first observed in the following quarter actually began in that quarter. We used the ratio for the spell length distribution observed for spells with available data to determine the number of spells terminating during the two quarters hidden by the missing data. We also used an additional correction to account for the possibility that a spell spanning a quarter with missing data actually discontinued prior to the missing quarter but that the individual began a new job spell with the same employer immediately after the missing quarter, based on experiments in which we omitted a quarter of actual data.
5. We have chosen not to adjust the first-quarter hazard rate for the expected date of job start because there are clearly other serious inaccuracies that are relevant in the first period. It is known that the hazard of job loss increases after the first few weeks on a job, followed by a dramatic decline. Since the biases are similar across sites, there is no difficulty in comparing first-period hazards.
6. For these analyses, only job spells that could last a full eight quarters can be included, and so spells used in the analysis for the period 1998–1999 differ by site according to data availability. For Baltimore, Fort Lauderdale, and Kansas City, only spells beginning in the first quarter of 1998 are included. For Chicago, spells beginning in the first three quarters of 1998 are included, and for Atlanta, spells beginning in 1998 and the first quarter of 1999 are included. No information on spells beginning in Houston in 1998 or 1999 is available. Houston data for 1996–1997 include only information on spells beginning through the first quarter of 1997.
7. All reported earnings have been adjusted to real dollars for the fourth quarter of 1999 based on the U.S. Consumer Price Index for urban consumers. As in the previous analyses, in Houston, information on job spells as reported in Tables 4.1 and 4.2 extends only through the first quarter of 1997.
8. There is no government-sanctioned measure comparable to the Consumer Price Index that attempts to capture cost of living differences across cities in the United States. One commercially produced index suggests that, among our sites, cost of living is highest in Fort Lauderdale, nearly 30 percent above the U.S. average, with Chicago about 20 percent above the national average, and Atlanta 10 percent above the national average. Our other sites are 5 to 10 percent below the national average (http://houseandhome.msn.com). These cost-of-living estimates bear little relation to observed differences in earnings reported in Table 4.1. It should be recognized that not only are such measures crude, but they are not designed to reflect the costs faced by welfare recipients.
9. As above, special calculations were necessary to obtain earnings measures in

Fort Lauderdale because of missing data in two quarters. We omit any spell beginning in a missing quarter or any spell beginning in the following quarter. For job spells beginning in other quarters, we constructed the spell omitting the missing quarters of data, allowing a job to span across the missing quarter. For all analyses in which second quarter earnings were matched, we omitted any spell with a second quarter corresponding to the quarter with missing data. For analyses reported in Tables 4.1–4.3, which require measures of earnings for each job, we substituted data from other spells in Fort Lauderdale as follows: For a spell missing earnings data in a particular quarter, we selected spells beginning within the same general time period (1992–1993, 1994–1995, or 1996–1997) that were of the same length as the spell in question and with the same welfare status, filling in the data from a random spell. Where we were unsure of the length of the spell because the spell ended immediately prior to a missing quarter, we matched the spell randomly with a spell from a population containing spells of both possible lengths, taking from the matching spell both the spell length, and, when necessary, the missing earnings information.

10. Chapter 5 examines the impact of individual and employer characteristics on job stability for welfare recipients.
11. Employers are defined according to the account maintained in the state unemployment insurance program. In general, all employees of a firm within a state are defined as being employed by a single employer, while employees outside the state are excluded.
12. Even if weekly earnings were the same for welfare recipients and those in matched job spells, we would expect welfare recipients to earn less in the initial quarter since they are more likely to leave the job during the quarter. Calculations show that if the only difference were due to such differential stability, earnings in the first quarter for welfare recipients would be within 10 percent of the earnings for others.
13. Unlike the comparison provided in Table 4.3, the two groups are not perfectly matched by employer.
14. In most sites, earnings decline by 1 to 5 percent due to dropping these spells, although declines in Chicago and Fort Lauderdale are over 10 percent in some periods. In the case of Fort Lauderdale, we omitted spells when data were not available for certain quarters, and this may reflect weaknesses in our methods of dealing with this problem.
15. The data used in this analysis for Houston require additional comment. We made a special data request to undertake matching by earnings, and we were able to obtain information on spells that began as late as the third quarter of 1998. Hence, in contrast to statistics reported in Tables 4.1 and 4.2, in which Houston data extend only through the first quarter of 1997, in Table 4.3, spells beginning in all quarters of 1996–1997 are included. Spells underlying the Houston analysis reported in Figure 4.6 extend through the third quarter of 1998.
16. Expected spell length in Table 4.5 is calculated as the expected number of quarters an observed job will last, based on the survival function calculated from all available spells 1992–1999 at a particular site. The measure is extrapolated to a

40-year working life based on the assumption that the geometric mean survival function in the last 10 quarters applies to the remainder of the 40 years.

17. Columns 4 and 7 in Table 4.5 both refer to welfare job spells, but they may differ at all sites because only spells that could be matched are included in column 7. In Fort Lauderdale and Houston, because data on earnings were missing for certain quarters, the matching sample was further restricted.
18. Recall that, in Fort Lauderdale, missing earnings data in two quarters required that we apply special methods.

5
Explaining Job Stability for Welfare Recipients

(Coauthored with Shiferaw Gurmu)

In the previous chapter, we examined the structure of the job spells of welfare recipients in our six sites, comparing them with job spells for others working for the same employers. This chapter focuses on the determinants of job stability, earnings within a job, and changes in the determinants of job stability over time. The analysis considers first the basic demographic factors influencing job stability and the impact of employer industry. We also control for personal characteristics by examining those individuals who held multiple jobs; we control for employer characteristics by examining job spells of different workers with the same employers.

These analyses allow us to shed light on the important question of how important it is for welfare recipients to obtain "good" jobs. One polar view suggests that the job an individual lands is of little or no importance because the source of job instability is the individual. According to this view, even if welfare recipients get good jobs, they will lose them in short order because of behavioral, skill, or other personal issues. The opposing view is that instability is a characteristic of the job, so that if welfare recipients can obtain stable jobs, perhaps those with "good" employers, they will be able to achieve stable employment.

While our results confirm the importance of individual factors, they show that industry and other employer characteristics are important determinants of job stability. This suggests that although individual characteristics and behaviors have important impacts on stability, it is also the case that "jobs matter." We also examine changes in the determinants of job stability over the 1990s, finding that changes are relatively modest. Finally, we examine the determinants of earnings on a job for welfare recipients, considering how earnings and job stability across industries are related. We confirm that stable jobs generally provide higher earnings, although there are some notable exceptions.

METHODS

Our definition of a job spell is the same as that in the previous chapter: one or more consecutive quarters in which an individual received earnings from a particular employer. The analysis will be limited to welfare job spells, those for which the employee was a welfare recipient during the first quarter that she received earnings from the employer. We do not require that the individual remain a recipient after the initial quarter of employment.

Our concern in this analysis focuses on the factors that produce stable employment. In the previous chapter, we examined the survival functions for job spells, finding that—across sites and over time—even when survival curves differed substantially, they seldom crossed. This implies that the relative stability of groups of spells can be ranked by average length. In the analysis here, our measure of job stability will be the length of the job spell, measured in quarters, truncated at eight quarters. Among welfare job spells, about half of all spells last just one quarter, while fewer than 1 in 10 lasts for eight quarters or more. For some of our analyses we also examine the total earnings during the life of a job, up to eight quarters, with earnings adjusted for inflation.[1]

Table 5.1 provides means and standard deviations for the sample of spells that we will be examining at each of the sites. Recall that an individual can contribute more than one spell to this sample, and spells for a given person may be overlapping. The mean spell length varies from 2.06 quarters in Kansas City to 2.65 quarters in Chicago, with a standard deviation of around 2 in all sites.[2] There are also substantial differences in earnings across sites, with rankings corresponding largely to those of spell length.

Means for age and number of children in a family and the proportion with high school degree (where available) are remarkably similar across the sites.[3] In contrast, the proportion of the spells coded minority differs across sites, ranging from 75 percent in Kansas City to 97 percent in Atlanta. Equally important, although not identified in the table, in Atlanta, Baltimore, and Kansas City, almost all of the minority recipients are black, whereas in Chicago, Houston, and Fort Lauderdale, a substantial portion is Hispanic.

The lower portion of Table 5.1 provides information on the industrial structure of employment for each of the sites. The industries iden-

tified correspond to one-digit Standard Industrial Classification (SIC) codes, except that certain detailed industries have been separated out because they each account for a relatively large share of welfare recipients. In particular, employment for SIC major group 58, eating and drinking places, has been tabulated separately, accounting for between 9 and 17 percent of all spells at our sites; the remainder of retail trade accounts for up to 19 percent of spells. In all sites, upwards of half of recipients' spells are in firms classified as service, so we have divided service industry jobs into four categories: hotels and motels, temporary help firms, nursing homes, and other service firms.

There are substantial differences among the sites in the industrial structure of jobs. Temporary help service firms account for more than one in five spells in Atlanta and Kansas City, but the proportion is as low as 1 in 10 in other sites. Overall, however, the basic employment structure of the sites is quite similar. One exception is Chicago, where use of an alternative industrial classification code made it difficult for us to translate the code into the SIC categories.[4] As a result, we were unable to determine industry in over a third of the spells, and these are simply classified together as "not ascertained."

Demographic Determinants of Job Stability

Table 5.2 presents coefficients for regression equations predicting spell length at each of the six sites. Both age and education have the expected effects. Those with high school degrees have spells that are up to a fifth of a quarter longer in the sites where it is available. Older individuals have longer job spells, although in Atlanta, Baltimore, and Kansas City, the effect of age increases at older ages, whereas it decreases in Chicago and Fort Lauderdale.[5] Nonetheless, the overall effects are similar, with coefficients implying that at age 35 predicted spell length is 0.25 to 0.34 quarters longer than that for an individual with similar characteristics who is age 25. The effect of children is estimated to be negative at all sites, but it is substantively small for each site.

Other variables display somewhat different impacts across sites. The effect of minority status has the expected negative sign in Atlanta, Chicago, and Kansas City, but is essentially zero in Baltimore and positive and substantial in Fort Lauderdale and Houston. The effect of being a long-term recipient is negative in Atlanta, Baltimore, Fort Lauderdale,

Table 5.1 Means and Standard Deviations for Spells across Six Sites

	Atlanta 1994:1–1998:4		Baltimore 1992:1–1998:1		Chicago 1995:3–1998:4		Fort Lauderdale 1995:1–1998:1		Houston 1994:4–1998:4		Kansas City 1992:1–1998:1	
Variable	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Length of spell (up to 8 quarters)	2.12	1.83	2.50	2.13	2.65	2.25	2.43	2.04	2.35	1.99	2.06	1.78
Total earnings (1999:4 dollars)	2,676	7,812	4,871	9,378	4,974	9,175	4,084	7,763	3,900	7,604	3,243	7,069
Minority (nonwhite or Hispanic)	0.97	0.17	0.91	0.29	0.91	0.29	0.80	0.40	0.84	0.37	0.75	0.43
Age	28.81	7.49	29.19	7.51	28.92	7.17	28.98	7.13	27.97	7.41	27.86	7.13
Age^2	885.96	491.97	908.45	497.55	887.28	458.44	890.44	460.29	837.40	480.08	827.00	457.71
Number of children	2.05	1.22	1.80	0.99	2.01	1.19	2.01	1.16	1.96	1.11	1.97	1.15
High school graduate	—	—	—	—	0.58	0.49	—	—	0.61	0.49	0.60	0.49
Long-term recipient	0.51	0.50	0.51	0.50	0.56	0.50	0.27	0.44	0.31	0.46	0.38	0.48
Industry												
1. Eating, drinking estab.	0.17	0.38	0.10	0.29	0.14	0.35	0.11	0.31	0.17	0.38	0.09	0.29
2. Agriculture, mining, const.	0.01	0.09	0.01	0.10	0.01	0.07	0.02	0.13	0.02	0.14	0.01	0.10
3. Manufacturing	0.02	0.16	0.05	0.22	0.03	0.18	0.03	0.18	0.02	0.15	0.04	0.20
4. Transportation, etc.	0.03	0.17	0.02	0.14	0.02	0.15	0.03	0.17	0.02	0.14	0.03	0.17
5. Wholesale trade	0.02	0.15	0.02	0.13	0.01	0.11	0.03	0.16	0.02	0.13	0.04	0.19
6. Retail trade, not eating estab.	0.14	0.35	0.16	0.37	0.11	0.31	0.16	0.37	0.19	0.39	0.13	0.34

7. Finance, ins., real estate	0.02	0.12	0.02	0.15	0.02	0.14	0.03	0.18	0.02	0.14	0.03	0.16
8. Hotels, motels	0.05	0.22	0.03	0.17	0.01	0.11	0.03	0.16	0.02	0.13	0.04	0.20
9. Temporary help firms	0.21	0.41	0.17	0.37	0.09	0.29	0.13	0.34	0.16	0.37	0.22	0.41
10. Skilled nursing, inter. care	0.03	0.16	0.05	0.23	0.05	0.21	0.02	0.13	0.02	0.15	0.07	0.26
11. Other service	0.26	0.44	0.34	0.48	0.19	0.39	0.30	0.46	0.34	0.47	0.29	0.45
12. Public administration	0.02	0.15	0.03	0.16	0.01	0.08	0.11	0.32	0.00	0.07	0.01	0.08
13. Industry not ascertained	0.02	0.15	0.00	0.00	0.31	0.46	0.00	0.00	0.00	0.04	0.00	0.00
Number of spells	85,382		100,963		281,479		26,127		142,754		95,809	

NOTE: — = data unavailable

Table 5.2 Estimated Impacts of Welfare Recipients' Characteristics on Job Spell Length

	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
Independent variables	Beta	Beta	Beta	Beta	Beta	Beta
Minority (nonwhite or Hispanic)	−0.098	0.009	−0.178	0.119	0.206	−0.101
	(0.038)	(0.023)	(0.015)	(0.032)	(0.014)	(0.013)
Age	0.020	−0.000	0.065	0.085	0.039	0.004
	(0.005)	(0.006)	(0.005)	(0.012)	(0.004)	(0.006)
Age^2	0.000	0.001	−0.001	−0.001	−0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of children	−0.021	−0.004	−0.010	−0.029	−0.016	−0.008
	(0.005)	(0.007)	(0.004)	(0.012)	(0.005)	(0.005)
High school graduate	—	—	0.227	—	0.107	0.222
			(0.009)		(0.011)	(0.012)
Long-term recipient	−0.073	−0.035	0.163	−0.149	−0.043	0.057
	(0.013)	(0.014)	(0.009)	(0.029)	(0.012)	(0.012)
R^2	0.0221	0.0160	0.0188	0.0147	0.0185	0.0202
Adjusted R^2	0.0218	0.0157	0.0187	0.0143	0.0184	0.0198

NOTE: Also controlled: quarter dummies. Standard errors are in parentheses. — = data unavailable.

and Houston; it is positive although small in Kansas City and strongly positive in Chicago.

It is natural to ask whether differences across sites in observed recipient characteristics are responsible for variation across sites in spell lengths. We used the coefficients reported in Table 5.2 for the regressions at each site to identify the extent to which mean differences in the independent variables for a given site could explain the deviation from the mean spell length across sites. Spell lengths in Kansas City and Chicago differ most dramatically, by nearly 0.6 quarters, but our calculations show that essentially none of this difference can be explained by the demographic factors controlled in this equation.

The impact of the basic demographic variables is expected to operate, in part, through the kinds of jobs that individuals obtain. We discuss the industrial composition of jobs as well as other differences across employers in some detail below, but at this point it is worth noting how the estimated effects of the basic demographic variables change when we control for employer.[6] In all of our sites, part of the effect of age is mediated by employer, but at least two-thirds of the effect remains. Similarly, in our sites where we have information on education, we find that about a third of its effect works through employer. For the other measures, the extent to which the effect operates through employer is variable, although the basic pattern of results is not changed dramatically by controls for employer.

EMPLOYER AND INDUSTRY AS DETERMINANTS OF JOB STABILITY

What role do industry and employer play in determining the job stability of welfare recipients? Table 5.3 shows impact estimates for all 12 industries, where each identifies an effect relative to the average industry.[7] The regressions control for minority status, age and age squared, and number of children, allowing full comparability across sites. In those sites where data for education and long-term welfare status are available, the coefficients of the industry variables change very little, generally much less than their standard errors, when these are controlled, so it is clear that the results would not be altered by including these additional measures.

Table 5.3 Estimated Impacts of Major Industry on Job Spell Length

Independent variable	Atlanta Beta	Baltimore Beta	Chicago Beta	Fort Lauderdale Beta	Houston Beta	Kansas City Beta
Industry						
1. Eating, drinking estab.	−0.052 (0.013)	−0.164 (0.019)	0.171 (0.010)	−0.110 (0.034)	−0.089 (0.011)	−0.072 (0.016)
2. Agriculture, mining, const.	−0.316 (0.068)	−0.616 (0.064)	−0.304 (0.057)	−0.484 (0.087)	−0.196 (0.036)	−0.205 (0.050)
3. Manufacturing	0.411 (0.038)	0.001 (0.026)	0.520 (0.022)	0.151 (0.066)	0.329 (0.033)	0.213 (0.025)
4. Transportation, etc.	0.537 (0.035)	0.478 (0.044)	0.855 (0.027)	0.401 (0.069)	0.452 (0.037)	1.214 (0.029)
5. Wholesale trade	0.349 (0.040)	0.012 (0.046)	0.645 (0.038)	0.253 (0.073)	0.290 (0.039)	−0.135 (0.027)
6. Retail trade, not eating estab.	−0.031 (0.015)	−0.174 (0.014)	0.158 (0.012)	0.138 (0.027)	0.006 (0.011)	0.028 (0.013)
7. Finance, ins., real estate	1.090 (0.048)	0.560 (0.040)	1.307 (0.030)	0.394 (0.065)	0.568 (0.037)	0.737 (0.031)

8. Hotels, motels	0.168	−0.349	0.375	−0.332	0.010	−0.386
	(0.027)	(0.034)	(0.036)	(0.072)	(0.039)	(0.025)
9. Temporary help firms	−0.559	−0.818	−0.804	−0.719	−0.608	−0.535
	(0.012)	(0.014)	(0.013)	(0.031)	(0.012)	(0.010)
10. Skilled nursing, inter. care	0.166	0.455	−0.176	0.345	−0.192	−0.065
	(0.038)	(0.026)	(0.019)	(0.090)	(0.035)	(0.019)
11. Other service	0.165	0.290	0.467	0.281	0.224	0.237
	(0.010)	(0.009)	(0.008)	(0.018)	(0.007)	(0.008)
12. Public administration	1.332	1.852	1.034	−0.237	2.423	1.536
	(0.041)	(0.038)	(0.053)	(0.034)	(0.077)	(0.064)
13. Industry not ascertained	−0.080		−0.424		0.064	
	(0.039)		(0.006)		(0.117)	
R^2	0.066	0.073	0.062	0.042	0.047	0.068
Adjusted R^2	0.066	0.072	0.062	0.042	0.047	0.068

NOTE: Also controlled: minority, age, age squared, number of children, and quarter dummies. Beta values are estimated impacts relative to the average industry. Standard errors are in parentheses.

Despite some differences across sites, the patterns of coefficients are remarkably similar. Those working in agriculture, mining, and construction have lower job stability than the average, but these industries are relatively unimportant, since fewer than 2 percent of the spells fall in this category. Manufacturing jobs, which make up between 2 and 5 percent of all spells, are appreciably more stable than other jobs at five of the six sites, with spells 0.2 to 0.5 quarters longer.

The two major industries that display the greatest job stability are transportation, communications, electric, gas, sanitary services, and public administration (except in Fort Lauderdale). Each of these makes up a very small portion of the spells at most sites. The exception is that in Fort Lauderdale, public administration makes up 11 percent of spells, in contrast to proportions below 4 percent at all other sites, suggesting that this category may be defined differently there.

The effect of jobs in wholesale trade is not consistent across sites. The very high stability that Chicago workers in wholesale trade experience appears less significant when it is observed that fewer than 1 percent of Chicago workers are in such jobs, as compared with 2 percent at other sites. Retail trade outside of eating and drinking establishments offers greater stability than the average in Chicago, Fort Lauderdale, and Kansas City, but the effect on stability is small at two of the other sites, and negative at one.

Of particular interest is the job stability offered by service firms, which contribute a very large share of welfare recipients' jobs in all sites. Temporary help service firms alone make up 9 to 22 percent of all spells in the six sites. Stability is lower in that category than any other, with spells at least half a quarter shorter than the average and a full quarter shorter than jobs in manufacturing. This is a substantial difference, given that the mean spell length is under 3. Among other service jobs, nursing homes and hotels and motels display inconsistent patterns across sites. On the other hand, the residual category of service jobs provides substantially greater levels of job stability at all sites, spells being longer than the average by 0.2 to 0.5 quarters. Eating and drinking establishments have slightly lower stability than the industry average in all sites but Chicago, where such jobs appear to be more stable than the average.

While the differences observed among these job categories are large, one may wonder whether using more detailed industry categories

would alter the results. In order to test this, we considered a variety of detailed controls for employer industry. Table 5.4 reports the proportion of variance explained (as indicated by adjusted R^2) for various models predicting length of job spell. When only the basic demographic characteristics are controlled, just 1 to 2 percent of the variance is explained by the model (line 1). Controlling for major industry (categories listed in Table 5.3), the explained variance is 4 to 7 percent (line 3). Lines 2 and 4 control for long-term recipiency, and, for the three sites where it is available, the indicator of high school graduation. These results suggest that including these variables would not alter our conclusions in any important way.

Line 5 shows that controlling for two-digit industry increases explained variance to between 8 and 11 percent, and line 6 shows that controlling for four-digit industry increases explained variance another two percentage points. It is clear that although the broad industry categories capture much of the differences between detailed industries, finer gradations are still of significance.[8]

Although line 6 provides controls for industry as defined by the most detailed measure available in our dataset, there may be other differences between employers not captured by SIC code. Line 7 in Table 5.4 presents adjusted R^2 in a model that controls for employer fixed effects, equivalent to fitting a separate dummy variable for each employer in the sample. Explained variance increases quite substantially, to around 20 percent at all sites. Still, more than half of the explained variance due to employers is explained by four-digit industry, and more than a third is explained by two-digit industry.

Overall, our conclusion is clear. Even after controls for recipient characteristics, the industry of the employer and other employer characteristics play an important role in predicting employment stability. Of course, we expect there are important unmeasured recipient characteristics that influence job stability and also influence the type of job. Insofar as these are important, we expect that job type, per se, is less important than these analyses suggest.

Table 5.4 Proportion of Variance Explained for Models Predicting Spell Length

	Independent variables								Adjusted R^2					
Model	Industry/ employer controls	Quarter dummies	Minority	Age, age^2	Children	High school[b]	Long-term	All person effects	Atlanta	Baltimore	Chicago	Fort Lauderdale	Houston	Kansas City
(1)	None	X	X	X	X				0.021	0.016	0.016	0.013	0.018	0.019
(2)	None	X	X	X	X	X	X		0.022	0.016	0.019	0.014	0.018	0.020
(3)	Major industry	X	X	X	X				0.066	0.072	0.062	0.042	0.048	0.068
(4)	Major industry	X	X	X	X	X	X		0.066	0.073	0.064	0.042	0.048	0.068
(5)	2-digit industry and selected others[a]	X	X	X	X				0.093	0.111	0.078	0.083	0.077	0.100
(6)	4-digit industry	X	X	X	X				0.119	0.135	0.126	0.106	0.096	0.127
(7)	Employer	X	X	X	X				0.200	0.224	0.218	0.213	0.173	0.198
(8)	None	X			X			X	0.270	0.263	0.282	0.348	0.258	0.253
(9)	Major industry	X			X			X	0.294	0.293	0.306	0.360	0.277	0.280
(10)	2-digit industry and selected others[a]	X			X			X	0.312	0.319	0.315	0.378	0.297	0.302

[a] Selected industry detail SIC code:

53 5311 (dept. store)
53 all other
54 5411 (grocery stores)
54 all other
55 5541 (gasoline stations)
55 all other (automotive and boat sales)
70 7011 (motel, hotel)
70 all other
72 721 (laundry, dry cleaning)
72 all other
80 8051 (skilled nursing care)
8052 (intermediate care)
8062 (hospitals)
808 (home health care)
80 all other
83 8322 (individual and family social services)
8361 (residential care)
83 all other

[b] High school is available only for Chicago, Houston, and Kansas City.

PERSON FIXED EFFECTS

In order to address this issue, we estimated regression equations where fixed effects for each individual in the sample are controlled. Such a model estimates the effect of industry on the basis of those welfare recipients who have multiple job spells. Of course, one might imagine this is a fairly unusual population. In order for an individual to have multiple spells, it is necessary that the initial job did not result in a sufficiently good match to induce the individual to discontinue welfare receipt. For that reason, one might anticipate that differences between industries for this selected population would understate differences across all individuals.

Table 5.5 presents estimates of the effects of industry using this method to control for all person effects. Omitted from this specification are the basic demographic characteristics that do not change over time. Number of children and the quarter in which the spell starts are controlled. Age is excluded, since the effect of age and the quarter in which the spell begins are not separately identified in this fixed effects specification.[9]

Remarkably, industry effects reported in Table 5.5 closely parallel those obtained when simple demographic characteristics are controlled, with impacts only slightly reduced. For example, if we compare temporary help and manufacturing firms, we find that spells in temporary help firms are between 0.5 and 0.8 quarters shorter. This gap is only about 20 percent smaller than that obtained when only basic demographic factors are controlled. Consistent with the prior results, we find that public administration jobs are generally the most stable (again, with the exception of those in Fort Lauderdale), with jobs in transportation and related industries only slightly less stable.

It is worth stressing that these results are consistent with the view that individual characteristics play an important role in determining job stability, independent of job type. Lines 8–10 in Table 5.4 show that, in models controlling for individual fixed effects, the proportion of variance explained increases dramatically relative to other models. Individual effects are more important than firm effects at all sites; hence, knowing a recipient's job history is more important than knowing her employer's reputation for retaining workers. On the other hand, the difference in explained variance is less than one might expect. Knowing

Table 5.5 Estimated Impacts of Major Industry on Job Spell Length, Controlling Person Effects

Independent variable	Atlanta Beta	Baltimore Beta	Chicago Beta	Fort Lauderdale Beta	Houston Beta	Kansas City Beta
Industry dummies						
1. Eating, drinking estab.	0.062 (0.015)	−0.053 (0.024)	0.161 (0.013)	−0.019 (0.046)	−0.021 (0.014)	0.005 (0.018)
2. Agriculture, mining, const.	−0.354 (0.076)	−0.547 (0.080)	−0.147 (0.070)	−0.306 (0.116)	−0.103 (0.044)	−0.215 (0.055)
3. Manufacturing	0.345 (0.042)	−0.045 (0.033)	0.474 (0.027)	0.241 (0.087)	0.289 (0.039)	0.138 (0.028)
4. Transportation, etc.	0.418 (0.039)	0.449 (0.054)	0.705 (0.034)	0.318 (0.091)	0.360 (0.044)	1.061 (0.033)
5. Wholesale trade	0.225 (0.045)	0.023 (0.057)	0.594 (0.046)	0.130 (0.097)	0.225 (0.047)	−0.118 (0.030)
6. Retail trade, not eating estab.	−0.070 (0.017)	−0.240 (0.017)	0.035 (0.014)	0.060 (0.036)	−0.029 (0.013)	−0.004 (0.015)
7. Finance, ins., real estate	0.840 (0.054)	0.425 (0.050)	1.069 (0.036)	0.296 (0.086)	0.480 (0.044)	0.480 (0.035)

8. Hotels, motels	0.171 (0.030)	−0.067 (0.043)	0.266 (0.044)	−0.270 (0.095)	−0.007 (0.047)	−0.138 (0.028)
9. Temporary help firms	−0.447 (0.013)	−0.602 (0.017)	−0.546 (0.016)	−0.487 (0.041)	−0.514 (0.014)	−0.416 (0.011)
10. Skilled nursing, inter. care	0.130 (0.042)	0.441 (0.032)	−0.016 (0.023)	0.339 (0.119)	−0.093 (0.041)	0.040 (0.021)
11. Other service	0.098 (0.011)	0.195 (0.011)	0.314 (0.010)	0.159 (0.024)	0.171 (0.009)	0.131 (0.009)
12. Public administration	1.032 (0.046)	1.635 (0.047)	0.768 (0.064)	−0.140 (0.045)	2.287 (0.092)	1.417 (0.071)
13. Industry not ascertained	−0.111 (0.044)		−0.337 (0.008)		−0.022 (0.140)	
R^2	0.490	0.563	0.589	0.703	0.540	0.455
Adjusted R^2	0.294	0.293	0.306	0.360	0.277	0.280

NOTE: Controls: quarter dummies, number of children, person fixed effects. Beta values are estimated impacts relative to the average industry. Standard errors are in parentheses.

only the employer allows one to explain about a fifth of the variance in job stability, whereas knowing only about the employee explains about a quarter.

To what degree do individual differences play a role in determining what kinds of jobs individuals obtain? If, in fact, industry patterns that predict stability are largely determined by individual differences (e.g., if each individual who obtains multiple jobs is very likely to obtain those jobs in a given industry), controlling for industry will add relatively little explanatory power after individual effects are controlled. In this case, we could say that certain industries appear to offer stable employment because they hire stable people. Although the pattern of explained variances in Table 5.4 confirms the viewpoint that stable industries do in fact attract more stable individuals—the contribution of industry to explained variance is smaller when individual effects are controlled than when not—the extent of this effect is quite small. Most of the explanatory power of industry predicting job stability is independent of individual effects. This finding provides the strongest evidence that getting the "right" job is critical in assuring job stability for welfare recipients.[10]

As noted above, there are significant differences between sites in the average job spell length, with Kansas City displaying the shortest mean at 2.06 and Chicago the longest at 2.65. If we examine the distribution of jobs by industry (Table 5.1), it is clear that Kansas City has a much larger share of jobs in temporary help firms, the industry category with the lowest level of stability. Using the estimates of the impact of job type on stability reported in Table 5.5, we have predicted the extent to which these differences in industrial distribution are responsible for mean differences in spell length. We find that for Houston and Kansas City, the spell length is reduced by 0.05 to 0.06 because of the industrial distributions in these sites, whereas, for the other sites, the adjustment is less than 0.02.[11] Hence, industry—at least as gauged by broad categories—plays a relatively small role in explaining observed differences between sites.

CHANGES OVER TIME IN JOB SPELL LENGTH AND ITS DETERMINANTS

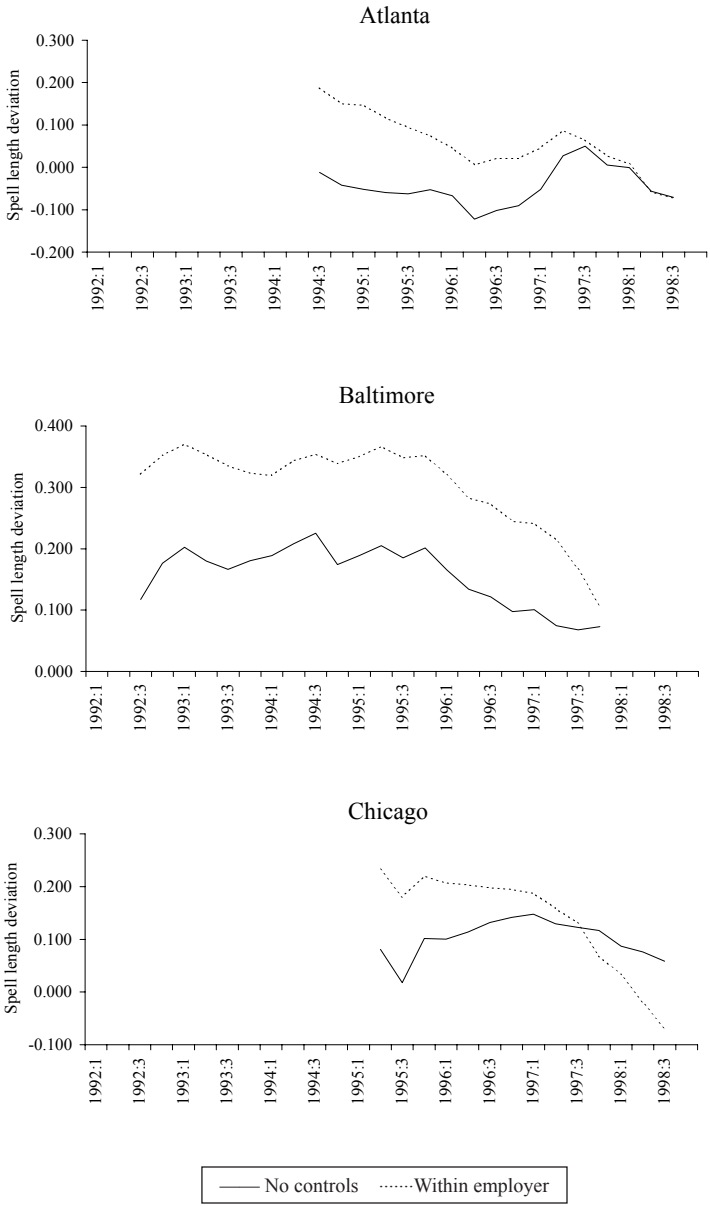
Over the 1990s, major policy reforms occurred in welfare policies and programs at each site, accompanied by dramatic improvements in local economic and labor market conditions. The previous chapter provided evidence of declines in job spell length at several sites among welfare recipients. Here, we consider this decline, identifying the extent to which it can be explained by changes in the characteristics of welfare recipients and the kinds of employers who hire them. We also consider the extent to which the factors explaining spell length have shifted over time.

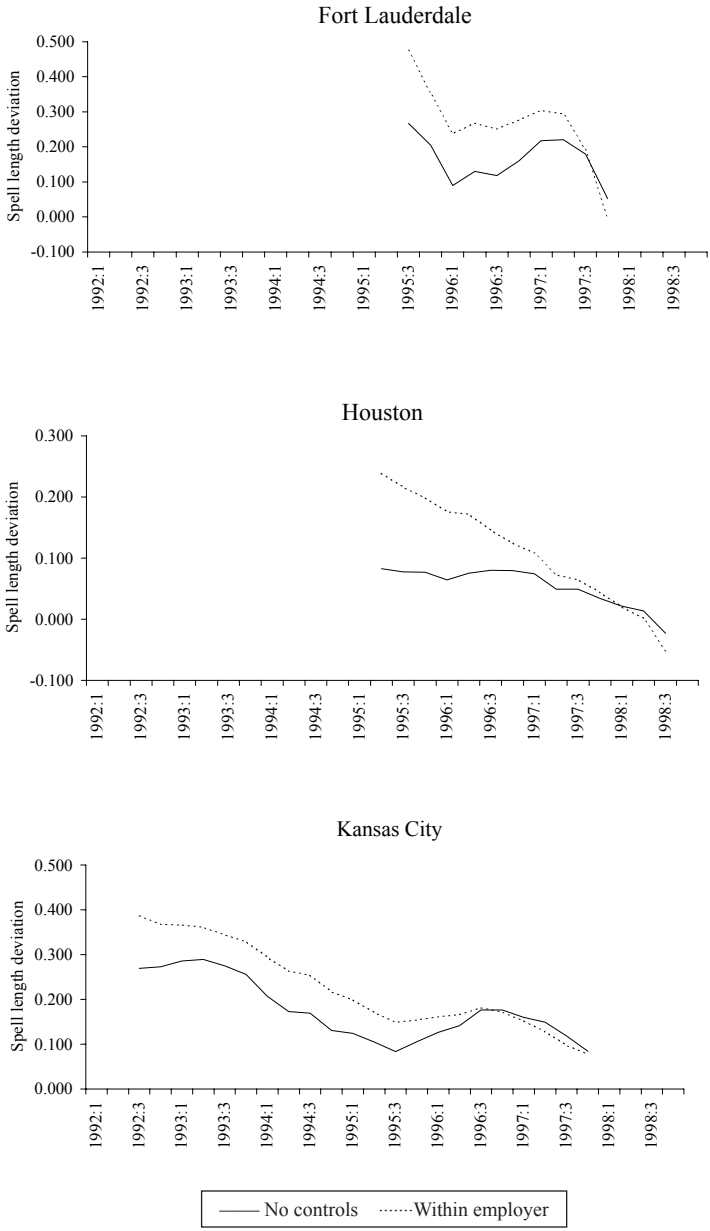
When we examine quarter coefficients predicting spell length, we find that there are consistent seasonal effects. Although there are some differences across sites, the basic patterns are remarkably similar. In all sites, job spells beginning in the fourth quarter are shorter than others, whereas those beginning in the first quarter are generally longer. Since employment and the number of new jobs are generally low in the first quarter (see Chapter 4), it should not be surprising if those who obtained such jobs are more likely to keep them. In contrast, jobs in the final quarter of the year may be temporary, reflecting retail job expansion in the holiday season. Despite the consistency, the size of these seasonal differences is modest. Controlling for individual characteristics, spells beginning in the first quarter of the year last about 0.2 quarters longer than those beginning in the fourth quarter at most sites, although the difference is as small as 0.1 (in Houston) and as large as 0.3 (in Chicago).

In order to account for seasonal effects and to reduce variation from quarter to quarter, Figure 5.1 graphs the four-quarter moving average of mean spell length for our sites over the 1990s.[12] The scale identifies spell length relative to that for 1997:4, and the solid line indicates spell length with no controls. We see that at each site, the spell length declines moderately over at least the latter portion of our period, with declines in the range of 0.1 to 0.2.

Since the population of recipients is changing over this period, one might ask to what extent differences can be traced to characteristics of recipients and the jobs they obtain. When we calculated quarter effects controlling for basic demographic characteristics and industry of em-

Figure 5.1 Spell Length Variation over Time




Fort Lauderdale
Spell length deviation
0.500
0.400
0.300
0.200
0.100
0.000
-0.100
1992:1
1992:3
1993:1
1993:3
1994:1
1994:3
1995:1
1995:3
1996:1
1996:3
1997:1
1997:3
1998:1
1998:3
Houston
Spell length deviation
0.300
0.200
0.100
0.000
-0.100
1992:1
1992:3
1993:1
1993:3
1994:1
1994:3
1995:1
1995:3
1996:1
1996:3
1997:1
1997:3
1998:1
1998:3
Kansas City
Spell length deviation
0.500
0.400
0.300
0.200
0.100
0.000
1992:1
1992:3
1993:1
1993:3
1994:1
1994:3
1995:1
1995:3
1996:1
1996:3
1997:1
1997:3
1998:1
1998:3
No controls
Within employer

ployer (as in regressions specified in lines 1–6 in Table 5.4), we found that the pattern reported in Figure 5.1 changed little, implying that these factors did not contribute much to observed patterns. Further controls for fixed employer effects, however (as in line 7 in Table 5.4), altered estimated trends. Figure 5.1 shows that when employer is taken into account, declines in job stability are greater, as indicated by the dashed line. This implies that recipients are more likely to obtain jobs with employers offering greater stability in the recent period, which served to lessen actual stability declines.

In order to examine whether the determinants of job stability have shifted, we present in Table 5.6 separate regressions for each of the sites predicting stability for spells beginning at different points in time as a function of demographic characteristics. Changes over time in effects appear to be idiosyncratic to each site. In Houston, the positive effect of minority status appears to decline, while the negative effect in Chicago increases. In Fort Lauderdale, the effect of age (as gauged by the difference between the ages of 25 and 35) increases by more than 50 percent in the most recent period, while the effect changes relatively little at other sites. In Kansas City, the effect of completing high school declines dramatically in the last year, but there are no similar changes in the other sites where education is available.

Nationally as well as in our sites, welfare reform emphasized the importance of employment for long-term recipients as a means of reducing welfare dependency. This focus may be reflected in the estimated effect of being on welfare for two years or more, our measure of long-term recipiency. In three of the sites, the estimated coefficient for this measure is positive in the final period and is appreciably greater than that estimated for earlier periods. The most extreme case is in Baltimore, where recipients on welfare at least two years in the final period have spells that last 0.22 quarters longer than similar individuals, whereas in the initial period their spells are actually 0.07 quarters shorter. However, in Fort Lauderdale, long-term recipients actually have shorter job spells than others and the effect may be increasing; in the other two sites, long-term recipiency has little effect.

Table 5.6 Estimated Impacts of Welfare Recipients' Characteristics on Job Spells for Spells Beginning in Various Periods

		Period job spell begins		
		1992–95	1996–97	1998
	Independent variables	Beta	Beta	Beta
Atlanta	Minority (nonwhite or Hispanic)	−0.081 (0.055)	−0.048 (0.059)	−0.343 (0.109)
	Age	0.000 (0.000)	0.028 (0.008)	0.020 (0.013)
	Age^2	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
	Number of children	−0.021 (0.008)	−0.020 (0.008)	−0.029 (0.013)
	Long-term recipient	−0.083 (0.019)	−0.077 (0.020)	−0.043 (0.033)
	R^2	0.019	0.025	0.025
	Adjusted R^2	0.018	0.024	0.025
Baltimore	Minority (nonwhite or Hispanic)	0.029 (0.030)	−0.022 (0.039)	−0.052 (0.127)
	Age	−0.007 (0.007)	0.024 (0.010)	0.032 (0.036)
	Age^2	0.001 (0.000)	0.000 (0.000)	−0.000 (0.001)
	Number of children	−0.011 (0.009)	0.006 (0.011)	−0.012 (0.034)
	Long-term recipient	−0.066 (0.018)	−0.006 (0.022)	0.222 (0.073)
	R^2	0.019	0.011	0.010
	Adjusted R^2	0.019	0.011	0.008
Chicago	Minority (nonwhite or Hispanic)	−0.160 (0.036)	−0.159 (0.020)	−0.228 (0.028)
	Age	0.083 (0.012)	0.069 (0.006)	0.046 (0.008)
	Age^2	−0.001 (0.000)	−0.001 (0.000)	−0.000 (0.000)
	Number of children	−0.010 (0.011)	−0.013 (0.005)	−0.005 (0.007)

Table 5.6 (continued)

		Period job spell begins		
		1992–95	1996–97	1998
	Independent variables	Beta	Beta	Beta
Chicago				
	High school graduate	0.255 (0.023)	0.215 (0.012)	0.231 (0.016)
	Long-term recipient	0.041 (0.023)	0.157 (0.012)	0.239 (0.017)
	R^2	0.016	0.018	0.022
	Adjusted R^2	0.015	0.018	0.022
Fort Lauderdale	Minority (nonwhite or Hispanic)	0.160 (0.066)	0.114 (0.038)	0.018 (0.128)
	Age	0.088 (0.026)	0.083 (0.015)	0.098 (0.044)
	Age^2	−0.001 (0.000)	−0.001 (0.000)	−0.001 (0.001)
	Number of children	−0.054 (0.025)	−0.024 (0.014)	−0.001 (0.040)
	Long-term recipient	−0.086 (0.059)	−0.175 (0.035)	−0.148 (0.122)
	R^2	0.011	0.015	0.026
	Adjusted R^2	0.010	0.014	0.023
Houston	Minority (nonwhite or Hispanic)	0.256 (0.023)	0.189 (0.021)	0.119 (0.038)
	Age	0.040 (0.007)	0.034 (0.007)	0.047 (0.010)
	Age^2	−0.000 (0.000)	−0.000 (0.000)	−0.000 (0.000)
	Number of children	−0.013 (0.009)	−0.021 (0.007)	−0.005 (0.011)
	High school graduate	0.086 (0.018)	0.122 (0.016)	0.118 (0.026)
	Long-term recipient	−0.097 (0.019)	−0.011 (0.017)	−0.003 (0.029)
	R^2	0.018	0.019	0.019
	Adjusted R^2	0.018	0.019	0.019

		Period job spell begins		
		1992–95	1996–97	1998
	Independent variables	Beta	Beta	Beta
Kansas City	Minority (nonwhite or Hispanic)	−0.100 (0.017)	−0.110 (0.022)	−0.020 (0.071)
	Age	0.008 (0.007)	−0.008 (0.010)	0.064 (0.032)
	Age^2	0.000 (0.000)	0.001 (0.000)	−0.001 (0.001)
	Number of children	−0.008 (0.007)	−0.003 (0.008)	−0.047 (0.028)
	High school graduate	0.220 (0.016)	0.241 (0.020)	0.076 (0.064)
	Long-term recipient	0.032 (0.016)	0.094 (0.020)	0.120 (0.065)
	R^2	0.071	0.019	0.010
	Adjusted R^2	0.021	0.019	0.008

NOTE: Standard errors are in parentheses.

EARNINGS ON A JOB

Our focus to this point has been on the stability of a job, as measured by the number of quarters an individual remains with a given employer, but it is clear that, in any attempt to assure self-sufficiency, earnings would be an important measure of employment success. As a measure of the overall benefits from a job, we have summed the total earnings obtained from the employer during the job spell for its first eight quarters and have fitted regressions that predict this measure.

Of course, we expect total earnings to be strongly correlated with the duration of the job, although the variation in total pay is appreciably greater than the variation in spell length. Table 5.1 indicates that the standard deviation of total earnings is about twice its mean, whereas the standard deviation of spell length is generally slightly less than its mean. Variation in hourly wage, hours of work per week, and weeks of work per quarter cause the two measures to differ, so there is no certainty that regressions for spell length and earnings will yield the same results in all analyses.

Table 5.7 presents coefficients for the industry of a job in regressions predicting total earnings. Estimates in the column on the left for each site include controls for our limited set of demographic characteristics; estimates in the column on the right control for person fixed effects. The pattern of coefficients is very similar to that predicting spell length. One notable exception is that spells in the category agriculture, mining, and construction (almost all in construction), which are generally shorter than those in other industries, provide earnings that are substantially higher than the average industry in three sites. This is consistent with the view that such jobs tend to provide higher wages in order to attract skilled individuals despite their obvious instability.

The most important result of this analysis is that industry effects on total earnings survive the inclusion of person effects, and that the basic pattern of coefficients does not change. At almost all of the sites, the coefficients decline in absolute value, indicating that some of the differences between industries are explained by person effects. Given that person effects explain more of the variation in earnings than in spell length (compare the estimates of adjusted R^2 in Table 5.7 with those in Table 5.5), it should not be surprising that some of the differences between jobs are also explained by person effects. Yet, our general conclusions for earnings are the same as for stability: When an individual gets the right kind of job, she experiences appreciably greater rewards.

Although the above conclusions suggest little difference between analyses using spell length and total earnings, their trends over time appear to differ somewhat. Figure 5.2 presents mean total earnings in each of the sites, based on when the spell begins. As in the case of spell length (Figure 5.1), the numbers represent deviations from mean total earnings in 1997:4, with four-quarter moving averages plotted in the figure. In Kansas City, average earnings per spell decline by nearly $400 from the early 1990s to 1995, but then increase to their previous level by the end of the period. This is in contrast to the trend in spell length, which shows an overall decline during the period. Controls for employer reveal a decline in total earnings, suggesting that part of the earnings gain is due to workers getting jobs with higher-paying employers. It should be stressed that earnings are adjusted for inflation, so changes are in real terms.

Houston and Chicago, like Kansas City, exhibit increases in total earnings even while spell length declines, indicating that workers are

receiving greater earnings in each quarter they work. Controls for employer reduce (Chicago) or reverse (Houston) this trend, suggesting that part of the observed increases in earnings at these sites is due to recipients obtaining jobs with higher-paying employers. Only in Baltimore does the trend for total earnings closely parallel that for spell length.

It may be of interest to consider the extent to which industrial differences in spell length parallel differences in earnings per quarter. We have calculated expected earnings for each of the major industry groupings as well as expected spell length, controlling person fixed effects, number of children, and quarter dummies. Since actual time worked during both the initial quarter and the final quarter of a spell will usually not extend the full length of the quarter, the predicted spell length, as we have measured it, generally overstates the actual continuous time that a job lasted. In calculating earnings per quarter, we have therefore adjusted spell length to represent continuous time.[13]

Figure 5.3 provides a scatter plot for each site of the expected length of a spell by the expected earnings per quarter for our major industry groupings. Although there is clearly variation around the trend line, the relationship is positive at each site. It is clear that if welfare recipients are placed in industries providing them with stable employment they also tend to have high earnings per quarter. (In each site, the industry category that contains construction workers is an obvious outlier, yielding relatively high earnings per quarter but low job stability.) Of course, our analyses do not allow us to identify the reason for the effect, and it may well reflect the incentives of workers to keep jobs that pay well. The important point is that we see no evidence that there is a trade-off between jobs that pay well and those that provide stability.

CONCLUSIONS

Job stability is an important factor in efforts to achieve self-sufficiency for welfare recipients. The analysis here shows that differences in job stability can be attributed both to individual characteristics and to the kinds of jobs individuals obtain. After controls for employer, both age and education have positive effects on stability, while the number of children generally has a negative effect. We find, in addition, that

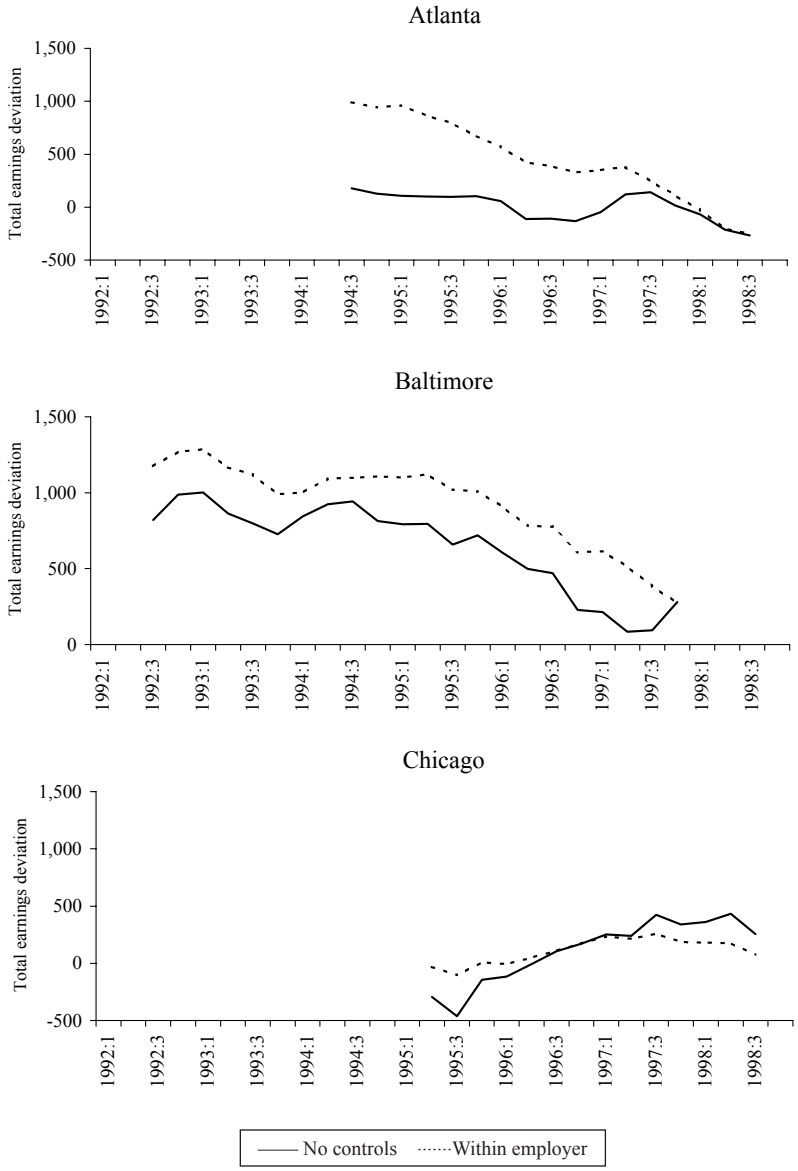
Table 5.7 Estimated Impacts of Welfare, by Major Industry, on Total Earnings in Job

	Atlanta		Baltimore		Chicago		Fort Lauderdale		Houston		Kansas City	
Independent variables	Beta	Beta	Beta	Beta	Beta	Beta	Beta	Beta	Beta	Beta	Beta	Beta
Industry dummies												
1. Eating, drinking estab.	−829	−134	−1,762	−979	−438	16	−1,407	−508	−1,193	−616	−1,043	−464
	(57)	(57)	(83)	(98)	(42)	(47)	(130)	(161)	(43)	(49)	(65)	(68)
2. Agriculture, mining, const.	−147	−179	58	−145	1,970	954	−869	−252	1,214	1,040	619	534
	(292)	(289)	(281)	(330)	(232)	(264)	(331)	(408)	(138)	(158)	(196)	(207)
3. Manufacturing	3,179	2,198	971	440	3,545	2,945	1,014	1,345	2,945	2,666	2,112	1,578
	(163)	(161)	(115)	(135)	(90)	(102)	(249)	(307)	(123)	(142)	(98)	(103)
4. Transportation, etc.	2,850	1,784	2,936	2,291	4,760	3,584	2,825	1,742	3,846	2,724	4,926	3,918
	(149)	(147)	(191)	(224)	(112)	(127)	(260)	(320)	(140)	(160)	(116)	(123)
5. Wholesale trade	2,199	1,672	987	726	4,014	3,247	1,929	1,036	2,578	2,070	308	309
	(171)	(169)	(201)	(236)	(153)	(174)	(277)	(341)	(149)	(171)	(105)	(111)
6. Retail trade, not eating estab.	−327	−349	−1,145	−1,196	−376	−470	−128	−168	−290	−313	−237	−215
	(64)	(64)	(61)	(72)	(48)	(54)	(103)	(127)	(41)	(47)	(53)	(56)
7. Finance, ins., real estate	5,470	4,236	4,372	2,934	7,805	5,897	3,158	2,073	4,343	3,223	4,324	2,873
	(205)	(203)	(175)	(206)	(121)	(137)	(245)	(301)	(140)	(160)	(123)	(130)
8. Hotels, motels	113	386	−2,857	−1,046	1,240	1,136	−3,147	−1,279	−396	−90	−2,405	−915
	(115)	(113)	(150)	(177)	(146)	(166)	(272)	(335)	(147)	(169)	(100)	(106)
9. Temporary help firms	−1,842	−1,499	−3,218	−2,441	−2,922	−2,242	−2,495	−1,847	−1,847	−1,807	−2,102	−1,747
	(50)	(50)	(61)	(71)	(54)	(61)	(117)	(144)	(45)	(52)	(39)	(42)
10. Skilled nursing, inter. care	432	119	1,968	1,894	−686	−67	1,162	1,499	−751	−209	−30	263
	(161)	(160)	(114)	(134)	(77)	(88)	(340)	(419)	(131)	(150)	(74)	(78)
11. Other service	530	277	1163	741	1,673	994	1,208	589	623	481	832	434
	(44)	(44)	(37)	(44)	(34)	(39)	(68)	(84)	(27)	(31)	(32)	(34)

12. Public administration	6,075 (175)	4,269 (173)	9,142 (164)	8,330 (193)	7,293 (213)	5,460 (242)	−532 (127)	−295 (157)	14,204 (291)	12,808 (334)	8,859 (251)	8,220 (264)
13. Industry not ascertained	−364 (167)	−401 (166)			−1,368 (25)	−1,047 (29)			2361 (443)	1693 (508)		
Controls												
Minority, age, age^2	X		X		X		X		X		X	
Number children, quarter dummies	X	X	X	X	X	X	X	X	X	X	X	X
All person effects		X		X		X		X		X		X
R^2	0.063	0.597	0.085	0.520	0.070	0.648	0.053	0.748	0.064	0.585	0.085	0.520
Adjusted R^2	0.062	0.441	0.085	0.365	0.070	0.405	0.052	0.456	0.064	0.348	0.085	0.365

NOTE: Beta values are estimated impacts relative to the average industry. Standard errors are in parentheses.

Figure 5.2 Earnings Variation over Time



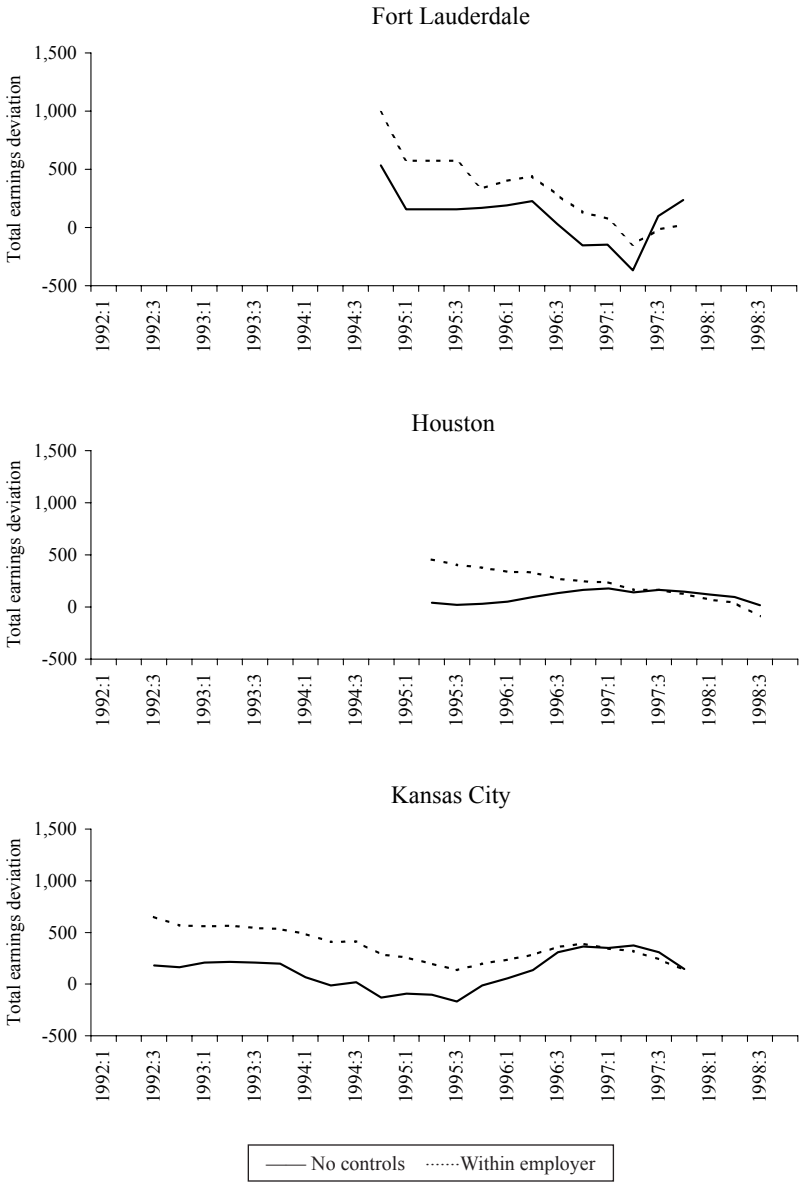
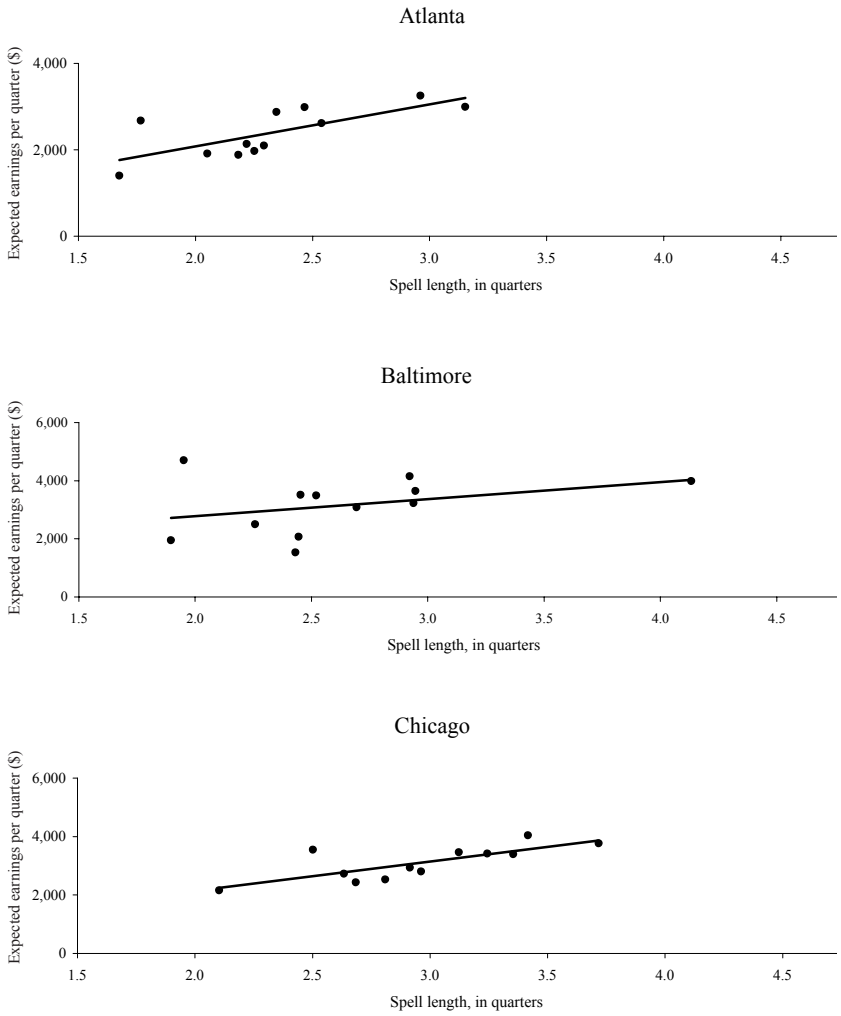
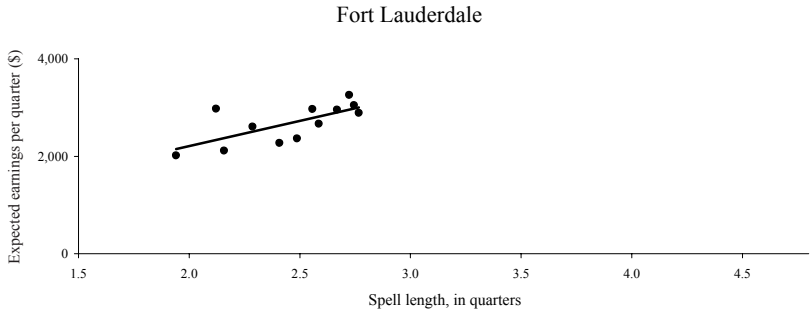
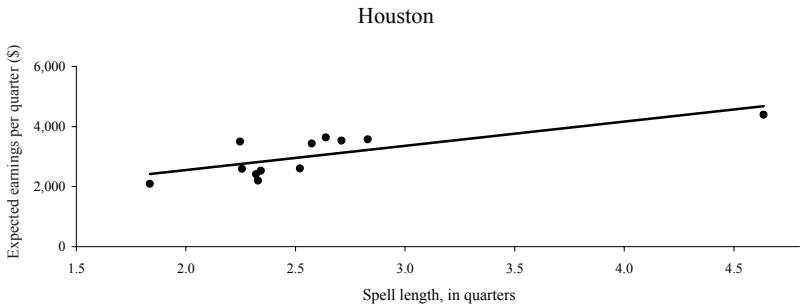
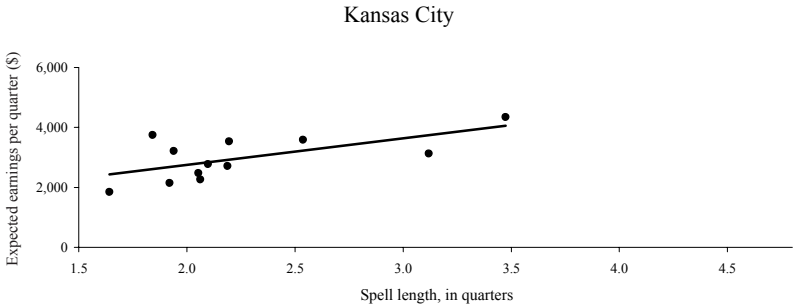

Fort Lauderdale
Houston
Kansas City
Total earnings deviation
1,500
1,000
500
0
-500
1992:1
1992:3
1993:1
1993:3
1994:1
1994:3
1995:1
1995:3
1996:1
1996:3
1997:1
1997:3
1998:1
1998:3
No controls
Within employer

Figure 5.3 Length of Job Spell and Quarterly Earnings for Major Industry Categories




Fort Lauderdale
Expected earnings per quarter ($)
4,000
2,000
0
1.5
2.0
2.5
3.0
3.5
4.0
4.5
Spell length, in quarters


Houston
Expected earnings per quarter ($)
6,000
4,000
2,000
0
1.5
2.0
2.5
3.0
3.5
4.0
4.5
Spell length, in quarters


Kansas City
Expected earnings per quarter ($)
6,000
4,000
2,000
0
1.5
2.0
2.5
3.0
3.5
4.0
4.5
Spell length, in quarters

there are important unmeasured individual characteristics that predict the stability of a job.

Perhaps the most important policy implication is that even in the face of large individual effects, the industry of the employer continues to play a substantial role. Three industry groups (public administration; finance, insurance, and real estate; and transportation, communication, electric, gas, and sanitary services) display the greatest job stability. Jobs in temporary help services and jobs in agriculture, mining, and construction are the least stable. Substantial portions of the estimated industry effects remain after controlling for person-specific effects as well as observed recipient characteristics that change over time. The analysis here provides strong evidence that getting the right job both in terms of industry and employer is critical in assuring job stability for welfare recipients.

Of course, our analyses that control fixed person effects obtain results based on individuals who began more than one job spell during the time they were receiving welfare. One may be hesitant to extend these results to individuals who have great difficulty obtaining any employment. On the other hand, by focusing on individuals who had multiple job spells while receiving welfare, we omit many of the most successful individuals, those who were able to obtain a stable job and immediately moved off welfare. Such selection—which reduces variation within the sample—should tend to reduce the estimated effect of all relevant variables. The strength of our findings is therefore of particular significance.

The analysis pertaining to changes over time in job stability shows that there are modest seasonal effects on job spell length, with job spells beginning in the fourth quarter shorter than others and those in the first quarter generally longer. We observe a decline in job stability over the period of our study in most sites. This decline is partly mitigated by a trend toward employment in firms offering more stable employment.

More generally, our results support the view that welfare recipients are responsive to opportunities in the environments they face. Although there are important differences that inhere in individual recipients that affect their labor market experiences, the industry and employer effects we have estimated suggest that if good jobs can be made available to them, recipients will benefit from them.

Notes

1. Since the proportion of job spells lasting more than eight quarters is small, we do not expect that our substantive results would be affected by including longer spells. Truncating spells to eight quarters allows us to compare spells beginning at different points in time without considering the impact of truncation.
2. Throughout the analysis, as in Chapter 4, spell length will be measured as a discrete variable indicating the number of quarters in which the individual received earnings from a particular employer. Since most individuals do not work the full quarter at the beginning and end of the spell, to obtain a continuous time estimate of the spell length would require a downward adjustment of slightly less than one quarter. Such an adjustment is applied where earnings per quarter are calculated; see the discussion of calculations for Figure 5.3.
3. All individual and job information is taken from records referring to the first quarter of the job spell. Number of children is permitted to differ across job spells for a given individual. Other personal characteristics are assumed constant across job spells. Although, in theory, educational attainment could differ across jobs if an individual obtained additional schooling between job spells, such differences are unusual. This is, in part, because of data limitations, as the education variable is infrequently updated after initial entry into welfare.
4. In Chicago, the North America Industrial Classification System (NAICS) is used.
5. This can be seen by examining how the slope of the quadratic function changes.
6. We use a fixed effects model, which controls for unobservable employer-specific effects by examining all job spells with the same employer. To the extent that job spells in a given firm tend to differ from those in other firms in ways not predicted by employee or employer characteristics, this approach attributes this to the impact of unmeasured employer characteristics.
7. See Kennedy (1986) for interpretation of dummy variable coefficients of this kind. Estimates of effects are normalized so that the weighted average of the coefficient estimates sum to zero, where the weight is the proportion of the sample in each industry.
8. The increase in explained variance is greater in Chicago. We used the NAICS code to construct codes equivalent to the two-digit SIC code, but in a third of all cases lack of correspondence forced us to group firms together in a missing category. Where the analysis at other sites used the four-digit SIC code, we used the original NAICS industry coding. Although it is not directly comparable to the SIC, it is of similar detail, with 898 separate categories, compared to approximately 600 SIC four-digit industries.
9. The long-term recipiency indicator is excluded; regressions including this measure show that results are not affected by its exclusion.
10. Bartik (1997) and Lane and Stevens (2001) examine impacts of job type on outcomes for welfare recipients. While neither analysis is directly comparable to ours, their results also suggest that industry effects may be important.

11. For each broad industry category, we have calculated the simple unweighted mean proportion across all six sites. Using the coefficients for a given site, we estimate how predicted spell length would change if the industry distribution at that site corresponded to this mean.
12. In Figures 5.1 and 5.2, the moving average for Fort Lauderdale has been included in the graph even where the average includes as few as two quarters because of the missing data for spells beginning in selected quarters.
13. If the chance of obtaining a job is uniform during the initial quarter and the chance of ending the job is uniform during the final quarter, this implies that, on average, an individual with a spell of recorded length of two or more quarters actually is on average employed for one quarter less than this discrete measure. For those who are recorded as working only one quarter, the assumption of continuous hazard implies an average actual length of one-third of a quarter. Since approximately half of spells end after one quarter (see Chapter 4), we subtract 0.834 from predicted spell length to obtain an estimate of the actual expected length measured in continuous units. The results are not sensitive to the details of this method.

6
Conclusions and Implications for Welfare and Beyond

The 1990s were a remarkable period in the United States in many respects, but nowhere more so than in the nation's large urban areas. The economy soared, and productivity posted significant gains. Employment grew at a remarkable pace, and unemployment dropped to levels not seen in decades. Welfare reforms at the federal, state, and local levels—bolstered by important contributing federal policy changes such as the EITC, Medicaid, and child care expansions—led to unprecedented declines in caseloads for cash assistance programs. Nationally, welfare caseloads fell to their lowest levels in three decades. Labor force participation rates for single mothers—never-married women as well as those who were separated or divorced—increased, approaching those of single women without children (Blank 2002). Even if the net result of all of these economic and policy changes failed to reduce income and earnings inequality (Marshall 2000), it was a truly remarkable era.

Our analysis has taken advantage of the availability of welfare and labor market data spanning most of the 1990s in six large urban areas. While the patterns we have presented for these areas may not be representative of all urban areas, they provide unique insights into welfare and work behaviors in a number of significant urban population centers that together account for 1 out of every 20 recipients in the nation. In important respects, they are a diverse set of cities, so we feel confident that those patterns that occur in all or most of our sites are common if not universal across urban areas. In addition, where we observe discrepant patterns, this provides an indication of how great variation across urban areas may be.

Our major conclusions tend to complement those from other welfare-to-work analyses that have been published recently, but they also provide intriguing nuances that shape our understanding of the existing body of research. We are able to identify recipient flows and patterns of employment in our six sites prior to and after welfare reform, so we

are able to make inferences about the impacts of reforms that go beyond existing studies. Furthermore, our analysis of job spells for both welfare and nonwelfare individuals provides unique information about the welfare-to-work experience and how it relates to work in low-wage jobs more generally. We believe our work leads to a number of major conclusions that policymakers and researchers should consider as the nation emerges from this period of dramatic reform and considers how to "fine tune" the welfare system. We should acknowledge as well that our conclusions are based on welfare recipients' experiences in one of the most robust economic expansions in decades, while policymakers are having to address welfare reauthorization in what some have referred to as a "jobless recovery." Their task is a challenging one.

This chapter draws on our analyses to answer broad questions about welfare and welfare reform in the United States. We begin by addressing how successful the welfare reforms of the 1990s were. First, we consider policy success in reducing the welfare rolls and, second, success in assuring that welfare recipients obtain employment and ultimately achieve self-sufficiency. We then turn to the more general question of whether welfare reform has succeeded in making those affected by the program—that is, recipients, potential recipients, and former recipients—better off. Even if many recipients are employed and receiving increased income, we may ask whether their gains are sufficient to compensate them for the burden of long hours of work.[1] Although we will reference others' findings on the impacts of welfare reform, our primary focus will be on the conclusions that follow from our results.

We next turn to a discussion of the policy implications of our findings. Welfare reform is an ongoing process at the federal, state, and, in some cases, local level. In fact, major elements of these reforms were implemented over several years in the 1990s and until the present. Can we identify any lessons about what policies will best achieve accepted goals? Tension continues to exist between the view that primary emphasis should be placed on getting a recipient into a job—any job—immediately, and the view that only training designed to build human capital is of real long-term value to recipients. Although our analysis does not allow us to estimate the efficacy of these alternative approaches, our results do suggest the limits and possibilities associated with them. We are able to provide insight into the value of helping recipients land "good jobs," addressing an issue of long-standing concern. Our find-

ings also suggest the degree to which policies that focus primarily on caseload reduction are supportive of the goal of self-sufficiency through employment, and under what circumstances such policies may serve to undermine it.

THE IMPACT OF WELFARE REFORM

Caseloads

Consistent with national trends, the caseload at each of our sites showed large declines both in the middle 1990s and following federal welfare reform. Observed patterns show both signs of substantial diversity across sites and remarkable consistency. Caseload declines in our sites vary from less than 50 percent to over 80 percent, but the pattern over time and the relative importance of welfare entry and exit rates in explaining caseload declines are broadly similar. At every site, the number of individuals entering welfare declined markedly, such that caseload declines would have been substantial even if exit rates had remained unchanged. However, exit rate increases also played an important role at every site, independent of the effects of demographic changes in the recipient population.

The marks of specific policies also are clear in our analysis. New requirements imposed on recipients in Atlanta led to a "spike" in welfare exits. In Fort Lauderdale, time limits clearly played an important role in increasing welfare exits, especially for long-term recipients. But many of the differences across sites are difficult to trace to specific policies. For example, in Houston, although the decline in entries was more important than at any other site and was substantially more important than the growth in exit rates in inducing observed caseload declines, we cannot link this to a particular policy change.[2]

Of course, any careful analysis of the incentive effects of welfare policy changes recognizes that flows both onto and off of welfare will be affected. In Fort Lauderdale, the decline in entries onto welfare would alone cause a larger decline in the caseload than declines observed at all sites but Houston. This decline in entries may be, at least partly, due to time limits. Under Florida's rules, those who left welfare because they exceeded the 24-month limit were not permitted to return over the next

two years. In fact, the proportion of those entering welfare in Fort Lauderdale who had received welfare in the prior two years declined precipitously in 1998, just when Florida's time limits would have become binding for those who had received aid continuously for two years. In addition to those directly affected by limits, those who wish to "bank" their remaining time on assistance may also be less likely to choose to accept aid (Grogger and Michalopoulos 2003).

Employment growth very likely played a role in reducing the caseloads, but the variation in economic conditions across sites is not the primary reason for differences in the caseload declines. For example, employment growth rates in Fort Lauderdale and Houston were not substantially greater than at all other sites, so this does not explain the greater caseload declines we observe at those sites. Perhaps more compelling than comparisons across our sites is the time pattern of caseload declines. Declines in rates of unemployment at our sites were generally quite modest in the period 1997–1999, yet caseloads at all sites declined at greater annual rates during that period than before.

Although we cannot trace out the specific roles of policy changes and economic factors in inducing caseload declines, our results are consistent with the view that welfare reforms induced substantial declines. State reforms in the 1990s prior to PRWORA display a host of common elements, and these were both supported and extended with the federal legislation, so the acceleration of caseload reductions is consistent with the importance of these factors (Blank 2002). We suspect that the greatest impacts are due to reforms that implemented various restrictions and requirements making welfare receipt less attractive; such changes would both reduce new applicants and increase exits from the rolls. We also believe that nationwide changes in attitudes, especially among workers in the social services, stressing the need for recipients to seek alternatives to government cash support, played an important role. While frontline welfare workers may have had great difficulty clearly communicating the specifics of various federal, state, and local reform provisions (Meyers, Glaser, and MacDonald 1998), it was very clear to all involved—workers and recipients alike—that the environment had changed dramatically in the 1990s, and that work was to be preferred over welfare.

Overall, our conclusions strongly support the consensus that the reforms were successful in reducing welfare caseloads. While other policy

reforms—especially increases in the EITC in the early 1990s—facilitated movements by individuals off of welfare, as did extraordinary and sustained economic growth, there is little question that, in the absence of welfare reforms, many more individuals would have continued to receive welfare payments.

Employment

Given explicit requirements under state waivers and TANF that recipients participate in training or paid employment, it is no surprise that employment rates of recipients increased over the period of reform. In addition, at each of our sites, we observe that welfare leavers are appreciably more likely to be employed in 1999 than in 1994. This suggests that the implicit goal of providing work as an alternative to welfare has been successful.

The timing of employment growth, however, is not supportive of the view that welfare reforms, defined narrowly, had a direct role. Evidence on caseloads, both at our sites and nationally, in conjunction with an analysis of state-level rules, suggests that reform was more intense in the period after TANF was implemented. If reforms operated largely by improving labor market opportunities for recipients, for example, through training and related programs, we would expect employment rates for leavers to grow particularly fast after federal reform. In fact, the growth in the employment rate is greater at all of our sites prior to 1997 than in the period 1997–1999. This experience is consistent with the view that the expansions in EITC, Medicaid, and child care that occurred earlier in the decade may have had a primary role in increasing employment of leavers by increasing the attractiveness of work.

Of course, we might expect that the economy would be of critical importance in fostering increased employment. As noted above, both nationally and at our sites, the unemployment rate declines were particularly precipitous beginning in the early to mid 1990s, and declines in unemployment rates were relatively modest at most sites after 1997. One exception is in Baltimore, where economic growth was somewhat less pronounced before 1998 but displayed an appreciable increase in 1998–1999. We observe an increase in employment for welfare leavers in Baltimore during this latter time period, suggestive of the role of the economy. Overall, the evidence is consistent with the view that eco-

nomic growth played a central role in causing the increase in employment of former recipients.

Still, it is clear that the economy is not the sole factor in explaining increased employment of welfare leavers. We observe dramatic improvements in employment levels of leavers in Chicago in the period 1996–1999, although declines in unemployment rates were only slightly greater than at most of the other sites. At three of our sites where the economy continued to be strong, we observe declines in leaver employment rates over the last two years of our analysis.

Our analysis of the dynamics of leaving welfare and obtaining employment provides no evidence that the basic relationship between work and welfare has shifted over the 1990s.[3] Throughout the period, those more likely to obtain employment are more likely to leave welfare, with little change in this relationship, despite the substantial increase in exit rates.

In the face of attempts to increase the employment focus of the welfare system, we might also expect important changes in the types of jobs individuals obtain. On the one hand, emphasis on "work-first" activities, in conjunction with increased pressure for employment, might well create incentives for individuals to obtain short-term employment, reducing movement into more stable employment. On the other hand, job readiness and training programs designed to increase recipients' long-term economic self-sufficiency might well help recipients obtain better jobs. Our results do not suggest substantial impacts of either kind: On net, welfare reform had little effect on the types of jobs individuals obtain. It would appear that welfare reform did little or nothing to change the underlying structure of the low-skilled, low-wage market faced by former recipients.

Throughout the period of our study, we observe that welfare recipients obtain jobs with very short expected durations. Only half of the jobs obtained by recipients last beyond the quarter in which they were obtained, and average quarterly earnings in these jobs are low. At several of the sites, we observe a decline in average employment duration over this period, but this seems to parallel declines observed for employees in the same firms who are not welfare recipients. More significantly, the declines in duration at our sites do not parallel declines in overall earnings on a job, suggesting that earnings improvements over the 1990s more than compensated for any declines in job stability.

Whatever changes occurred over the period, it is clear that there are few cases where recipients who leave jobs will usually be eligible for state unemployment insurance benefits.[4] It is worth recalling that the movement to reform welfare in recent decades has in part been a way of prodding recipients to join the economic mainstream and thus become eligible for first-tier safety net programs such as UI when times got tough (e.g., Murray 1984). Although analyses that focus on employment spells for former welfare recipients (e.g., Martinson 2000) suggest that many work continuously, our results show the flip side of this coin: The overwhelming majority of jobs obtained are short-term; few can be said to provide a source of economic stability.

Although we do not find evidence that welfare reform has forced recipients to accept jobs that are worse than those held by recipients in prior years, pressure for employment may be inducing recipients to seek jobs more aggressively. At every site, we observed increases in the proportion of recipients who had begun at least one new job in a given quarter. Such increases will occur if individuals obtain new jobs more quickly after leaving a prior job or if they are more likely to obtain a second job.

Are Individuals Better or Worse Off?

Supporters of welfare reform argued that the emphasis on employment would introduce, or reintroduce, recipients to the world of work, providing them with support and incentives to leave the welfare system. In the most optimistic picture, recipients would develop new work skills on the job, many eventually achieving self-sufficiency through employment, and former recipients would ultimately credit welfare reform with improving their lives. Opponents of welfare reform drew a sharply contrasting picture of low-skilled single parents forced onto the streets, competing in a labor market choked by a deluge of similarly desperate job seekers. In this environment, increasing numbers of individuals would face the prospect of extended unemployment, dependence on private and family charity as well as criminal activity, and, for some, homelessness or worse.

It should be clear that our results, consistent with other studies of welfare reform, allow us to unambiguously reject both of these polar views. Although we cannot provide a final answer of whether individu-

als are made better or worse off by reform, our results are consistent with a particularly simple view of how reform has influenced individuals' experiences. Overall, reform can be viewed as comprising two sets of changes: one imposing new constraints on recipients, and the other providing new resources supportive of work and self-sufficiency. The latter, sometimes referred to as "work supports," include training, child care and transportation subsidies, and related services.

The high rates of departure from the rolls and the steady or increasing employment rates, both for recipients and leavers, suggest that recipients are responsive to the combination of incentives provided by these reforms. On net, our evidence suggests that it is welfare reform's new constraints—such as requirements that people seek employment or engage in training, and limits on lifetime aid receipt—that are most important. The evidence for this view is based on two observations from our research. First, we find that caseloads have declined both because of an increase in the exit rates and because fewer individuals are entering welfare. If welfare reforms affected the caseload primarily by providing very attractive training or job opportunities for recipients, we would not expect such substantial declines in the numbers of individuals entering onto the rolls.

Second, at most of our sites, among those individuals who leave welfare and obtain a job in the following quarter, we observe an increase over the period 1996–1999 in the proportion that experiences a "support gap"; that is, those who exit welfare but do not obtain paid employment until some point in the following quarter. Such individuals very likely left welfare prior to finding employment. Although it may be legitimate to count these individuals as welfare-to-work successes, it is hard to argue that most were drawn off of welfare by attractive employment opportunities. Rather, it is more reasonable to assume that their exit from welfare reflected other factors, and, in the face of necessity, they responded by obtaining employment.

If this interpretation is correct—that is, if welfare reform has had its impact largely because it imposed new constraints on welfare recipients—we believe that individuals are not likely to have been made better off by reform. Since the option of exiting welfare was available to recipients prior to the latest reforms, this was a choice many judged to be inferior. In general, policy changes that operate by reducing the

attractiveness of one of the options—welfare receipt—will not yield a net benefit to single parents.

Whatever new constraints recipients face, our results imply that many are responding constructively, pursuing increased employment as their best chance in the new policy environment. They are securing employment at rates at least as high as leavers prior to reform. In most important respects, these former welfare recipients have done what was asked of them under welfare reform.

Welfare recipients have always faced a daunting set of barriers that limit their personal life chances. They generally suffer from low job skills, inadequate and unreliable child care, the burdens of family often embedded in a chaotic and disorganized household unit, family violence, and personal and psychological problems that limit their ability to cope in these difficult circumstances. Welfare reform certainly has not changed this, nor has it changed the nature of labor market demand for their services, which seldom provides earnings sufficient to pull a family out of poverty. But it has shown that such problems do not preclude moving substantial numbers of such individuals from welfare to work.

It is now typical for middle-class families with children to have both parents in the labor force. Whatever taxpayer support there once was for a program allowing poor single parents to withdraw from paid work has now evaporated. Welfare reforms represent an attempt to bring welfare in line with these general sentiments and mores, and they have certainly achieved that end.

POLICY IMPLICATIONS

What lessons does our study of urban welfare and work patterns provide about the efficacy of various kinds of reform? Variation across our sites gives us some indication of the impacts of alternative policies. Our analysis also provides us with information about the kinds of jobs welfare recipients obtain and the determinants of job stability. In this section, we discuss the implications of our findings for choices that policymakers face in their ongoing efforts to reauthorize U.S. welfare programs.

The Trade-Off between Caseload Reduction and Employment

From the inception of the latest round of debate on welfare reform in the early 1990s, one of the central questions has revolved around the potential trade-off between the goal of reducing caseloads and that of increasing recipient employment and assuring economic self-sufficiency. Those expressing caution argued that time limits and other restrictions would merely exacerbate the already-desperate conditions of the poorest single parents, while reform supporters argued that such restrictions would create incentives to push and pull recipients into the labor market and find their own paths to independence. When welfare reform is defined broadly, including the substantial expansions of EITC, Medicaid, and child care in the early part of the 1990s, we find strong support for the latter view.

In our examination of specific programs, however, we do see evidence of trade-offs between case reduction and movement into employment. This is clear for Atlanta, where regulations requiring recipients to sign self-sufficiency pacts in 1998 led to both increases in exit rates and, very likely, declines in employment rates. Similarly, in Fort Lauderdale, when the two-year time limit began to bind, there were declines in employment rates for leavers, along with continuing high exit rates. Finally, in Houston, although we cannot identify a specific policy that is responsible, the very large caseload declines are associated with unchanging employment rates among leavers, in contrast to the upward trend at other sites.

These patterns confirm that ever-more-stringent welfare policies will not automatically lead to higher levels of employment. Despite the overall positive record, some state policies have had the effect of reducing caseloads at the cost of recipient employment. In contemplating further policy changes, all states will need to recognize that the remaining recipients may be limited in their ability to adapt to the labor market and the new welfare regime. Time limits, or other reforms that make welfare receipt less attractive, may leave an increasing number of poor single parents with options that are dramatically worse.

Of course, reforms providing services and continued support to recipients, allowing them to expand or enhance their job skills or subsidizing their job search and work activities, provide an alternative approach to reducing caseloads over the longer term. We turn next to a discussion

of how our results bear on the question of what kinds of programs and strategies support successful transitions from welfare to work.

Work-First versus Human Capital Development

At each of our sites, recipients are required to participate in programs designed to provide skills to aid in obtaining employment that will ultimately allow them to leave welfare and obtain economic self-sufficiency. The underlying philosophy of these programs varies from those that are focused on merely getting people into any job ("work-first") as soon as possible to those that attempt to first augment their job skills through training. Although there are differences in focus across our sites, none of the programs provide long-term job skills training to a substantial share of welfare recipients. A recent review of evaluations of training programs for welfare recipients and others argues that serious efforts to improve human capital through training have not really been attempted: participants in HCD programs evaluated as part of the National Evaluation of Welfare-to-Work Strategies (NEWWS) were mainly provided basic and adult education services (40 percent), rather than occupational skill training (28 percent) (King 2004). Further, NEWWS sites were only able to boost participation in vocational training by 5 percentage points, while adult education increased by fully 20 percentage points. Supporters argue that skill development programs are essential to improving the long-term labor market prospects of current welfare recipients (for example, in addition to King 2004, see Hotz, Imbens, and Klerman 2000; Krueger 2004; and Martinson and Strawn 2002).

Our results provide some evidence in support of this claim. We find that the jobs obtained by welfare recipients are appreciably less stable and provide lower pay than jobs obtained at the same time by others with the same employer. Of course, such differences reflect the fact that welfare recipients may differ on average from other employees on a variety of personal characteristics, including education, gender, and family structure. However, we find that even when we compare welfare recipients with one another, after controlling for characteristics we can observe, unmeasured personal characteristics have a large effect on job stability and earnings. Two welfare recipients with the same external character-

istics who obtain a job with the same employer may differ dramatically in their expected success on the job.

These results suggest that there is substantial room for benefits from any program that can succeed in building individual job skills. If long-term training can, in fact, augment individual job skills, in effect making the least successful recipients more like those who are successful, such training will provide generous returns, possibly in the range of 10 percent or more (Krueger 2004). While our work opens up the possibility of such benefits, it provides no indication of whether programs are in fact successful. Our reading of the literature (Barnow and King 2000; King 2004) is that some programs yield modest and continuing returns, while others have great promise, but results are hardly definitive. Our own results are also consistent with the alternative view that stable—and largely unalterable—personal characteristics play the primary role in determining who among welfare recipients will be successful in the labor market.

Does this mean that "work-first" programs do not work? Although our results indicate clear limits on their efficacy, there remains much room for such programs to affect recipients' success in the labor market, certainly in the near term. We turn next to a discussion about the kinds of directed job search that may be most valuable to recipients.

The Value of a "Good" Job

One of the guiding principles of work-first programs is that immediate paid employment—almost any paid employment—should be encouraged. Supporters argue that recipients without job experience will learn the everyday work skills that enable them to continue in employment, and recipients with prior job experience will avoid unemployment and the personal debilitation and stigmatization that it often entails. In fact, recent evaluations indicate that near-term (i.e., 1–3 years) gains in employment and earnings from such programs are significant and substantial (Hamilton 2002; Hotz, Imbens, and Klerman 2000). This view is often stated as "Get a job, any job," or sometimes, "Get a job, get a better job, get a career."

However, our results suggest that, in at least some cases, recipients may benefit by waiting for "better" job offers. Even after controlling for all personal characteristics (using a person fixed-effects model), we

found that jobs in certain industries offered higher pay and more stable employment. Furthermore, high pay and stability tend to go together, so it is seldom necessary to sacrifice one to get the other. While this finding may accord with common sense, our results are unique in confirming the powerful effect of type of job in the welfare environment. Previous work has seldom controlled for unmeasured differences across people, nor has there been work focusing on the experiences of welfare recipients.

More generally, programs that focus on helping recipients obtain those jobs with the greatest expected pay and stability may have substantial benefits. Such benefits are particularly attractive, because they may occur without costly investment in new job skills. Of course, such programs should not be viewed as a panacea. Our analysis refers only to the returns from a single job, not to the future pattern of employment. It appears likely that even a short-term job is better than an extended period without any job.[5] A program that increases placements in better jobs without reducing the likelihood of employment would almost certainly benefit workers. This is also consistent with the results from two key studies. The NEWWS evaluation found larger longer-term employment and earnings impacts from its Portland site, a hybrid employment- and training-oriented program that encouraged participants to sort job prospects carefully and select those offering better pay, benefits, and chances for advancement. And, King et al. (2000) found that more stable employment and higher earnings resulted from programs that stressed skills training and more selective job search strategies in Illinois and Texas.

CONCLUDING OBSERVATIONS

Our results support the growing consensus that American welfare reform has met many of the primary goals of its supporters, and it has avoided the dire predictions of its severest critics. We have made major strides in terms of reducing welfare caseloads and promoting employment among former and potential welfare recipients.

It must be recognized that this success occurred against the backdrop of an extraordinarily strong economy and earlier federal policy changes (e.g., EITC and Medicaid expansions), which greatly facili-

tated increasing labor market involvement for welfare recipients. Future reforms may not face such a supportive environment, and policy choices may be more likely to face difficult trade-offs between the goals of caseload reduction and employment.[6]

Our work also highlights the poor earnings and unstable employment of former welfare recipients and others at the bottom of the earnings distribution in six large urban areas. The success of welfare reform has resulted in most former recipients joining the ranks of the working poor and introduced them to the daily stresses that all single parents must face when juggling work and family demands. The next policy frontier we face is addressing the enormous challenges that collectively confront the working poor. Policies that deal with work and family issues have a far larger audience and presumably far greater public appeal than those that are focused solely on recipients of cash assistance. Policymakers at the national, state, and local levels will have to join forces in meeting these challenges, as will employers as a group.

Notes

1. This point is emphasized by Boushey and Gunderson (2001) and Edin and Lein (1997).
2. For an attempt to identify causal impacts of policy choices in Texas, see Schexnayder's (2003) report, which is based on random assignment of recipients to alternative rules under the Achieving Change for Texans waiver demonstration.
3. This view is echoed in Blank (2002) and Schexnayder et al. (2002).
4. Evidence from TANF leaver studies suggests that a larger share of former welfare recipients is eligible for UI benefits in the postreform era, but the proportion remains small. Rangarajan, Razafindrakoto, and Corson (2002), reporting results from New Jersey during the late 1990s when the state's economy was still booming, found that between three-fifths and three-quarters of TANF leavers would potentially be monetarily eligible for UI, but that as many as 60 percent of these would be ineligible for nonmonetary reasons (e.g., voluntary quits).
5. Heinrich, Mueser, and Troske (forthcoming) show that welfare recipients in temporary help services jobs, those that we find to be the least stable here, do appreciably better in terms of ultimate employment than do recipients without jobs, and only slightly less well than the average employed recipient.
6. State fiscal difficulties are considered by Chernick and Reschovsky (2002).

Appendix A
Data Sources

Our data are for AFDC/TANF cases in each central county in six metropolitan areas: Fulton County, Georgia (Atlanta); Baltimore City, Maryland (county equivalent unit); Cook County, Illinois (Chicago); Broward County, Florida (Fort Lauderdale); Harris County, Texas (Houston); and Jackson County, Missouri (Kansas City). In each case, the county contains all or almost all of the central city population. With the exception of Baltimore, the county also contains substantial population outside the central city, although a large share of the county's welfare recipients resides in the central city. The proportion of the metropolitan population included in the central county varies from less than one-fifth (for Fulton County in the Atlanta metropolitan area) to nearly three-quarters (for Harris County in the Houston metropolitan area). Although we follow the convention of referencing each site by the name of its central city, all information on welfare participation applies to the central county, unless explicitly noted otherwise.

We have limited our focus to families headed by females aged 18 but less than 65 years who received AFDC-Basic or TANF cash payments. These selection criteria omit all men as well as recipients who received aid as part of the AFDC-Unemployed Parent program or its TANF successor. We retain cases where the head has been coded as "inactive." In contrast to other analyses, this approach includes cases that have been sanctioned and cases where the case head is not the parent of the dependent children ("child only" cases).

The unit of analysis can be viewed as the family or as the case head, who is the mother or female payee. We omit those who received only noncash benefits even if they were reported as AFDC or TANF recipients. We have aggregated monthly benefit payments to quarterly totals, so that anyone who received payments in any month in a quarter is counted as receiving payments in that quarter. This allows greater comparability with quarterly earnings data and smoothes over much of the administrative "churning" inherent in welfare data.

In order to examine the employment experiences of welfare recipients, we obtained quarterly total earnings for all individuals in jobs covered by unemployment insurance (UI) in these states, matching these to the records of AFDC/TANF recipients. For the Kansas City analysis, both Missouri and Kansas earnings data were accessed and used. The vast majority of employment in each of the states is covered by these data, although illegal employment, self-employment, and several classes of nonprofit and federal employment are not

covered.[1] The files also fail to identify employment for individuals who left the state. Out-of-state employment for residents in our sites is not expected to be significant, except in Kansas City, where Jackson County residents often have jobs across the border in Kansas.

Because administrative practices regarding the archiving of data differ across states, the period of coverage for our sites varies somewhat. AFCD/TANF data for Baltimore and Kansas City are available beginning in 1990, Atlanta and Houston in 1992, Fort Lauderdale in 1993, and Chicago in 1995. In all sites, welfare data extend through the end of calendar year 1999. However, where we have used employment data in our analysis, in many cases necessary data do not extend to the end of 1999, as noted in the text.

As an indicator of the general economic climate in the region, we use the unemployment rate for the primary metropolitan area. Our decision to use the metropolitan area rather than the county stems from our concern that our measure of the local economy not be influenced by welfare policy. Whereas unemployment for a single county might be influenced by an influx of former welfare recipients and by intra-metropolitan mobility and local demographic changes, such effects will be much smaller at the level of the metropolitan area.

The measures underlying caseload and employment are summarized in Table A.1. As noted above, those women receiving any welfare cash payments in a quarter are viewed as recipients. Given monthly turnover, the caseload measured this way for a given quarter will be slightly greater than the highest monthly caseload. In examining movements onto and off of welfare, we define an individual as leaving welfare in a given quarter if she received welfare during that quarter but not during the following quarter. Similarly, an individual is defined as entering welfare if she was not receiving welfare in that quarter but was receiving welfare in the following quarter. This structure implies that caseload can be identified as changing according to the equation of motion:

$$\text{Caseload}(t + 1) = \text{Caseload}(t) - \text{Exits}(t) + \text{Entries}(t),$$

where Exits(t) and Entries(t) are defined by comparison between quarter t and quarter $t + 1$.[2] The rate of exit is calculated as Exits(t) / Caseload(t), so that it indicates the chance that an individual receiving welfare in quarter t receives no welfare in the following quarter. In order to examine exit rates for long-term recipients, we define the exit rate analogously for all individuals who had received welfare payments continuously for eight quarters prior to the quarter in question.

The extent of welfare recidivism is determined by the numbers of those entering welfare (i.e., who received welfare in a given quarter but not the prior

quarter) who had also received welfare payments at any point in the prior eight quarters, divided by the total entering welfare in the quarter in question. Our determination of prior welfare experience is limited to the same county, since we did not identify those who had received welfare elsewhere.

Our measure of the rate of employment for welfare leavers is the proportion receiving earnings in quarter $t + 1$ among those who received welfare payments in quarter t but not in quarter $t + 1$. As indicated above, our measure of earnings is limited to employment within the state (or two adjoining states in the case of Kansas City) that is reported to the state Unemployment Insurance system. This measure includes both individuals who obtained jobs prior to or immediately after leaving welfare, as well as some individuals who left welfare but found a job only after a period of unemployment, perhaps as long as five months.

Table A.1 also provides definitions for welfare exit rate for those who are employed, and our measure of the proportion of employed leavers who likely experienced a "support gap."

Table A.1 Summary of Measures of Welfare Dynamics

Caseload in quarter *t* is defined as the total number of families (or case heads) receiving AFDC/TANF payments and fitting our selection criteria at any point during the quarter.

Exits in quarter *t* is defined as the number of case heads who received payments in quarter *t* but not in *t* + 1.

Entries for quarter *t* is defined as the number of case heads who received welfare payments in quarter *t* + 1 but not in *t*.

Exit rate is the number of exits at quarter *t* divided by the caseload in quarter *t*.

Exit rate for long-term recipients is defined as above but applies to individuals who had been receiving welfare payments in at least eight quarters continuously prior to quarter *t*.

Employment rates of welfare leavers is the number of individuals leaving welfare in quarter *t* and receiving earnings during quarter *t* + 1 divided by the number of individuals leaving welfare in *t*.

Welfare exit rates for employed recipients is the number of individuals leaving welfare in quarter *t* and receiving earnings in quarter *t* + 1 divided by the number receiving welfare in quarter *t* and receiving earnings in quarter *t* + 1.

Proportion with support gap is the proportion not employed in *t* among those employed in *t* + 1 who exited welfare in *t*.

Notes

1. For example, program officials estimate that UI coverage exceeds 98 percent of state wage and salary employment in Texas (King and Schexnayder 1998). Kornfeld and Bloom (1999) find that using wage record data rather than survey information does not impose serious biases on estimates of program effects.
2. This labeling convention implies that those entries indexed by *t* first appear in the caseload at *t* + 1, whereas exits indexed by *t* last appear in the caseload at *t*.

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The Authors

Christopher T. King is director of the Ray Marshall Center for the Study of Human Resources and a lecturer at The University of Texas at Austin's Lyndon B. Johnson School of Public Affairs, where he holds the Mike Hogg Professorship in Urban Management. He has written widely on education, workforce, and social policy, including *Improving the Odds: Increasing the Effectiveness of Publicly Funded Training* (Urban Institute Press 2000, with Burt S. Barnow) and forthcoming reports on the implementation of the Workforce Investment Act and the impacts of workforce participation on employment and earnings. He has a B.A. in economics from The University of Texas at Austin and both an M.A. and Ph.D. in economics from Michigan State University.

Peter Mueser is an associate professor of economics at the University of Missouri, where he has taught since 1985. He received his Ph.D. in sociology from the University of Chicago. His research focuses on poverty, welfare, job training programs, and migration. His work is published in *The Review of Economics and Statistics, The Journal of Regional Science, The American Journal of Political Science, The Journal of Urban Economics*, and other social science journals.

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The W.E. Upjohn Institute for Employment Research is a nonprofit research organization devoted to finding and promoting solutions to employment-related problems at the national, state, and local levels. It is an activity of the W.E. Upjohn Unemployment Trustee Corporation, which was established in 1932 to administer a fund set aside by the late Dr. W.E. Upjohn, founder of The Upjohn Company, to seek ways to counteract the loss of employment income during economic downturns.

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